Demography (2017) 54:1007–1028
DOI 10.1007/s13524-017-0566-2

New Evidence Against a Causal Marriage Wage Premium

Alexandra Killewald[1] · **Ian Lundberg**[2]

Published online: 22 March 2017

Abstract Recent research has shown that men's wages rise more rapidly than expected prior to marriage, but interpretations diverge on whether this indicates selection or a causal effect of anticipating marriage. We seek to adjudicate this debate by bringing together literatures on (1) the male marriage wage premium; (2) selection into marriage based on men's economic circumstances; and (3) the transition to adulthood, during which both union formation and unusually rapid improvements in work outcomes often occur. Using data from the National Longitudinal Survey of Youth 1979, we evaluate these perspectives. We show that wage declines predate rather than follow divorce, indicating no evidence that staying married benefits men's wages. We find that older grooms experience no unusual wage patterns at marriage, suggesting that the observed marriage premium may simply reflect co-occurrence with the transition to adulthood for younger grooms. We show that men entering shotgun marriages experience similar premarital wage gains as other grooms, casting doubt on the claim that anticipation of marriage drives wage increases. We conclude that the observed wage patterns are most consistent with men marrying when their wages are already rising more rapidly than expected and divorcing when their wages are already falling, with no additional causal effect of marriage on wages.

Keywords Marriage · Divorce · Wages · Transition to adulthood · Panel data models

Electronic supplementary material The online version of this article (doi:10.1007/s13524-017-0566-2) contains supplementary material, which is available to authorized users.

✉ Alexandra Killewald
killewald@fas.harvard.edu

[1] Department of Sociology, Harvard University, Cambridge, MA 02138, USA

[2] Department of Sociology and Office of Population Research, Princeton University, Princeton, NJ 08544, USA

Introduction

The benefits of marriage for men's wages are of long-standing interest to both economists and sociologists (Cheng 2016; Chun and Lee 2001; Cohen 2002; Cornwell and Rupert 1997; Dougherty 2006; Ginther and Zavodny 2001; Gray 1997; Hersch and Stratton 2000; Killewald and Gough 2013; Korenman and Neumark 1991; Loh 1996). Traditional fixed-effects models typically estimate that marriage increases men's wages by about 5 % to 7 % (Cornwell and Rupert 1997; Dougherty 2006; Killewald 2013; Killewald and Gough 2013).

Taking a more nuanced approach, Dougherty (2006) and Cheng (2016) tracked men's wages in the years leading up to marriage and through subsequent marital durations. Both found that men's wages are already rising more rapidly than expected several years prior to marriage, and both researchers argued that when this premarital increase is included as part of the marriage premium, the long-term marriage wage premium is approximately 20 %—much higher than estimated by traditional fixed-effects models. The interpretation of this descriptive pattern, however, is ambiguous. Both Cheng and Dougherty acknowledged three possible sources of the positive association between marriage and wages for men: (1) a causal effect of marriage on wages, (2) a causal effect of wages on marriage, or (3) a spurious association between wages and marriage due to a latent maturation process. Furthermore, Cheng and Dougherty used the same data set—the National Longitudinal Survey of Youth 1979 (NLSY79)—to estimate their models. Despite the similarity of theoretical framing, data, and research design, however, the authors arrived at different conclusions. Dougherty (2006) argued that because the increase in wages associated with marriage begins five or more years prior to marriage, household specialization is unlikely to be responsible for the gains. If specialization is the only possible mechanism for a causal effect of marriage on wages, Dougherty's results suggest that marriage does not affect wages. However, marriage may affect wages through pathways other than specialization, such as by changing men's priorities and preferences, and these effects may begin prior to the marriage date. Cheng (2016) found that men's wages begin to increase as early as four years before marriage, but she concluded that marriage has a causal effect on men's wages, accelerating wage growth.

We take as a starting point the finding from Dougherty and Cheng that men's wages begin to increase more rapidly than expected several years before marriage, and we develop new tests to evaluate whether the pattern of wage growth in the years surrounding marriage is more consistent with a causal effect of marriage that predates marriage or with the alternative reverse causality or latent maturation perspectives. Using the same data set as Dougherty and Cheng (NLSY79), we begin by replicating their findings of unexplained premarital wage increases. Next, we show that men who marry after age 26 experience no premarital increase in wages and no wage premium following marriage. This finding is consistent with the hypothesis that marriage has no causal effect on men's wages and that the premarital wage gains observed for young husbands instead are due to spurious rapid growth in wages in early adulthood, whereas men who marry later have largely completed this period well prior to their marriage and therefore experience no premarital wage increase. We also compare the experiences of men whose marriage is followed by a birth within seven months—"shotgun marriages"—to those who do not have a birth shortly following their marriage. If the

premarital rise in wages is due to anticipating marriage, men entering shotgun marriages should not experience this rise given that their marriage is less anticipated. Instead, we find similar patterns for both groups.

We also estimate the wage benefits of staying married. Prior research has suggested that at least some of the marriage premium dissipates following divorce (Killewald 2013; Korenman and Neumark 1991). However, we find that wage declines predate divorce. These results are consistent with reverse causality and with prior research finding that job loss or lack of full-time employment increases men's risk of divorce (Charles and Stephens 2004; Killewald 2016).

We argue that our results are more consistent with the hypothesis that marriage, including its anticipation, has no effect on men's wages. These findings call for situating transitions to marriage within the more general life stage of the transition to adulthood and recognizing that divorce may be a consequence rather than cause of men's falling wages.

Background

Married men experience wage advantages compared with both never-married and divorced men (Cheng 2016; Chun and Lee 2001; Cohen 2002; Cornwell and Rupert 1997; Ginther and Zavodny 2001; Hersch and Stratton 2000; Killewald and Gough 2013; Korenman and Neumark 1991), with some evidence of marriage premia increasing with marital duration (and the reverse for divorce) (Cheng 2016; Ginther and Zavodny 2001; Korenman and Neumark 1991).

In this section, we first describe why marriage might increase men's wages and discuss what this means for likely effects of staying married compared with divorce and for changes in wages in advance of the marriage date. We then discuss the possibility that the association between marriage and wages is driven by selection rather than a causal effect of marriage.

Why Might Marriage Affect Men's Wages?

Specialization—the most prominent theory to explain a causal male marriage wage premium—posits that wives' contributions to unpaid labor allow husbands to increase effort in paid labor (Becker 1991), increasing husbands' wages relative to single men who do not benefit from the sexual division of labor. Consistent with this perspective, marriage is associated with less time in household labor for men, particularly in female-typed tasks (Gupta 1999; Hersch and Stratton 2000). Cohabiting men might also experience wage gains from specialization compared with men living singly; but for men living singly, no premarital increase in wages is predicted. Because divorce is associated with increases in men's housework time compared with remaining married (Gupta 1999; Hersch and Stratton 2000), specialization also predicts a decline in men's wages following divorce.

Employer discrimination may also cause higher wages for married men, especially if marriage generates biased perceptions by employers of men's stability or competence. Although this perspective is difficult to test empirically, it predicts no premarital marriage premium and a likely erosion of the premium following divorce.

Cheng (2016) and Dougherty (2006) documented that men's wages begin to increase more rapidly than expected several years before marriage, with no evidence that the marriage date itself immediately increases wages or accelerates wage growth. These findings cast doubt on the specialization and discrimination perspectives for the male marriage premium because neither can explain the premarital wage increase and both suggest that the formation of the marital union itself should have wage consequences. Based on his findings, Dougherty (2006) argued against attributing the male marriage premium to specialization. Other research has similarly rejected the specialization explanation. For example, Killewald and Gough (2013) used the fact that married women, too, experience a wage premium to argue against the specialization explanation for married men's higher wages. More directly, Hersch and Stratton (2000) and Pollmann-Schult (2011) showed that men's time spent in housework does not substantially mediate the male marriage premium. Given these results, we assume that if marriage does affect men's wages, it does not do so primarily through household specialization or discrimination.

However, the male marriage premium may arise for other reasons. Because wage-earning and financial providership are normative for husbands (Nock 1998), marriage may motivate men to invest more in wage-earning. Following divorce, motivation for wage-earning may decline, thus reducing wages. Cheng (2016:32) described this as the "motivation theory" of the male marriage premium. Consistent with this perspective, pay is ranked a more important job trait by married than unmarried men (Gorman 2000). This perspective, unlike specialization or discrimination, allows that men's wages might increase prior to marriage "as grooms-to-be shrug off bachelor habits and begin to assume the outlook and priorities of married men" (Waite and Gallagher 2000:100). Killewald and Gough (2013), like Dougherty, focused on ruling out the specialization hypothesis, finding that both men and women experience higher wages in the year immediately prior to marriage than in other premarriage years. Although they interpreted their results primarily as indicative of a causal marriage premium, they noted, "These results suggest that some of the returns to marriage may not be due to the marital union itself, but instead to the benefits of romantic partnerships more generally, or to time-varying selection into unions following wage increases" (p. 493).

Thus, for scholars arguing against the specialization explanation for the male marriage premium, the premarital increase in wages is treated as either unproblematic or even further supportive evidence. These analyses, however, do not clarify whether there is a causal marriage premium for reasons other than household specialization, such as changing attitudes and motivation. When the potential mechanisms for the effect of marriage on wages are expanded beyond specialization, a premarital increase in wages (such as that found by Dougherty, Cheng, and Killewald and Gough) no longer rules out a causal marriage wage premium.

Petersen et al. (2011), using a sample of Norwegian men, found that to-be-married men's wages already exceed those of their peers who never marry and increase little at the transition to marriage, consistent with selection or with preemptive action by men who will ultimately marry. They observed, "What is clear . . . is that the premia to marriage and parenthood occur even before entrance into marriage and having children, and that these premia on balance are stronger than the premia for actually entering into marriage and fatherhood when using the results from within-individual changes in wages. The empirical fact is solid. How this is interpreted, however, is still open" (p. 302). Our article aims to provide evidence on the appropriate interpretation.

Selection Into Marriage

Although premarital wage gains may reflect causal anticipation, an alternative interpretation is that the marriage premium arises due to selection of men into marriage on the basis of unobserved traits that are also positively associated with wages (Cheng 2016; Dougherty 2006). Fixed-effects or first-difference models, commonly used to estimate the marriage premium, alleviate bias of this kind only if the unobserved characteristics are time-invariant rather than time-varying. For example, men's employment and earnings are positively associated with subsequent marriage (Oppenheimer 2003; Oppenheimer et al. 1997; Sweeney 2002; Xie et al. 2003). Given that unmarried couples discuss postponing marriage until they have achieved financial goals (Edin and Kefalas 2005; Smock et al. 2005), a premarital increase in men's wages is equally consistent with men's wages affecting the transition to marriage as with an anticipatory effect of marriage on men's wages. A similar problem may arise for divorce given that job loss and lack of full-time employment are associated with heightened risk of divorce (Charles and Stephens 2004; Killewald 2016). Thus, reverse causality poses a substantial threat to estimates of the effect of marriage on wages.

An association between marriage and wages may also be purely spurious, resulting from a heterogeneous maturation process (Cheng 2016; Dougherty 2006). Marriage typically occurs during young adulthood, a demographically dense time of high rates of both union formation and work instability and growth (Rindfuss 1991; Shanahan 2000). Men may experience several years of rapid changes in both work and family formation because transitions to partnership and steady work are likely to be gradual or blurred, taking place over a prolonged period rather than crisply at a particular moment in time (Rindfuss 1991). These transitions happen in different orders and at different ages for different individuals (Hogan and Astone 1986; Rindfuss 1991; Shanahan 2000), in part because some individuals experience greater challenges navigating the transition to adulthood than others, particularly in employment (Oppenheimer et al. 1997). If blurred transitions to marriage and careers overlap in the transition to adulthood, men's wages will begin to rise more rapidly than usual in the years prior to marriage but not because of a causal effect of marriage anticipation on wages. Because the transition to adulthood is individual-specific in timing and duration, even flexible controls for age-wage associations will not fully net out this pattern.

Although there is no clear age at which the transition to adulthood ends, age 30 may be used as an approximation (Rindfuss 1991). In our analytic sample, men are age 32, on average, at time of divorce, compared with age 25 at marriage; thus, we expect that the association between divorce and wages will not suffer as much from this spurious association because most men approaching divorce have already completed the transition to adulthood. Men marrying later should also be less susceptible to this source of bias.

Plan for Analyses

As described earlier, both Cheng and Dougherty acknowledged the possibility that the male marriage premium may be driven by a causal effect, reverse causality, or spurious maturation processes. Although both researchers observed increases in men's wages prior to marriage, Dougherty interpreted this pattern as evidence against the male

marriage premium being driven by household specialization, implicitly arguing against a causal effect of marriage on wages; Cheng argued that marriage exerts a long-term effect on men's wages, although the process begins before the marriage date. Our analyses begin by replicating the findings of Dougherty and Cheng. We then perform additional analyses that seek to adjudicate between these interpretations. First, we examine trends in wages prior to marriage for men marrying at different ages. If the premarriage trends are due to the transition to adulthood, we hypothesize that the ramp-up will be greater for men marrying earlier, while men marrying later will have already completed their period of rapid wage gains, experiencing no additional gains just prior to marriage. If the premarital ramp-up in wages is due to anticipation or reverse causality, we expect that it will be shared by all marrying men, regardless of their age at marriage. Of course, marriage may induce greater premarital motivation for wages among younger grooms (causal anticipation); or perhaps younger grooms select more strongly into marriage on the basis of wage growth (reverse causality), possibly because their premarriage earnings are below a normative threshold. However, the transition to adulthood is the most parsimonious theory that would predict a stronger premarriage ramp-up among those who marry young.

Second, we take advantage of a group of marriages that follow a different selection process: shotgun marriages (Ginther and Zavodny 2001). We argue that marriages following a premarital conception are less anticipated than other marriages. If the premarital rise in wages is due to either a causal effect of marriage anticipation on wages or selection into marriage following wage gains, the premarital wage increase for men forming shotgun marriages should be smaller than for other husbands because shotgun marriages are, on average, less anticipated and deliberately timed. Similar wage gains for both groups are more consistent with a spurious association.

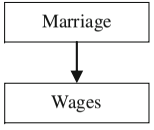
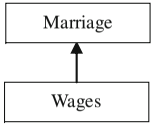
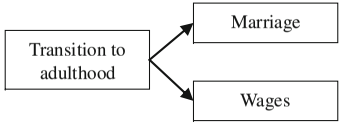
Separately, we examine men's wages in the years surrounding divorce (or permanent separation). We expect that if marriage has a causal effect on wages, it will fade following divorce, leading to declining wages. If divorce is prompted by declines in economic resources—the reverse causality perspective—we expect wage declines that predate divorce rather than follow it. Finally, because men are older at divorce than marriage, the transition to adulthood perspective suggests stability in wages in the years surrounding divorce given that most men are no longer experiencing rapid wage growth. Table 1 summarizes these empirical tests and the three theories described earlier.

Data and Methods

Our analyses use the 1979–2012 waves of the National Longitudinal Survey of Youth 1979 (NLSY79) (Bureau of Labor Statistics 2016a). The NLSY79 is a nationally representative panel study that follows individuals from 1979, when they were aged 14–22, through the present. We include observations from men's entry into the adult labor force through the end of data collection at ages 47–56. We define labor force entry as the first year when the respondent was not enrolled in full-time school, provided that he remained out of full-time school in the next interview. We exclude observations prior to labor force entry,[1] prior to age 18, and when the respondent was

[1] This restriction excludes 78 men who never entered the labor force.

Table 1 Theories relating marriage to wages

	Causal Marriage Premium: Anticipation (1)	Reverse Causality: Wages Affect Marriage (2)	Spurious Association: Transition to Adulthood (3)
Theory			
	Men's work behavior changes due to marriage, including in anticipation of taking on the husband role.	Men delay marriage until financial goals are achieved.	During the transition to adulthood, men mature rapidly, often marry, and experience wage increases.
Relevant Literature	Cheng (2016), Waite and Gallagher (2000)	Edin and Kefalas (2005), Oppenheimer (2003), Oppenheimer et al. (1997), Smock et al. (2005), Sweeney (2002), Xie et al. (2003)	Hogan and Astone (1986), Rindfuss (1991), Shanahan (2000)
Empirical Predictions			
Marriage exit	Wages decline after divorce.	Wages decline before divorce.	Wages flat through divorce.
Heterogeneity by timing	Wages increase prior to marriage regardless of marriage timing.	Wages increase prior to marriage regardless of marriage timing.	Wages increase more among those who marry young.
Shotgun marriage	Smaller premarital wage increases for shotgun marriages than others.	Smaller premarital wage increases for shotgun marriages than others.	Similar premarital wage increases, regardless of shotgun marriage status.

a full-time student or active-duty military. We exclude person-year observations from the self-employed and those working not for pay or in a family business because for this population, wages are an incomplete measure of labor income.

As described later in more detail, we use fixed-effects models, which rely on within-person variation. Therefore, we exclude from the models of marriage men who were followed through 2012 but never married (10.6 %) or who attrited from the sample prior to 2012 and had not married by the time they attrited (13.9 %). We also exclude observations that lack wage values because men were not employed (8.7 %). Finally, we exclude 0.4 % of the remaining observations because they did not contribute at least two observations for the fixed-effects model. The analytic sample for marriage entry includes 49,649 observations on 4,218 men.

Similarly, we exclude from the models of divorce those who were followed through 2012 but never divorced (31.2 % of those who married), attrited from the sample prior to 2012 and without having divorced (24.5 % of those who married), or ended their first marriage because of widowhood (2.1 % of those whose first marriage ended). We exclude 9.5 % of observations for nonemployment and 0.8 % of remaining observations for providing only one wage observation. The analytic sample for divorce includes 15,312 observations on 1,755 men.

Our dependent variable is the log of hourly wages in the respondent's current or most recent job since the last interview.[2] We use the Bureau of Labor Statistics Consumer Price Index (CPI) to adjust to 2012 dollars, and we bottom- and top-code hourly wages at $1 and $100 to reduce the effect of outliers.

To adjust for age differences between married and unmarried individuals, we control for *potential experience*, defined as the respondent's age minus the age at which he entered the labor market, plus 1 (so that all values are nonzero), and then logged.[3] If men with unusually rapid wage growth tend to marry early, conventional fixed-effects models will produce upwardly biased estimates of the marriage premium (Loughran and Zissimopoulos 2009). To address this source of selection bias, we allow the background potential experience-wage profile to vary by educational attainment, academic achievement, race, and age at marriage (under 23, 23–26, and over 26). We measure education with four categories: no high school degree, a high school degree (either GED or high school diploma) but no postsecondary education, some college, and four or more years of college. Academic achievement is measured with percentile on the Armed Forces Qualification Test (AFQT). We measure race as classified by the NLSY79 during household screening: non-Hispanic white or other, non-Hispanic black, and Hispanic. For the 154 observations missing education data, we use surrounding years to impute values. We multiply impute missing AFQT values.

We include controls for other family formation changes (cohabitation and parenthood) in order to isolate the effects of marriage. We measure cohabitation with a dummy variable set to 1 if the individual is currently unmarried and reports a partner in the household roster. We measure parenthood with the respondent's number of own

[2] For respondents with concurrent jobs, we choose the job in which the respondent reports the most hours per week; in the event of a tie, we choose the job in which the respondent has the longest tenure (National Longitudinal Surveys 2016).

[3] The log specification fit the data better than linear, linear spline, and quadratic specifications in the marriage model. We assume respondents already in the labor market in 1979 with less than a high school degree, a high school degree, some college, or a college degree entered the labor market at age 16, 18, 20, or 22, respectively.

coresidential children. Last, we measure period or business cycle effects with year indicators.

Models

To examine wage patterns in the years leading up to and following marriage or divorce, we estimate wage trajectories at the time of marital status transitions using linear splines with knots one year before and after the transition. Similar to Duncan et al.'s (2006) study of marriage and drug use, this approach allows us to report wage growth rates in the years before, during, and after marriage. Our approach also builds on Dougherty's (2006) notion that fixed-effects models should be sufficiently flexible to describe the temporal structure of wage growth relative to marriage.[4] The marriage model is represented by Eq. (1), with vectors in bold. The outcome is logged hourly wages for respondent i at time t [$\ln(Y_{it})$].

$$\ln(Y_{it}) = M_{it}^{[-5,-1]}\beta_1 + M_{it}^{[-1,1]}\beta_2 + M_{it}^{[1,5]}\beta_3 + M_{it}^{[5,10]}\beta_4 + \mathbf{X}_{it}\boldsymbol{\nu} + \alpha_i + \delta_t + \varepsilon_{it}. \quad (1)$$

Whereas conventional fixed-effects models measure marriage with a dummy variable for current marital status, or current status plus years spent in that status, our key independent variables are a linear spline for years since marriage, represented by the M terms in the equation. Each of the M terms is a truncated measure of how long before or after the marriage date the current year is, with each term representing a different range for which values are not truncated, reflected in the numbers in superscript. For example, the first term, $M_{it}^{[-5,-1]}$, is untruncated for observations one to five years before marriage; for these years, the value is coded −1 through −5. For observations after the year prior to marriage, it is coded −1; and for observations more than five years prior to marriage, it is coded −5. This approach makes interpretation simple: rates of log wage growth before, during, after, and long after marriage, net of control variables, are captured by β_1, β_2, β_3, and β_4, respectively. Throughout, when we refer to "wage growth" in the years surrounding marital transitions, we mean growth that is not explained by the control variables, including aging. The reference category is those who will marry in five or more years, and respondents are censored after the ends of their first marriages.

Like conventional fixed-effects models, our models estimate unique intercepts for each person (α_i) and year (δ_t) to net out time-invariant, unobserved characteristics of individuals and years associated with both wages and the independent variables. Time-varying control variables ($\mathbf{X}_{it}$) are as described earlier. All models are weighted, and unweighted results are similar. Standard errors are clustered at the individual level.

The models analyzing wage changes around divorce, represented by Eq. (2), are analogous. We censor respondents prior to the start of their first marriage and after the start of their second marriage, if one exists. Therefore, the sample includes observations

[4] Our quantity of interest differs somewhat from Dougherty's. Dougherty conditioned on characteristics such as work experience and job tenure, identifying the association between marriage and wages net of these mediators. We are interested in the total effect of marriage on men's wages, some of which may operate through increased work experience and job tenure, so we do not condition on these variables.

from only those individuals who are currently married and will divorce, or who are currently divorced but have not yet remarried. Because respondents who divorce often do so within a few years of marriage, and remarriage often occurs shortly after divorce, we estimate wage trajectories for a shorter period than in the marriage models: three years before to five years after divorce.

$$\ln(Y_{it}) = D_{it}^{[-3,-1]}\gamma_1 + D_{it}^{[-1,1]}\gamma_2 + D_{it}^{[1,5]}\gamma_3 + \mathbf{X}_{it}\boldsymbol{\nu} + \alpha_i + \delta_t + \varepsilon_{it}. \quad (2)$$

Our choice of a spline specification is designed to strike a middle ground between parsimony and flexibility in the shape of wage changes during the years surrounding marital transitions. Both Dougherty and Cheng used single-year dummy variables for each of the years surrounding marital transitions, maximizing flexibility.[5] Our spline models closely replicate the patterns of these more flexible specifications (see Online Resource 1).

An advantage of the linear spline specification is that it facilitates clear tests of our hypotheses. In our first analysis of transitions to marriage, we confirm Cheng and Dougherty's findings that wages increase more rapidly than expected in the years prior to marriage ($\beta_1 > 0$). We subsequently test whether it is possible to reject the null hypothesis of linear increases in wages throughout the entire period from 5 years before through 10 years after marriage ($\beta_1 = \beta_2 = \beta_3 = \beta_4$). If we cannot reject this hypothesis, it indicates at a minimum no strong evidence that the marriage itself speeds wage increases, relative to the years just before.

As in the marriage model, we test in our divorce model whether we can reject the null hypothesis of a linear relationship in wages throughout the entire period, from three years before to five years following divorce ($\gamma_1 = \gamma_2 = \gamma_3$). If the wage pattern is linear, this suggests that divorce does not exert a unique effect on wage patterns, whereas a greater decline in wages in the periods during and following divorce suggests a divorce wage penalty. We additionally examine whether wages begin to fall prior to the divorce ($\gamma_1 < 0$), consistent with reverse causality—divorce following wage declines. Alternatively, if we see no evidence that wages change unusually in the years surrounding divorce ($\gamma_1 = \gamma_2 = \gamma_3 = 0$), this is most consistent with no causal association between wages and divorce in either direction.

Next, we return to the marriage models to examine how wage growth in the years surrounding marriage varies by age at marriage. As described earlier, the transition to adulthood perspective suggests that wages should increase more rapidly in the years prior to marriage, on average, for men marrying younger $\left(\beta_1^{young} > \beta_1^{old}\right)$. If preparation for marriage encourages wage growth or wage growth encourages marriage, by contrast, we might expect similar premarital wage increases, regardless of age at marriage. To test this hypothesis, we test for significant differences in wage patterns in the years surrounding marriage by terciles of age at marriage, particularly examining whether wages in this period are flatter for men marrying older than those marrying younger.[6]

[5] Cheng subsequently smoothed the coefficients from these models for graphical presentation.

[6] In these models, we estimate trajectories beginning only three years before marriage so that the youngest group of grooms can provide valid observations for the entire trajectory.

Finally, we test whether men whose marriage is followed by a first birth within seven months experience a similar premarital wage increase to other grooms. In this analysis, men who became fathers before marriage are excluded.[7] In our sample, 42 % of men with premarital conceptions enter a shotgun marriage. If a causal anticipatory effect of marriage drives the observed premarital run-up in wages, these premarital gains should be smaller for men entering shotgun marriages, which are less likely than other marriages to be anticipated $\left(\beta_1^{shotgun} < \beta_1^{not\ shotgun}\right)$. Smaller premarital wage increases for men entering shotgun marriages is likewise consistent with reverse causality: these grooms are less likely to have timed their marriage to a period when their wages are already rising unusually quickly. On the other hand, if premarital wage gains are similar regardless of shotgun marriage status, this is most consistent with the transition to adulthood perspective, suggesting that the marriage process is incidental to other factors shaping wage growth.

Results

Table 2 provides weighted descriptive statistics for the samples used to evaluate the marriage wage premium at entry to and exit from a first marriage. Compared with when they are not yet married, men are older (34 vs. 25) and have higher average wages ($24.66 vs. $16.71) when they are married. Men are older when divorced than when married but not yet divorced (36 vs. 29), but their average wages are similar ($19.60 vs. $19.88).

Figure 1 shows modeled wage trajectories in the years surrounding the entry to and exit from a first marriage. Tabular results are available in Online Resource 1. Because the outcome is the log of hourly wages, we exponentiate the predictions and subtract 1 to yield the predicted percentage change in wages for each year, relative to five or more years prior to marriage or three or more years prior to divorce. The shaded areas show 95 % confidence intervals for the estimated effects.

Panel a in Fig. 1 shows men's changing wages in the years prior to and following entry into marriage. For comparison, we estimate a traditional fixed-effects model with an indicator variable for being married and found a 3.8 % marriage wage premium, similar to prior results that use this method. In the more flexible spline specification, between five years before marriage and one year before marriage, men's wages increase each year 0.5 % more than expected, although the rate of increase is not statistically distinguishable from 0. In the years immediately before and after the marriage, men's wages increase each year 1.5 % more than expected, which is statistically significantly different from 0. Descriptively, men accumulate a wage premium of 2 % by the year before marriage, which grows to 5 % by the year after marriage. The premium increases only very modestly over the next nine years, stabilizing at about 6 %. The results do not lend themselves to clear interpretation. On the one hand, wage growth is most rapid in the years immediately surrounding marriage, consistent with a causal marriage

[7] For all models that stratify by subgroup, we exclude individuals missing information on subgroup membership (e.g., men for whom we cannot determine the relative timing of marriage and first birth).

Table 2 Descriptive statistics of sample, by marital status

	Marriage Premium		Divorce Penalty	
	Never Married	Married	Married, Will Divorce	Divorced
Cohabiting	0.12	—	—	0.20
Number of Children	0.04	1.16	1.03	0.24
	(0.28)	(1.09)	(1.03)	(0.65)
Age	24.59	33.72	29.34	35.89
	(4.71)	(8.38)	(6.20)	(8.30)
Potential Experience	5.05	13.81	10.23	16.83
	(4.55)	(8.24)	(6.14)	(8.19)
Education				
Less than high school	0.14	0.11	0.19	0.17
High school	0.48	0.44	0.49	0.53
Some college	0.18	0.18	0.18	0.19
4+ years of college	0.20	0.27	0.14	0.12
Race				
Hispanic	0.06	0.06	0.07	0.07
Black	0.12	0.11	0.13	0.13
White/other	0.82	0.83	0.80	0.80
Hourly wage (2012$)	16.71	24.66	19.88	19.60
	(10.21)	(15.96)	(11.23)	(12.91)
Number of Observations	16,749	32,900	9,016	6,296
Number of Respondents	3,359	4,006	1,566	1,470

Notes: Samples are restricted to observations on employed respondents who are included in the models in Fig. 1. Observations are weighted using sample weights to be representative of the national population within the age range of the cohort. The married columns refer to individuals in a first marriage. The divorced column refers to those whose first marriage has ended and who have not yet remarried. The racial distribution is estimated at the respondent level. All other descriptive statistics are estimated at the observation level.

premium. On the other hand, the general upward trend in wages throughout the years surrounding marriage is similar to that found by Dougherty (2006) and Cheng (2016), and we cannot reject the null of constant wage gains throughout the entire period, with no special increase at the transition to marriage.[8]

Panel b in Fig. 1 shows the results from our divorce model. We find a statistically significant decline in wages of 2.2 % per year in the years leading up to divorce. We cannot reject the null hypothesis that this trajectory is linear over the entire period; descriptively, the rate of decline is slower during and following divorce than in the

[8] Cheng (2016) and Dougherty (2006) both tracked wages over a longer interval, and neither included controls for calendar year. Both of these differences contribute to their larger estimate of total wage accumulation in the years surrounding marriage (approximately 20 %) compared with our estimate (approximately 6 %). Results estimated through 20 years after marriage and excluding the calendar year controls are shown in Online Resource 1. We prefer the shorter interval because of small sample sizes at the longest marital durations. We believe that the calendar year controls are useful, although we acknowledge that the collinearity of potential experience, years since marriage, and calendar year makes statistical identification of the separate effects challenging.

Fig. 1 Spline wage trajectories at marriage and divorce. Shaded areas represent 95 % confidence intervals. Models are linear regression models with person fixed effects estimated with sampling weights. Figures show the predicted percentage change in wages, relative to the value five or more years prior to marriage or three or more years prior to divorce. The marriage model excludes respondents who never marry and observations after the end of a first marriage. The divorce model excludes respondents who never divorce, observations prior to the start of a first marriage, and observations after the start of a second marriage. Conventional fixed-effects models with dummy variables for current marital status show a 3.8 % marriage premium and a 5.0 % divorce penalty. We cannot reject the joint null hypothesis of linearity throughout the entire window in either model, nor can we reject the null of linearity at any of the individual knots in either model. Tabular results are available in Online Resource 1, Tables S1 and S2

years prior to divorce. Our comparison conventional fixed-effects model estimates a divorce penalty compared with remaining married of 5.0 %. Our models, however, reveal that divorce appears to be the consequence rather than cause of falling wages.

In panel a of Fig. 2, we separate wage patterns by age at marriage. Table 3 presents numerical results and indicates the significance of hypothesis tests for both within-group wage patterns and between-group differences in these patterns. Men who marry after age 26 (the top third of the distribution) experience no unusual wage gains prior to or during marriage. In other words, wage growth among men who marry after age 26 is entirely explained by the control variables, and we cannot reject the null hypothesis that all the marriage terms are jointly 0. In contrast, men who marry at younger ages experience faster than expected wage growth leading up to marriage and through the first several years of their marriages, and we can reject the null hypothesis that all the marriage terms are jointly 0 for both of the younger groups. For these men, the years immediately prior to marriage are the late teens and early 20s—a time when we might expect the most rapid wage changes. Men who marry at age 27 or later are more likely to be approaching marriage after their improvements in wages have stabilized and are

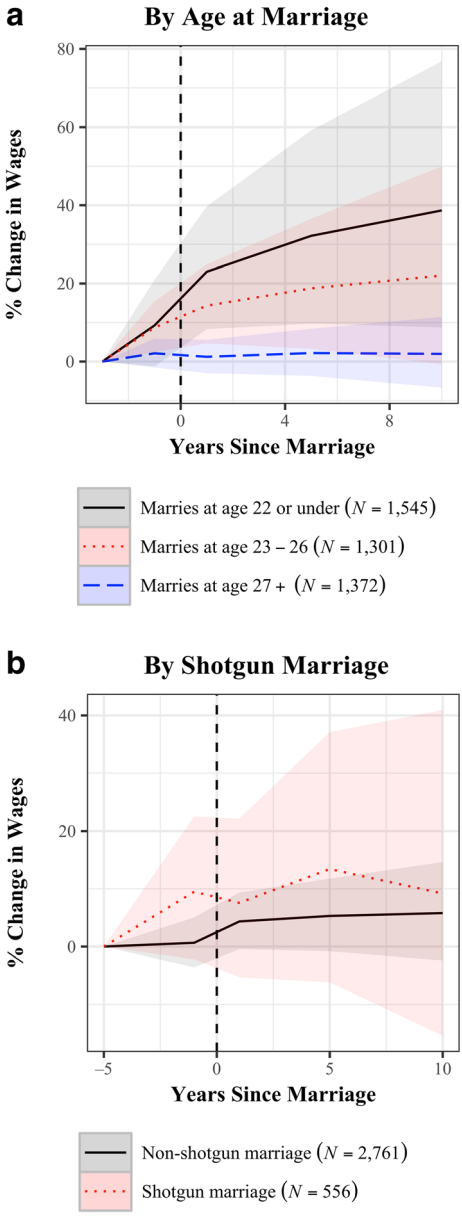


Fig. 2 Heterogeneous wage patterns: Anticipation of marriage or a transition to adulthood? Shaded areas represent 95 % confidence intervals. Models are linear regression models with person fixed effects estimated with sampling weights, analogous to those in Fig. 1. Figures show the predicted percentage change in wages, relative to the value three or more years prior to marriage (panel a) or five or more years prior to marriage (panel b). The models exclude respondents who never marry and observations after the end of a first marriage. The analysis by age at marriage begins at three years before marriage because many men who marry at age 22 or younger do not have valid wage observations more years prior to marriage. Models are estimated separately within subgroups defined by terciles of the age at first marriage distribution (panel a) and by whether the marriage was a shotgun marriage, defined as a marriage followed by a first birth within seven months (panel b). Tests of the significance of differences between slopes are provided in Tables 3 and 4. Tabular results are available in Online Resource 1, Table S3

well captured by our controls. Descriptively, unexplained premarital wage gains are larger for the younger two groups, but the confidence intervals are wide, and the differences are not statically significant. However, we can reject the joint null hypothesis that the three overall trajectories are equal and, specifically, that they have equal slopes in the two years surrounding entry into marriage: the rate of wage growth for men marrying later is slower during this period than for the other two groups.

Table 3 Heterogeneity in wage trajectories by age at marriage

	Marries at Age 22 and Under (1)	Marries at Ages 23–26 (2)	Marries at Age 27+ (3)	H_0: (1) = (2) = (3) (4)
A. Premarriage Slope (−3, −1)	0.044	0.042**	0.010	ns
	(0.026)	(0.015)	(0.009)	
e^{β}	1.045	1.043	1.011	
B. During-marriage Slope (−1, 1)	0.059***	0.025*	-0.004	**
	(0.017)	(0.013)	(0.009)	
e^{β}	1.061	1.025	0.996	
H_0: (A) = (B)	ns	ns	ns	—
C. Immediate Postmarriage Slope (1, 5)	0.018	0.010	0.002	ns
	(0.010)	(0.009)	(0.006)	
e^{β}	1.018	1.010	1.002	
H_0: (B) = (C)	**	ns	ns	—
D. Long-term Postmarriage Slope (5, 10+)	0.010	0.005	-0.000	ns
	(0.008)	(0.009)	(0.006)	
e^{β}	1.010	1.005	1.000	
H_0: (C) = (D)	ns	ns	ns	—
Joint Test of Linearity H_0: (A) = (B) = (C) = (D)	**	*	ns	—
Joint Test of 0 Growth H_0: (A) = (B) = (C) = (D) = 0	**	*	ns	—
N	1,545	1,301	1,372	

Notes: Models are fixed-effects models analogous to the spline models in Fig. 1. Models are estimated separately within subgroups defined by terciles of the age at first marriage distribution. Trajectories begin three years before marriage because many men who marry at age 22 or younger do not have valid wage observations more years prior to marriage. We can reject the null hypothesis that trajectories are equal across age at marriage groups. Additional results are available in Online Resource 1, Table S3.

*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests)

These results are consistent with the claim that marriage is associated with wage gains simply because the timing of marriage is correlated with the transition to adulthood. It may also be consistent with delay of marriage until financial thresholds are met, which may especially affect younger men, who have lower average wages.

In panel b of Fig. 2, we test whether the premarital increase in wages is shared by men who enter shotgun marriages (again, likely to be less deliberately timed and anticipated). Table 4 presents numerical results and indicates the significance of hypothesis tests, again making both within- and between-group comparisons. Men entering shotgun marriages experience premarital wage gains similar to other grooms, and we cannot reject the null hypothesis that the two trajectories are equal. The lack of support for causal anticipation is not only due to wide confidence intervals: the point estimates for premarital wage gains are actually greater among men experiencing a shotgun marriage, despite their marriages being less likely to be deliberately timed. Again, the results cast doubt on the claim that premarital wage gains are due to the

Table 4 Heterogeneity in wage trajectories by shotgun marriage status

	Not Shotgun Marriage (1)	Shotgun Marriage (2)	H_0: (1) = (2) (3)
A. Premarriage Slope (−5, −1)	0.002	0.023	ns
	(0.005)	(0.014)	
e^{β}	1.002	1.023	
B. During-marriage Slope (−1, 1)	0.018**	-0.009	ns
	(0.007)	(0.020)	
e^{β}	1.018	0.991	
H_0: (A) = (B)	ns	ns	—
C. Immediate Postmarriage Slope (1, 5)	0.002	0.013	ns
	(0.004)	(0.012)	
e^{β}	1.002	1.013	
H_0: (B) = (C)	ns	ns	—
D. Long-term Postmarriage Slope (5, 10+)	0.001	-0.008	ns
	(0.004)	(0.010)	
e^{β}	1.001	0.992	
H_0: (C) = (D)	ns	ns	—
Joint Test of Linearity (A) = (B) = (C) = (D)	ns	ns	—
Joint Test of 0 Growth (A) = (B) = (C) = (D) = 0	ns	ns	—
N	2,761	556	

Notes: Models are fixed-effects models analogous to the spline models in Fig. 1. Models are estimated separately by whether the marriage was a shotgun marriage, defined as a marriage followed by a first birth within seven months. We cannot reject the null hypothesis that the trajectories are equal by shotgun marriage status. Additional results are available in Online Resource 1, Table S3.

*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests)

anticipation of entering marriage. Because premarital conceptions may also lead couples to marry under less than ideal financial circumstances, the results also suggest that the premarital increase in wages is not entirely due to men delaying marriage until financial thresholds are met. Instead, premarital wage gains shared by husbands with anticipated and less-anticipated marriages are most consistent with marriage often occurring during the transition to adulthood, when wages are rapidly growing.

Supplementary Models

To test the robustness of our results, we estimated a variety of supplementary models. Online Resource 1 describes these models in more detail and shows their results.

Heterogeneous Effects by Subgroup

Even if marriage has no average effect on the wages of men who marry, it might benefit some subgroups of men, including men who did not marry but who would have

benefited from marriage. We found no statistically significant variation in wage patterns in the years surrounding marriage by race, education, AFQT score groups, wife's employment status at the start of marriage, or gender role attitudes.

We estimated men's probability of marriage based on family background (including race), AFQT score, health, and marital, employment, and educational expectations in 1979 (for more detail, see Online Resource 1) and stratified respondents by the predicted probability (see the methodology and discussion in Brand and Xie 2010). We found no evidence of a larger wage premium for men less likely to marry. These results cast doubt on the hypothesis that men who do not marry would benefit if they did, assuming that men with lower probabilities of marriage are more similar to men who never marry than are men with higher probabilities of marriage.

We also matched married men to similar men observed through age 40 who never married. For each matched pair, we assigned the never-married man the hypothetical age of marriage observed in his matched counterpart. We found that men who married experienced significantly more rapid wage growth between one and five years after marriage than matched men at the same ages who did not marry, which might suggest that men who did not marry would have benefited from marriage. However, they may also have missed or not yet experienced the latent maturation process that spurred both wage growth and marriage for their peers.

Our analyses, therefore, do not lead to firm conclusions about possible wage gains from marriage for the unmarried. We caution that the men in the NLSY79 who did not marry are a subgroup likely to be particularly disadvantaged in the labor force: 31 % are black, compared with 12 % in the married sample; 19 % lack a high school degree at age 25, compared with 12 % among those who marry; and they average approximately a decile lower on the AFQT. Our subgroup analyses do not suggest that these traits lead to larger wage gains from marriage. Furthermore, given the distinctiveness of the unmarried group on observed characteristics, they may differ on unobserved traits as well, and we hesitate to speculate about the likely wage outcomes of these men if they were to marry.

Of course, the absence of statistically significant group differences does not rule out the possibility that subgroups may have true differences in wage patterns or that subgroups of men not identified by our analyses may experience a causal marriage premium. Descriptively, we observe somewhat more rapid wage growth throughout the years surrounding marriage (including before marriage) for men with more traditional gender role attitudes, and more rapid wage growth following marriage for men with the highest probabilities of marriage. Future research is needed to further investigate potential heterogeneity in the causal effect of marriage on men's wages.

Alternative Measures of Anticipation

In 1979 and 1982, respondents were asked questions about whether they expected to marry in a given time frame in the future. We found that men who expected to marry, yet did not, experienced wage trajectories that were not statistically distinguishable from those who followed through on the marriage. Using a similar approach, we examined the wage trajectories of men who married during this period, stratified by whether they expected it. Here, we found some statistically significant differences in overall wage trajectories, but differences did not always favor the men who expected marriage, and some statistically significant differences occurred after entry into marriage rather than during the period in

which marriage was anticipated (or not). Sample sizes were small and confidence intervals were wide; however, we found no strong evidence to counteract our conclusions from the main analyses: (1) anticipation of marriage is not driving the premarital increase in wages, because wage gains in the years prior to marriage are largely shared regardless of whether marriage was expected; and (2) being married has no additional effect on wages given that men "left at the altar" experience similar wage outcomes as men who actually marry.

A More Recent Cohort

Using traditional fixed-effects models and data from the National Longitudinal Survey of Youth 1997 (NLSY97) (Bureau of Labor Statistics 2016b), Budig and Lim (2016) found that men born 1980–1984 receive a marriage premium. Furthermore, among those marrying during 2004–2010 (the group with the relevant data available), the premium was larger for men in male-breadwinner marriages (husband but not wife employed full-time, full-year) than for those in dual-earner marriages. This finding raises two questions: whether our results are particular to the NLSY79 cohort, and whether the marriage premium requires household specialization. We replicated our analyses on the NLSY97 cohort and, as in the NLSY79, found no evidence in favor of a causal marriage premium. Descriptively, as in the NLSY79, wages rise beginning prior to marriage and do not accelerate following marriage, whereas wages begin to decline prior to divorce. For this cohort, we did find significant variation in wage trajectories across specialization categories. Men with non-employed wives have the fastest wage growth one year before to one year after marriage, but then their wage growth changes significantly to a slight decline in the post-marriage period; by five years after marriage, the predicted marriage premium for men with non-employed wives is descriptively less than for men with wives employed either full-time or part-time. This trajectory is the opposite of the prediction that one would make based on a specialization argument (additional results are available in Online Resource 1). We caution that the NLSY97 cohort remains young (ages 28–34 at the 2013 survey wave), so conclusions remain tentative and estimates are imprecise. Future research is needed to investigate the experiences of the NLSY97 cohort as they age.

Alternative Measures of Wages and Events

We estimated models that (1) imputed in various ways wages for men who were not employed, (2) described wage patterns relative to the start of a first coresidential partnership (either marriage or cohabitation), and (3) did not censor individuals at divorce in the marriage model or at marriage and remarriage in the divorce model. In each case, we found results very similar to those in the main models.

We also examined transitions to second marriages. The estimates are less precise because of the smaller sample size but descriptively show a drift upward in wages that predates the marriage itself, similar to first marriages.

Conclusion

Recent research on the marriage wage premium has identified that men's wages begin to grow at accelerated rates at least several years prior to marriage (Cheng 2016; Dougherty

2006), inconsistent with the claim that the marriage premium is driven by household specialization or employer discrimination. However, the premarital increase in wages may be interpreted either as evidence of no causal effect of marriage on wages (Dougherty 2006) or as a real effect of marriage that predates the marriage itself (Cheng 2016). In this article, we brought together three related but largely independent lines of research in an attempt to adjudicate between these interpretations: the effect of marriage on men's wages, the economic determinants of marriage, and the transition to adulthood.

We found that divorce penalties estimated by conventional fixed-effects models in fact predate divorce, suggesting that prior evidence of a divorce wage penalty is likely due to failure to account for this reverse causality. Thus, we found no evidence that *staying* married has a causal wage benefit for men.

Next, we performed two analyses to evaluate whether the premarital run-up in men's wages is likely to be due to a causal anticipation of marriage, versus either timing marriage to follow wage gains or an unobserved latent maturation process that leads to rapid wage gains around the same time as marriage. We found that men who marry after age 26 do not experience any unusual wage growth in the years surrounding marriage: their wages grow as predicted by other characteristics. By contrast, younger grooms experience more rapid wage gains than expected in the years leading up to and through their entrance into marriage. It is possible that anticipation of marriage acts to "settle down" young men, whereas by their late 20s, men experience this transition in work regardless of marital status. In this case, marriage might shift men's period of wage growth earlier, whereas their peers catch up later. Likewise, it is possible that reverse causality occurs only among younger grooms, whose earnings are more likely to be below a normative threshold, whereas older grooms do not need to wait for wage growth before marriage. However, such an interpretation is difficult to reconcile with the lack of variation in trajectories by other wage-relevant traits, including race, education, and AFQT score. We believe that the observed variation by age at marriage is most consistent with the transition to adulthood argument, which suggests that the spurious association between marriage and rapid wage growth is particularly likely for men marrying in the late teens and early 20s. Men who marry later are more likely to have already experienced that period of rapid wage gains prior to their approach to marriage.

This interpretation is reinforced by our finding that the premarital increase in wages is shared by men who enter shotgun marriages and men whose marriages are not as rapidly followed by a birth. If the period of rapid wage increases before marriage is due to anticipation, we would not expect that men with less anticipated marriages would share fully in these anticipatory effects. Furthermore, we would not expect that men entering shotgun marriages have as much discretion as other men about delaying marriage until financial thresholds are met. Our results thus cast doubt on the explanation that the premarital increase in wages is due to marriage being timed to follow economic improvements.

Of course, our analyses have limitations. The sample size limits our ability to test the effects of rarer transitions (such as to second marriages) and to estimate longer-term trends (such as more than a decade after marriage). When we are unable to reject null hypotheses of linear wage trends through the years surrounding marriage or similar wage patterns between groups, this may be because of low statistical power rather than truly similar patterns between groups. This challenge is heightened in models that allow greater flexibility in the estimated wage patterns surrounding marriage, providing a richer picture

but decreasing the precision of estimates. For example, in the pooled marriage model, wage gains are descriptively faster in the year just before and just after marriage, which could be interpreted as a wage premium of approximately 2 % to 3 % at the time of marriage, although we believe that our later results are less consistent with this interpretation than with a spurious maturation process driving wage gains in the years surrounding marriage.

We recognize that it is impossible to establish causal effects with complete certainty in the absence of random assignment. In a theoretical piece outlining the challenges of causal inference based on fixed-effects estimates of the male marriage premium, Sobel (2012) argued that current panel data sets suitable for estimating the male marriage premium do not include information on relevant time-varying confounders, such as mental health and substance abuse. Furthermore, for scholars interested in studying the wage effects of an entire marital history, Sobel noted that alternative methods, such as marginal structural models, are needed to account for the fact that marital status follows a dynamic selection process: factors such as occupation and work experience may be predictors of marital status in one period and its outcomes in the next. Rather than attempt to perfectly identify the effect of marriage on wages, we present a set of empirical analyses that provide evidence that is, to different degrees, consistent with various theoretically informed perspectives on the appropriate interpretation of men's observed wage trajectories at marriage and divorce.

Our analyses support the argument that men simply tend to marry at times when wages are already increasing particularly rapidly. For each result, it is possible to tell an alternative story of how the observed pattern is consistent with a causal effect of marriage, although that effect may be limited to particular years or particular subgroups. Taking the findings together, however, shows how complicated and limited such a causal story would have to be to be consistent with our results. Because our main focus is questioning whether marriage affects wages, we are less concerned with adjudicating between the reverse causality and transition to adulthood perspectives. We believe that our results favor the transition to adulthood interpretation for marriage and the reverse causality perspective for divorce, but our core claim—that there is no population-average causal effect of anticipating, getting, or staying married on men's wages—does not depend on which alternative process is at work.

Although we find no individual-level effect of marriage on men's wages, we are unable to evaluate the effect of marriage culture on all men's wages, regardless of whether they personally marry. For example, if wage-earning is normative for husbands, it may also become a more general expectation of adult masculinity, particularly if most men expect to marry and therefore perceive themselves as potential future husbands, regardless of whether they eventually marry. Because 96 % of the men in our sample expect to marry, we cannot separately identify the effects of marital expectations on men's wages, or what men's wages would be if the marriage rate were substantially lower.

Our results have implications for research on the economic determinants of marriage. If wages rise prior to marriage for spurious reasons, positive associations between men's current economic circumstances and marriage in the next year may be biased just as the wage premium is. Alternatively, if men time marriage in response to changes in financial circumstances, more research is needed to understand which aspects of financial change and achievement, or which financial thresholds, are particularly relevant to enabling marriage, and whether these thresholds vary by subgroup.

Although we cannot provide definitive evidence that marriage does not benefit men's wages, our analyses show that the empirical patterns of men's wage changes in the years surrounding marriage are entirely consistent with an alternative explanation: men marry during a transition to adulthood when their wages are already improving, and they tend to divorce after their wages are already declining.

Acknowledgments We are grateful to Javier García-Manglano, Florencia Torche, Christopher Winship, and anonymous *Demography* reviewers for helpful comments, and to Siwei Cheng for both comments and sharing an early version of her manuscript. Lundberg received support from the Undergraduate Research Scholars program of the Institute for Quantitative Social Science at Harvard University and from the Eunice Kennedy Shriver National Institute of Child Health and Human Development of the National Institutes of Health under Award Number P2CH0047879 and under Award Number T32HD007163. The content is solely the responsibility of the authors and does not necessarily represent the official views of the National Institutes of Health. An earlier version of this article was presented at the annual meeting of the Population Association of America, May 2014, Boston, MA. Replication code is available on the Harvard Dataverse at doi:10.7910/DVN/X9528P (http://dx.doi.org/10.7910/DVN/X9528P).

References

Becker, G. S. (1991). *A treatise on the family* (Enlarged ed.). Cambridge, MA: Harvard University Press.

Brand, J. E., & Xie, Y. (2010). Who benefits most from college? Evidence for negative selection in heterogeneous economic returns to higher education. *American Sociological Review, 75,* 273–302.

Budig, M. J., & Lim, M. (2016). Cohort differences and the marriage premium: Emergence of gender-neutral household specialization effects. *Journal of Marriage and Family, 78,* 1352–1370.

Bureau of Labor Statistics, U.S. Department of Labor. (2016a). *National Longitudinal Survey of Youth 1979 cohort, 1979–2012* (Rounds 1–25). Columbus: Center for Human Resource Research, The Ohio State University [producer and distributor].

Bureau of Labor Statistics, U.S. Department of Labor. (2016b). *National Longitudinal Survey of Youth 1997 cohort, 1997–2013* (Rounds 1–16). Chicago, IL National Opinion Research Center, the University of Chicago [producer]; and Columbus: Center for Human Resource Research, The Ohio State University [distributor].

Charles, K. K., & Stephens, M., Jr. (2004). Job displacement, disability, and divorce. *Journal of Labor Economics, 22,* 489–522.

Cheng, S. (2016). The accumulation of (dis)advantage: The intersection of gender and race in the long-term wage effect of marriage. *American Sociological Review, 81,* 29–56.

Chun, H., & Lee, I. (2001). Why do married men earn more: Productivity or marriage selection? *Economic Inquiry, 39,* 307–319.

Cohen, P. N. (2002). Cohabitation and the declining marriage premium for men. *Work and Occupations, 29,* 346–363.

Cornwell, C., & Rupert, P. (1997). Unobservable individual effects, marriage and the earnings of young men. *Economic Inquiry, 35,* 285–294.

Dougherty, C. (2006). The marriage earnings premium as a distributed fixed effect. *Journal of Human Resources, 41,* 433–443.

Duncan, G. J., Wilkerson, B., & England, P. (2006). Cleaning up their act: The effects of marriage and cohabitation on licit and illicit drug use. *Demography, 43,* 691–710.

Edin, K., & Kefalas, M. (2005). *Promises I can keep: Why poor women put motherhood before marriage*. Berkeley: University of California Press.

Ginther, D. K., & Zavodny, M. (2001). Is the male marriage premium due to selection? The effect of shotgun weddings on the return to marriage. *Journal of Population Economics, 14,* 313–328.

Gorman, E. H. (2000). Marriage and money: The effect of marital status on attitudes toward pay and finances. *Work and Occupations, 27,* 64–88.

Gray, J. S. (1997). The fall in men's return to marriage: Declining productivity effects or changing selection? *Journal of Human Resources, 32,* 481–504.

Gupta, S. (1999). The effects of transitions in marital status on men's performance of housework. *Journal of Marriage and the Family, 61,* 700–711.

Hersch, J., & Stratton, L. S. (2000). Household specialization and the male marriage wage premium. *Industrial and Labor Relations Review, 54,* 78–94.

Hogan, D. P., & Astone, N. M. (1986). The transition to adulthood. *Annual Review of Sociology, 12,* 109–130.

Killewald, A. (2013). A reconsideration of the fatherhood premium: Marriage, coresidence, biology, and fathers' wages. *American Sociological Review, 78,* 96–116.

Killewald, A. (2016). Money, work, and marital stability: Assessing change in the gendered determinants of divorce. *American Sociological Review, 81,* 696–719.

Killewald, A., & Gough, M. (2013). Does specialization explain marriage penalties and premiums? *American Sociological Review, 78,* 477–502.

Korenman, S., & Neumark, D. (1991). Does marriage really make men more productive? *Journal of Human Resources, 26,* 282–307.

Loh, E. S. (1996). Productivity differences and the marriage wage premium for white males. *Journal of Human Resources, 31,* 566–589.

Loughran, D. S., & Zissimopoulos, J. M. (2009). Why wait? The effect of marriage and childbearing on the wages of men and women. *Journal of Human Resources, 44,* 326–349.

National Longitudinal Surveys. (2016). *Jobs & employers*. Washington, DC: Bureau of Labor Statistics, U.S. Department of Labor. Retrieved from https://www.nlsinfo.org/content/cohorts/nlsy79/topical-guide/employment/jobs-employers

Nock, S. L. (1998). *Marriage in men's lives*. New York, NY: Oxford University Press.

Oppenheimer, V. K. (2003). Cohabiting and marriage during young men's career-development process. *Demography, 40,* 127–149.

Oppenheimer, V. K., Kalmijn, M., & Lim, N. (1997). Men's career development and marriage timing during a period of rising inequality. *Demography, 34,* 311–330.

Petersen, T., Penner, A. M., & Høgsnes, G. (2011). The male marital wage premium: Sorting vs. differential pay. *Industrial and Labor Relations Review, 64,* 283–304.

Pollmann-Schult, M. (2011). Marriage and earnings: Why do married men earn more than single men? *European Sociological Review, 27,* 147–163.

Rindfuss, R. R. (1991). The young adult years: Diversity, structural change, and fertility. *Demography, 28,* 493–512.

Shanahan, M. J. (2000). Pathways to adulthood in changing societies: Variability and mechanisms in life course perspective. *Annual Review of Sociology, 26,* 667–692.

Smock, P. J., Manning, W. D., & Porter, M. (2005). "Everything's there except money": How money shapes decisions to marry among cohabitors. *Journal of Marriage and Family, 67,* 680–696.

Sobel, M. E. (2012). Does marriage boost men's wages?: Identification of treatment effects in fixed effects regression models for panel data. *Journal of the American Statistical Association, 107,* 521–529.

Sweeney, M. M. (2002). Two decades of family change: The shifting economic foundations of marriage. *American Sociological Review, 67,* 132–147.

Waite, L. J., & Gallagher, M. (2000). *The case for marriage: Why married people are happier, healthier, and better off financially*. New York, NY: Broadway Books.

Xie, Y., Raymo, J. M., Goyette, K., & Thornton, A. (2003). Economic potential and entry into marriage and cohabitation. *Demography, 40,* 351–367.