On the Marriage Wage Premium[*]

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Abstract

We use a novel instrument based on social norms towards marriage to present a new finding: being married has a positive causal effect on the wages of both men and women. Despite the striking changes in the labor market and the composition of families over the past decades, the premium for men is sizable and has only slightly diminished over time. In contrast, we document a stark change for women: the effect of being married has evolved from a penalty into a premium. We present new evidence to uncover the mechanisms behind these causal patterns. The premium for men can be mainly rationalized as a byproduct of household specialization à la Becker (1985, 1993): within married couples, men tend to concentrate on market work, which leads them to supply more market hours than their single counterparts and earn higher wages. In the past, the penalty for women was also a consequence of household specialization, with married women working significantly fewer market hours than their single counterparts. The degree of specialization between spouses has decreased over time, allowing married women to work virtually as many hours as singles and leading to the disappearance of the penalty. The marriage wage premium for women in recent years can be explained by one of the channels in Pilossoph and Wee (2021): because of income pooling with their husbands, married women have a higher reservation wage, which allows them to select better-paid job opportunities.

Keywords — Wage, Marriage, Marriage Wage Premium.
JEL Codes — J11, J12, J16, J30.

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1 Introduction

Over the past decades, the United States, along with many other countries, has experienced a pronounced shift in both the structure of families and the role of women. The marriage, divorce, fertility, and assortative mating patterns have all markedly changed, while women's labor market outcomes have also evolved significantly (Lundberg and Pollak, 2007; Greenwood, Guner, and Vandenbroucke, 2017). The economic role of women in the labor market is more prominent now than ever before.[1] One aspect of this transformation that has received less attention is the evolution of the relationship between wages and being married.[2] While several authors study the fact that married men earn higher wages than their single counterparts, the so-called marriage wage premium (MWP), there is less work on this relationship for women.[3]

Knowledge of the causal link between being married and wages for women is key to understanding the secular changes that have occurred over the past decades. Many of the changes in the economic role of women reflect, in fact, the transformation in the economic role of *married* women (Goldin, 1990). For example, most of the increase in female labor force participation that occurred after the Second World War can be accounted for by the growth in the employment of married women. Hence, it is crucial to analyze the relationship between wages and being married to understand the economic transformation of the past decades.

We use 42 years of Current Population Survey (CPS) data (1977-2018) to study the effect of being married on the hourly wages of both men and women. Our work targets two key objectives. The first is to document, for both men and women, the evolution of the causal effect of being married on wages over time. The second is identifying the main underlying mechanisms explaining the marriage wage premium.

To achieve our first aim, we face three empirical challenges. First, a sizable share of women do not participate in employment. The underlying economic decision generating this outcome implies that the sample of women's wages is not a random draw from the population. Hence, sample-selection bias might distort the estimated coefficients. Second, unobservable variables may affect both the propensity to be married and wages. That is, the estimates may suffer from omitted variable bias. Third, there may be reverse causality if wages also affect the probability of being married. We simultaneously tackle these three issues by combining a Heckman (1979) sample-selection correction with an instrumental variables approach that uses a novel instrument for being married based on social norms.

Social groups and communities differ in their valuation of marriage, their practices around it, and their preferences toward unwedded life. These differences are often expressed as social norms that define desirable behavior of the individuals in the community (Burke and Young, 2011). Therefore, the individual propensity to be married is crucially affected by the social norms in their group or community.[4] Our premise is that individuals who live in a social milieu of prevalent marriage are more likely to be married due to the social incentives in the group or community. To systematically proxy social norms towards marriage, we compute the share of married people who have the same sex, live in the same state, and have the same values for the indicators of college education and children but are 6 to 15 years older than a given individual in our analysis.[5] The proxy thus captures the reference group's propensities to enter

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[1] A vast literature studies the evolution of women's labor market outcomes. See Olivetti (2006), Attanasio, Low, and Sánchez-Marcos (2008), Blau and Kahn (2007, 2017), Fernández (2013), and Goldin (2014).

[2] Some authors study the relationship between being married and other outcomes. For example, Choi and Valladares-Esteban (2018, 2020) or Guner, Kulikova, and Llull (2018).

[3] See Hill (1979), Dolton and Makepeace (1987), Korenman and Neumark (1992), Loughran and Zissimopoulos (2009), Ginther and Sundström (2010), Killewald (2013), Juhn and McCue (2016a, 2017), and Pilossoph and Wee (2021).

[4] There is a growing literature that studies how social norms affect family outcomes. Fernández and Fogli (2009) study how second-generation American women's work and fertility behavior is shaped by the social norms of the country of origin of their parents. Adamopoulou (2012) estimates the effect of peers on marital decisions, while Adamopoulou and Kaya (2018) analyze how peer effects shape living arrangements. Vickery and Anderberg (2021) model social norms as conformity preferences to study inter-ethnic marriages in the United Kingdom.

[5] Acemoglu, He, and le Maire (2022) use an instrument in the same spirit to identify how peer effects mediate

and exit marriage.

Using the General Social Survey (GSS), we cross-validate the instrument's soundness as a proxy for social norms towards marriage. We use the available data in the GSS to show that the instrument strongly predicts favorable opinions and attitudes regarding marriage, which supports its use as a proxy for social norms toward marriage.

The baseline specification is a log-wage equation with a set of individual controls and state and year fixed effects, which we estimate by instrumental variables (IV) for men and a selection-corrected IV for women. Our first key finding is to document a change in the sign of the causal impact of being married on women's wages. Until the mid-1980s, being married caused a sizable wage *penalty* for women. In the next twenty years, the effect was close to null, and then a notable wage *premium* emerged from the mid-2000s. For men, we find a sizable positive causal effect throughout the period we analyze that has slightly decreased over time.

We provide extensive evidence to support each identifying assumption underlying the instrumental variable approach, paying particular attention to the exclusion restriction. We show that our identification strategy can recover the null effect of marriage on two placebo tests. We start estimating the effect of being married on height. This setup is informative for two reasons. First, we know height is determined during childhood and adolescence, i.e., the causal effect of being married on height is zero. Nevertheless, there is a robust correlation between being married and height, likely driven by selection bias. Taller men are more likely to be married, while the opposite is true for women. Second, height is highly correlated with cognitive and non-cognitive abilities, two key determinants of wages that are unobservable in our data.[6] In contrast to the Ordinary Least Squares (OLS) estimates, the IV approach returns the null effect of being married on height. This placebo exercise indicates that, in the main specification, the instrument impacts wages only through being married and not via other unobservable variables. In a second placebo test, we show that the instrument also recovers the null effect of being married on nativity. Next, we use the plausibly exogenous method of Conley, Hansen, and Rossi (2012) to systematically evaluate the implications of violations of the exclusion restriction. The key patterns implied by the results –the persistence of the male premium and, for women, the evolution of the penalty into a premium– are robust to wide-ranging violations of the exclusion restriction.

All equations in the baseline specification include year fixed effects to absorb the systematic business cycle fluctuations that might affect hourly wages, marriage prevalence, and employment. We also include state fixed effects to prevent long-lasting differences across states from impacting our estimates. A particular case of these differences across states could be the spatial sorting of individuals with a higher likelihood of being married and earning higher wages. The baseline results are robust to such sorting patterns. Next, we show that the results derived from the baseline are insensitive to including much richer fixed-effects specifications.

First, the baseline results are virtually unaffected by the inclusion of state-by-year fixed effects, i.e., state-specific business cycles are not a driver of our main results. Second, during the period we study, the labor market outcomes of non-college-educated men declined noticeably. Therefore, a natural question is whether the type of shocks identified in Autor, Dorn, and Hanson (2013) and their impact on wages and marriage (Autor, Dorn, and Hanson, 2019) affect our estimates. We find that such shocks do not impact our main estimates – the main causal patterns for both men and women are robust to including state-by-year-by-college fixed effects.

Finally, we consider localized shocks affecting wages and marriage prospects, such as the fracking boom studied by Kearney and Wilson (2018). When these shocks mainly impact one education group, the state-by-year-by-college specification shows that they do not threaten our main results. We further show that the baseline results stand even if these shocks affect a whole generation or age group, irrespective of education level, in a given state at a particular time. Namely, the estimates are robust to the inclusion

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the hiring of managers with business degrees.

[6]See Persico, Postlewaite, and Silverman (2004), Case and Paxson (2008), and Schick and Steckel (2015).

of state-by-year-by-age-group fixed effects. Hence, a shock, such as the fracking boom, that may raise the wages and marriage prospects of the youth in a given state does not significantly impact the inference we draw from our identification strategy.

We then turn to consider the underlying mechanisms that may be driving our estimates of the effect of being married on wages, and assess all the mechanisms proposed in the literature to explain the MWP. As a result, we identify and present new evidence for the two main mechanisms that can reconcile the existence and evolution of the causal effect of being married on wages over the last decades for men and women jointly.

A central hypothesis in the literature is that the MWP for men may be generated by household specialization à la Becker (1985, 1993): one spouse concentrates on home production (non-market work and childcare), allowing the other to supply more market work.[7] Therefore, the spouse who specializes in market work can out-compete singles. We use data on market hours from the CPS and the time allocation information from the American Time Use Survey (ATUS) to show that there is a significant degree of specialization among spouses. The average husband concentrates on market work, while the average wife focuses more on home production. However, the degree of specialization has decreased steadily over time.[8] We show that, through the last decades, being married has led men to work more market hours than their single counterparts, albeit the difference has slightly reduced.[9] This difference in market hours is consistent with Becker's household specialization theory. Therefore, we conclude it is a crucial mechanism behind the premium for men.[10]

Our analysis of market hours also reveals that, while married women worked significantly less for pay than their single counterparts in the past, the gap has virtually closed.[11] This pattern in market hours is consistent with the reduction in household specialization we find. Thus, we conclude that the reduction in the degree of specialization among spouses drives the disappearance of the penalty for women.

Next, we examine the joint search theory of the marriage wage premium of Pilossoph and Wee (2021). In their model, married individuals achieve higher wages because they can afford to wait for better job offers. As married individuals share resources within the household, their implicit reservation wage is higher than that of a single individual. In addition, married people climb the job ladder faster by putting more effort into searching for better jobs while employed. Married agents understand that when they achieve a higher wage, they incentivize their spouses to find a better job by increasing their reservation

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[7]Ginther and Zavodny (2001) and Antonovics and Town (2004) find that the MWP for men is not due to omitted-variable bias and suggest that being married causes men to earn higher wages. Chun and Lee (2001), Gupta, Smith, and Stratton (2007), and Bardasi and Taylor (2008) present evidence in favor of the household-specialization hypothesis. In contrast, Loh (1996) and Hersch and Stratton (2000) find that the data do not support the household specialization theory well.

[8]Gray (1997) finds that the marriage premium for men in the late 1970s can be partly explained by specialization but concludes that this channel declined over time and that in the early 1990s, most of the premium was due to selection into marriage.

[9]Several authors present evidence showing that married men devote more resources to their careers than singles. Akerlof (1998) finds that, after marriage, men in the NLSY work more hours and earn higher wages. Lundberg and Rose (2002) show that in the Panel Study of Income Dynamics (PSID), men work more hours and earn higher wages after marriage and the arrival of the first offspring. Ahituv and Lerman (2007) show, using the NLSY, that marriage leads men to work more hours. Mazzocco, Ruiz, and Yamaguchi (2014) combine a collective model with PSID data to show that marriage induces dynamic changes in the labor supply of men and women. Olivo-Villabrille (2022) exploits the differences in no-default divorce legislation across states to show that marriage leads men to increase hours worked. Blandin, Jones, and Yang (2023) use a structural lifecycle model of marriage and labor supply to show that the number of hours men work increases due to marriage.

[10]Becker's theory is agnostic on the precise pathway from higher hours to higher wages. The literature offers some possible explanations. Korenman and Neumark (1991) find that married men have higher job performance, which leads to a higher likelihood of promotion and wages, using data from a company personnel file. Eckstein, Keane, and Lifshitz (2019) use CPS data to estimate a structural lifecycle model with endogenous education, employment, marriage, divorce, and fertility decisions, in which half of MWP for men is due to higher hours of work and the increase in human capital that these higher hours bring about. Goldin (2014) and Bick, Blandin, and Rogerson (2022) provide other mechanisms that link higher hours with higher wages.

[11]Juhn and McCue (2016a,b) show that the total earnings of married women are still lower than those of single women, although there has been convergence.

wage. We use the CPS's panel dimension to examine the marriage wage premium of individuals who move from not having a job to employment. Consistent with the higher-reservation-wage mechanism of Pilossoph and Wee (2021), we find that the MWP of newly employed individuals increases with spousal earnings. For women, all the marriage wage premium is explained by this mechanism.

Finally, starting with Hill (1979), some authors have conjectured that married individuals might receive higher wages due to employer discrimination. We thoroughly examine the discrimination hypothesis in a companion paper (McConnell and Valldares-Esteban, 2023). Using the National Survey of the Youth 1979 (NLSY), we show that being married is positively correlated with measures of cognitive and non-cognitive abilities measured in early adulthood. We use the framework of Altonji and Pierret (2001) and Pinkston (2009) to test whether employers use the worker's marital status as an attribute against which to discriminate.[12] We find no evidence indicating that employers discriminate in favor of (or against) married men or women. Therefore, we conclude that employer discrimination is not an important mechanism underpinning the MWP.

We make two key contributions. First, we contribute to the literature studying the marriage wage premium for men by presenting robust empirical evidence on the positive causal effect of being married on men's wages. Starting with Hill (1979), several authors have examined whether the positive relationship between being married and men's wages is causal or spurious. Some analyses, such as Korenman and Neumark (1991) or Antonovics and Town (2004), suggest that marriage causes higher wages, while others, like Nakosteen and Zimmer (1987) or Krashinsky (2004), find no evidence of a causal link. We present an empirical strategy that is able to tackle concerns about omitted-variable and reverse-causality biases. We also provide evidence supporting the household specialization mechanism to rationalize the premium for men.

Secondly, by studying the evolution of the causal relationship between being married and women's wages, we contribute to the literature that examines the stark changes in women's labor market outcomes over the last decades (Lundberg and Pollak, 2007; Goldin, 2014; Blau and Kahn, 2017). Hill (1979) and Loughran and Zissimopoulos (2009) study the negative impact that being married used to have on women's wages, what we label as the marriage wage penalty for women.[13] Juhn and McCue (2016a, 2017) document the evolution of women's earnings vis-á-vis marital stauts and motherhood. Using OLS and fixed effects methodologies, they find that being married is negatively correlated with earnings for older cohorts of women. However, this conditional correlation is positive in younger cohorts except for women with young children. We depart from their analysis in that the results we present demonstrate the causal effect of being married on women's hourly wages. Further, we uncover the crucial mechanisms that reconcile the evolution of this causal effect from a penalty into a premium. The systematic assessment of the mechanisms that rationalize the MWP contributes to the broader literature that studies the interaction between the family and economic outcomes (Doepke and Tertilt, 2016; Greenwood et al., 2017).

The rest of the paper is organized as follows. In Section 2, we describe the data we use and the sample restrictions we impose. In Section 3, we describe the instrument and present the causal patterns of the effect of being married on wages. In Section 4, we assess the mechanisms that have been proposed to rationalize the marriage wage premium. Finally, Section 5 concludes.

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[12]Maasoumi, Millimet, and Sarkar (2009) use the PSID to estimate the MWP for men along the wage distribution. Because they find a premium only at the lower end of the wage distribution, they conclude that discrimination is likely the cause of the premium. de Linde Leonard and Stanley (2015) run a meta-analysis of the literature on the MWP for men and conjecture that discrimination might be the source of the premium as the meta-analysis indicates that the MWP increases with years of marriage. However, these studies do not formally test the employer discrimination hypothesis.

[13]Dolton and Makepeace (1987) and Korenman and Neumark (1992) conclude that marriage does not affect women's wages negatively but having children does. Killewald (2013) reports a positive effect of marriage on women's wages.

2 Data

We use data from the March Supplement of the CPS from 1977 to 2018.[14] Our sample consists of white non-Hispanic civilians in their prime working age (between 25 and 54 years old), not living in group quarters, and for whom we have no missing data on relevant demographic characteristics. We exclude self-employed workers, individuals working in the private household sector, and agricultural workers from the sample. The group of married individuals consists of people that declare to be married and living with their spouse in the same household.[15] The non-married group is composed only of never-married individuals to keep consistency with the literature on the MWP for men.[16]

Using the information on weeks worked last year and usual work hours per week, we build a variable that proxies the total number of hours worked last year for each individual on the sample. Then, we divide the non-allocated total labor income last year, expressed in 1999 US dollars, by the total number of hours worked last year to measure hourly wages. As it is common in the literature, we trim the top and bottom 1% of our measure of hourly wages to limit the influence of outliers. We disregard the hourly wage measure of individuals who report less than 100 hours of work last year and consider them never employed last year. We use the Annual Social and Economic Supplement weights throughout the analysis.

Tables 1 and 2 present key descriptive statistics for men and women, respectively. We divide the sample into six periods (each spanning seven years) to study the evolution of the effect of being married on wages over time. The observed patterns, both for men and women, are consistent with well-documented trends in the US labor market during the past decades. Namely, the decrease in the share of married individuals, the increase in female labor force participation, the increase in educational attainment, and the reduction in the number of children.

Table 1: Descriptive Statistics for Men

	1977-1983	1984-1990	1991-1997	1998-2004	2005-2011	2012-2018
Sample Size	121,172	120,080	112,404	129,390	137,634	110,952
Married	0.841	0.791	0.760	0.745	0.709	0.662
Hourly Wage (1999 Dollars)	19.81	19.29	18.40	19.63	19.81	19.81
	(9.31)	(9.94)	(9.88)	(10.71)	(11.33)	(11.82)
Age	37.55	37.13	38.05	39.27	39.53	39.10
	(8.83)	(8.38)	(8.24)	(8.40)	(8.78)	(8.93)
Highest Level of Education:						
HS Dropout	0.168	0.116	0.085	0.070	0.062	0.051
HS Graduate	0.368	0.378	0.335	0.310	0.310	0.277
Some College	0.187	0.201	0.262	0.277	0.274	0.275
College Graduate	0.150	0.171	0.209	0.238	0.243	0.269
Advanced Graduate	0.126	0.134	0.110	0.105	0.111	0.129
Number Children, 0–4	0.297	0.296	0.268	0.248	0.250	0.238
	(0.592)	(0.599)	(0.570)	(0.557)	(0.562)	(0.549)
Number Children, 5–17	1.12	0.898	0.842	0.828	0.788	0.753
	(1.29)	(1.12)	(1.08)	(1.08)	(1.07)	(1.08)
Children, 18 and over	0.160	0.141	0.129	0.127	0.122	0.114

Notes: Data used: CPS, 1977-2018. We report means and, for continuous variables, we report standard deviations in parentheses.

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[14]The CPS data are made publicly available by Flood, King, Ruggles, and Warren (2015).

[15]In the CPS, it is not possible to reliably identify cohabiting couples for most of the years in the sample. Moreover, cohabitants are not subject to the legal obligations of marriage. Therefore, we ignore cohabitation.

[16]Separated, divorced, and widowed individuals are excluded from the sample. In Appendix Section A.1, we reproduce the main analysis using a sample in which the non-married group includes never-married, divorced, separated, and widowed individuals. The main results are robust to changing the definition of the non-married group.

Table 2: Descriptive Statistics for Women

	1977-1983	1984-1990	1991-1997	1998-2004	2005-2011	2012-2018
Sample Size	132,308	130,154	121,247	142,474	152,620	121,234
Married	0.901	0.864	0.846	0.829	0.803	0.751
Employed	0.619	0.713	0.763	0.774	0.759	0.751
Hourly Wage (1999 Dollars)	11.91	12.86	13.65	15.18	15.79	16.26
	(6.25)	(7.24)	(8.04)	(9.08)	(9.55)	(10.14)
Age	37.80	37.28	38.19	39.44	39.80	39.31
	(8.83)	(8.39)	(8.22)	(8.34)	(8.73)	(8.92)
Highest Level of Education:						
HS Dropout	0.162	0.104	0.073	0.053	0.045	0.037
HS Graduate	0.475	0.447	0.362	0.305	0.262	0.219
Some College	0.173	0.205	0.275	0.295	0.295	0.283
College Graduate	0.121	0.153	0.203	0.244	0.271	0.300
Advanced Graduate	0.068	0.091	0.087	0.103	0.126	0.161
Number Children, 0–4	0.270	0.288	0.272	0.252	0.261	0.255
	(0.569)	(0.589)	(0.570)	(0.559)	(0.570)	(0.564)
Number Children, 5–17	1.25	1.01	0.958	0.948	0.926	0.903
	(1.30)	(1.13)	(1.10)	(1.10)	(1.11)	(1.12)
Children, 18 and over	0.203	0.178	0.161	0.154	0.156	0.150

Notes: Data used: CPS, 1977-2018. We report means and, for continuous variables, we report standard deviations in parentheses.

3 The Causal Effect of Being Married on Wages

We start this section by providing context for the instrument we use. Then, we explain the empirical specification. After providing support for each of the key identifying assumptions, we present the main results. Finally, we present placebo tests that indicate that the exclusion restriction is likely satisfied and estimate the consequences for the estimates of violations of the exclusion restriction using the method of Conley et al. (2012).

3.1 The Measurement of Social Norms Towards Marriage

To the best of our knowledge, no data set exists with a direct measure of social norms towards marriage at the individual level and the systematic information on wages and demographic characteristics we need to implement our identification strategy. Hence, we use the information on the CPS to construct a proxy for social norms toward marriage. To do so, we proceed as follows. For each individual in the sample, we compute the share of married people of the same sex, who live in the same state, are observed in the same survey year, and report the same coarse level of education and children in the household but are 6 to 15 years older. That is, we use the marriage rate of people with identical demographic characteristics but slightly older than the individuals in the sample to proxy the prevalence of social norms toward marriage. The intuitive idea is that because social norms are persistent over time, the marriage rate of older cohorts is influenced by norms relevant to the cohort we analyze. When we define the reference cohort, we balance two criteria. First, we require that the reference cohort is old enough to minimize competition in the age-based marriage market. Second, the reference cohort needs to be close enough to the individual in the sample so that norms that affect the reference cohort persist in affecting the individuals in the sample.

The GSS periodically asks a nationally-representative sample of individuals their stance on several issues, including marriage, religion, and political leaning. These questions offer a more direct measure of the social norms towards marriage held by the population. However, the relevant questions are not asked systematically, and the GSS does not provide information on the wages of the respondents. We use the

available data to correlate the information in the GSS with the CPS-based instrument to validate that the instrument is indeed a proxy of social norms towards marriage. To compute these correlations, we proceed as follows. First, to ease interpretation, we normalize the coding of the responses to reflect a coherent stance on marriage. For example, all answers which show a positive regard towards marriage are coded with the same number. Second, we group respondents by year, division, and college-indicator categories. Due to the sample sizes of the GSS, we cannot use finer cells. For each cell and question, we compute the average answer and correlate it with the instrument, which we compute using data from the CPS.

The results in Table 3 show that the instrument has a strong positive correlation with questions that directly ask about attitudes towards marriage. There is a 55.6% correlation between agreeing with the statement that marriage makes people happier and the instrument and a 59.2% correlation between agreeing with the position that married people who want children should get married and the instrument. These correlations indicate that the instrument is a good proxy of social norms toward marriage. Additionally, the instrument is not a particularly good predictor of political and policy stances. That is reassuring as it indicates that the instrument is not broadly capturing individual traits that might be associated with conservatism and might potentially impact wages too.

However, the instrument is also highly correlated with religiosity. On the one hand, this is a predictable outcome as stances towards marriage are interlinked with religious beliefs. On the other hand, that might threaten the identification of causal effects as religiosity has been hypothesized to be a determinant of economic outcomes.[17] If the instrument was a proxy not only of norms towards marriage but also of religiosity, and religiosity was a determinant of productivity, the exclusion restriction would be violated. We do not take a stand on whether religiosity affects wages. Instead, in Section 3.6, we show that the patterns describing the evolution of the MWP over the last decades are robust to violations of the exclusion restriction. Hence, even if it were the case that religiosity affects wages directly, the MWP patterns we present can be interpreted as causal.

Table 3: The Instrument Is a Good Proxy of Positive Attitudes Toward Being Married

	Correlation
Marriage	
Agree with Statement: Married People Generally Happier Than Unmarried	.556
Agree with Statement: People Who Want Children Should Get Married	.592
Agree with Statement: Husband Should Work, Wife Should Look After Home	.1
Agree with Statement: Homosexuals Shouldn't Have Right To Marry	.397
Religion	
Have Any Religious Faith	.689
Have Christian-Based Faith	.641
Attend Religious Service Monthly Or More	.474
Strong Religious Affiliation	.633
Pray Weekly Or More	.243
Agree with Statement: Supreme Court Wrong To Ban Lord's Prayer In Schools	.147
Politics And Policy	
Republican	.087
Think Of Self As Conservative	.103
Agree with Statement: We Spend Too Much On Welfare	.215
Agree with Statement: We Spend Too Much On Childcare Assistance	.061
Agree with Statement: We Spend Too Much On Foreign Aid	.459
Agree with Statement: We Spend Too Much On The Military	-.069
Agree with Statement: We Spend Too Much On Space Exploration	.297
Agree with Statement: We Spend Too Much On Supporting Science	-.276

Notes: Data used: CPS and GSS. Cells are division × college × year groupings of GSS and CPS respondents. Christian-based faith encompasses the following religious preferences listed in the GSS: Protestant, Catholic, Christian and Orthodox-Christian. Each cell is weighted by the sum of the proportion of the CPS cell size to the total sample size in CPS and the proportion of the CPS cell size to the total sample size in the GSS.

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[17]See Iyer (2016) for a review of the literature on the economics of religion.

3.2 Empirical Specification

For men, we run the following two-stage least squares (2SLS) specification:

$$M_i = \pi_1 Z_{M,i} + X_i' \pi_2 + \theta_{1s} + \phi_{1t} + \mu_i, \tag{1}$$

$$y_i = \alpha M_i + X_i' \beta + \theta_{2s} + \phi_{2t} + \epsilon_i. \tag{2}$$

Equation 1 is the first stage, where M_i is a dummy variable that equals 1 when an individual reports to be married and living with their spouse, $Z_{M,i}$ is the value of the instrument for individual i, X_i is a set of demographic controls which consists of education-category dummies, the number of children below the age of 5, the number of children aged 5-17, a dummy for a child over the age of 18, and dummies for years of potential experience.[18] θ_{1s} and ϕ_{1t} are state and year fixed effects respectively. Equation 2, the second stage, describes how the logarithm of hourly wages, y_i, depends on being married and the same covariates as in the first stage.

For women, we run a selection-corrected version of the 2SLS specification described in Equation 1 and Equation 2:

$$E_i = \mathbb{1}\{\kappa_1 Z_{E,i} + \kappa_2 Z_{M,i} + X_i' \kappa_3 + \theta_{1s} + \phi_{1t} + \xi_i > 0\} = \mathbb{1}\{Z_i' \kappa + \xi_i > 0\}, \tag{3}$$

$$M_i = \pi_1 Z_{M,i} + X_i' \pi_2 + \theta_{2s} + \phi_{2t} + \pi_5 \lambda(Z_i' \kappa) + \mu_i, \tag{4}$$

$$y_i = \alpha M_i + X_i' \beta + \theta_{3s} + \phi_{3t} + \sigma_{13} \lambda(Z_i' \kappa) + \epsilon_i. \tag{5}$$

Because we treat being married as an endogenous variable, we use the instrument ($Z_{M,i}$) instead of the dummy for being married (M_i) in the employment equation (Equation 3). We start by estimating the employment decision (Equation 3) using a probit. Then, we recover the estimated coefficients to compute $\lambda(Z_i' \kappa) = \phi(Z_i' \kappa)/\Phi(Z_i' \kappa)$. Finally, we estimate the two systems of equations, Equations 1-2 and Equations 4-5 using 2SLS. We bootstrap the standard errors in the selection-corrected 2SLS procedure.[19]

In the literature that studies women's labor market outcomes, it is common to use a dummy variable for the presence of own children in the household as the exclusion restriction to correct for employment selection. However, this option is incompatible with controlling for children in the wage equation. When we think about the constraints that affect women's employment decisions, it seems plausible that the time a mother needs to devote to children decreases with the child's age. For example, a newborn requires more time (is more likely to affect the employment margin) than a teenager. Given this consideration, we use the age of the youngest own child in the household as the exclusion restriction. We note two relevant points. First, the dummies for the age of the youngest child are jointly significant in the employment equation. Secondly, the set of controls (X_i) in the wage equation includes the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over the age of 18. Hence, because we already control for the presence of children, we think it is safe to exclude $Z_{E,i}$ from the wage equation. The implicit assumption is that what affects wages is whether there are young (0-4) and older (5-17) children in the household but not the age of the youngest, which is only relevant to the employment decision.

The decision of whether to control for the presence of children is not inconsequential. On the one hand, there is an extensive literature that documents that having children negatively affects the earnings of married and single women, while it has a null or positive impact on the earnings of men.[20] Moreover, the theories proposed to rationalize the marriage wage premium do not include the presence of children as

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[18]As it is standard in the literature, we compute potential experience as age minus years of education minus seven.

[19]The instrument is computed prior to running the 2SLS procedure. Note that in the case of a generated instrument (which enters only the first stage and the selection equation), we do not need to adjust the standard errors of the 2SLS estimates as is the case with a generated regressor in the wage equation.

[20]See Angelov, Johansson, and Lindahl (2016), Chung, Downs, Sandler, and Sienkiewicz (2017), Killewald (2013), Kuziemko, Pan, Shen, and Washington (2018), Kleven, Landais, and Søgaard (2019), or Kleven (2022).

an element of the theory. Therefore, these two reasons suggest defining the treatment of being married as net of the effect of children on wages. On the other hand, the positive correlation between being married and having children might create a bias in the estimates. In Appendix Section A.2, we replicate the main results while not controlling for the presence of children in the household. The coefficients we obtain and the patterns implied by the estimates align with those from the main specification. We also use the NLSY to provide estimates of the potential bias that controlling for children might have on the estimates. We show that among those with children, the pre-marriage wages of those who will end up having children while married and those who have children while remaining unwedded are not statistically different. This finding suggests that the potential bias, if existent, is likely to be small.

In Appendix Section A.3, we reproduce the main results while adding controls for industry and occupation. We do not include these controls in the main specification because we consider the choice of occupation/industry endogenous to the treatment of being married. Moreover, industry and occupation are ill-defined for non-employed individuals, which precludes using these controls when correcting for sample-selection bias. In Section 3.5, we present a battery of richer fixed-effects specifications. We show that the main results are not driven by the effects of the negative shocks that have impacted the labor market outcomes of non-college-educated men (Autor et al., 2019) or the positive localized shocks, such the fracking boom (Kearney and Wilson, 2018), that might have generated expansions in the economy of a particular state.

We posit being married may have heterogeneous effects on wages and, thus, consider the coefficient estimates a measurement of a local average treatment effect (LATE).[21] In the next section, we highlight the main features of the supporting evidence we provide in Appendix B for the assumptions that identify a well-defined LATE. In Section 3.6, we discuss the exclusion restriction and assess the consequences of its violation on the results.

3.3 Support for the Identifying Assumptions

First Stage. We provide evidence supporting the instrument's ability to predict being married in three places. First, we present first-stage F-statistics in Table 4 and Table 5 in Section 3.4. Second, in Appendix Section B.1, Figure B1 and Figure B2 show the first stage graphically, as well as, presenting information on the distribution of the instrument. For both men and women, there is evidence of a strong relationship between the instrument and individual marriage decisions (conditional on the other relevant covariates discussed in Section 3.2). Finally, the first column of Table B2, in Appendix Section B.1, presents the first stage coefficient for each of the subperiod samples we use.

Conditional Independence. Table B1, in Appendix Section B.1, examines the stability of the first stage parameter as we condition on additional covariates. These additional variables are only available for a subset of the periods we analyze. Hence, we do not include these in the main specification. However, they are variables that can plausibly impact marriage decisions and productivity. To the extent that the instrument is randomly assigned, adding these variables to the first stage should not appreciably impact the point estimate on the instrument. The estimates in Table B1 indicate that there is no impact on the first stage coefficient of including these additional regressors, which we interpret as supportive evidence of the conditional independence assumption.

Monotonicity. In the empirical specification in Section 3.2, monotonicity implies that any individual getting married when the instrument's value is low also marries when it is high. It also implies that individuals not marrying when the instrument's value is high do not marry when it is lower. A growing literature on judge severity instruments (Dahl, Kostøl, and Mogstad (2014); Bald, Chyn, Hastings, and Machelett (2019); Bhuller, Dahl, Løken, and Mogstad (2020)), which employs a setup of an endogenous

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[21]See Imbens and Angrist (1994).

binary regressor and a continuous instrument as we do, notes that monotonicity implies we should see a non-negative first stage coefficient for any sub-sample. In Appendix Section B.1, Table B2 presents the first stage coefficient for a variety of different sub-samples. In all cases, the coefficient is non-negative, supporting the monotonicity assumption.

3.4 Results

We present the results for men in Table 4. We divide the years in the sample into six subperiods to examine the evolution of the estimates over time. The first row of Table 4 reports the OLS coefficients, i.e. we estimate Equation 2 in isolation and report $\hat{\alpha}$. One of the differences between the OLS estimates and the IV coefficients is that the latter are estimated from the complier population while the first are based on the whole population. In order to understand how much of the difference between the OLS and 2SLS estimates is due to the distinct populations from which they are estimated, the second row in Table 4 presents OLS estimates based on reweighting the main sample to reflect the observable characteristics of the complier population.[22] Finally, we report the 2SLS coefficients obtained from estimating Equations 1 and Equation 2 in the third row of Table 4. The results in Table 4 show that being married has a sizable positive causal impact on men's wages, although the effect is likely to have slightly decreased over time.

Table 4: Being Married Has a Positive Causal Effect on the Wages of Men

	(1)	(2)	(3)	(4)	(5)	(6)
	1977-1983	**1984-1990**	**1991-1997**	**1998-2004**	**2005-2011**	**2012-2018**
OLS:						
Married	0.213***	0.208***	0.214***	0.209***	0.222***	0.200***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
CW-OLS:						
Married	0.201***	0.195***	0.204***	0.198***	0.217***	0.197***
	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)
2SLS:						
Married	0.306***	0.269***	0.246***	0.226***	0.266***	0.237***
	(0.013)	(0.015)	(0.013)	(0.016)	(0.017)	(0.021)
First-Stage F-Statistic	1461.3	2116.8	1304.2	1425.5	1426.4	1128.9
Adjusted R^2	0.198	0.238	0.263	0.249	0.268	0.265
Observations	104,970	104,545	96,210	112,807	120,606	94,896

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the natural log of wages. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. For the complier reweighted regressions, we first separate each sample into six mutually exclusive groups based on education and age, as outlined in Appendix Section B.2.2. Then, we estimate the proportion of compliers in each sub-group and then reweight the main estimation samples so that the complier proportion in each of the six sub-groups matches the proportion of the main sample for the same sub-group.

The results for women are in Table 5. In the first row, we report the OLS estimates. The second row contains the coefficients corrected by sample-selection bias. In the third row, we present the selection-corrected and complier-reweighted OLS estimates. Finally, the 2SLS selection-corrected coefficients estimated jointly using Equations 3, 4, and 5 are in the fourth row. Being married causally decreased women's wages in the late 1970s and early 1980s. From the mid-1980s until the mid-2000s, the impact was not statistically different from zero, while it became a notably positive effect from the mid-2000s onwards. That is, the effect of being married on women's wages has evolved from a sizable penalty to a considerable premium over the last decades.

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[22]In Appendix Section B.2, we provide the details on how we back out the observable characteristics of the complier population.

Table 5: The Causal Effect of Being Married on Women's Wages Has Evolved From a Penalty Into a Premium

	(1)	(2)	(3)	(4)	(5)	(6)
	1977-1983	**1984-1990**	**1991-1997**	**1998-2004**	**2005-2011**	**2012-2018**
OLS:						
Married	−0.053***	−0.001	0.031***	0.054***	0.070***	0.079***
	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)
Heckman:						
Married	−0.044***	0.000	0.033***	0.052***	0.068***	0.080***
	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)	(0.006)
CW-Heckman:						
Married	−0.025***	0.019**	0.051***	0.061***	0.079***	0.087***
	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)
2SLS-Heckman:						
Married	−0.149***	−0.058	−0.018	−0.033	0.104***	0.086***
	(0.022)	(0.037)	(0.028)	(0.040)	(0.031)	(0.032)
First-Stage F-Statistic	352.6	485.8	265.2	417.4	311.0	236.8
Adjusted R^2	0.149	0.200	0.237	0.227	0.225	0.227
Observations	70,872	83,482	83,791	101,175	109,624	85,427

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the natural log of wages. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. The exclusion restrictions for the employment equation are a series of dummies for the age of the youngest child in the household from 1-18, where age less than 1 is the base category, a dummy for ages 19-24, a dummy for 25 and over, and no children are also included. In this case, we bootstrap standard errors, allowing for clustering at the state level and using 500 iterations. For the complier reweighted regressions, we first separate each sample into six mutually exclusive groups based on education and age, as outlined in Appendix Section B.2.2. Then, we estimate the proportion of compliers in each sub-group and then reweight the main estimation samples so that the complier proportion in each of the six sub-groups matches the proportion of the main sample for the same sub-group.

The IV estimates (the "2SLS" for men and the "2SLS-Heckman" for women) indicate a moderately larger causal effect of being married on wages than what the OLS coefficients suggest. In other words, according to the IV estimates, both the marriage wage premium (for men and women) and the marriage wage penalty that women experienced in the late 1970s and early 1980s are larger than what is estimated from OLS. This difference between IV and OLS estimates may appear through three different channels. First, if marriage has heterogeneous effects, it is possible that, in the complier population, marriage has a larger effect than in the whole population. The complier-reweighted OLS estimates indicate this is not the case for men or women. Second, if there are unobservable factors that increase (decrease) wages while decreasing (increasing) the probability of being married, the OLS estimates are downward biased. For women, there is evidence that certain traits and behavior that are positively associated with career success may reduce the likelihood of marriage.[23] On the other hand, for men, the literature on the MWP suggests that the effect of unobservable variables downward biases the OLS estimates. Thirdly, if an exogenous increase (decrease) in wages reduces (increases) the probability of being married, the OLS coefficients are downward biased due to reverse causality. Theoretically, an exogenous change in wages affects the material resources that a single person brings to the marriage, changing their option value of marriage and how appealing they might be to potential spouses. At the same time, it also changes their option value to remain single, which implies that the direction of the simultaneity bias is ambiguous. Regalia, Ríos-Rull, and Short (2011), Salcedo, Schoellman, and Tertilt (2012), and Greenwood, Guner, Kocharkov, and Santos (2016) present quantitative models in which the improvement of the option value of singlehood drives down the rate of marriage. Hence, it is plausible that the OLS coefficients are

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[23]Bursztyn, Fujiwara, and Pallais (2017) study the behavior of MBA students and find that single females express less willingness to conform to the demands of high-paying jobs in front of single male peers. Taylor, Hart, Smith, Whalley, Hole, Wilson, and Deary (2005) show that IQ at age 11 is negatively associated with women's marriage probability during adulthood.

downward biased due to reverse causality.[24]

The key conclusion we derive from the results is that being married has a sizable positive effect on men's wages and the effect on women has evolved from a sizable penalty to a substantial premium.

3.5 Robustness to Different Types of Shocks

The baseline specification we present in Section 3.2 includes year fixed effects that absorb US-wide systematic business cycle fluctuations, along with state fixed effects that neutralize static differences across states. In this section, we show that the baseline results of Section 3.4 are robust to various economic shocks affecting wages, employment, and marriage decisions. To do so, we demonstrate that the baseline results are virtually unaffected by including richer sets of fixed-effects specifications.

Figure 1 presents different estimates for the causal effect of being married on log wages, that is, $\hat{\alpha}$ from equivalent specifications to those in Equation 2 and 5 using different fixed-effects specifications. Figure 1a depicts the results for men and Figure 1b for women.[25] Each figure contains six panels for each time sub-period, from 1977-1983 to 2012-2018. In each panel, we plot four different estimates of the causal effect of marriage on hourly wages. The first, labeled "S, Y," is that from the baseline specification, i.e., that reported in Tables 4 and 5 which includes year and state fixed effects. The second coefficient, labeled "SxY," is computed using state-by-year fixed effects. In this case, the fixed effects absorb any business cycle fluctuations at the state level. The third coefficient, labeled "SxYxC," includes state-by-year-by-college fixed effects. There is compelling evidence that, over the last decades, the rise in trade with China and the changes in production technologies have negatively affected the labor market outcomes and the marriage prospects of non-college-educated men (Autor et al., 2013, 2019). This specification compares individuals within state-year-education categories, that is, it does not use any cross-education-category variation to identify the causal effect of being married on hourly wages. Finally, the fourth coefficient, labeled "SxYxA," is from a specification including state-by-year-by-age-group fixed effects. In this case, the fixed effects neutralize local shocks that might disproportionately impact a given generation, such as the fracking boom analyzed by Kearney and Wilson (2018).

The coefficients we depicted in Figure 1a indicate that the effect of being married on men's hourly wages is not affected by the fixed effects specification. The coefficients associated with state-by-year-by-college ("SxYxC") fixed effects are particularly important to clarify the drivers of the causal impact of being married on men's wages. The MWP is not driven by the worsening of economic opportunity that non-college-educated men have experienced over the last decades.

For women, in Figure 1b, we see that the pattern identified in the main results of Tables 5 – the evolution from a penalty into a premium– is also robust to the inclusion of several fixed-effects specifications.

Finally, we highlight that both men's and women's baseline results survive the inclusion of state-by-year-by-age-group fixed effects ("SxYxA"). Our instrument uses the cross-generation persistence of social norms toward marriage to proxy the prevalent social norms in a given age group. However, when we disregard age-group variation to identify the causal effect of being married, the estimated coefficients do not vary substantially from the baseline results. That is, the identification in the main results does not fundamentally originate in comparing wages across different age groups. It is important to emphasize this result as it clarifies that shocks that may affect one generation's wages and marriage decisions do not interfere with identifying the causal effect of being married on the next generation.

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[24]Folke and Rickne (2020) show that, in Sweden, women elected for public office, which under certain conditions can be understood as an exogenous positive shock to earnings, are more likely to divorce.

[25]Figure A1 in Appendix Section A.4 presents the F-test statistics associated with the estimation of each coefficient.

Figure 1: The Main Results Are Robust to the Existence of a Wide Variety of Shocks
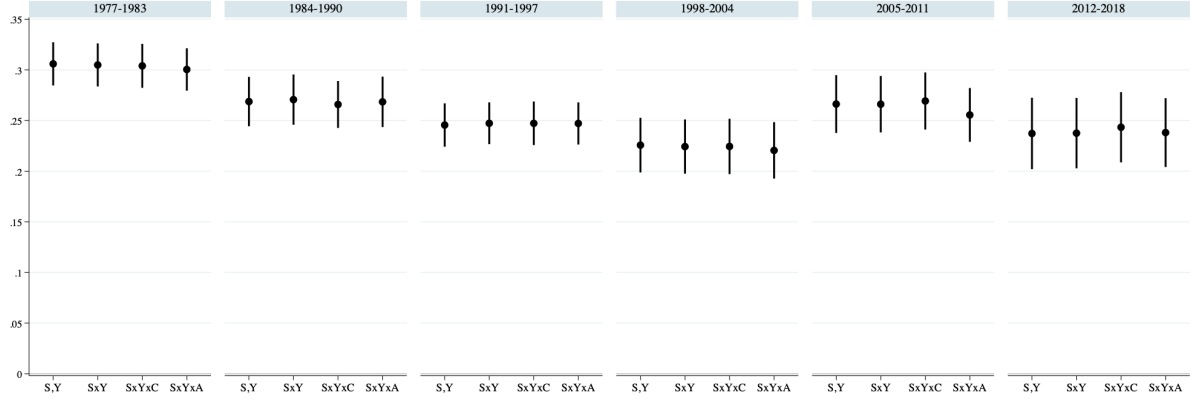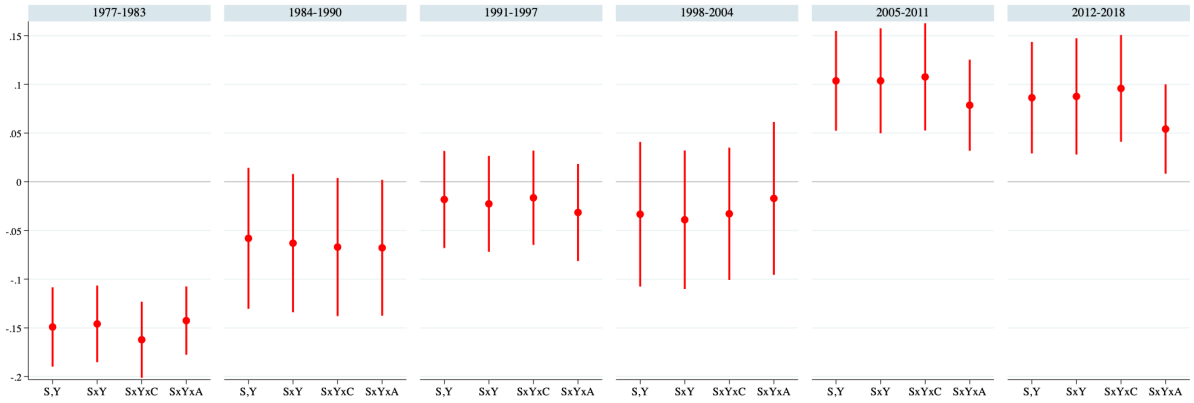

(a) 2SLS Coefficients, Men

(b) 2SLS-Heckman Coefficients, Women

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the natural log of wages. We present 4 sets of fixed effects specifications: the baseline specification of state and year fixed effects (labelled S,Y), state-by-year fixed effects (labelled SxY), state-by-year-by-college fixed effects (labelled SxYxC), and state-by-year-by-age-group fixed effects (labelled SxYxA). The age groups used in the third specification comprise six groups, each spanning five years of age. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. For all IV-Heckman specifications, the exclusion restrictions for the employment equation are a series of dummies for the age of the youngest child in the household from 1-18, where age less than 1 is the base category, a dummy for ages 19-24, a dummy for 25 and over, and no children are also included.

3.6 The Exclusion Restriction

As is the case in any IV empirical strategy, we rely on the exclusion restriction (the social norms measured by the instrument only affect wages through marriage) to guarantee the validity of the estimates while being unable to test it. In this section, we provide evidence of the credibility of this assumption. Moreover, we show that the broad patterns presented in Section 3.4 (the emergence of a premium for women and the sustained presence of a premium for men) are robust to violations of the exclusion restriction.

3.6.1 Placebo Tests

We start by showing that the instrument can estimate the causal effect of being married on height. This placebo test is particularly relevant for three reasons. First, as with wages, a significant correlation exists between height and being married. Taller men are more likely to be married, while the opposite is true for women. Secondly, while we do not know the causal relationship between being married and wages, we do know that being married has a null effect on height. Stature is determined by genes and environmental factors during childhood (Steckel 1995). Thirdly, height is strongly correlated with cognitive and non-cognitive abilities, two determinants of wages that are unobservable in our setting (Persico et al. 2004, Case and Paxson 2008, Schick and Steckel 2015). In other words, height is an excellent proxy for variables that one could suspect may appear in the error term of the wage equation (Equations 2 and 5) affecting the instrument and wages simultaneously. The fact that the instrument can recover the true null causal effect of being married on height indicates that the instrument can likely deal with many of the omitted-variable-bias concerns one could have when estimating the effect of being married on wages. In particular, those related to time-invariant omitted variables.

We use data from the National Health Interview Survey (NHIS), which contains data on height and all the variables we need to replicate the identification strategy we use for wages. In order to compute the instrument, we need information on the geographic location of the individuals in the sample. Because the NHIS does not provide the interviewees' state of residence, we use metropolitan statistical areas (MSA) as the geographical identifier, only available from 1997 to 2001. We report the conditional correlation between height and being married in columns (1) through (4) of Table 6. As with wages, the OLS estimates are significantly different from zero. To construct the instrument with NHIS data, we need to restrict the sample to observations reporting geographical location, i.e., a particular MSA. Therefore, in columns (3) and (4) of Table 6, we repeat the OLS analysis on the sub-sample of observations for which we can compute the instrument. The estimated coefficients demonstrate that this sub-sample presents a very similar relationship between height and being married to the entire sample. Columns (5) and (6) of Table 6 present the 2SLS estimates. These indicate that there is no significant causal effect of being married on height, as we know it is the case. Moreover, the first-stage F-statistics confirm the robustness of the instrument as a predictor of being married in the NHIS sample.

Table 6: The Instrument Recovers the Null Causal Effect of Being Married on Height

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample		**Large MSA Only Sample**			
	OLS		**OLS**		**2SLS**	
	Men	Women	Men	Women	Men	Women
Married	0.239***	−0.172***	0.206***	−0.178**	−0.258	0.345
	(0.029)	(0.036)	(0.069)	(0.082)	(0.357)	(0.563)
First-Stage F-Statistic					220.538	40.105
Adjusted R^2	0.015	0.022	0.014	0.023	0.009	0.016
Observations	16,538	18,704	6,060	7,149	6,060	7,149

Notes: Data used: NHIS, 1997-2001. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by MSA and reported in parentheses. The dependent variable is height in inches. Year and MSA fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual.

Using the data available in the CPS, we perform another placebo test. Starting in 1994, the CPS asked interviewees whether they were US- or foreign-born. As with height, there is a significant correlation between nativity and being married, while being married does not causally affect nativity. Columns (1)

and (2), in Table 7, present the conditional correlation between being married and nativity. Columns (3) and (4) report the 2SLS estimates. The IV specification finds that being married does not have a statistically significant effect on nativity for both men and women. The nativity placebo test is also relevant for the analysis because nativity might be correlated to the determinants of earnings if immigrants are self-selected (Borjas 1987). The two placebo tests indicate that the IV strategy effectively tackles the concerns about omitted-variable bias and reverse causality that might affect the OLS estimates.

Table 7: The Instrument Recovers the Null Causal Effect of Being Married on Nativity

	(1)	(2)	(3)	(4)
	OLS		**2SLS**	
	Men	Women	Men	Women
Married	−0.016***	−0.028***	0.002	−0.019
	(0.003)	(0.005)	(0.006)	(0.012)
First-Stage F-Statistic			2062.275	422.988
Adjusted R^2	0.029	0.030	0.028	0.030
Observations	422,828	453,433	422,828	453,433

Notes: Data used: CPS, 1994-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is a dummy that takes the value 1 for individuals that report being born in the US and 0 otherwise. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual.

3.6.2 The Implications of Violating the Exclusion Restriction

We have presented two placebo tests that indicate that the exclusion restriction is likely satisfied. Next, we assess how robust are the main patterns we derive from the estimates to violations of the exclusion restriction using the plausible exogenous approach by Conley et al. (2012). Conley et al. (2012) present the 2SLS equations allowing for an instrument Z that is non-excludable and abstracting from other control variables as:

$$Y = M\alpha + Z\gamma + \epsilon, \tag{6}$$

$$M = Z\Pi + V, \tag{7}$$

where $E[M\epsilon] \neq 0$, $E[Z\epsilon] = 0$ and the exclusion restriction can be thought of $\gamma \equiv 0$.

Conley et al. (2012) consider four different strategies to implement their technique, which handles deviations from $\gamma \equiv 0$. We consider the Local to Zero Approximation (LTZ) which assumes a prior on γ that follows a normal distribution, with mean μ_γ and variance Ω_γ, where the uncertainty regarding γ reduces with sample size. In this setting, we can write the plausibly exogenous estimator as:

$$\hat{\alpha} \sim N(\alpha_{2SLS} + A\mu_\gamma, W_{2SLS} + A\Omega_\gamma A'), \tag{8}$$

where $A = (X'Z(Z'Z)^{-1}Z'X)^{-1}(X'Z)$, α_{2SLS} is the 2SLS point estimate, and W_{2SLS} is the 2SLS variance-covariance matrix.

Given the evidence we have presented on the instrument's reliability, our prior is that γ is likely to be very close to zero. The aim now is to establish a realistic worst-case scenario for the estimates. To that end, we start by estimating how large γ in Equation 6 needs to be to invalidate the inference, that is, to crowd out the direct effect of being married on wages (α). We denote this particular γ as γ^*, and we compute a different γ^* for each sex and subperiod we consider in the main results of Section 3.4. We report the estimated γ^*s for men in Table 8 and Table 9 for women. All γ^*s for men are positive and are of similar magnitude as the IV estimates (γ^* is about 4/5 of the IV coefficient). The γ^*s are positive because

the IV coefficients (α in Equation 6) are also positive and, hence, to crowd out the effect of being married on wages, γ needs to be positive. The fact that the γ^*s are of similar magnitude to the IV estimates indicates that a *large* violation of the exclusion restriction is needed to obtain a null causal effect of being married on men's wages. For women, the γ^*s have the same sign as the IV coefficient and are also of the same order of magnitude. In the 1977-1983 subperiod, when the IV estimate is significantly smaller than zero, γ^* is around 2/3 of the IV coefficient. In the last two periods, 2005-2011 and 2012-2018, when we observe a significant premium for women, γ^* is about half of the IV estimates. That is, although the estimates for women do not present the same degree of resilience to violations of the exclusion restriction as those for men, there is still a considerable margin for a violation to occur while still finding that the causal effect of being married on the wages of women has moved from a penalty to a premium.

To estimate what would be a reasonable deviation from the $\gamma \equiv 0$ prior, we use the approach of van Kippersluis and Rietveld (2018). They note that if there exists a sub-group of the sample for whom the first stage is zero, i.e., a group for whom the instrument does not impact the marital status, then the reduced form for this group can be used to back out a plausible estimate for γ. We refer to this sub-sample as the zero-first-stage sub-sample.[26] We consider those with predicted marriage in the lowest quartile as the zero-first-stage sub-sample because the instrument has little predictive power for this sub-group. We denote the γ we estimate from this procedure $\hat{\gamma}_{ZFS}$.[27]

If we assume homogeneous direct effects of the instrument (i.e., the $\hat{\gamma}$ estimated from the zero-first-stage sub-sample is informative of γ for the entire sample), then we can assess the impact of a violation of the exclusion restriction on the 2SLS estimates for the full sample. Specifically, this means we set $\mu_\gamma = \hat{\gamma}_{ZFS}$. In the second row of Tables 8 and 9, we present the effect of being married on wages when γ, the magnitude of the violation of the exclusion restriction, is derived from the zero-first-stage sub-samples. Note that for each sex and seven-year subperiod, we compute a different $\hat{\gamma}_{ZFS}$. The coefficients indicate that the patterns derived from the main results (in the first row) are robust to the violations of the exclusion restriction informed by $\hat{\gamma}_{ZFS}$. We also use $\hat{\gamma}_{ZFS}$ to anchor the interpretation of γ^*, the violation of the exclusion restriction that invalidates the inference. For men, $\gamma^*/\hat{\gamma}_{ZFS}$ is larger than 2 in all periods, that is, the violation of the exclusion restriction that renders the inference invalid needs to be at least two times larger than the most reasonable magnitude of the exclusion restriction we can estimate. For women, $\gamma^*/\hat{\gamma}_{ZFS}$ is larger than one in all cases. In the later periods, when the MWP for women has emerged, the violation of the exclusion restriction needs to be more than 50% larger than the estimate of γ that can be drawn from the data to invalidate the patterns drawn from the main results.

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[26]To see how this approach is informative of the exclusion restriction, consider a reduced form equation for wages where we substitute Equation 7 into Equation 6:

$$Y = Z(\gamma + \alpha\Pi) + (\epsilon + \alpha V). \tag{9}$$

For the zero-first-stage sub-sample, $\Pi = 0$, the reduced form estimate of the instrument is given by γ.

[27]In Appendix Section B.3, we describe the details of this sub-sample.

Table 8: The Premium for Men Is Robust to Violations of the Exclusion Restriction

	(1)	(2)	(3)	(4)	(5)	(6)
	1977-1983	**1984-1990**	**1991-1997**	**1998-2004**	**2005-2011**	**2012-2018**
2SLS	0.306***	0.269***	0.246***	0.226***	0.266***	0.237***
	(0.013)	(0.015)	(0.013)	(0.016)	(0.017)	(0.021)
Plausibly Exogenous	0.264***	0.230***	0.214***	0.194***	0.254***	0.225***
	(0.015)	(0.016)	(0.014)	(0.017)	(0.018)	(0.022)
$\hat{\gamma}_{ZFS}$	0.034	0.034	0.026	0.026	0.010	0.010
γ^*	0.248	0.234	0.207	0.188	0.221	0.187
$\gamma^*/\hat{\gamma}_{ZFS}$	7.261	6.851	7.896	7.172	22.033	18.643
Observations	104,970	104,545	96,210	112,807	120,606	94,896

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the natural log of wages. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. As discussed in Appendix Section B.3, given the lack of a zero-first-stage sample for men in the first two periods, we take the maximum value of $\hat{\gamma}_{ZFS}$ across all other specifications. This procedure returns that of women in the same first two periods. We specify $\hat{\gamma}$ as the reduced form estimate from the zero-first-stage sample. For the plausibly exogenous sepcifications, we set $\Omega_\gamma = (.125\sqrt{S_0^2 + S_{-0}^2})^2$, where S_0 is the standard error on $\hat{\gamma}$ for the zero-first-stage sub-sample and S_{-0} is the equivalent for the remainder of the sample. γ^* is the value of γ that yields a coefficient on being married equal to zero.

Table 9: The Evolution of the Causal Effect of Being Married on Women's Wages Is Robust to Violations of the Exclusion Restriction

	(1)	(2)	(3)	(4)	(5)	(6)
	1977-1983	**1984-1990**	**1991-1997**	**1998-2004**	**2005-2011**	**2012-2018**
2SLS-Heckman	−0.149***	−0.058	−0.018	−0.033	0.104***	0.086***
	(0.022)	(0.037)	(0.028)	(0.040)	(0.031)	(0.032)
Plausibly Exogenous	−0.197***	−0.106**	0.001	−0.010	0.153***	0.139***
	(0.026)	(0.044)	(0.031)	(0.045)	(0.032)	(0.036)
$\hat{\gamma}_{ZFS}$	0.034	0.034	−0.013	−0.013	−0.026	−0.026
γ^*	−0.107	−0.042	−0.012	−0.018	0.055	0.043
$\gamma^*/\hat{\gamma}_{ZFS}$	−3.133	−1.230	0.959	1.438	−2.093	−1.637
Observations	70,872	83,482	83,791	101,175	109,624	85,427

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the natural log of wages. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. The exclusion restrictions for the employment equation are a series of dummies for the age of the youngest child in the household from 1-18, where age less than 1 is the base category, a dummy for ages 19-24, a dummy for 25 and over, and no children are also included. We specify $\hat{\gamma}$ as the reduced form estimate from the zero-first-stage sample. For the plausibly exogenous sepcifications, we set $\Omega_\gamma = (.125\sqrt{S_0^2 + S_{-0}^2})^2$, where S_0 is the standard error on $\hat{\gamma}$ for the zero-first-stage sub-sample and S_{-0} is the equivalent for the remainder of the sample. γ^* is the value of γ that yields a coefficient on being married equal to zero.

Overall, we interpret the results of the plausibly exogenous approach as strong evidence that the patterns we derive from the core analysis are robust to wide-ranging violations of the exclusion restriction.

4 Mechanisms

In this section, we assess the extent to which the theories proposed to rationalize the marriage wage premium are consistent with the data. We start with the hypothesis that the MWP is a byproduct

of household specialization among spouses as proposed by Becker (1985, 1993). Then, we look at the mechanisms in Pilossoph and Wee (2021), which pose that higher wages for married individuals steam from different job-search behavior between married and single individuals. Starting with Hill (1979) and Bartlett and Callahan (1984), some have hypothesized that the marriage wage premium could result from positive employer discrimination. We carefully test this hypothesis in a companion paper (McConnell and Valldares-Esteban, 2023). Using the NLSY, we document that individuals with higher cognitive and non-cognitive skills are more likely to be married. This fact opens the possibility for employers to use martial status, a characteristic that is easier to observe than skills, to statistically discriminate against workers. We expand the empirical strategies of Altonji and Pierret (2001) and Schönberg (2007) – Pinkston (2009) to the case of marriage but find no evidence to support the discrimination hypothesis. In fact, the results indicate that the marriage wage premium increases with labor market experience, the opposite of what statistical discrimination implies.

4.1 Household Specialization

The household specialization theory in Becker (1985, 1993) aims to explain the division of labor within couples. Becker (1985) briefly discusses the implications of the theory for the wage differences between married and single individuals, although this is not one of the central predictions. Becker's theory can be described as follows. Each individual, single or married, allocates their limited effort among different activities. To simplify, let us consider three activities: market work, home production, and leisure. The central insight of Becker's theory is that when individuals are part of a couple, there are gains in trading the fruits of market work and home production between the spouses. Therefore, the optimal strategy is for each spouse to specialize in one activity. It is important to note two implications of Becker's theory. First, the optimal degree of specialization depends on the relative comparative advantages between the two spouses. For two identical individuals, the degree of specialization, i.e., the relative time devoted to market work vis-à-vis home production, is lower than for a couple in which one member has a relative advantage in market work while the other member has an advantage in home production. Secondly, when couples are allowed to exchange the fruits of their work, the incentive to specialize also implies an incentive to produce more, as the possibility boundary or production-possibility frontier shifts outwards. Therefore, Becker's theory predicts that married couples produce more (market work plus home production), i.e., enjoy less leisure than their single counterparts. The natural implication of Becker's theory for the MWP is that the spouse specialized in market work devotes more effort to their carrier than a comparable single. This higher investment naturally leads to higher wages.

Figure 2 presents the evolution of time allocation patterns among married couples over time. On the left panel (2a), we plot the difference in yearly market hours between husbands and wives using CPS data. While in the late 1970s, husbands worked an average of around 1,300 hours per year more than wives, this difference has steadily decreased over time. In the late 2010s, husbands work around 700 more hours per year on average than wives. On the right panel (2b), we use ATUS data to compute the difference in time spent in home production between husbands and wives on an average week. We follow the classification of activities in Aguiar and Hurst (2007) and consider four broad categories: market work, non-market work, childcare, and leisure. We label home production the time allocated to non-market work and childcare. Husbands spend less time in home production activities than wives. However, the difference has been reducing over time. In the late 1970s, wives devoted over two hours a day more to home production than husbands, while in the 2010s, the difference has reduced to around 80 minutes. The fact that the average husband provides more market work (relative to the average wife), while the average wife spends more time on home production (relative to the average husband) is consistent with the household specialization model by Becker (1985, 1993). At the same time, the fact that the time allocation patterns between husbands and wives are converging indicates that the intensity of specialization has declined

over time.[28]

Figure 2: Household Specialization Has Decreased Over Time

(a) Market Work (hours per year)   (b) Home Production (minutes per day)
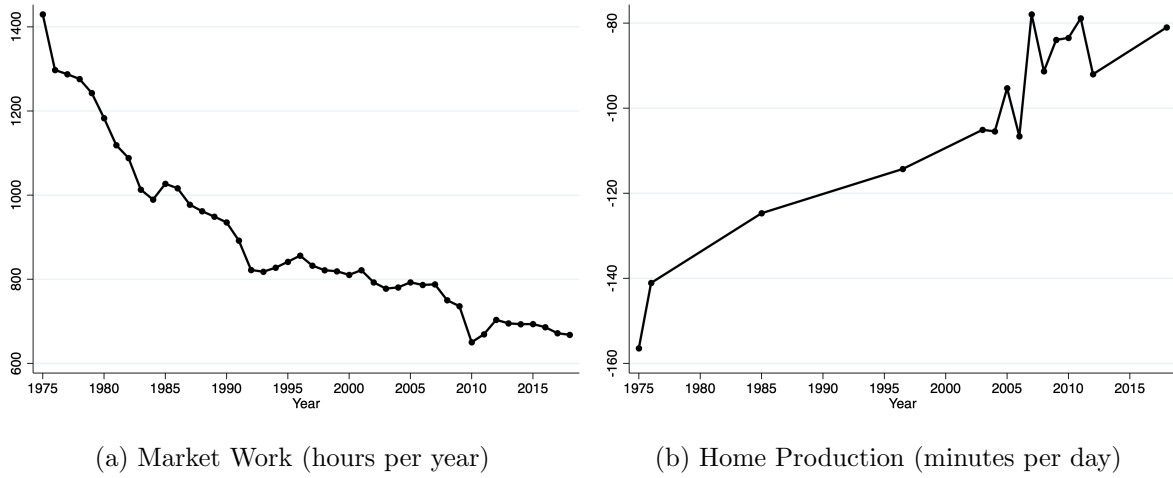
Notes: Data used for annual hours worked: CPS, 1975-2018. Data used for home production: ATUS, 1975-2018.

Having established that there is specialization among spouses, we now examine how it manifests in differences between married and single individuals. In Table 10, we estimate the difference in annual hours worked between married and single men over time. To ease comparison to the main MWP results in Section 3.4, we use a similar strategy to that presented in Section 3.2. In Table 10, the main dependent variable is annual hours worked instead of hourly wages. We present three sets of coefficients: OLS (first row), complier-reweighted OLS (second row), and 2SLS (third row). The three sets of coefficients indicate that married men work more hours than single men. In the late 1970s and early 1980s, the difference was around 3.6 to 5.5 hours a week, while in the 2010s, the difference was around 3 to 3.5 hours a week.[29] These results are consistent with the hypothesis that household specialization leads the spouse specialized in market work, the husband, to devote more resources to his career than his single counterparts. It is worth noting that in Becker (1985, 1993), agents allocate effort, not hours. That is, workers with the same amount of hours might end up with different wages because they put different amounts of effort into their jobs.[30] We focus on hours worked in the market as it is the best empirical counterpart to effort in the theory.

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[28]There is an extensive literature studying the evolution of time allocation within married couples over time. See Greenwood et al. (2017) for a review.

[29]Ahituv and Lerman (2007) and Olivo-Villabrille (2022) also find that being married leads men to work more market hours.

[30]Korenman and Neumark (1991) have access to personnel data from a company and find that married men consistently receive higher job performance ratings. Because it is natural to think that more effort leads to higher performance, the results in Korenman and Neumark (1991) support the implications of Becker's theory.

Table 10: Being Married Increases the Market Hours Worked by Men

	(1)	(2)	(3)	(4)	(5)	(6)
	1977-1983	**1984-1990**	**1991-1997**	**1998-2004**	**2005-2011**	**2012-2018**
OLS						
Married	191.2***	191.4***	198.4***	174.8***	180.7***	165.2***
	(8.8)	(7.9)	(7.7)	(6.9)	(9.5)	(7.2)
CW-OLS:						
Married	189.4***	190.3***	198.4***	170.4***	177.0***	160.6***
	(7.7)	(8.2)	(8.0)	(7.3)	(10.0)	(7.3)
2SLS:						
Married	284.0***	207.4***	209.2***	196.8***	213.8***	178.7***
	(20.7)	(15.4)	(17.4)	(15.5)	(17.0)	(23.9)
First-Stage F-Statistic	1461.3	2116.8	1304.2	1425.5	1426.4	1128.9
Adjusted R^2	0.1	0.1	0.1	0.1	0.1	0.1
Observations	104,970	104,545	96,210	112,807	120,606	94,896

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the total hours worked in the previous year. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. For the complier reweighted regressions, we first separate each sample into six mutually exclusive groups based on education and age, as outlined in Appendix Section B.2.2. Then, we estimate the proportion of compliers in each sub-group and then reweight the main estimation samples so that the complier proportion in each of the six sub-groups matches the proportion of the main sample for the same sub-group.

The theory in Becker (1985, 1993) is agnostic about the mechanisms that might bring higher wages given higher effort. One candidate to link higher hours to higher wages is experience or human capital in a learning-by-doing framework. Both Eckstein et al. (2019) and Blandin et al. (2023) build structural models in which this mechanism is a crucial determinant of the MWP of men. Both models highlight the dynamic nature of the mechanism. Early in the lifecycle, married men start working more than their single counterparts. As time progresses, the stock of human capital accumulated (or experience) grows faster for married men than for their single counterparts. Conditional on human capital (or experience), the wages of married men are not higher than those of single. However, marriage *causes* higher human capital or experience and, therefore, wages. This dynamic mechanism presents an important implication for empirical work on whether the regressions measuring the MWP should have detailed information on accumulated experience as controls. Higher experience is a causal implication of marriage. In our framework, we proxy experience with years after education. Hence, the MWP we measure includes the effects on additional human capital accumulation that might accrue given the differential in hours worked.

Table 11 presents the differences in hours worked between married and single women. In the late 1970s and early 1980s, single women worked between 3.6 and 12.5 hours per week less than single women. This gap has decreased drastically over the last four decades. By the 2010s, married women worked between 0.2 and 1.5 hours per week less than their single counterparts. Moreover, the OLS and 2SLS coefficients in 2012-2018 are not statistically different from zero, suggesting that the difference in hours worked between married and single women has virtually disappeared.

The marriage wage penalty for women we observe in the late 1970s and its subsequent reduction is consistent with the convergence in market hours between single and married women. Using Becker's theory, we can rationalize most patterns we observe in the data as follows. At the beginning of the sample period, in the late 1970s and early 1980s, the average married couple presented a significant degree of specialization among spouses. This specialization led married men to work significantly more market hours than their single counterparts, while the opposite was true for women. These differences in market hours imply a wage premium for married men and a wage penalty for married women. As the degree of specialization within married couples decreases, the differences in market hours worked between married and single individuals also diminish. The convergence between married and single individuals

has been relatively small for men. Consequently, the wage premium for married men has only decreased slightly. However, married women have mostly caught up in terms of market hours relative to their single counterparts. This convergence is consistent with the disappearance of the wage penalty for married women. However, what Becker's theory cannot explain is why, nowadays, married women receive a wage premium while working almost as much as their single counterparts. We provide an explanation in the next section.[31]

Table 11: The Gap in Marked Hours Worked Between Single and Married Women Has Reduced Over Time

	(1)	(2)	(3)	(4)	(5)	(6)
	1977-1983	**1984-1990**	**1991-1997**	**1998-2004**	**2005-2011**	**2012-2018**
OLS						
Married	−216.6***	−144.0***	−83.8***	−81.7***	−28.8***	−11.5
	(13.2)	(8.1)	(8.1)	(7.4)	(8.1)	(7.3)
CW-OLS:						
Married	−191.9***	−129.1***	−72.4***	−78.9***	−23.5**	−13.5*
	(14.5)	(7.9)	(7.8)	(7.8)	(9.8)	(7.1)
2SLS:						
Married	−653.1***	−523.0***	−313.9***	−326.4***	−144.1***	−79.0
	(45.4)	(48.0)	(36.6)	(56.3)	(47.7)	(65.0)
First-Stage F-Statistic	361.9	478.3	291.4	425.6	378.0	283.9
Adjusted R^2	0.1	0.1	0.1	0.1	0.1	0.1
Observations	70,872	83,482	83,791	101,175	109,624	85,427

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the total hours worked in the previous year. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. For the complier reweighted regressions, we first separate each sample into six mutually exclusive groups based on education and age, as outlined in Appendix Section B.2.2. Then, we estimate the proportion of compliers in each sub-group and then reweight the main estimation samples so that the complier proportion in each of the six sub-groups matches the proportion of the main sample for the same sub-group.

4.2 Joint Household Search

Pilossoph and Wee (2021) propose a theory that can rationalize a marriage wage premium for both spouses due to different search behavior between single and married individuals. The theory of Pilossoph and Wee (2021) is built on two mechanisms. First, because spouses pool resources, they have higher reservation wages, especially when one spouse is employed, and the other is looking for a job. A higher reservation wage implies a willingness to wait for higher-paying jobs. Secondly, married individuals have an extra incentive to climb the job ladder, i.e., to look for better opportunities while employed, as a higher wage not only increases the current resources of the household but also incentivizes the spouse to find a higher-paying job by providing a higher reservation wage. Pilossoph and Wee (2021) demonstrate that these mechanisms can generate a marriage wage premium for men and women.

In this section, we examine these mechanisms and whether the data support their broad implications. Naturally, the sample period of interest is that in which we find a marriage premium for both men and women, that is, from 2005 to 2018. Therefore, all the data we use in this section comprises these years. We start examining by examining a crucial implication of the higher-reservation-wage mechanism. If higher reservation wages arise due to income pooling within the couple, the reservation wage should increase with spousal earnings. The empirical implication is that the marriage wage premium of newly employed individuals should increase with their partner's income. The CPS follows households for two

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[31]In Appendix Section C.1, we provide further evidence that the data is consistent with Becker's theory by showing that married individuals devote less time to leisure activities than their single counterparts.

four-month periods with an eight-month exclusion in between. Therefore, we can estimate the MWP of individuals who switch from not being employed to having a job and how it relates to their spouse's income. We present two types of job finders in Table 12. We estimate the coefficients in the second and fourth columns using all the individuals without a job in one month and employed in the next. In the first and third columns, we restrict to the subsample of individuals that have not been employed for one quarter, i.e., the maximum amount of time that the panel structure of the CPS allows us to observe. On the first row of Table 12, we present the OLS coefficient associated with being married, while on the second row, we interact being married with spousal earnings. Apart from this interaction term, the regression is as the wage equations 2 and 5.[32]

For recently employed men, being married is associated with a sizable married premium even when spousal earnings are null, i.e., the wife is not working. At the same time, as predicted by the higher-reservation-wage mechanism, the MWP increases with spousal income. For recently employed women, being married alone has a negligible effect on wages when spousal earnings are zero, while the MWP increases steeply with spousal income. The results in Table 12 are consistent with the higher-reservation-wage mechanism. However, how much the mechanism matters to understand the overall MWP of men and women is quite different. For men, being married is associated with a premium independently of spousal earnings, which is consistent with the premium for men being driven by another factor, like the household specialization mechanism discussed in Section 4.1. Instead, there is no direct evidence of another factor for women, as married women whose spouses have zero earnings do not exhibit a premium. Although there is evidence of a higher-reservation-wage mechanism for both husbands and wives, the mechanism is likely playing a major role in determining the overall MWP for women than for men, as the fraction of non-employed husbands is much lower than that of wives. Among married men, the rate of non-employment is around 7% between 2005 and 2018, while it is around 26% for married women. In other words, much fewer husbands are in a position to benefit from the higher-reservation-wage mechanism than wives.

Table 12: The MWP of Newly Employed Individuals Increases With Spousal Earnings

	Men		Women	
	Not Employed in Past Quarter	Not Employed in Past Month	Not Employed in Past Quarter	Not Employed in Past Month
Married	0.122***	0.111***	0.010	−0.006
	(0.013)	(0.023)	(0.011)	(0.016)
Married×Spousal Earnings	0.064***	0.069***	0.055***	0.055***
	(0.015)	(0.023)	(0.011)	(0.015)
Adjusted R^2	0.176	0.177	0.213	0.202
Observations	20,253	8,154	21,965	8,954

Notes: Data used: CPS, 2005-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the total hours worked in the previous year. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18.

We further examine the higher-reservation-wage mechanism in Appendix Section C.2. In particular, we analyze the time devoted to job search between non-employed married and single individuals. The results indicate that the mechanism is likely to play a stronger role for women than for men. We also examine the implications of the job-ladder-climbing mechanism broadly understood, but we do not find any support for the mechanism. Married individuals do not switch jobs more often than their single counterparts. Moreover, husbands and wives do not devote more time to job searching while employed

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[32]We restrict the attention to OLS estimates because the presence of spousal earnings generates a host of endogeneity issues that our framework is not designed to address.

than their single counterparts.

5   Conclusions

We present a novel instrument based on social norms towards marriage to study the causal relationship between being married and hourly wages for men and women over the last decades. Despite the significant changes in the family and the labor market, the marriage wage premium for men has remained sizable and has only slightly declined. For women, the effect of being married has evolved from a penalty to a premium. We provide ample evidence to back up the assumptions behind the instrumental variable strategy. We pay particular attention to the exclusion restriction. We show that the instrument is successful in two placebo treatments, height and nativity, which are likely to involve similar confounders to those in the main setup with wages. Further, we show that the qualitative patterns of the causal effect of being married on wages are robust to ample violations of the exclusion restriction.

We assess the main theories proposed to account for the marriage wage premium. We find that household specialization à la Becker (1985, 1993) can rationalize the existence of a premium for men, its slight decline over time, and the disappearance of the marriage penalty for women. At the beginning of the sample period, the late 1970s and early 1980s, the degree of specialization among spouses implied that married men worked significantly more market hours than their single counterparts, while the opposite was true for women. These differences in market hours likely led to a premium for men and a penalty for women. Over time, the degree of specialization among married couples diminished. Notably, the decline in specialization has led to only a small reduction in the gap in market hours between married and single men. In contrast, the difference between married and single women has faded. The convergence in hours worked by married and single women can rationalize the disappearance of the penalty for women but cannot explain the current marriage wage premium. We find that the premium for women is likely related to job-search behavior. The data is consistent with the mechanism in Pilossoph and Wee (2021) posing that married women can wait longer for better-paying jobs as their husbands' income increases their reservation wage.

The results we present provide a consistent set of facts to continue developing the models of the household and the new literature on the joint search behavior of married couples. Our analysis suggests that the marriage wage premium is a consequence of both the time allocation patterns induced by household specialization and joint-search incentives. We do not quantify how much of the observed premium can be accounted for each mechanism, but a model unifying these two theories could be used to that end. Further, we do not explore how the marriage wage premium interacts with fertility and carrier interruptions. The marriage wage premium and the motherhood penalty need to be reconciled by developing a more comprehensive theory and empirical tests. The effect of the different dimensions of the family on wages is crucial to understand the effect of many public policies ranging from the taxation of household income to the provision of unemployment insurance.

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Appendix

A Robustness

A.1 Alternative Definition of the Unmarried Group

As discussed in Section 2, the literature defines the MWP as the difference in wages between married individuals and those who are never married. That is, separated, divorced, and widowed individuals are not included in the unmarried group. This section shows that the main results are robust to including these individuals in the sample.

In Table A1, we compare the main results for men of Table 4, in rows labeled "Baseline", with those obtained with the sample that includes separated, divorced, and widowed men, in rows named "Alternative". As with the core sample, the MWP for men is sizable and has only slightly decreased over time. When the sample includes separated, divorced, and widowed men, the MWP is around 15-20% lower than when the unmarried group includes only never-married individuals. This lower MWP suggests that the effect of being married on men's wages might have lasting effects that might outlive marriage. This lasting effect is consistent with the household-specialization mechanism discussed in Section 4.1. Being married leads men to accumulate more experience/human capital than their single counterparts. This higher experience/human capital level does not vanish with separation, divorce, or the spouse's passing.

Table A1: The Main Results for Men Are Robust to the Inclusion of Separated, Divorced, and Widowed Individuals in the Unmarried Group

	(1)	(2)	(3)	(4)	(5)	(6)
	1977-1983	**1984-1990**	**1991-1997**	**1998-2004**	**2005-2011**	**2012-2018**
OLS:						
Baseline	0.213***	0.208***	0.214***	0.209***	0.222***	0.200***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Alternative	0.171***	0.172***	0.181***	0.181***	0.189***	0.174***
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)
CW-OLS:						
Baseline	0.201***	0.195***	0.204***	0.198***	0.217***	0.197***
	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)
Alternative	0.167***	0.169***	0.177***	0.176***	0.187***	0.173***
	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)
2SLS:						
Baseline	0.306***	0.269***	0.246***	0.226***	0.266***	0.237***
	(0.013)	(0.015)	(0.013)	(0.016)	(0.017)	(0.021)
Alternative	0.278***	0.266***	0.248***	0.229***	0.282***	0.230***
	(0.011)	(0.014)	(0.014)	(0.016)	(0.018)	(0.022)
First-Stage F-Statistic	2619.0	1998.7	1225.1	1372.1	1309.5	1115.7
Adjusted R^2	0.192	0.232	0.253	0.242	0.256	0.255
Observations	114,160	116,523	109,127	127,932	135,660	106,526

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the natural log of wages. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. For the complier reweighted regressions, we first separate each sample into six mutually exclusive groups based on education and age, as outlined in Appendix Section B.2.2. Then, we estimate the proportion of compliers in each sub-group and then reweight the main estimation samples so that the complier proportion in each of the six sub-groups matches the proportion of the main sample for the same sub-group.

We present the baseline and alternative estimates for women in Table A2. In the specifications that do not take into account the endogeneity of marriage ("OLS", "Heckman", and "CW-Heckman"), the

coefficients imply a smaller penalty at the beginning of the sample and a lower premium at the end when we include separated, divorced, and widowed women in the sample. Instead, the causal coefficients ("2SLS-Heckman") indicate that the penalty was more prominent in the late 1970s and early 1980s, while the premium is larger in the last subperiod of the sample (2012-2018). These differences are also consistent with the mechanisms presented in Section 4. At the beginning of the sample period, being married led women to work fewer hours than their single counterparts, i.e., they accumulated less experience/human capital. That handicap did not disappear with the end of a marriage. At the end of the period, married women are already working virtually as much as singles, that is, there is no disadvantage in terms of experience/human capital related to being married. However, married women still have a higher reservation wage than their separated, divorced, and widowed counterparts, which allows them to find better-paying jobs.

Table A2: The Main Results for Women Are Robust to the Inclusion of Separated, Divorced, and Widowed Individuals in the Unmarried Group

	(1)	(2)	(3)	(4)	(5)	(6)
	1977-1983	**1984-1990**	**1991-1997**	**1998-2004**	**2005-2011**	**2012-2018**
OLS:						
Baseline	−0.053***	−0.001	0.031***	0.054***	0.070***	0.079***
	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)
Alternative	−0.031***	−0.005	0.026***	0.043***	0.052***	0.067***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Heckman:						
Baseline	−0.044***	0.000	0.033***	0.052***	0.068***	0.080***
	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)	(0.006)
Alternative	−0.019***	−0.000	0.018***	0.040***	0.050***	0.070***
	(0.007)	(0.008)	(0.006)	(0.005)	(0.004)	(0.006)
CW-Heckman:						
Baseline	−0.025***	0.019**	0.051***	0.061***	0.079***	0.087***
	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)
Alternative	−0.013*	0.011	0.039***	0.053***	0.065***	0.079***
	(0.007)	(0.008)	(0.006)	(0.007)	(0.005)	(0.007)
2SLS-Heckman:						
Baseline	−0.149***	−0.058	−0.018	−0.033	0.104***	0.086***
	(0.022)	(0.037)	(0.028)	(0.040)	(0.031)	(0.032)
Alternative	−0.209***	−0.077	−0.060	−0.053	0.098**	0.105**
	(0.044)	(0.057)	(0.050)	(0.054)	(0.041)	(0.044)
First-Stage F-Statistic	478.7	484.7	154.2	209.5	195.1	175.6
Adjusted R^2	0.130	0.198	0.231	0.224	0.225	0.228
Observations	86,641	102,490	102,775	124,193	132,973	102,415

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the natural log of wages. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. The exclusion restrictions for the employment equation are a series of dummies for the age of the youngest child in the household from 1-18, where age less than 1 is the base category, a dummy for ages 19-24, a dummy for 25 and over, and no children are also included. In this case, we bootstrap standard errors, allowing for clustering at the state level and using 500 iterations. For the complier reweighted regressions, we first separate each sample into six mutually exclusive groups based on education and age, as outlined in Appendix Section B.2.2. Then, we estimate the proportion of compliers in each sub-group and then reweight the main estimation samples so that the complier proportion in each of the six sub-groups matches the proportion of the main sample for the same sub-group.

A.2   Controlling for Children

A.2.1   Excluding Children-Related Controls

We compare the main results for men from Section 3.4 to those from specifications in which we do not include any control for the presence of children in the household in Table A3. The main results are in the

rows labeled "Baseline" while those from the specification that do not include children-related controls are in rows named "Alternative". In line with the literature that studies the effect of children on fathers' wages, we find that not controlling for the presence of children in the household leads to higher estimates of the MWP. Without controlling for children, the MWP for men is around 15-25% higher. However, the main pattern of the MWP for men described in Section 3.4 remains. The MWP for men is sizable and has only moderately decreased over the last decades.

Table A3: The Main Results for Men Are Robust to Excluding Children-Related Control Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	1977-1983	**1984-1990**	**1991-1997**	**1998-2004**	**2005-2011**	**2012-2018**
OLS:						
Baseline	0.213***	0.208***	0.214***	0.209***	0.222***	0.200***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Alternative	0.234***	0.228***	0.243***	0.241***	0.251***	0.226***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)
CW-OLS:						
Baseline	0.201***	0.195***	0.204***	0.198***	0.217***	0.197***
	(0.006)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)
Alternative	0.222***	0.215***	0.232***	0.231***	0.241***	0.219***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
2SLS:						
Baseline	0.306***	0.269***	0.246***	0.226***	0.266***	0.237***
	(0.013)	(0.015)	(0.013)	(0.016)	(0.017)	(0.021)
Alternative	0.314***	0.286***	0.299***	0.300***	0.316***	0.292***
	(0.009)	(0.012)	(0.010)	(0.012)	(0.012)	(0.012)
First-Stage F-Statistic	2204.648	3208.384	3735.189	3274.400	4583.162	3850.434
Adjusted R^2	0.196	0.236	0.260	0.246	0.265	0.262
Observations	104,970	104,545	96,210	112,807	120,606	94,896

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the natural log of wages. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment and dummies for years of potential experience. When children controls are included, these are the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. For the complier reweighted regressions, we first separate each sample into six mutually exclusive groups based on education and age, as outlined in Appendix Section B.2.2. Then, we estimate the proportion of compliers in each sub-group and then reweight the main estimation samples so that the complier proportion in each of the six sub-groups matches the proportion of the main sample for the same sub-group.

The same comparison for women is in Table A4. Because of the motherhood penalty, when we do not include controls for the presence of children in the household, the estimates imply a higher penalty at the beginning of the sample period and a lower premium at the end. Nonetheless, the key pattern implied by the main results in Section 3.4 is unchanged. The effect of being married on women's wages has evolved from a sizable penalty to a substantial premium.

Table A4: The Main Results for Women Are Robust to Excluding Children-Related Control Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	1977-1983	**1984-1990**	**1991-1997**	**1998-2004**	**2005-2011**	**2012-2018**
OLS:						
Baseline	−0.053***	−0.001	0.031***	0.054***	0.070***	0.079***
	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.006)
Alternative	−0.105***	−0.049***	0.001	0.031***	0.057***	0.077***
	(0.009)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
Heckman:						
Baseline	−0.044***	0.000	0.033***	0.052***	0.068***	0.080***
	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)	(0.006)
Alternative	−0.059***	−0.019**	0.011	0.035***	0.057***	0.071***
	(0.009)	(0.008)	(0.007)	(0.008)	(0.007)	(0.006)
CW-Heckman:						
Baseline	−0.025***	0.019**	0.051***	0.061***	0.079***	0.087***
	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)
Alternative	−0.043***	−0.000	0.029***	0.045***	0.070***	0.077***
	(0.009)	(0.008)	(0.007)	(0.009)	(0.007)	(0.007)
2SLS-Heckman:						
Baseline	−0.149***	−0.058	−0.018	−0.033	0.104***	0.086**
	(0.024)	(0.041)	(0.031)	(0.045)	(0.032)	(0.035)
Alternative	−0.192***	−0.123***	−0.084***	−0.092***	0.036*	0.067***
	(0.032)	(0.043)	(0.027)	(0.036)	(0.020)	(0.022)
First-Stage F-Statistic	301.841	505.448	405.901	429.022	477.665	475.824
Adjusted R^2	0.137	0.186	0.227	0.217	0.221	0.224
Observations	70,872	83,482	83,791	101,175	109,624	85,427

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the natural log of wages. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment and dummies for years of potential experience. When children controls are included, these are the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. The exclusion restrictions for the employment equation are a series of dummies for the age of the youngest child in the household from 1-18, where age less than 1 is the base category, a dummy for ages 19-24, a dummy for 25 and over, and no children are also included. In this case, we bootstrap standard errors, allowing for clustering at the state level and using 500 iterations. For the complier reweighted regressions, we first separate each sample into six mutually exclusive groups based on education and age, as outlined in Appendix Section B.2.2. Then, we estimate the proportion of compliers in each sub-group and then reweight the main estimation samples so that the complier proportion in each of the six sub-groups matches the proportion of the main sample for the same sub-group.

A.2.2 An Estimate of the Potential Bias Induced by Controlling for Children

We follow the approach of Angrist and Pischke (2009) to illustrate the effect that controlling for the presence of children might have on the estimates we present in Section 3.4. Let W_{0i} and W_{1i} be the respective potential outcomes of wages when unmarried ($M_i = 0$) and married ($M_i = 1$). Let K_{0i} and K_{1i} be the analogous potential outcomes for having children. Then, we can write:

$$W_i = M_i W_{1i} + (1 - M_i)W_{0i}$$
$$K_i = M_i K_{1i} + (1 - M_i)K_{0i}.$$

We illustrate the effect of being married on wages while controlling for children by conditioning on the presence of children ($K_i = 1$). The empirical strategy we present in Section 3.2 measures the effect of being married on wages by comparing the wages of the marred and unmarried while controlling for children, that is, $E[W_i \mid K_{1i} = 1, M_i = 1] - E[W_i \mid K_{0i} = 1, M_i = 0]$. However, this expression is actually

composed of two elements:

$$E[W_i \mid K_{1i} = 1, M_i = 1] - E[W_i \mid K_{0i} = 1, M_i = 0]$$
$$= E[W_{1i} \mid K_{1i} = 1] - E[W_{0i} \mid K_{0i} = 1]$$
$$= \underbrace{E[W_{1i} - W_{0i} \mid K_{0i} = 1]}_{\text{Causal Effect}} + \underbrace{E[W_{0i} \mid K_{1i} = 1] - E[W_{0i} \mid K_{0i} = 1]}_{\text{Selection Bias}},$$

where $E[W_{1i} - W_{0i} \mid K_{0i} = 1]$ is the causal effect of being married on wages, the object of interest in the analysis. In the case that $E[W_{0i} \mid K_{1i} = 1] - E[W_{0i} \mid K_{0i} = 1] \neq 0$, the estimates we obtain do not only reflect the causal effect of being married on wages but also include a bias. The critical issue is that we cannot directly quantify the selection bias term, as we do not observe the potential outcome W_{0i} for all individuals in the data. We observe this term only for the unmarried.

We use the data in the NLSY to compute an approximation of the selection bias term. The critical term we aim to approximate in this exercise is W_{0i}. We do so for *all* individuals, not just those who never marry. We start by following analogous sample restrictions and variable definitions to those described in Section 2 to obtain comparable estimates to those from the CPS sample. Next, we restrict the sample to individuals who have at least one child at some point in their life ($K_i = 1$). Further, we select wage observations only from periods in which the individuals are currently unmarried. That is, in the resulting sample, all wages are from individuals who are currently unmarried. However, we exploit the panel structure to divide them between those who will marry ($K_{1i} = 1$) and those who will not ($K_{0i} = 1$). Using this sample, we estimate the following regression equation using OLS:

$$y_i = \kappa \text{After}_i + X_i' \beta + \epsilon_i,$$

where y_i is the logarithm of hourly wages, After_i is a dummy variable that takes value one for those who have their first child after marriage, and X_i is a battery of controls which includes time and a complete set of age or experience dummies.

The results from this exercise are in Table A5. We estimate $\hat{\kappa}$ at 60, 48, 36, 24, and 12 months before marriage, as some of the effects of being married might occur before the official marriage date if there is unobserved cohabitation that leads to specialization or if transfers between the future spouses influence job-search behavior. We report the results for men in Panel A and those for women in Panel B. Further, we compute the coefficients using two different sets of controls. In panels labeled "i. Limited (CPS) Controls", we use a coarse set of controls analogous to those available in the CPS. Notably, we use age dummies to proxy labor market experience. Instead, in panels "ii. Extended (NLSY) Controls", we use the richer set of variables available in the NLSY, including dummies for years of actual experience. All the estimates we compute for both men and women are statistically not significantly different from zero. At the same time, all the point estimates are negative 60 months before marriage, while all are positive 12 months before marriage. We interpret these results, along with the coefficients we report in Table A3 and Table A4, as a solid indication that the main results are robust to not including dummies for the presence of children in the household as controls.

Table A5: There Is No Evidence of Selection Bias From Conditioning on Children

	(1)	(2)	(3)	(4)	(5)
	Log-wage Regressions, M Months Prior to Marriage				
	60 Months	48 Months	36 Months	24 Months	12 Months
A. Men					
i. Limited (CPS) Controls					
First Child After Marriage	−0.003	0.009	0.028	0.040	0.052
	(0.049)	(0.045)	(0.042)	(0.039)	(0.036)
Adjusted R^2	0.110	0.115	0.112	0.118	0.118
ii. Extended (NLSY) Controls					
First Child After Marriage	−0.029	−0.020	−0.002	0.011	0.023
	(0.046)	(0.042)	(0.039)	(0.037)	(0.034)
Adjusted R^2	0.183	0.179	0.167	0.167	0.161
$E[W_0|K_0 = 1]$	18.381	18.168	17.986	17.903	18.449
Observations	2,548	2,969	3,462	4,058	4,783
B. Women					
i. Limited (CPS) Controls					
First Child After Marriage	−0.027	−0.006	0.006	0.031	0.050
	(0.052)	(0.047)	(0.045)	(0.044)	(0.039)
Adjusted R^2	0.152	0.148	0.147	0.157	0.172
ii. Extended (NLSY) Controls					
First Child After Marriage	−0.000	0.014	0.026	0.042	0.052
	(0.046)	(0.042)	(0.040)	(0.038)	(0.035)
Adjusted R^2	0.226	0.207	0.211	0.211	0.222
$E[W_0|K_0 = 1]$	12.887	12.971	12.934	12.887	12.869
Observations	1,770	2,078	2,477	2,974	3,574

Notes: Data used: NLSY79. All individuals in the sample have at least one child. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the individual level and reported in parentheses. The controls under are dummies for age, region of residence in 1979, year, education level, and a dummy for living in an urban area. Under , dummies for years of actual experience are used instead of age. The following controls are added: dummies for the month of birth, maternal education categories, paternal education categories, an indicator if the mother was present at home and working when the respondent was 14, and if the father was present at home and working when the respondent was 14.

A.3   Industry and Occupation Controls

The empirical specification in Section 3.2 does not include industry and occupation controls for two reasons. First, correcting for women's employment selection would require information on the industry/occupation of the non-employed. For many non-employed workers, we do not have information on the industry/occupation of their last job. However, the more severe issue is that industry/occupation are ill-defined for the non-employed, as the process of finding a new job might involve a change of industry or occupation with respect to the last job. Secondly, we consider the choice of industry/occupation as potentially endogenous to the effect of being married. For example, consider the effect of household specialization discussed in Section 4.1. The market-specialized spouse might choose to work in an industry/occupation where long hours are rewarded more favorably. In contrast, the household-specialized spouse might decide to find a job in an industry/occupation where part-time work is not particularly penalized.

Despite these considerations, in Table A6, we present IV estimates for men and women that are compatible with including industry and occupation controls. For men, the empirical specification is analogous to that described in Section 3.2. For women, we cannot do not apply the sample-selection correction. Therefore we use the same specification as that of men. We present the estimates without industry/occupation controls in the row labeled "Baseline". For men, the coefficients are the same as those in Table 4 (row "2SLS"), while for women the coefficients are very similar to those in Table 5 as the

correction for sample-selection bias does not have a significant impact on the estimates. The following rows report the estimates while controlling for industry dummies only, occupation dummies only, and industry and occupation dummies simultaneously. The key inference we draw from the main results in Section 3.4 are robust to industry/occupation controls. Namely, the effect of being married on men's wages is significant in size and has only slightly decreased over time, and for women, there has been an evolution from a sizable penalty to a substantial premium.

Table A6: The Main Results Are Robust to the Inclusion of Industry and Occupation Controls

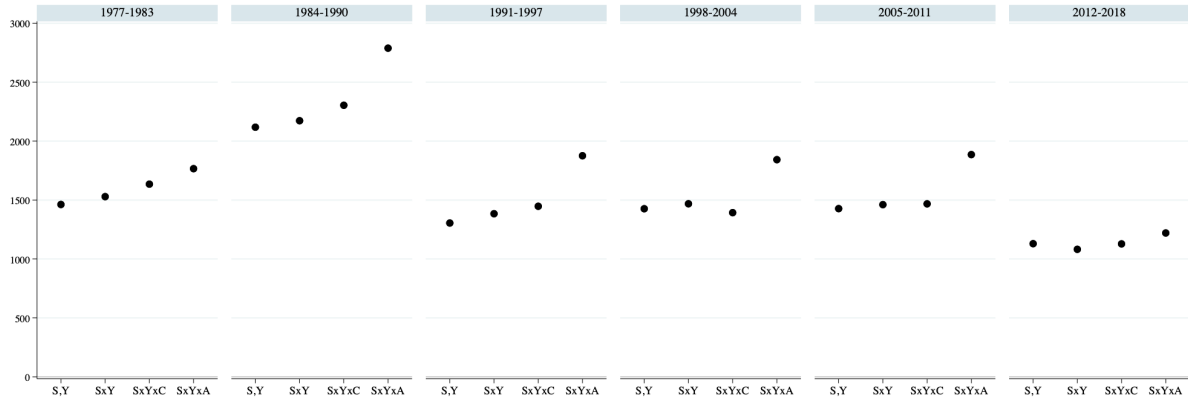|	(1)	(2)	(3)	(4)	(5)	(6)
	1977-1983	**1984-1990**	**1991-1997**	**1998-2004**	**2005-2011**	**2012-2018**
A. Men: 2SLS						
Baseline	0.306***	0.269***	0.246***	0.226***	0.266***	0.237***
	(0.013)	(0.015)	(0.013)	(0.016)	(0.017)	(0.021)
+ Industry	0.274***	0.227***	0.214***	0.196***	0.230***	0.199***
Groups	(0.011)	(0.012)	(0.013)	(0.016)	(0.017)	(0.021)
+ Occupation	0.284***	0.250***	0.227***	0.216***	0.257***	0.233***
Groups	(0.012)	(0.014)	(0.012)	(0.015)	(0.016)	(0.020)
+ Industry	0.251***	0.215***	0.201***	0.189***	0.224***	0.200***
& Occupation	(0.011)	(0.012)	(0.012)	(0.015)	(0.016)	(0.020)
Observations	104,969	104,545	96,209	112,807	120,606	94,896
B. Women: 2SLS						
Baseline	−0.159***	−0.061	−0.030	−0.034	0.078***	0.088***
	(0.025)	(0.045)	(0.029)	(0.044)	(0.027)	(0.032)
+ Industry	−0.138***	−0.063*	−0.038	−0.037	0.040	0.051*
Groups	(0.026)	(0.037)	(0.028)	(0.040)	(0.028)	(0.029)
+ Occupation	−0.166***	−0.070*	−0.044*	−0.046	0.058**	0.090***
Groups	(0.023)	(0.041)	(0.025)	(0.039)	(0.027)	(0.031)
+ Industry	−0.136***	−0.064*	−0.042*	−0.039	0.034	0.063**
& Occupation	(0.023)	(0.035)	(0.024)	(0.037)	(0.028)	(0.029)
Observations	70,871	83,482	83,789	101,175	109,624	85,427

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the natural log of wages. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment and dummies for years of potential experience. When children controls are included, these are the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. Additional industry control variables are comprised of a set of 14 industry group dummies. Additional occupational controls are comprised of a set of 7 industry group dummies.

We take a parsimonious approach to the inclusion of industry and occupation controls. For industry, we use the 1990 industry codes supplied by the CPS and construct fourteen categories of industries. These are: Agriculture, forestry, and fisheries; Mining; Construction; Manufacturing; Transportation, communications, and other public utilities; Wholesale trade; Retail trade; Finance, insurance, and real estate; Business and repair services; Personal services; Entretainment and recreation services; Professional and related services; Public administration; Active duty military. For occupation, we use the 1990 occupation code supplied by the CPS and construct seven categories of occupations: Managerial and professional specialty; Technical, sales, and administrative support; Service occupations; Farming, forestry, and fishing; Precision production, craft, and repair; Operators, fabricators, and laborers; Military occupations.
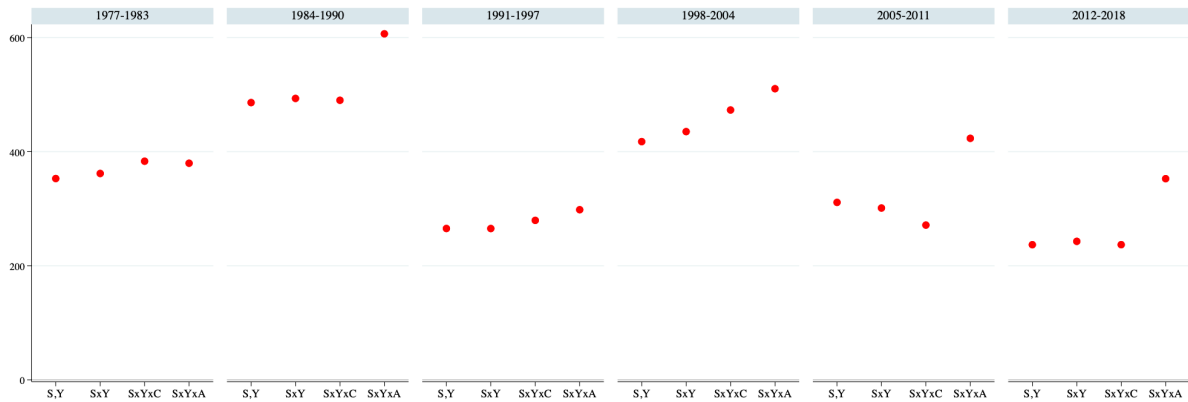
A.4 F-Test statistics From Fixed-Effects Robustness

In this section, we present the F-test statistics associated with the coefficients in Figure 1. All fixed-effects specifications are associated with robust first stages.

Figure A1: first stage f-stats need to be put somewhere

(a) 2SLS Coefficients, Men

(b) 2SLS-Heckman Coefficients, Women

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the natural log of wages. We present 4 sets of fixed effects specifications: the baseline specifation of state and year fixed effects (labelled S,Y), state-by-year fixed effects (labelled SxY), state-by-year-by-college fixed effects (labelled SxYxC), and state-by-year-by-age-group fixed effects (labelled SxYxA). The age groups used in the third spefication comprise six groups, each spanning five years of age. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. For all IV-Heckman specifications, the exclusion restrictions for the employment equation are a series of dummies for the age of the youngest child in the household from 1-18, where age less than 1 is the base category, a dummy for ages 19-24, a dummy for 25 and over, and no children are also included.

B   More on the Instrumental Variable Setup

B.1   Support for the Identifying Assumptions

Figure B1: First Stage Relationship Between Being Married and the Proportion of Married Individuals in Reference Group for Men

(a) 1977-1983.   (b) 1984-1990.   (c) 1991-1997.

(d) 1998-2004.   (e) 2005-2011.   (f) 2012-2018.

Notes: Data used: CPS, 1977-2018. The solid lines are local linear regressions of residualized being married dummy variable on the residualized instrument, a flexible version of the first-stage 2SLS equation. Both the being married dummy and the instrument are residualized on year and state fixed effects, dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. The dashed lines are 95% confidence intervals. The histogram of the (residualized) instrument is shown in the background, with the top and bottom 2% excluded from the figure.

Figure B2: First Stage Relationship Between Being Married and the Proportion of Married Individuals in Reference Group for Women
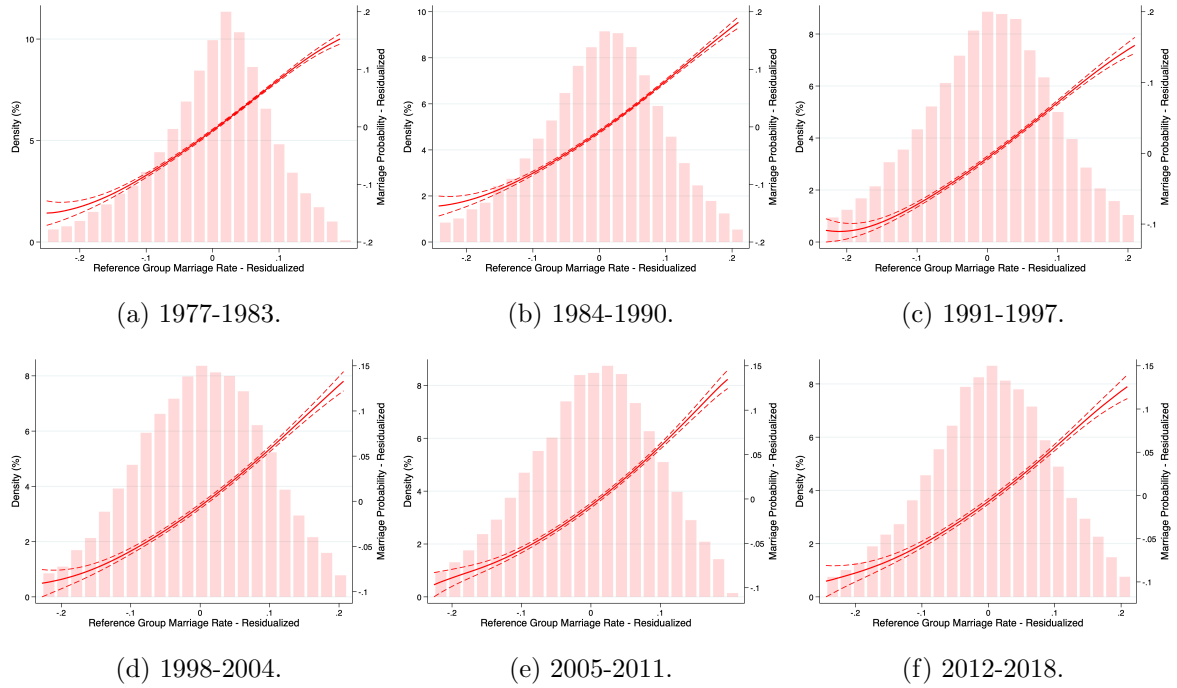
(a) 1977-1983.

(b) 1984-1990.

(c) 1991-1997.

(d) 1998-2004.

(e) 2005-2011.

(f) 2012-2018.

Notes: Data used: CPS, 1977-2018. The solid lines are local linear regressions of residualized being married dummy variable on the residualized instrument, a flexible version of the first-stage 2SLS equation. Both the being married dummy and the instrument are residualized on year and state fixed effects, dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. We include the Inverse Mills Ratio as a covariate. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. The dashed lines are 95% confidence intervals. The histogram of the (residualized) instrument is shown in the background, with the top and bottom 2% excluded from the figure.

Table B1: Alternative 2SLS First-Stage Specifications

Men	1998-2004	1998-2004	2005-2011	2005-2011	2012-2018	2012-2018	2012-2018
Instrument	0.832***	0.830***	0.824***	0.822***	0.758***	0.758***	0.758***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.023)	(0.023)	(0.023)
U.S. Born		−0.029***		−0.041***		−0.045***	−0.045***
		(0.006)		(0.007)		(0.007)	(0.007)
Health Status:							
Excellent		0.051***		0.048***		0.036***	0.035***
		(0.003)		(0.004)		(0.005)	(0.005)
Very Good		0.030***		0.033***		0.025***	0.025***
		(0.003)		(0.004)		(0.004)	(0.004)
Fair		−0.010		−0.014		−0.019**	−0.018**
		(0.007)		(0.010)		(0.008)	(0.008)
Poor		−0.038*		−0.013		−0.026	−0.020
		(0.020)		(0.020)		(0.024)	(0.025)
Difficulty:							
Hearing							−0.022
							(0.013)
Vision							−0.068***
							(0.020)
Physical							−0.023*
							(0.013)
p-value: Extra Covariates = 0	-	0.000	-	0.000	-	0.000	0.000
Observations	112,807	112,807	120,606	120,606	94,896	94,896	94,896

Women	1998-2004	1998-2004	2005-2011	2005-2011	2012-2018	2012-2018	2012-2018
Instrument	0.524***	0.526***	0.519***	0.528***	0.471***	0.471***	0.474***
	(0.026)	(0.026)	(0.029)	(0.029)	(0.031)	(0.031)	(0.031)
U.S. Born		−0.086***		−0.123***		−0.161***	−0.155***
		(0.008)		(0.011)		(0.012)	(0.013)
Health Status:							
Excellent		0.049***		0.032***		0.015**	0.020***
		(0.004)		(0.005)		(0.006)	(0.006)
Very Good		0.018***		−0.001		−0.010*	−0.004
		(0.005)		(0.005)		(0.006)	(0.006)
Fair		0.056***		0.103***		0.152***	0.119***
		(0.011)		(0.013)		(0.015)	(0.012)
Poor		0.215***		0.306***		0.382***	0.299***
		(0.025)		(0.029)		(0.049)	(0.044)
Difficulty:							
Hearing							−0.026
							(0.027)
Vision							0.056*
							(0.028)
Physical							0.166***
							(0.029)
p-value: Extra Covariates = 0	-	0.000	-	0.000	-	0.000	0.000
Observations	101,175	101,175	109,624	109,624	85,427	85,427	85,427

Notes: Data used: CPS, 1998-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is a dummy for being married. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. For women, we include the Inverse Mills Ratio as a covariate. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. The *p*-value is from a joint test of the statistical significance of all additional covariates.

Table B2: 2SLS First Stage for Various Sub-Samples

		Predicted Married		**Region**				**Age**		**College**	
A. Men	(1) Full Sample	(2) Below Median	(3) Above Median	(4) North-East	(5) Mid-West	(6) South	(7) West	(8) 25-39	(9) 40-54	(10) No	(11) Yes
1977–1983	0.839***	0.509***	0.163***	0.855***	0.813***	0.760***	0.939***	0.819***	1.115***	0.861***	0.815***
	(0.022)	(0.042)	(0.029)	(0.019)	(0.016)	(0.027)	(0.037)	(0.022)	(0.100)	(0.025)	(0.034)
1984–1990	0.906***	0.465***	0.230***	0.911***	0.909***	0.866***	0.964***	0.893***	0.840***	0.935***	0.875***
	(0.020)	(0.032)	(0.040)	(0.023)	(0.013)	(0.043)	(0.062)	(0.020)	(0.051)	(0.019)	(0.031)
1991–1997	0.867***	0.308***	0.247***	0.967***	0.872***	0.796***	0.839***	0.863***	0.875***	0.878***	0.856***
	(0.024)	(0.039)	(0.036)	(0.020)	(0.047)	(0.042)	(0.055)	(0.020)	(0.073)	(0.026)	(0.027)
1998–2004	0.832***	0.252***	0.277***	0.889***	0.812***	0.777***	0.885***	0.817***	0.944***	0.819***	0.832***
	(0.022)	(0.029)	(0.026)	(0.032)	(0.026)	(0.028)	(0.067)	(0.022)	(0.052)	(0.026)	(0.030)
2005–2011	0.824***	0.208***	0.382***	0.856***	0.812***	0.794***	0.844***	0.788***	0.979***	0.766***	0.851***
	(0.022)	(0.045)	(0.033)	(0.063)	(0.017)	(0.024)	(0.060)	(0.020)	(0.050)	(0.022)	(0.027)
2012–2018	0.758***	0.216***	0.529***	0.766***	0.729***	0.765***	0.791***	0.697***	0.903***	0.648***	0.802***
	(0.023)	(0.038)	(0.044)	(0.041)	(0.020)	(0.031)	(0.088)	(0.024)	(0.049)	(0.022)	(0.026)
B. Women											
1977–1983	0.746***	0.247***	0.038**	0.790***	0.713***	0.613***	0.875***	0.728***	0.759***	0.790***	0.653***
	(0.040)	(0.039)	(0.016)	(0.055)	(0.043)	(0.058)	(0.140)	(0.041)	(0.125)	(0.045)	(0.041)
1984–1990	0.739***	0.194***	0.059***	0.842***	0.737***	0.638***	0.724***	0.740***	0.574***	0.772***	0.692***
	(0.034)	(0.039)	(0.016)	(0.069)	(0.040)	(0.054)	(0.118)	(0.034)	(0.097)	(0.038)	(0.039)
1991–1997	0.661***	0.174***	0.066***	0.797***	0.631***	0.590***	0.600***	0.717***	0.453***	0.681***	0.636***
	(0.041)	(0.034)	(0.016)	(0.065)	(0.053)	(0.070)	(0.144)	(0.037)	(0.096)	(0.045)	(0.048)
1998–2004	0.524***	0.128***	0.093***	0.583***	0.462***	0.466***	0.558***	0.574***	0.418***	0.388***	0.541***
	(0.026)	(0.032)	(0.017)	(0.050)	(0.047)	(0.045)	(0.048)	(0.027)	(0.054)	(0.037)	(0.025)
2005–2011	0.519***	0.239***	0.088***	0.585***	0.514***	0.487***	0.456***	0.561***	0.410***	0.297***	0.553***
	(0.029)	(0.030)	(0.019)	(0.049)	(0.060)	(0.040)	(0.092)	(0.031)	(0.053)	(0.028)	(0.037)
2012–2018	0.471***	0.171***	0.193***	0.553***	0.401***	0.480***	0.444***	0.440***	0.422***	0.230***	0.479***
	(0.031)	(0.037)	(0.021)	(0.037)	(0.021)	(0.048)	(0.114)	(0.033)	(0.056)	(0.038)	(0.034)

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is a dummy for being married. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. For women, we include the Inverse Mills Ratio as a covariate. Columns (2) and (3) present results based on whether individuals were above or below the median based on predicted marriage. This procedure involves running a linear probability model of the being married dummy on all key covariates but not the instrument.

B.2 The Complier Sample

The estimates we present in Section 3.4 reflect the average causal effect of being married on wages for the compliers. While it is infeasible to identify the individual compliers, we can characterize the compliant sub-population by calculating its size and certain observable characteristics of this group. We follow the approach in Dahl et al. (2014).

B.2.1 Complier Types

In this section, we calculate the fraction of compliers (those whose marriage decision was impacted by their value of $Z_{M,i}$), always takers (those who would marry irrespective of their value of $Z_{M,i}$), and never takers (those who would never marry irrespective of their value of $Z_{M,i}$). For compliers, we can write their proportion as:

$$\pi_c \equiv Pr(M_i = 1 | Z_{M,i} = \overline{Z}) - Pr(M_i = 1 | Z_{M,i} = \underline{Z}) = Pr(M_i(\overline{Z}) > M_i(\underline{Z})), \tag{10}$$

where $\underline{Z}$ and $\overline{Z}$ are the minimum and maximum values of the instrument, respectively. By conditional independence and monotonicity, we can also write the proportion of always-takers:

$$\pi_a \equiv Pr(M_i = 1 | Z_{M,i} = \underline{Z}) = Pr(M_i(\overline{Z}) = M_i(\underline{Z}) = 1), \tag{11}$$

and the proportion of never-takers:

$$\pi_n \equiv Pr(M_i = 1 | Z_{M,i} = \overline{Z}) = Pr(M_i(\overline{Z}) = M_i(\underline{Z}) = 0). \tag{12}$$

Table B3 presents these proportions of compliers for each sample, using both a local linear and a linear model, and using a variety of definitions for the values of $\underline{Z}$ and $\overline{Z}$. The local linear model is a flexible version of the first stage equation, Equation 1 for men's non-selection corrected 2SLS approach and Equation 4 for women's selection-corrected counterpart. After residualizing both marriage and the instrument with respect to the control variables, we run a local linear regression of residualized marriage on the residualized instrument.[33]

We can also calculate these proportions using a linear model (i.e., Equations 1 and 4). In this case, we use the parameters from the first stage regression to calculate $\pi_c = \hat{\pi}_1(\overline{Z} - \underline{Z})$, $\pi_a = \hat{\pi}_{2,0} + \hat{\pi}_1\underline{Z}$ and $\pi_n = 1 - \hat{\pi}_{2,0} - \hat{\pi}_1\overline{Z}$, where $\hat{\pi}_{2,0}$ is the first stage constant and $\hat{\pi}_1$ the coefficient on the instrument. The proportion of compliers is typically larger when using the linear model. The impact of the instrument is broadly linear but does taper off towards the higher values. The local linear model captures this feature, while the linear model does not.

It should be noted that the calculated proportion of compliers is large, a consequence of the fact that the instrument is age-dependent. The exercise implicit in the calculations involves giving an individual the lowest and highest levels of the instrument, $\underline{Z}$ and $\overline{Z}$, and tracing out the impacts on marriage decisions. This exercise can never fully map to reality, as it involves changing the ages (and education levels) of individuals in order that they are exposed to a different reference group.

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[33]Figure B1 and Figure B2 present the local linear regression relation between residualized marriage and local social norms in marriage underlying these calculations.

Table B3: Sample Share of Compliers

Model:	Local Linear			Linear		
Top/Bottom:	(1) 1%	(2) 2%	(3) 5%	(4) 1%	(5) 2%	(6) 5%
Men						
1977 - 1983	0.54	0.49	0.42	0.60	0.52	0.42
1984 - 1990	0.60	0.53	0.44	0.63	0.56	0.44
1991 - 1997	0.58	0.51	0.42	0.60	0.53	0.42
1998 - 2004	0.51	0.45	0.38	0.55	0.48	0.39
2005 - 2011	0.51	0.45	0.37	0.54	0.47	0.38
2012 - 2018	0.46	0.41	0.34	0.50	0.44	0.35
Women						
1977 - 1983	0.35	0.31	0.25	0.39	0.33	0.24
1984 - 1990	0.36	0.32	0.26	0.39	0.33	0.26
1991 - 1997	0.29	0.27	0.23	0.34	0.29	0.23
1998 - 2004	0.24	0.21	0.18	0.27	0.23	0.18
2005 - 2011	0.22	0.20	0.17	0.25	0.22	0.17
2012 - 2018	0.18	0.18	0.17	0.25	0.21	0.17

Notes: Data used: CPS, 1977-2018. The dependent variable in Columns (1)-(3) is the residualized dummy for being married. In Columns (4)-(6) is a dummy for being married. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. For women, we include the Inverse Mills Ratio as a covariate. In Columns (1)-(3), the instrument is the residualized marriage rate in the reference group of each individual. In Columns (4)-(6), the instrument is the marriage rate in the reference group of each individual. For women, the exclusion restrictions for the employment equation are a series of dummies for the age of the youngest child in the household from 1-18, where age less than 1 is the base category, a dummy for ages 19-24, a dummy for 25 and over, and no children are also included. The local linear approach uses an Epanechnikov kernel with a bandwidth of .15 and a polynomial of degree 2.

B.2.2 Characterizing Compliers

Although we cannot identify compliers individually, we can characterize the compliant sub-population. This exercise is useful, given that we can only identify the 2SLS estimate of the MWP based on the compliers rather than the entire sample. The statistic of interest to characterize the compliers is $\frac{P[X=x|complier]}{P[X=x]}$, which describes how much more likely a complier is to have a given characteristic compared to the sample as a whole. In order to calculate the numerator, we calculate several ancillary statistics:

$$ P[X = x|complier] = \frac{P[complier|X = x] \times P[X = x]}{P[complier]}, \tag{13} $$

where $P[complier] = \hat{\pi}_1(\overline{Z} - \underline{Z})$ is calculated as described in Section B.2.1 and $P[X = x]$ is the probability that $X = x$. $P[complier|X = x] = \hat{\pi}_{1,x}(\overline{Z} - \underline{Z})$, where $\pi_{1,x}$ is the first stage coefficient on the instrument based on the sub-sample $X = x$. We create six mutually exclusive sub-groups based on education (college and no college) and age (three 10-year bands) group categories. Table B4 presents the results.

Table B4: Complier Characteristics

Education:	No College			College		
Age Group:	(1) 25-34	(2) 35-44	(3) 45-54	(4) 25-34	(5) 35-44	(6) 45-54
Men						
1977 - 1983	1.070	0.813	0.346	0.980	0.889	0.366
1984 - 1990	1.119	0.833	0.280	0.926	0.847	0.325
1991 - 1997	1.147	0.945	0.284	0.940	0.962	0.369
1998 - 2004	1.078	1.042	0.286	0.938	1.119	0.459
2005 - 2011	0.965	1.083	0.441	0.939	1.116	0.681
2012 - 2018	0.851	1.086	0.569	0.875	1.155	0.746
Women						
1977 - 1983	0.943	0.688	0.279	1.041	1.024	0.544
1984 - 1990	0.959	0.620	0.167	1.079	0.973	0.340
1991 - 1997	1.153	0.656	0.192	1.163	0.976	0.322
1998 - 2004	0.837	0.528	0.117	1.151	1.318	0.447
2005 - 2011	0.661	0.412	0.245	1.165	1.255	0.402
2012 - 2018	0.458	0.530	0.144	0.900	1.176	0.485

Notes: Data used: CPS, 1977-2018. In order to generate the conditional probabilities, we run a series of first-stage equations for the entire sample and the six mutually exclusive sub-groups. The dependent variable in all specifications is a dummy for being married. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. For women, we include the Inverse Mills Ratio as a covariate. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. For women, the exclusion restrictions for the employment equation are a series of dummies for the age of the youngest child in the household from 1-18, where age less than 1 is the base category, a dummy for ages 19-24, a dummy for 25 and over, and no children are also included. The values for $\underline{Z}$ and $\overline{Z}$ are defined for each respective sub-sample and are based on the 5% and 95% percentile of the instrument, respectively.

B.3 The Zero-First-Stage Sub-Sample

We need to specify uncertainty regarding the direct effect of the instrument, Ω_γ. To do so, we follow a suggestion from Imbens and Rubin (2015) regarding normalized differences between covariates in a treatment and control group in a regression framework not exceeding 0.25. Applied to our setting, we specify $\Omega_\gamma = (.125\sqrt{S_0^2 + S_{-0}^2})^2$, where S_0 is the standard error on $\hat{\gamma}$ for the zero-first-stage sub-sample and S_{-0} is the equivalent for the remainder of the sample.

When observing the first stage results for various data sub-samples, we noted that the instrument was not predictive of marriage for very low predicted marriage sub-samples.[34] Hence, we consider those with predicted being married in the lowest quartile as a candidate sub-sample for the zero-first-stage sub-sample.

Table B5 highlights that, except for the earliest period for men, we can consider this group as the zero-first-stage sub-sample because the instrument is not a good predictor of being married for this sub-sample.[35] Thus, we use the reduced form estimate on the instrument based on this sample as $\hat{\gamma}_{ZFS}$, our estimate for μ_γ. The reduced form estimates are also presented in Table B5. In all cases but one, the estimate of the direct effect of the instrument on wages is insignificantly different from zero. The single exception is for men in the 1977-1990 period, i.e., the only period for which we could not establish a meaningful zero-first-stage group.

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[34]We do not use the instrument when predicting being married in this case.

[35]For completeness, we still present the results of this exercise for men in the 1977-1990 period below. However, given that we did not manage to construct a zero-first-stage sub-sample for men in this period, one should disregard the results for this group.

Table B5: The Zero-First-Stage Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Men			**Women**		
	1977-1990	**1991-2004**	**2005-2018**	**1977-1990**	**1991-2004**	**2005-2018**
First Stage	0.062**	0.021	0.042	0.000	−0.006	0.041
	(0.029)	(0.031)	(0.026)	(0.030)	(0.034)	(0.038)
Reduced Form	0.096***	0.026	0.010	0.034	−0.013	−0.026
	(0.027)	(0.033)	(0.037)	(0.029)	(0.023)	(0.025)
Observations	48,721	44,901	39,015	35,596	41,096	36,245

Notes: Data used: CPS, 1977-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable in rows is a dummy for being married, while it is the natural log of wages in rows. Year and state fixed effects are included in all regressions. The following additional controls are included: dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. For women, we include the Inverse Mills Ratio as a covariate. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual. For women, the exclusion restrictions for the employment equation are a series of dummies for the age of the youngest child in the household from 1-18, where age less than 1 is the base category, a dummy for ages 19-24, a dummy for 25 and over, and no children are also included. The zero-first-stage sample is constructed by predicting marriage from a regression specification with all model controls listed but not the instrument. Quartiles of the predicted married variable and the first quartile from the zero-first-stage sample are created.

C More on Mechanisms

C.1 Household Specialization

In this section, we examine how Becker's theory fits the patterns of market work and household specialization we observe starting in the 2000s. On the one hand, in Figure 2, we see that although the degree of specialization has decreased over time, nowadays, the average husband is specialized in market work (relative to his wife), while the average wife is specialized in home production (relative to her husband). On the other hand, in Table 11, we show that, nowadays, married women work almost as much in the market as single women. The apparent contradiction here is that married women work in the market almost as much as single women while specializing in home production relative to their husbands. As highlighted in Section 4.1, Becker's theory implies an expansion of the household production-possibility frontier. That is, married individuals devote more time to production (market plus home hours) than singles because the fruits of work are enhanced due to trade between spouses. The empirical implication is that married individuals devote less time to leisure, as shown in Table C1. We use data from ATUS for the sample period in which we observe a premium for married women, from 2005 to 2018. In Columns 1 and 4, we report the average unconditional difference between married and single individuals in daily time devoted to leisure activities. Columns 2 and 5 present the difference conditional on observable characteristics, and in columns 3 and 6, we report the IV estimates. All coefficients show that married individuals spent less time on leisure activities than their single counterparts. For men, the difference ranges between 38 minutes and around 1.5 hours, while for women, it is between 17 and 75 minutes.

Table C1: Married Individuals Enjoy Less Leisure

	(1)	(2)	(3)	(4)	(5)	(6)
	Men			**Women**		
	OLS, No Controls	OLS	2SLS	OLS, No Controls	OLS	2SLS
Married	−91.090***	−49.649***	−38.169	−47.921***	−17.001***	−75.174
	(3.865)	(5.723)	(32.511)	(5.777)	(6.004)	(46.401)
First-Stage F-Statistic			276.563			85.178
Observations	16,061	16,061	16,061	18,145	18,145	18,145

Notes: Data used: ATUS 2005-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the time spent on leisure activities in minutes per day. In columns (1) and (4), a dummy for married is the only regressor. In the remaining columns, we include year and state fixed effects, dummies for the highest level of educational attainment, dummies for age, the number of children below the age of 5, and the number of children aged 5-17. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual.

C.2 Joint Household Search

This section provides further tests for the higher-reservation-wage and job-ladder-climbing mechanisms. In Table C2, we use the ATUS to compute differences in the time devoted to searching for a job between non-employed married and single individuals. Columns 1 and 4 report unconditional differences, columns 2 and 5 conditional on observables, and columns 3 and 6 present 2SLS estimates. For men, we compute very noisy coefficients due to the small sample size. For transparency, we report the IV coefficient in column 3, but we note that the F-statistics indicates a (too) weak first stage. Therefore, we do not draw any inference from the male sample. That is, we cannot ascertain whether married men are likely to devote more or less time to job search than their single counterparts. In the case of women, the coefficients are more informative, although the IV estimates are also quite noisy. Married women are likely to spend less time searching for jobs than their single counterparts. The estimates indicate that the difference is not quantitatively large, although the IV coefficient suggests a larger difference. We view

these estimates as consistent with the higher-reservation-wage mechanism for women and this mechanism having a bigger role for women than for men.[36]

Table C2: Non-employed Married Women Likely Spend Less Time Job Searching

	(1)	(2)	(3)	(4)	(5)	(6)
		Men			Women	
	OLS, No Controls	OLS	2SLS	OLS, No Controls	OLS	2SLS
Married	2.073	−1.892	−96.217	−6.660***	−4.198*	−35.582
	(4.959)	(6.205)	(85.555)	(2.257)	(2.215)	(22.068)
First-Stage F-Statistic			5.644			25.996
Observations	1,318	1,318	1,318	4,380	4,380	4,380

Notes: Data used: ATUS 2005-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the time spent on job search in minutes per day. In columns (1) and (4), a dummy for married is the only regressor. In the remaining columns, we include year and state fixed effects, dummies for the highest level of educational attainment, dummies for age, the number of children below the age of 5, and the number of children aged 5-17. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual.

Next, we examine the job-ladder-climbing mechanism. Generally understood, the mechanism implies two testable predictions. First, employed married individuals put more effort into job searching than their single counterparts. Second, married individuals switch to new jobs at higher rates (climb the job ladder faster) than singles.

Table C3 presents an analogous analysis to that in Table C2 but for employed individuals. Columns 1 and 4 report unconditional differences, columns 2 and 5 conditional on observables, and columns 3 and 6 present 2SLS estimates. All the OLS coefficients are statistically significantly lower than zero, while the IV estimates are noisier but also negative. We interpret time devoted to job search as the natural empirical counterpart to search effort. Therefore, we conclude that the results in Table C3 are at odds with the prediction that married individuals devote more effort to job search when employed, the first prediction associated with the job-ladder-climbing mechanism.

Table C3: Employed Married Individuals Do Not Spend More Time Job Searching

	(1)	(2)	(3)	(4)	(5)	(6)
		Men			Women	
	OLS, No Controls	OLS	2SLS	OLS, No Controls	OLS	2SLS
Married	−1.154**	−1.235**	−2.342	−1.325**	−1.297***	−5.642
	(0.468)	(0.507)	(1.579)	(0.514)	(0.471)	(3.695)
First-Stage F-Statistic			335.655			49.617
Observations	14,743	14,743	14,743	13,765	13,765	13,765

Notes: Data used: ATUS 2005-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable is the time spent on job search in minutes per day. In columns (1) and (4), a dummy for married is the only regressor. In the remaining columns, we include year and state fixed effects, dummies for the highest level of educational attainment, dummies for age, the number of children below the age of 5, and the number of children aged 5-17. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual.

Next, we examine whether married individuals switch jobs more often, i.e., climb the job ladder faster than singles. We follow Wilmers and Kimball (2022) and exploit the CPS questions on job mobility to identify job switches. The CPS follows households for two four-month periods with an eight-month sample exclusion in between. For individuals in households surveyed the previous month, the CPS asks whether

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[36]See Chapter 5 in Pissarides (2000).

the individual has switched employer and, if not, whether the activities and duties of the job have changed with respect to the previous month. Using these questions, one can identify job-to-job transitions, both between and within employer. In Table C4, we estimate the difference in the probability of switching jobs, that is, going through a job-to-job transition, between married individuals and their single counterparts. Panel A focuses on between-employer transitions, while Panel B considers within-employer switches. Columns 1 and 4 report unconditional differences, columns 2 and 5 present conditional differences, and columns 3 and 6 are IV estimates. All the coefficients we estimate indicate that married individuals are less likely to switch jobs than their single counterparts, which is at odds with the job-ladder-climbing mechanism.

Table C4: Married Individuals Are Less Likely to Switch Jobs

	(1)	(2)	(3)	(4)	(5)	(6)
	Men			**Women**		
	OLS, Un-conditional	OLS	2SLS	OLS, Un-conditional	OLS	2SLS
A.) Between-Employer						
Married	−0.007***	−0.004***	−0.013***	−0.006***	−0.004***	−0.014***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.002)
First-Stage F-Statistic			3,048			510
Adjusted R^2	0.000	0.001	0.001	0.000	0.001	0.000
Observations	1,329,376	1,329,376	1,329,376	1,129,688	1,129,688	1,129,688
B.) Within-Employer						
Married	−0.004***	−0.004***	−0.004***	−0.004***	−0.003***	−0.003*
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.002)
First-Stage F-Statistic			3,017			518
Adjusted R^2	0.000	0.001	0.001	0.000	0.001	0.001
Observations	1,295,764	1,295,764	1,295,764	1,102,021	1,102,021	1,102,021

Notes: Data used: CPS, 2005-2018. *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered by state and reported in parentheses. The dependent variable in Panel A is a dummy indicating a between-employer job transition, and in Panel B is a dummy indicating a within-employer job transition. In columns (1) and (4), a dummy for married is the only regressor. In the remaining columns, we include year and state fixed effects, dummies for the highest level of educational attainment, dummies for years of potential experience, the number of children below the age of 5, the number of children aged 5-17, and a dummy for a child over 18. As described in Section 3.1, the instrument is the marriage rate in the reference group of each individual.

Taken together, the results in Table C3 and Table C4 suggest that there is no empirical support for the job-ladder-climbing mechanism. Therefore, we conclude that this mechanism cannot be a major contributor to the marriage wage premium.