

# Who Benefits from Marriage?

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May 2007

## Abstract

The phenomenon that married men earn higher average wages than unmarried men, the so-called marriage premium, is well known. However, the robustness of the marriage premium across the wage distribution and the underlying causes of the marriage premium deserve closer scrutiny. Focusing on the entire wage distribution and employing recently developed semi-nonparametric tests for quantile treatment effects, our findings cast doubt on the robustness of the premium. We find that the premium is explained by selection above the median, whereas a positive premium is obtained only at very low wages. We argue that the causal effect at low wages is probably attributable to employer discrimination.

**JEL Classifications:** C14, C21, C33, D31, J12

**Key words:** Marriage Premium, Quantile Treatment Effects, Stochastic Dominance, Selection

**Word Count:** 9950

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# 1 Introduction

Married men, *on average*, earn more than single men in the labor market, seemingly even after controlling for some observable attributes; a phenomenon commonly referred to as the marriage premium (MP). The current notion of the MP is rather widely held as is exemplified by Loh (1996, p. 566): “Virtually all cross-sectional wage studies find that currently married men typically earn a higher wage rate than their unmarried counterparts in the labor market.” Cornwell and Rupert (1997, p. 285) similarly note: “Married men earn more than unmarried men. This fact is unassailable and is robust across data sets and over time.” While the existence of the MP (as is defined) may not be controversial, one salient question has not been asked by researchers (to our knowledge) and another has yet to be answered. First, to what extent does the MP hold across the entire wage distribution, and is it uniform? Second, what are the underlying sources of the MP? Answering the former question is vital, for if some men do not benefit, or benefit to different degrees, a narrow focus on an average measure of the MP creates a false sense of robustness, and may be inconsistent with a uniform ranking of wage distributions across reasonable classes of utility functions. With respect to the latter question, Cornwell and Rupert (1997, p. 285) continue: “While there is compelling evidence that married men earn more than unmarried men, the source of this premium remains unsettled.” More recently, Stratton (2002, p. 199) states: “Research has failed as yet to reach a consensus regarding the nature of these differentials.”

Given the lack of consensus, labor economists continue to seek the underlying sources of the MP – typically on the order of a 10 to 40 percent *average* wage differential – for four main reasons. First, knowledge of the premium’s source(s) contributes to our understanding of the general process of wage determination. Second, understanding the MP furthers our knowledge of the role played by gender in the labor market as the premium constitutes about one-third of the entire gender wage gap (Korenman and Neumark 1991). Third, if the MP reflects true productivity differences, then changes in marital trends in the US and elsewhere may foreshadow changes in future productivity. Finally, while marriage may not seem to be a policy-relevant ‘treatment’ at first glance, many US policies seek to support and encourage marriage (although presumably for reasons other than higher male wages).<sup>1</sup>

Several hypotheses for the MP have been put forth, and these may be loosely classified into three categories: (i) causal explanations, (ii) unobserved covariates/selection explanations, and (iii) reverse causation

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<sup>1</sup>For example, the US Department of Health & Human Services maintains that one of the four primary objectives of Temporary Aid for Needy Families (TANF) is “encouraging the formation and maintenance of two-parent families” (see [http://www.acf.hhs.gov/opa/fact\\_sheets/tanf\\_printable.html](http://www.acf.hhs.gov/opa/fact_sheets/tanf_printable.html)). See Rasul (2005) for a list of over 20 laws or activities undertaken in different states that seek to promote marriage.

explanations. Hypotheses that are based on a causal effect of marriage on wages center predominately on the productivity-enhancing impact of marriage on men arising from intra-household specialization. Under this argument – originating in the work by Becker (1973, 1974, 1991) – marriage enables men to specialize in labor market activities (due to their comparative advantage in market work), while women specialize in home production. A second, causal explanation attributes the MP to employer discrimination in favor of married men.<sup>2,3</sup> Unobserved covariates/selection explanations note the selective nature of marriage, and focus on the possibility that marriage may be correlated with unobservable attributes that are valued in both the labor and marriage markets (e.g., interpersonal skills, integrity, reliability, work ethic, etc.). Finally, arguments based on reverse causation center on the possibility that single women may seek out high-earning men as potential partners (e.g., Ginther and Zavodny 2001).

In previous research, several studies document evidence in favor of a small, productivity-enhancing effect of marriage after controlling for self-selection, typically by employing parametric, fixed effects panel methods (Korenman and Neumark 1991; Daniel 1995; Stratton 2002; Krashinsky 2004).<sup>4</sup> Antonovics and Town (2004) find a large, causal effect of marriage – and no evidence favoring the selection hypothesis – upon estimating a fixed effects model using data on monozygotic twins; Krashinsky (2004), however, finds the opposite. Isacson (2007) also finds that the entire MP in Sweden may be explained by selection when utilizing individual panel data, but not when using twins to control for selection. In further support of the specialization hypothesis, Jacobsen and Rayack (1996), Gray (1997), Chun and Lee (2001), among others, find that the MP declines with wife’s labor supply.<sup>5</sup> Moreover, several studies have documented a decline in the MP over time (Blackburn and Korenman 1994; Loh 1996; Gray 1997; Cohen 2002), which may be additional evidence in favor of the specialization hypothesis given the rise in female labor force participation in the US. In terms of the employer discrimination hypothesis, Jacobsen and Rayack (1996) and Loh (1996) test for the existence of the MP amongst self-employed workers, and find an insignificant or even negative MP, perhaps lending some support to the hypothesis.<sup>6</sup> Finally, Ginther and Zavodny (2001)

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<sup>2</sup> According to this reasoning, employers may view married men as more reliable, more honest, less mobile, etc.

<sup>3</sup> An additional causal explanation rests on the theory of compensating differentials, asserting that married men tend to forego non-monetary work benefits (e.g., flexible hours) for greater monetary compensation. Duncan and Holmud (1983) and Hersch (1991) find little support for such claims.

<sup>4</sup> Fixed effects methods control for selection based on time invariant wage levels, but not selection into marriage based on wage growth.

<sup>5</sup> Loh (1996), on the other hand, fails to uncover evidence of a consistent relationship between wife’s labor supply and husband’s wages. Hersch and Stratton (2000) find that while the use of individual fixed effects does not substantially reduce the marriage premium, controlling for time spent in household production has little impact on the magnitude of the marriage premium. The authors, therefore, conclude that selection plays a minimal role, but reject the specialization as the underlying source of the causal relationship.

<sup>6</sup> As indicated by the authors, such conclusions should be interpreted cautiously, given the difficulties that arise in econometric studies of the self-employed (e.g., measurement error in wages, self-selection, pooling self-employed and non-self-employed workers, etc.).

use ‘shotgun weddings’ to circumvent the selection issue, finding that selection accounts for less than ten percent of the MP.<sup>7</sup> Conversely, several researchers conclude that self-selection is the primary explanation for the existence of the MP (Nakosteen and Zimmer 1987; Cornwell and Rupert 1997), particularly in the 1990s (Gray 1997).<sup>8</sup> Finally, little systematic evidence exists *per se* to support the reverse causation explanation. However, consonant with Becker (1976), Cornwell and Rupert (1997) and Krashinsky (2004) find that men who are ‘to-be-married’, on average, earn wages comparable to married men, and Nakosteen and Zimmer (1997) and Ginther and Zavodny (2001) note that earnings are positively correlated with the likelihood of marriage.

In this paper, we hope to shed light on these questions in two important ways. First, we revisit the claim that the MP is robust by assessing the effects of marriage across the entire wage *distribution* utilizing recent tests for quantile treatment effects (QTE) and stochastic dominance (SD). The former allows one to test for statistically significant effects of marriage on particular *quantiles* of the wage distribution, rather than just the mean. The latter provides a means of aggregating information across the quantiles in order to make summary statements about how one distribution compares to another. Thus, the distributional approach utilizes all available information, assesses the potentially heterogeneous returns to marriage across subgroups of men, and makes explicit the welfare framework being employed by different observers who may place different weights on different subgroups<sup>9</sup>. Second, we revisit and distinguish the underlying explanations of the return to marriage within this broader *distributional* analysis of the MP.

To perform the analysis, we begin by using panel data from the Current Population Survey (CPS) for 1992 – 2001. We offer the following comparisons of *unconditional* distributions: (i) single versus ‘to-be-married’ men (men who marry within the next year), (ii) single versus newly married men, and (iii) the distribution of wage changes for single versus newly married men. Next, we re-examine the same pairwise comparisons after adjusting for a host of observable covariates that may be correlated with both marital status and/or labor market performance. This two-part strategy enables us to (i) support (or refute) the existence of a *uniform* MP, and (ii) comment on the underlying causes of the MP (assuming it exists). Specifically, if the MP represents a causal relationship (with marriage affecting wages), then there should be no difference in the distribution of wages between single and ‘to-be-married’ men. But

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<sup>7</sup>A ‘shotgun wedding’ is defined in Ginther and Zavodny (2001) as a marriage that is followed by the birth of a child within the subsequent seven months.

<sup>8</sup>Gray (1997) finds that the marriage premium represents a productivity effect in the late 1970s, but is attributable to selection in the early 1990s.

<sup>9</sup>The richness of the SD analysis has led to their growing application. For example, Maasoumi and Millimet (2005) examine changes in US pollution distributions over time and across regions at a point in time. Maasoumi and Heshmati (2000) analyze changes in the Swedish income distribution over time as well as across different population subgroups. Abadie (2002) analyzes the impact of veteran status on the distribution of civilian earnings.

the distribution of married men should ‘dominate’ that of single men. On the other hand, a ‘dominant’ distribution of wages amongst ‘to-be-married’ men would suggest a role for both selection and reverse causation explanations. If there is some validity to all the proposed explanations, then we might observe a modest disparity in the distribution of wages favoring ‘to-be-married’ men (versus singles), followed by an even greater disparity after marriage. Thus, our analysis can help assess the relative role of causal versus correlation-based explanations of the MP across the entire wage distribution.

The above strategy separates the causal and selection components of the MP through comparisons of wage changes before and after marriage, thereby eliminating time invariant unobservables as in fixed effects methods. An alternative strategy is to use an instrumental variable (IV) for marital status. The advantage of employing an IV strategy is that it controls for time-varying unobservables that may be correlated with both marital status and wages. Abadie (2002) shows how one can utilize a binary instrument to perform distributional tests. Thus, for comparison, we use several instruments and re-examine the return to marriage. In light of the instruments utilized, we switch to the Panel Study of Income Dynamics (PSID) and use data from 1994.

The results are striking. In particular, we reach four conclusions. First, the MP persists even at the distributional level in the CPS data in the 1990s, even after adjusting for a lengthy vector of observable attributes. However, there is some evidence that the return to marriage may not be uniform across the wage distribution; the gains from marriage are largest for those in the *lower tail* of the wage distribution. Second, the MP is no longer statistically significant above roughly the median once selection on time invariant attributes is admitted, but the MP persists in the *lower tail* of the distribution. Third, controlling for time invariant and time-varying unobservables correlated with marital status and labor market performance further amplifies this result; the MP is at-most a phenomenon associated with the extreme *lower tail* of the wage distribution, and the MP may even be negative in the upper tail. This implies that typical fixed effects methods alone – which are prominent in the MP literature – may be insufficient to give a complete picture of the returns to marriage. In addition, our results draw attention to the fact that statistically significant estimates of the *average* MP are really capturing effects occurring only at low quantiles of the wage distribution. Finally, the disappearance of the MP over the majority of the distribution once unobservables are addressed suggests an important role of selection in explaining the MP. However, the MP in the lower tail does appear to represent a causal relationship. Moreover, given the strong negative correlation between married women’s labor supply and husband’s wage, the MP in the lower tail is not likely driven by household specialization, but rather may reflect employer discrimination in low wage

labor markets (e.g., Blau and Kahn 2005). In other words, in such labor markets, the absence of other signals (e.g., schooling) to differentiate among workers implies a large weight placed by employers on a worker's marital status. The remainder of the paper is organized as follows. Section 2 details the empirical methodology. Section 3 discusses the data and the benchmark average estimates. Section 4 presents the results from the distributional analysis. Section 5 concludes.

## 2 Empirical Methodology

### 2.1 Potential Outcomes Framework

To contrast various estimators in terms of what they estimate, and under what assumptions, we utilize the potential outcomes framework often adopted in the program evaluation literature. Let  $w_{1i}$  denote the log wage of individual  $i$  if married (denoted as  $D_i = 1$ ), and  $w_{0i}$  denote the log wage of individual  $i$  if single (denoted as  $D_i = 0$ ). The effect of marriage is given by  $\tau_i \equiv w_{1i} - w_{0i}$ . However, only one potential outcome is observed for a given individual at a particular point in time; one observes  $w_i = D_i(w_{1i}) + (1 - D_i)w_{0i}$ .

### 2.2 Regression Approach

To proceed within a regression framework, we begin by specifying a structural relationship for the potential outcomes. Define

$$\begin{aligned} w_{0i} &= \mu_0(x_i) + u_{0i} \\ w_{1i} &= \mu_1(x_i) + u_{1i} \end{aligned} \tag{1}$$

where  $E[w_j|x_i] = \mu_j(x_i)$ ,  $j = 0, 1$ , and  $x_i$  is a vector of observable attributes of individual  $i$  (including an intercept). Thus,  $u_j$  captures the impact of unobservable attributes on wages when  $D = j$ ,  $j = 0, 1$ . Following Heckman et al. (1999), if one assumes that  $\mu_j(x_i) = x_i\beta_j$ ,  $j = 0, 1$ , and  $\beta_0 = \beta_1$  except for the intercept terms, then one obtains the following regression model

$$w_i = x_i\beta_0 + \tau D_i + [u_{0i} + D_i(u_{1i} - u_{0i})] \tag{2}$$

where  $\tau$  is the constant treatment effect. OLS estimation of (2) yields a consistent estimate of  $\tau$  under these functional form assumptions if

- (i)  $\text{Cov}(D, u_0) = 0$ , and

$$(ii) \text{ Cov}(D, u_1 - u_0) = 0.$$

The former requires marital status to be independent of unobservables that impact wages when single. The latter requires marital status to be independent of unobserved, individual-specific gains from marriage.

In contrast, a consistent estimate of  $\tau$  may be obtained under an alternative sets of assumptions (although still maintaining the functional form restrictions). First, given the presence of balanced panel data, suppose we observe individuals in two time periods, where all individuals are single in the first period, and some individuals are married in the second period. Wages in each period are given by

$$\begin{aligned} w_{it'} &= x_{it'}\beta_0 + u_{0it'} \\ w_{it} &= x_{it}\beta_0 + \tau D_{it} + [u_{0it} + D_{it}(u_{1it} - u_{0it})] \end{aligned}$$

where  $t'$  indicates the initial period when all individuals are single, and  $t$  indexes the later period when some individuals are married ( $t > t'$ ). First-differencing yields

$$w_{it} - w_{it'} = \Delta w_{it} = \Delta x_{it}\beta_0 + \tau D_{it} + [\Delta u_{0it} + D_{it}(u_{1it} - u_{0it})]. \quad (3)$$

OLS estimation of (3) provides a consistent estimate of  $\tau$  if

$$(i) \text{ Cov}(D, \Delta u_0) = 0, \text{ and}$$

$$(ii) \text{ Cov}(D, u_{1t} - u_{0t}) = 0.$$

While the latter is unchanged from above, the former now requires only that marital status be uncorrelated with *changes* over time in unobservables impacting wages when single. Thus, as is well known in models with unobserved individual effects, identification is achieved even if the treatment is correlated with time-invariant unobservables that impact wages when untreated.

The second alternative set of assumptions is couched within an IV approach. If an excluded variable,  $Z$ , is available such that

$$(i) \text{ Conditional Independence: } w_0, w_1, D(z) \perp Z|X$$

$$(ii) \text{ Correlation: } E[D|X, Z] \text{ is a non-trivial function of } X \text{ and } Z, \text{ and}$$

$$(iii) \text{ Monotonicity: } D(z') \geq D(z'') \text{ or } D(z') \leq D(z'') \forall i, z' \neq z'',$$

where  $D(z)$  is random variable indicating the value of  $D$  when  $Z = z$ , then  $\tau$  may be consistently estimated.

Under the assumption of a constant treatment effect, all three models estimate the same parameter, but differ in the assumptions required for identification. However, if the effect of the treatment is heterogeneous, then the various models estimate different parameters. In particular, while the first two estimate the ATT in such a scenario, IV estimates the local average treatment effect (LATE) unless  $E[u_1 - u_0|X, Z, D = 1] = E[u_1 - u_0|X, D = 1]$  and  $E[u_0|X, Z] = E[u_0|X]$ , where the former follows from the assumption that the probability of marriage is independent of the unobserved, individual-specific gains to marriage conditional on  $X$  and  $Z$  and the latter states that  $u_0$  may depend on  $X$  but not  $Z$ .

### 2.3 Distributional Approach

**Quantile Treatment Effects** When the treatment effect is heterogeneous, the preceding regression-based approaches focus on specific summary measures of the treatment effect distribution. In light of recent advances in the program evaluation literature, additional information concerning the impact of marriage on wages can be uncovered. To that end, we undertake several pairwise comparisons of the distributions of log wages, distinguished by marital status, and analyze the quantile treatment effects (QTEs). To begin, let  $W_0$  and  $W_1$  denote two wage variables to be compared;  $W_0$  ( $W_1$ ) may represent potential log wages if single (married).  $\{w_{0i}\}_{i=1}^{N_0}$  is a vector of  $N_0$  observations of  $W_0$  (denoted by  $D_i = 0$ );  $\{w_{1i}\}_{i=1}^{N_1}$  is an analogous vector of realizations of  $W_1$  (denoted by  $D_i = 1$ ). Let  $F_0(w) \equiv \Pr[W_0 < w]$  represent the cumulative density function (CDF) of  $W_0$ ; define  $F_1(w)$  similarly for  $W_1$ . The  $p^{th}$  quantile of  $F_0$  is given by the smallest value  $w_0^p$  such that  $F_0(w_0^p) = p$ ;  $w_1^p$  is defined similarly for  $F_1$ . Under this notation, the QTE for quantile  $p$  is given by  $\Delta_p = w_1^p - w_0^p$ , which is simply the horizontal difference between the CDFs at probability  $p$ .<sup>10</sup> Estimates,  $\hat{\Delta}_p$ ,  $p = 0.01, \dots, 0.99$ , are obtained using the sample analogues, obtained using the empirical CDFs given by

$$\hat{F}_{jN_j}(w) = \frac{1}{N_j} \sum_{i=1}^{N_j} \mathbf{I}(W_j \leq w), \quad j = 0, 1. \quad (4)$$

The estimates,  $\hat{\Delta}_p$ , are consistent if  $W_0, W_1 \perp D$ , and if the CDFs of potential wages are continuous and monotonically increasing (at the quantiles for which the QTE is estimated).

Since dependence between marital status and other determinants of wages most certainly invalidates

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<sup>10</sup>It is important to note that the QTEs do not correspond to quantiles of the distribution of the treatment effect unless the assumption of *rank preservation* holds (Heckman et al. 1997; Firpo 2007). Absent this assumption, whereby the ranking of individuals in the wage distribution would remain unchanged across marital states, the QTE simply reflects differences in the quantiles of the two marginal distributions.



the preceding identification assumption, we also estimate the QTEs under several alternative sets of assumptions. First, we obtain estimates of the QTEs adjusting for covariates using inverse propensity score weighting (IPW). Such estimates are consistent under the now familiar conditional independence (CIA) and common support (CS) assumptions, in addition to the previous requirement that the distributions of potential wages be continuous and monotonically increasing (at the quantiles for which the QTE is estimated). Specifically, we require

(i) CIA:  $W_0, W_1 \perp D|X$

(ii) CS:  $p(x_i) \in (c, 1 - c)$  for all  $i$  and for some  $c > 0$ .

To proceed, we follow Bitler et al. (2006) and estimate the empirical CDF for  $W_j$  by

$$\hat{F}_{jN_j}(w) = \frac{\sum_{i=1}^{N_j} \hat{\eta}_i \mathbf{I}(W_j \leq w)}{\sum_{i=1}^{N_j} \hat{\eta}_i}, \quad j = 0, 1 \quad (5)$$

where the weights,  $\hat{\eta}_i$ , are given by

$$\hat{\eta}_i = \frac{D_i}{\hat{p}(x_i)} + \frac{1 - D_i}{1 - \hat{p}(x_i)} \quad (6)$$

and  $D_i$  is the indicator variable for marital status defined above, and  $\hat{p}(x_i)$  is the propensity score (i.e., the predicted likelihood of observation  $i$  being married given a set of observed attributes,  $x_i$ , from a first-stage probit model) (see also Firpo 2007).

Second, since the CIA assumption may still be too stringent, we relax this assumption to account for both observable covariates,  $X$ , as well as time invariant unobservables. As in the previous section, suppose we observe individuals in two time periods, where all individuals are single in the first period, and some individuals are married in the second period. Define  $W_{0t}$  ( $W_{1t}$ ) as potential log wages at time  $t$  if single (married), and  $W_{0t'}$  as potential log wage if single at time  $t'$ , where  $t'$  indicates the initial period when all individuals are single, and  $t$  indexes the later period when some individuals are married ( $t > t'$ ). We can now define two new potential outcomes,  $\Delta W_{0t}$  and  $\Delta W_{1t}$ , where the former (latter) represents the change in wages across time periods if one remains single (marries). Estimating QTEs based on the empirical CDFs of  $\Delta W_{0t}$  and  $\Delta W_{1t}$  without adjusting for covariates or with adjusting for covariates requires  $\Delta W_{0t}, \Delta W_{1t} \perp D$  or  $\Delta W_{0t}, \Delta W_{1t} \perp D|X$ , respectively.

Finally, since selection into marriage may occur on the basis of expected future growth in wages, even accounting for time invariant unobservables may be insufficient. Thus, we turn to an IV approach

based on Abadie (2002). According to Imbens and Rubin (1997), when a binary instrumental variable is available, the potential distributions of the outcome variable are identified for the subpopulation (referred to as compliers) whose treatment assignment (in this case, marital status) is affected by variation in the instrument. This is analogous to the LATE in the regression context.

As before, let  $W_0$  and  $W_1$  denote the potential outcomes if single and married, respectively, with  $w_i(0)$  and  $w_i(1)$  representing specific values for observation  $i$ ,  $i = 1, \dots, N_0 + N_1$ , from the respective distribution. Let  $Z_i$  be a binary instrument, where  $D_i(z)$  is the value of  $D_i$  if  $Z_i = z$ ,  $z \in \{0, 1\}$ . With this setup, not only is only one of the two potential outcomes observed for each observation, but only one of the two potential treatment assignments. Lastly, let  $F_0^c(w)$  and  $F_1^c(w)$  represent the CDFs of potential wages for single and married compliers, which are defined as follows:

$$\begin{aligned} F_0^c(w) &= E[I\{w_i(0) \leq w\} | D_i(1) = 1, D_i(0) = 0] \\ F_1^c(w) &= E[I\{w_i(1) \leq w\} | D_i(1) = 1, D_i(0) = 0] \end{aligned} \quad (7)$$

If  $Z_i$  satisfies the following three assumptions:

- (i) Independence:  $w_i(0), w_i(1), D_i(0), D_i(1) \perp Z_i$
- (ii) Correlation:  $\Pr(Z_i = 1) \in (0, 1)$  and  $\Pr(D_i(0) = 1) < \Pr(D_i(1) = 1)$
- (iii) Monotonicity:  $\Pr(D_i(0) \leq D_i(1)) = 1$ ,

then the QTEs based on the distributions  $F_0^c(w)$  and  $F_1^c(w)$  identify the causal effect of marriage for the subpopulation of compliers (Imbens and Angrist 1994; Angrist et al. 1996). Moreover, as shown in Abadie (2002), distributional tests conducted on the distributions  $F_0^c(w)$  and  $F_1^c(w)$  are equivalent to tests conducted on the distributions  $G_0(w)$  and  $G_1(w)$ , where  $G_0$  ( $G_1$ ) represents the distribution of wages for individuals with  $Z_i = 0$  ( $Z_i = 1$ ). Thus, under the IV approach, the only difference is that now we are working with the empirical CDFs of  $G_0$  and  $G_1$  in (4) and (5) rather than  $F_0$  and  $F_1$ .<sup>11</sup>

In the results below, we plot  $\hat{\Delta}_p$  obtained under each set of identifying assumptions, as well as 90% confidence intervals based on a simple bootstrap technique, similar to Bitler et al. (2006). When adjusting for covariates, the first-stage probit model and resulting weights are estimated anew during each bootstrap replication.

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<sup>11</sup>Note, we also utilize the IV approach adjusting for covariates using IPW. In this case, assumption (i) is replaced by a conditional independence assumption.

**Test of Equality** In addition to examining the QTEs at each integer quantile, we test the joint null  $H_o : \Delta_p = 0 \ \forall p \in (0, 1)$ , or equivalently  $H_o : F_0 = F_1$ , utilizing a two-sample Kolmogorov-Smirnov (KS) statistic.<sup>12</sup> The test is based on the following:

$$d_{eq} = \sqrt{\frac{N_0 N_1}{N_0 + N_1}} \max_k \{|\hat{F}_{1N_1}(w_k) - \hat{F}_{0N_0}(w_k)|\} \quad (8)$$

where  $\hat{F}_{jN_j}$ ,  $j = 0, 1$ , is obtained using either (4) or (5). Inference for the test of equality of the distributions is conducted using the bootstrap procedure outlined in the Appendix.

**Stochastic Dominance** While examination of the QTEs is of great interest, policy implications may be ambiguous if equality of the CDFs is rejected and the QTEs vary in sign or statistical significance across the distribution. Different observers and policymakers, all of whom may prefer higher wages, may place different weights on different parts of a distribution. How does one compare changes at the top end of the distribution versus the bottom end versus those affecting individuals in the middle? To address this issue, we perform tests for first and second order stochastic dominance. Tests for SD offer the possibility of making limited, but robust, welfare comparisons of distributions. Such comparisons are *robust* in that they are insensitive to the exact preference function within a large class and/or the underlying distribution of wages. However, they are *limited* to the extent that they do not extend attention beyond the outcome of interest (wages) in the application, ignoring other outcomes associated with marriage. That said, SD tests are nonetheless extremely powerful as they highlight *exactly* what can be said about the entire distributions being compared and revealing policy preferences.

Several tests for SD have been proposed in the literature; the approach herein is based on a generalized Kolmogorov-Smirnov (KS) test.<sup>13</sup> To begin, assuming general von Neumann-Morgenstern conditions, let  $\mathcal{U}_1$  denote the class of (increasing) ‘welfare’ functions  $u$  that are increasing in wages (i.e.  $u' \geq 0$ ), and  $\mathcal{U}_2$  the sub-class of functions in  $\mathcal{U}_1$  such that  $u'' \leq 0$  (i.e. concavity). Concavity represents an aversion to wage inequality, a risk aversion for policy makers. Note that  $u$  refers to the welfare function of a policymaker, not the individual.

Under this notation, wages  $W_0$  First Order Stochastically Dominate  $W_1$  (denoted  $W_0 \text{ FSD } W_1$ ) *iff*  $E[u(W_0)] \geq E[u(W_1)]$  for all  $u \in \mathcal{U}_1$ , with strict inequality for some  $u$ . Equivalently,

$$F_0(\omega) \leq F_1(\omega) \quad \forall \omega \in \Upsilon, \text{ with strict inequality for some } \omega \quad (9)$$

<sup>12</sup>In the IV approach following Abadie (2002), we are testing  $H_o : G_0 = G_1$ .

<sup>13</sup>Maasoumi and Heshmati (2000) provide a brief review of the development of alternative tests.

where  $F(\bullet)$  continues to denote the CDF and  $\Upsilon$  denotes the union of the supports of  $W_0$  and  $W_1$ , or

$$\Delta_p \leq 0 \quad \forall p \in (0, 1), \text{ with strict inequality for some } p. \quad (10)$$

If  $W_0$  FSD  $W_1$ , then the expected social welfare from  $W_0$  is at least as great as that from  $W_1$  for all increasing welfare functions.

Further  $W_0$  Second Order Stochastically Dominates  $W_1$  (denoted as  $W_0$  SSD  $W_1$ ) iff  $E[u(W_0)] \geq E[u(W_1)]$  for all  $u \in \mathcal{U}_2$ , with strict inequality for some  $u$ . Equivalently,

$$\int_{-\infty}^{\omega} F_0(v)dv \leq \int_{-\infty}^{\omega} F_1(v)dv \quad \forall \omega \in \Upsilon, \text{ with strict inequality for some } \omega \quad (11)$$

or

$$\int_0^p \Delta_v dv \leq 0 \quad \forall p \in (0, 1), \text{ with strict inequality for some } p. \quad (12)$$

If  $W_0$  SSD  $W_1$ , then the expected social welfare from  $W_0$  is at least as great as that from  $W_1$  for all increasing and concave utility functions in the class  $\mathcal{U}_2$ . Of course, FSD implies SSD and higher orders.

To test for FSD and SSD, we utilize the following generalizations of the KS test statistic:

$$d = \sqrt{\frac{N_0 N_1}{N_0 + N_1}} \min \sup_{\omega \in \Upsilon} [\hat{F}_{1N_1}(\omega) - \hat{F}_{0N_0}(\omega)] \quad (13)$$

$$s = \sqrt{\frac{N_0 N_1}{N_0 + N_1}} \min \sup_{\omega \in \Upsilon} \int_{-\infty}^{\omega} [\hat{F}_{1N_1}(v) - \hat{F}_{0N_0}(v)] dv \quad (14)$$

where min is taken over  $\hat{F}_{1N_1} - \hat{F}_{0N_0}$  and  $\hat{F}_{0N_0} - \hat{F}_{1N_1}$ . If  $d > 0$ , then there is no observed ranking in the sample in the first-order sense. Similarly, if  $s > 0$ , then there is no observed second-order ranking in the sample. In order to attach probabilities to an inference about SD, we conduct two well-established bootstrap procedures outlined in the Appendix.

### 3 Data

We utilize two different data sets to implement the various estimators. For the panel estimators, we make use of matched data from the Current Population Survey (CPS). For the IV estimators, we make use of data from the Panel Study of Income Dynamics (PSID).

### 3.1 Current Population Survey

The CPS data come from the March Supplement. The CPS interviews households who do not change residence for two consecutive periods.<sup>14</sup> For the comparisons we wish to make, we require males who were single in both survey periods, as well as males who were single in the first period and married in the second period. The difficulty is that many males who marry in between survey years change residences, and therefore drop out of the CPS sample. Consequently, the to-be-married/married males we do observe may not be a random sample; we shall return to this below. To circumvent the small sample size issue, we utilize matched pairs of individuals over the period 1992–2001.<sup>15</sup> We restrict the sample to include only employed, native born males between ages 25 and 65; omitting those in school, enrolled in the military, employed in agriculture, disabled, and self-employed.

The outcome of interest is the hourly wage, which is constructed using data on annual wages and salaries, usual number of hours worked per week, and number of weeks worked last year. All wages were converted to (1982) real dollars using the CPI. To eliminate the effect of outliers, we drop observations with wages below \$1/hr and above \$100/hr (Loh 1996).<sup>16</sup> The final sample contains 11,033 observations: 4,813 (5,669) single individuals in period one (two), and 257 (294) to-be-married (married) individuals in period one (two). The balanced sample utilized in the first-difference comparisons contains 4,676 (252) single (married) individuals.

To obtain the propensity score weights,  $\hat{\eta}_i$ , we utilize an extensive set of individual, family, occupational, and location variables available in the CPS. The vector  $x$  in (6) includes the following variables (in addition to a constant term): racial dummies (white, black, American-Indian, Asian or Pacific Islander, and other), age, age squared, educational attainment dummies (less than high school, some college without degree, college and above), number of own children younger than six, dummies for type of housing (own or rent), occupation dummies, full-time employee indicator, dummy for membership in a labor union or similar employee association, class of worker dummies (private, federal, state and local government), urban and year dummies, and nine regional dummies. Note that unlike typical methods such as standard regression analysis, the variable of interest – marital status – is not included in the regressions estimated herein. Summary statistics are provided in Table 1. Interestingly, to-be-married men earn approximately two percent more per hour than single men; married men earn nearly 19 percent more.

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<sup>14</sup>The CPS uses the 4-8-4 rotation process for interviewing households. A particular household is interviewed for four consecutive months, then kept out for the following eight months and re-interviewed for another four months. Each year there is an incoming rotation group and an outgoing rotation group.

<sup>15</sup>For further details on the data construction, see Millimet et al. (2003).

<sup>16</sup>We use two other methods to trim outliers: (i) drop men earning wages outside the 5<sup>th</sup> and 95<sup>th</sup> quantiles, and (ii) substitute the value of wages at the 5<sup>th</sup> (95<sup>th</sup>) quantile for wages below (above) the 5<sup>th</sup> (95<sup>th</sup>) quantile. Results are similar.

As noted above, given the nature of the CPS, there are concerns about the representativeness of our sample. To investigate, we also display in Table 1 summary statistics for all married and single males in the incoming rotation group of the 1996 CPS (the year was chosen at random). In addition, we provide  $p$ -values from  $t$ -tests testing the equality of means across our single (married) sample and the 1996 complete incoming rotation group of single (married) men. The three most obvious differences between our sample and the 1996 full sample is that our married sample (i) is younger (32.80 years versus 42.10 years), (ii) has fewer children under age six (0.23 versus 0.38), and (iii) is much more likely to rent, rather than own, a home (55% ownership versus 79%). The majority of other differences are either statistically insignificant, or, where statistically significant, are minor in magnitude. To further assess the impact of the selection criteria utilized, Table 2 reports the results of OLS and fixed effect (FE) regressions using our sample. In addition, benchmark results from Korenman and Neumark (1991) are displayed for comparison. While the set of controls are slightly different, we obtain a MP of 0.151 (s.e. = 0.034) using OLS and our period two sample, versus 0.11 (s.e. = 0.02) for Korenman and Neumark (1991). Estimating a fixed effects model, we obtain a premium of 0.068 (s.e. = 0.038), versus 0.06 (s.e. = 0.03) for Korenman and Neumark (1991). Thus, in practice, our sample does not appear to be overly selective.<sup>17</sup>

In addition to highlighting the representativeness of our sample, the OLS and FE results indicate that the assumption of  $\text{Cov}(D, u_0) = 0$  versus  $\text{Cov}(D, \Delta u_0) = 0$  matters in practice. In particular, the fact that the MP coefficient falls by more than 50% in the FE model suggests that marriage is strongly correlated with time invariant unobservables that affect wages in the unmarried state. The results are only suggestive, however, as this conclusion hinges on the other modeling assumptions holding (e.g., constant treatment effect and linear functional form).

### 3.2 Panel Study of Income Dynamics

To allow for the possibility that marriage is correlated time-varying unobservables, we use cross-sectional data from the PSID from 1994. We switch from the CPS to the PSID since the latter has information on an individual's parents' marital history (if the individual is a child of an original sample member), whereas the former does not.<sup>18</sup> Moreover, we choose the 1994 wave for no particular reason other than that it is

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<sup>17</sup>Neumark and Kawaguchi (2001) document (in a parametric regression framework) that sample attrition in the matched CPS data may understate the marriage premium as the magnitude of the premium appears to be larger for men who change residences after marriage. Given the comparability of our findings to previous studies utilizing other panel data sets (such as the various NLS data sets or PSID), the size of any bias appears small at best. Furthermore, we condition on housing type in our analysis to further reduce any bias arising from non-random attrition.

<sup>18</sup>The CPS provides information only for those parents who reside with their children, which introduces additional sample selection issues.

fairly centrally spaced with respect to the time range of the CPS sample. Upon dropping observations according to the same criteria as applied to the CPS data, we obtain a sample of 976 male family heads, of which 806 are currently married and 170 are never married. Hourly wages are constructed and trimmed as before, and a similar set of observable attributes are utilized. Summary statistics are provided in Table 3. Table 4 reveals that the OLS estimate of the MP is 0.185 (s.e. = 0.047), consonant with the previous literature and our CPS result.

To perform the IV analysis, we utilize four different binary instruments based on parental marital stability, sex ratio in the state, ‘No-fault’ divorce requirement in state for property settlement and alimony, and equitable property division law in the state.<sup>19</sup> All four IVs are defined such that we expect a positive correlation between the instrument and marriage. The first instrument (IV1) is constructed from an individual’s parents’ marital status. Specifically, we trace the parents’ marital history for every male head and record the parent’s marital status from the time of the son’s birth onwards. We define  $Z_i$  as zero if there is an incidence of parental divorce or separation during the son’s lifetime, and a value of unity if the parents’ marital union remained intact through 1994. A vast sociology literature is divided on the impact of family structure on offspring marital timing. While some find that parental divorce makes marriage more likely, others show that it delays or deters marriage (e.g., Kobrin and Waite 1984; Goldscheider and Waite 1986, 1991; Avery et al. 1992; Li and Wojtkiewicz 1994; South 2001; Cunningham and Thornton 2006). Much of this ambiguity, however, has been shown to relate to the age and cohort of the individuals under study. In particular, Wolfinger (2003) finds that parental divorce greatly increased the probability of marriage in the early 1970s, but lowered the likelihood of marriage in the mid-1990s. Furthermore, Wolfinger (2003) documents that parental divorce raises the likelihood of teenage marriage, but reduces the probability of marriage conditional on remaining single until age 20. Given that our sample contains individuals over age 20 from the mid-1990s, we suspect that coming from an intact family should raise the likelihood of marriage.

Aside from the issues of correlation (which is testable) and monotonicity (which is not), the instrument must also satisfy the independence assumption. Although also untestable, Manski et al. (1992, p. 35) provide evidence that our “exogeneity assumption is not far off the mark.” Manski et al. (1992) analyze the impact of parental marital status on children’s educational outcomes, concluding that previous research on the topic that assumed the exogeneity of the parents’ marital status yielded fairly accurate inference. Given the fact that the marital decisions of children tend to occur later in life than educational decisions (Manski

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<sup>19</sup> A number of other instruments based on unilateral divorce law, as well as law requiring separation before divorce, were found to be insignificant in affecting the propensity to marry, and hence were not included in the analysis.

et al. (1992) focus on high school completion), the exogeneity assumption seems even less problematic in the current context.

The next three instruments use state-level data and hence are less likely to raise concerns regarding exogeneity. The second instrument (IV2) is based on the state-level sex ratio relative to the sample mean. Besides the strong link between sex ratio and marriage rates suggested by anecdotal and quantitative evidence, a number of studies have established a significant positive influence of sex ratio on marriage rates for women (e.g., Angrist 2002). Although sex ratio is traditionally defined as the number of males per female, the argument establishing a relationship between the marriage rate and the relative availability of marriageable partners could be made for men as well. The intuition can be traced back to Becker (1991) where the relative abundance of women is likely to tilt intra-household bargaining power in favor of men, thereby raising their incentives to marry. Since our focus is on marital formation for men, IV2 is a dummy variable set to unity if the state level sex ratio – defined as the ratio of women to men in age group 25-65 – exceeds the sample average.

IV3 is a dummy variable indicating the presence of a no-fault divorce law in the state of residence;  $Z_i$  is zero if the state divorce laws do not require fault to be established for property settlement and alimony, and unity otherwise. Rasul (2005) examines the impact of moving from fault-based to no-fault divorce on marriage rates. Since no-fault requirements for property settlement and alimony reduce *ex post* bargaining costs, and financial penalties can no longer be imposed on at-fault spouses, such a regime could lower the costs of exiting marriage leading to an increase in the incidence of marital formation. However, as individuals realize that fault need not be established as a factor for property settlement, they may be more reluctant to marry, with this effect being magnified as women become economically better off relative to men. We find that the latter effect prevails in our data, and thus define the instrument such that at-fault states are coded as  $Z = 1$ .

Finally, IV4 is an indicator for a divorce regime in which property is ‘equitably’ divided;  $Z_i$  is zero if state divorce laws invoke equitable division of property and assets, and unity otherwise. This law requires the division of property to be either explicitly equal or be divided in a “just and reasonable manner” in the spirit of modern equitable property division, according to what the judge deems fair. While the move from common law property and community property regimes in the 1970s was aimed towards improving the welfare of divorcees, little attention was paid towards the incidence on marriage. Rasul (2005) finds that the presence of equitable property division laws has a significant and independent (of other divorce



laws) negative effect on marriage rates, consonant with our findings.<sup>20</sup>

Table 4 displays the results obtained via Two-Stage Least Squares (TSLS). In addition to the MP estimates, we also display the first-stage coefficient on the instrument in each specification, as well as the Wald test associated with the exclusion restriction. In all four cases, the coefficient on the instrument in the first-stage is positive and statistically significant at at least the  $p < 0.09$  level. With all four IVs, the estimated MP is not overly different from the OLS estimate; the IV estimates range from 0.149 to 0.191, and are statistically significant at the 90% confidence level using IV1 and IV4 (IV1: coefficient = 0.180, s.e. = 0.106; IV4: coefficient = 0.191, s.e. = 0.107).<sup>21</sup> Thus, relaxing the assumption of independence between marriage and unobservables affecting wages in the single state does not affect conclusions regarding the existence or magnitude of the MP.

## 4 Distributional Results

The regression results discussed thus far estimate average (conditional mean) effects, at least for some subpopulation, under specific functional form assumptions. To assess the robustness of the results to the relaxation of these restrictions, we turn to the distributional analysis.

### 4.1 CPS

The QTEs obtained using the CPS data are plotted in Figure 1; Panel A displays the unconditional QTEs, and Panel B displays the QTEs adjusting for covariates. Moreover, within each panel, the first column uses data from the first period only and compares the to-be-married men with those who remain single in the second period. The second column uses data from period two only and compares married versus single men. Finally, the last column compares the change in wages for men who marry between the first and second periods to the change in wages for those who remain single. The top panel in Tables 5 (unconditional) and 6 (adjusted for covariates) gives the results from the tests for equality and stochastic dominance.

Comparing the unconditional distributions of wages in the first period (Figure 1, Panel A), we find positive and statistically significant QTEs, favoring to-be-married men, at nearly all quantiles below roughly the 60<sup>th</sup> quantile. This pattern of QTEs gives rise to an observed SSD ranking. In addition, we easily reject equality of the CDFs ( $p = 0.028$ ), and the simple bootstrap suggests that the SSD ranking is statistically meaningful ( $\Pr\{s \leq 0\} = 0.984$ ). Finally, it is striking that the QTEs are largest in the bottom tail of

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<sup>20</sup>Data for states that adopted an equitable property division regime are obtained from Gray (1998).

<sup>21</sup>In all four cases, we fail to reject the null of marital status being exogenous;  $p > 0.70$  in each case.

the wage distribution and decline steadily as wages rise, eventually favoring single men in the extreme upper tail. Thus, while previous research has documented a positive relationship between wages and the likelihood of marriage (e.g., Nakosteen and Zimmer 1997; Ginther and Zavodny 2001), this relationship is not uniform across the wage distribution and is clearly strongest in the bottom tail of the distribution.

Comparing the unconditional distributions of wages in the second period, we observe a similar pattern for the QTEs as in period one except now the QTEs remain positive and statistically significant, favoring married men, up through the 85<sup>th</sup> quantile. As with the first period comparison, we obtain a statistically significant SSD ranking using the simple bootstrap ( $\Pr\{s \leq 0\} = 0.998$ ), and easily reject equality of the CDFs ( $p = 0.000$ ).

Since the wage distributions favor to-be-married and married men, relative to single men, it is necessary to examine the distributions of wage changes to see if marriage is associated with any additional wage benefit. Here, we find positive and statistically significant QTEs up to roughly the 60<sup>th</sup> quantile. Again, this pattern gives rise to a statistically significant SSD ranking according to the simple bootstrap ( $\Pr\{s \leq 0\} = 1.000$ ), and we reject equality of the CDFs at conventional levels ( $p = 0.064$ ).

While the unconditional results are interesting, they are merely suggestive as they fail to control for time-varying observables correlated with both marital status and wages. For instance, Table 1 indicates that to-be-married men are better educated, more likely to belong to a union, more likely to work full-time, and are more likely to be white. To replace the assumption that potential outcomes must be independent of treatment assignment with a conditional independence assumption, we adjust for covariates. The QTE results for the first period differ markedly from the corresponding unconditional results (Figure 1, Panel B). Specifically, while the QTEs remain positive across the majority of the distribution for the first period, they are rarely statistically significant. In addition, while we observe an SSD ranking in this case, it is no longer statistically meaningful according to either bootstrap method; we also fail to reject equality of the CDFs ( $p = 0.308$ ). Thus, the unconditional wage advantage associated with to-be-married men disappears once we adjust for observables. However, the results for the second period and the first-difference model are qualitatively unchanged from the unconditional results.

In sum, the distributional analysis using the CPS yields three main findings. First, the results confirm the existence of a MP at the distributional level, but highlight its non-uniformity. The SSD rankings in period two and in the first-difference model indicate that the MP is less robust than previously documented in that such rankings are only obtained when one moves beyond simple mean wage comparisons and incorporates dispersion into the welfare criteria. The introduction of dispersion into the discussion is

necessary given the insignificant differences (or significant crossings) in the wage distributions of single and married men at high wages. Second, even at lower quantiles, the MP is not uniform; the MP is largest in magnitude in the lower tail of the wage distribution and declines nearly monotonically across the quantiles. Finally, in line with Becker (1976), Cornwell and Rupert (1997), Krashinsky (2004), and others, we find a large wage advantage enjoyed by married men prior to marriage. However, except at very low quantiles, this advantage is completely explained by differences in observable attributes.

## 4.2 PSID

The QTEs obtained using the PSID data are plotted in Figure 2; Panel A displays the QTEs without utilizing any instrumental variable, and Panel B displays the IV results. Within each panel, the first column displays the unconditional estimates, while the second column adjusts for covariates. The bottom panel in Tables 5 (unconditional) and 6 (adjusted for covariates) gives the results from the tests for equality and stochastic dominance.

Comparing the unconditional distributions of wages without utilizing any instruments (Figure 2, Panel A, left column), we obtain similar results to the CPS data (Figure 1, Panel A, middle column). In particular, we obtain positive QTEs at all quantiles except for in the extreme upper tail. However, below the 90<sup>th</sup> quantile, the QTEs are more uniform than in the CPS data, as well as slightly higher in magnitude. In terms of the statistical tests, we easily reject equality of the distributions ( $p = 0.000$ ), but we do not find any evidence of FSD or SSD.<sup>22</sup>

When we adjust for covariates in the PSID data (Figure 2, Panel A, right column), the point estimates for the QTEs are negative between roughly the 20<sup>th</sup> and 80<sup>th</sup> quantiles, although the confidence intervals are wide and encompass zero at nearly all quantiles. Despite the width of the confidence intervals, we continue to reject equality of the CDFs ( $p = 0.000$ ) and, as in the unconditional case, we do not find any evidence of FSD or SSD.

The differences between the CPS and PSID results may be a reflection of the younger, more selective CPS sample. Given that average age of the married men is nearly four years higher in the PSID sample, one might be concerned that unobservables correlated with both marital status and wages are more likely to be present in the PSID sample. Thus, we turn to the IV results.

When we do not adjust for covariates (Figure 2, Panel B, left column), we reject equality of the CDFs only when using IV1 based on parent's marital stability. In this case, the QTEs are virtually unchanged

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<sup>22</sup>Note, while the pattern of the QTEs suggests SSD, the QTEs are only displayed at integer quantiles, whereas our tests for SD utilize check for crossings of the CDFs using a much finer grid, including points below the first quantile.

from the unconditional QTEs displayed in Panel A without using any instruments; the QTEs are positive at all quantiles except in the extreme upper tail. For the other three instruments, not only do we fail to reject equality at conventional levels, but the individual QTEs are rarely statistically significant, and the point estimates for the QTEs are sometimes positive and sometimes negative. We find no evidence of FSD or SSD in any case.

When we adjust for covariates and continue to use our instruments (Figure 2, Panel B, right column), we obtain three noteworthy findings. First, we reject equality of the CDFs using IV2 (state-level sex ratio) and IV3 (no-fault divorce law), although there is no evidence of FSD or SSD using any of the four instruments. Second, using the two IVs for which we reject equality of the CDFs, we obtain positive (negative) QTEs at lower (higher) quantiles. Thus, the positive, but statistically insignificant TSLS estimates reported in Table 4 are misleading; they reflect the ‘average’ of a positive (negative) MP at low (high) wages. Finally, while we do not reject equality of the CDFs using IV1 (parent’s marital stability), we also obtain positive, statistically significant QTEs at quantiles in the lower tail (roughly below the 20<sup>th</sup> quantile).

### 4.3 Discussion

Viewing the complete set of results, we conclude that the MP is less robust than suggested by its lengthy literature. Under various independence assumptions between potential wages and marital status – either unconditionally or conditional on observables – as well as assumptions concerning functional form, we do obtain positive and statistically significant estimates of the MP. However, allowing for the presence of time invariant unobservables correlated with both marital status and wages explains over half of the MP, whereas allowing for time varying unobservables does little to impact estimates of the ‘average’ MP. Thus, estimating mean treatment effects within parametric frameworks indicates that while some of the MP may be due to selection, there is a role for causal-based explanations (i.e. specialization or employer discrimination).

The distributional results reveal a more subtle MP. When selection of unobservable attributes is disregarded, we continue to find a fairly robust, positive MP across nearly the entire distribution in the CPS data. The PSID data is more noisy, and thus difficult to interpret. When we allow for selection on both observables as well as time invariant unobservables, we obtain a sizeable, positive MP only in the extreme lower tail of the wage distribution; a positive, but much smaller MP over the remainder of the distribution, which is statistically insignificant above roughly the median. Thus, in contrast to the parametric FE estimate of the MP, we find that selection on time invariant unobservables does not explain much, if any,

of the MP at lower quantiles, but explains most of the MP over the remainder of the distribution. Finally, when we allow for selection on both observables and time invariant and varying unobservables, we find only modest evidence of a positive MP at lower quantiles, and now a negative effect of marriage at higher quantiles. Thus, in the most flexible model, we find evidence of a causal MP only in the extreme lower tail, and that selection explains the entire MP over the remainder of the distribution.

The polar effects of marriage at different parts of the distribution, along with the positive and either insignificant or marginally significant TLS estimates of the average MP, highlights the gain to the distributional analysis. Moreover, the negative effects of marriage at higher wages is consistent with an increase in the value of non-market time, while the positive MP in the lower tail likely reflects employer discrimination rather than household specialization given the inverse relationship between male wages and female labor supply (e.g., Blau and Kahn 2005). Specifically, while workers in high wage labor markets may possess a number of informative signals concerning their productivity (e.g., schooling levels, school quality, previous experience, etc.), workers in low wage labor markets may lack such signals, leading employers to place greater emphasis on marital status as an indicator of the workers' value. Finally, the fact that the SD results fail to yield any rankings in the first or second order sense implies that conclusions regarding the wage benefits of marriage depend crucially on the weight placed by policymakers on different parts of the distribution.

## 5 Conclusion

The existence of a return to marriage for men in the labor market is one of the many long-standing, stylized facts in labor economics. While the marriage premium has been well studied, all such studies (to our knowledge) focus on only one descriptive measure of the distribution of wages: the (conditional) mean. We find such a narrow focus to be costly in that the MP varies considerably across the wage distribution once the presence of unobservables affecting both wages and marital status is addressed. Specifically, the positive MP is only robust at very low wages, and there is some evidence of a negative MP at higher wages. This finding suggests a potentially important role of employer discrimination in low wage markets, and a large role of selection over the remainder of the distribution.

There are two potential limitations to these findings. First, the IV results hinge on the validity of the instruments utilized, as well as identify effects only for individuals whose marital status is determined by the instruments. Validation of these findings to other subpopulations remains the goal of future work as new instruments are uncovered. Second, the nature of the CPS data led us to focus solely on the return

to the first year of marriage, while the nature of the IV estimation (and sample size) led us to pool all married men together (regardless of length of marriage). There is some prior research that suggests that the return to marriage increases with the length of marriage (typically viewed as evidence in favor of the specialization hypothesis); see, e.g., Korenman and Neumark (1991), Stratton (2002), and Isacsson (2007). However, Cornwell and Rupert (1997) conclude that the MP represents a one-time intercept shift, as the authors fail to find a significant effect of marital duration on wages. Nonetheless, application of the distributional methods to wage distributions differentiated by years of marriage may also prove fruitful in the future, particularly if an exogenous source of variation in marital duration is found (perhaps the gender of children as there is some evidence that divorce is less frequent when a couple has a son). In any event, given the results uncovered here, future work should continue at the distributional level.

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## A Appendix: Technical Details

### A.1 Test of Equality

Inference for the test of equality of two distributions is conducted using the bootstrap procedure detailed in Abadie (2002). Specifically, we pool the two samples, resample (with replacement) from the combined sample, split the new sample into two samples, where the first  $N_0$  represent  $W_0$  and the remainder represent  $W_1$ , and compute the KS statistic. This process is repeated  $B$  times, and the p-value is given by

$$p - value = \frac{1}{B} \sum_{b=1}^B \mathbf{I}(d_{eq,b}^* > d_{eq}) \quad (\text{A.1})$$

where  $d_{eq,b}^*$  is the value of the test statistics from bootstrap repetition  $b$  and  $d_{eq}$  is the value of the test statistic obtained from the original sample. The null hypothesis is rejected if the p-value is less than the desired significance level, say 0.10.

### A.2 Stochastic Dominance

#### A.2.1 Computation of $d$ and $s$

Our procedure is as follows:

- (i) compute the empirical CDFs using either (4) and (5), depending on if one wishes to adjust for covariates, at  $\omega_k$ ,  $k = 1, \dots, K$ ,
- (ii) compute the differences  $d_1(\omega_k) = F_{1N_1}(\omega_k) - F_{0N_0}(\omega_k)$  and  $d_2(\omega_k) = F_{0N_0}(\omega_k) - F_{1N_1}(\omega_k)$ ,
- (iii) obtain  $d = \sqrt{\frac{N_0 N_1}{N_0 + N_1}} \min \{ \max\{d_1\}, \max\{d_2\} \}$ ,
- (iv) calculate the sums  $s_{1k} = \sum_{l=1}^k d_1(\omega_l)$  and  $s_{2k} = \sum_{l=1}^k d_2(\omega_l)$ , over a finite number of support points  $k$ , and
- (v) obtain  $s = \sqrt{\frac{N_0 N_1}{N_0 + N_1}} \min \{ \max\{s_{1k}\}, \max\{s_{2k}\} \}$ .

If  $d \leq 0$  and  $\max\{d_1\} < 0$ , then  $W_0$  is observed to first-order dominate  $W_1$ ; if  $d \leq 0$  and  $\max\{d_2\} < 0$ , then the reverse is observed. If  $d > 0$ , then there is no observed ranking in the first-order sense. Similar interpretations are given to  $s$ ,  $\max\{s_{1k}\}$ ,  $\max\{s_{2k}\}$  with respect to second order dominance.

### A.2.2 Inference

Inference is conducted using two different bootstrap procedures to evaluate the null of FSD (SSD), which is equivalent to  $H_o : d \leq 0$  ( $H_o : s \leq 0$ ). The first follows Abadie (2002), and is identical to the approach described above for the test of equality. Specifically, we pool the two samples, resample (with replacement) from the combined sample, randomly split the new sample into two samples, and compute the test statistics in (13) and (14). This process, which approximates the distribution of the test statistics under the Least Favorable Case (LFC) of  $F_0 = F_1$ , is repeated  $B$  times, and the p-value is given by (A.1). The null is rejected if the p-value is less than the desired significance level. We refer to this procedure as the *equal bootstrap*. However, as noted in Linton et al. (2005), the composite boundary between the null and alternative hypotheses is generally larger than the LFC set. As such, bootstrap-based tests imposing the LFC are not asymptotically ‘similar’ on the boundary, implying that the test is biased. Specifically, because  $d = 0$  or  $s = 0$  may be true even when LFC fails to hold, the above test will not have the appropriate asymptotic size. Thus, we utilize a second procedure following Maasoumi and Heshmati (2000) and Maasoumi and Millimet (2005). Now, we resample (with replacement) from each individual sample,  $W_0$  and  $W_1$ . Thus, this procedure *does not* impose the LFC (or any other portion of the null). Consequently, we do not form p-values using (A.1). Instead, under this resampling scheme, if  $\Pr\{d \leq 0\}$  is large, say 0.90 or higher, and  $d \leq 0$ , we infer FSD to a desirable degree of confidence. This is a classic confidence interval test; we are assessing the likelihood that the event  $d \leq 0$  has occurred.  $\Pr\{s \leq 0\}$  is interpreted in a similar fashion. We refer to this procedure as the *simple bootstrap*.

Table 1. Summary Statistics: CPS.

Variable	Actual Sample						CPS 1996 Comparison				Test of Equality	
	Single		To-Be-Married		Married		Single		Married		Single	Married
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	p-value	
Hourly Wage	9.32	6.58	9.51	4.59	11.09	6.06	7.99	5.99	11.77	9.56	0.00	0.01
Labor Supply (hrs/wk)	42.29	8.46	44.31	8.39	42.93	7.02	41.66	9.19	44.31	9.15	0.00	0.03
Education												
< High School	0.07	0.26	0.06	0.23	0.07	0.25	0.12	0.32	0.11	0.32	0.00	0.00
High School	0.57	0.50	0.55	0.50	0.53	0.50	0.57	0.50	0.58	0.49	0.75	0.06
College +	0.35	0.48	0.40	0.49	0.41	0.50	0.31	0.46	0.31	0.46	0.00	0.00
Age	35.75	8.34	31.63	6.41	32.80	6.75	33.14	7.66	42.10	9.94	0.00	0.00
Class of Worker												
Private	0.85	0.36	0.85	0.36	0.83	0.37	0.87	0.33	0.84	0.37	0.00	0.43
Federal	0.03	0.18	0.02	0.14	0.04	0.19	0.02	0.15	0.04	0.20	0.00	0.03
State Govt.	0.04	0.20	0.06	0.23	0.05	0.23	0.04	0.20	0.05	0.21	0.42	0.60
Local Govt.	0.07	0.26	0.08	0.26	0.08	0.27	0.06	0.24	0.08	0.26	0.00	0.85
Union Member	0.05	0.21	0.08	0.26	0.07	0.26	0.03	0.18	0.05	0.21	0.00	0.04
Full Time Status (> 35 hrs/wk)	0.91	0.29	0.96	0.20	0.96	0.19	0.86	0.35	0.95	0.22	0.00	0.30
Metropolitan Status												
Urban	0.86	0.35	0.82	0.38	0.82	0.38	0.88	0.32	0.81	0.39	0.03	0.38
Rural	0.14	0.35	0.17	0.38	0.18	0.38	0.12	0.32	0.19	0.39	0.05	0.23
Race												
White	0.79	0.41	0.82	0.39	0.82	0.39	0.78	0.41	0.88	0.32	0.66	0.00
Black	0.17	0.37	0.11	0.31	0.11	0.31	0.16	0.37	0.08	0.27	0.19	0.04
American Indian	0.01	0.09	0.01	0.08	0.01	0.11	0.01	0.08	0.01	0.07	0.83	0.01
Asian or Pacific Isles	0.04	0.19	0.06	0.23	0.06	0.23	0.05	0.22	0.04	0.19	0.07	0.08
Region												
New England	0.07	0.25	0.06	0.23	0.05	0.22	0.06	0.24	0.05	0.22	0.01	0.02
Middle Atlantic	0.17	0.38	0.16	0.37	0.17	0.38	0.17	0.38	0.14	0.35	0.86	0.05
East North Central	0.18	0.39	0.16	0.36	0.15	0.36	0.16	0.37	0.18	0.38	0.01	0.19
West North Central	0.07	0.26	0.10	0.30	0.09	0.28	0.07	0.25	0.07	0.26	0.35	0.05
South Atlantic	0.17	0.38	0.20	0.40	0.19	0.40	0.16	0.36	0.18	0.38	0.04	0.87
East South Central	0.04	0.19	0.03	0.18	0.04	0.19	0.04	0.20	0.06	0.24	0.81	0.00
West South Central	0.08	0.26	0.08	0.28	0.08	0.27	0.09	0.28	0.11	0.31	0.01	0.01
Mountain	0.06	0.23	0.05	0.21	0.06	0.24	0.06	0.23	0.06	0.24	0.13	0.12
Pacific	0.17	0.37	0.18	0.38	0.17	0.38	0.20	0.40	0.15	0.36	0.00	0.84
Home Ownership												
Own	0.57	0.49	0.55	0.50	0.55	0.50	0.50	0.50	0.79	0.41	0.00	0.00
Rent	0.41	0.49	0.45	0.50	0.45	0.50	0.50	0.50	0.20	0.40	0.00	0.00
Number of Own Children												
Under 6	0.04	0.23	0.09	0.31	0.23	0.54	0.05	0.28	0.38	0.68	0.13	0.00
Number of Observations	10482		257		294		3782		15692			

Notes: 13 occupational categories are also utilized, but are not displayed, due to space considerations. Sample restricted to those with an hourly wage between \$1 - 100. CPS Comparison Sample is obtained from the 1996 incoming rotation group. Wages are in 1982 dollars. Appropriate sample weights utilized.

**Table 2. OLS and Fixed Effects Estimates of the Marriage Premium.**

Variable	CPS Sample						Korenman-Neumark			
							OLS		FE	
	OLS			FE						
	Pooled Sample		Period Two Only		Pooled Sample					
	Coeff	Std Error	Coeff	Std Error	Coeff	Std Error	Coeff	Std Error	Coeff	Std Error
<b>Married (1 = Yes)</b>	0.217***	0.091	0.151***	0.034	0.068*	0.038	0.11***	0.02	0.06**	0.03
<b>Other Covariates</b>										
Age, Age Squared			Yes				No			
Exper., Exper. Squared			No				Yes			
Region			9 region dummies				Dummy for South			
Urban			Urban/Rural dummies				Urban/Rural dummies			
Union			Union/Non-Union member dummies				Union/Non-Union member dummies			
Occupation			13 three-digit occupational categories				8 single-digit occupational categories			
Industry			No				11 industry dummies			
Year			9 Year dummies				3 Year dummies			
Non-Spouse Dependents			Number of own children under 6				Dummy for Non-Spouse Dependents			
Schooling			3 dummies for <HS, HS, College+				5 dummies			
Race			5 dummies				No			
Full-time Status			FT/PT status dummies				No			
Own/Rent Home			2 dummies				No			
Class of Worker			4 dummies for State, Local, Federal Govt. & Private				No			

Notes: \*\*\*/\*\*/\* denotes significance at the 1%, 5%, and 10% level, respectively. Korenman-Neumark results are taken from Korenman and Neumark (1991).

**Table 3. Summary Statistics: PSID.**

Variable	Single		Married	
	Mean	SD	Mean	SD
<b>Hourly Wage</b>	15.01	13.59	19.41	12.18
<b>Labor Supply (hrs/wk)</b>	43.84	8.30	45.41	8.51
<b>Parent's Marital Status</b> (1 = Remained Intact)	0.65	0.48	0.78	0.42
<b>Education</b>				
< High School	0.11	0.32	0.11	0.31
High School	0.57	0.50	0.55	0.50
College +	0.32	0.47	0.34	0.48
<b>Age</b>	33.13	6.27	36.67	6.83
<b>Class of Worker</b>				
Government	0.19	0.39	0.17	0.38
Private	0.81	0.39	0.83	0.38
<b>Union Member</b>	0.16	0.37	0.19	0.39
<b>Full Time Status</b> (≥ 40 hrs/wk)	0.83	0.38	0.93	0.25
<b>Race (White)</b>	0.79	0.41	0.92	0.28
<b>Region</b>				
Northeast	0.21	0.41	0.21	0.41
North Central	0.24	0.43	0.30	0.46
South	0.31	0.46	0.31	0.46
West	0.24	0.43	0.19	0.39
<b>Own Children under 18</b>	0.20	0.71	1.31	1.19
<b>Number of Observations</b>	170		806	

Notes: Data from 1994 wave. Appropriate sample weights utilized.

**Table 4. OLS and Instrumental Variables Estimates of the Marriage Premium (PSID Sample).**

Variable	OLS		IV #1		IV #2		IV #3		IV #4	
	Coeff	Std Error	Coeff	Std Error	Coeff	Std Error	Coeff	Std Error	Coeff	Std Error
<b>Married (1 = Yes)</b>	0.185***	0.047	0.180*	0.106	0.154	0.103	0.149	0.103	0.191*	0.107
<b>First-Stage</b>										
Coefficient on IV			0.231*	0.133	0.317**	0.152	0.306**	0.144	0.249*	0.135
Wald Test										
$\chi^2$ test statistic			3.02		4.320		4.500		3.380	
p-value			p = 0.082		p = 0.038		p = 0.034		p = 0.066	
<b>Other Covariates</b>										
Age, Age Squared					Yes					
Exper., Exper. Squared					No					
Region					4 region dummies					
Urban					No					
Union					Union/Non-Union member dummies					
Occupation					9 Three-digit occupational categories					
Industry					No					
Year					N/A					
Non-Spouse Dependents					Number of children under 18					
Schooling					3 dummies for <HS, HS, College+					
Race					2 dummies					
Full-time Status					FT/PT status dummies					
Own/Rent Home					No					
Class of Worker					2 dummies for Government & Private					

Notes: \*\*\*/\*\*/\* denotes significance at the 1%, 5%, and 10% level, respectively. N/A = not applicable. Data are from the 1994 wave of the PSID. Instrument #1 is a binary indicator for whether the mother's marital status was married when the individual was born and remained intact until the present. Instrument #2 is a binary indicator for whether the sex ratio in the individual's state of residence is greater than the sample average. Instrument #3 is a binary indicator for whether divorce laws require 'No Fault' for division of property and alimony in the state. Instrument #4 is a binary indicator for whether divorce laws require equitable division of property and assets in the state. Wald test refers to test of significance of the instrument in the first-stage. See the text for further details.



**Table 5. Distributional Tests of Unconditional Wages.**

Distributions		Observed Ranking	Test of Equality	First Order Dominance					Second Order Dominance				
X	Y			d <sub>1,MAX</sub>	d <sub>2,MAX</sub>	d	Pr{d≤0} (Simple Boot)	Pr{d*≥d} (Equal Boot)	s <sub>1,MAX</sub>	s <sub>2,MAX</sub>	s	Pr{s≤0} (Simple Boot)	Pr{s*≥s} (Equal Boot)
<b>CPS</b>													
<b>A. Wages in Period One</b>													
Single	To-Be-Married	SSD	p = 0.028	1.970	0.270	0.270	0.000	0.814	207.632	-0.003	-0.003	0.984	0.898
<b>B. Wages in Period Two</b>													
Single	Married	SSD	p = 0.000	2.887	0.154	0.154	0.000	0.946	369.437	0.000	0.000	0.998	0.984
<b>C. First-Difference in Wages</b>													
Single	Married	SSD	p = 0.064	1.739	0.169	0.169	0.000	0.916	133.346	-0.009	-0.009	1.000	0.870
<b>PSID</b>													
<b>A. No Instrument</b>													
Single	Married	None	p = 0.000	0.384	0.253	0.253	0.006	0.786	470.747	0.383	0.383	0.272	0.628
<b>B. IV #1: Parent's Marital Stability</b>													
Single	Married	None	p = 0.000	2.539	0.185	2.539	0.004	0.860	304.782	0.015	0.015	0.230	0.786
<b>C. IV #2: State-Level Sex Ratio</b>													
Single	Married	None	p = 0.104	0.182	1.278	0.182	0.784	0.888	3.935	154.766	3.935	0.540	0.424
<b>D. IV #3: No-Fault Divorce Law</b>													
Single	Married	None	p = 0.312	0.370	1.072	0.370	0.002	0.524	3.737	61.634	3.737	0.272	0.436
<b>E. IV #4: Equitable Property Division Divorce Law</b>													
Single	Married	None	p = 0.354	0.998	0.130	0.130	0.000	0.916	87.188	6.494	6.494	0.090	0.348

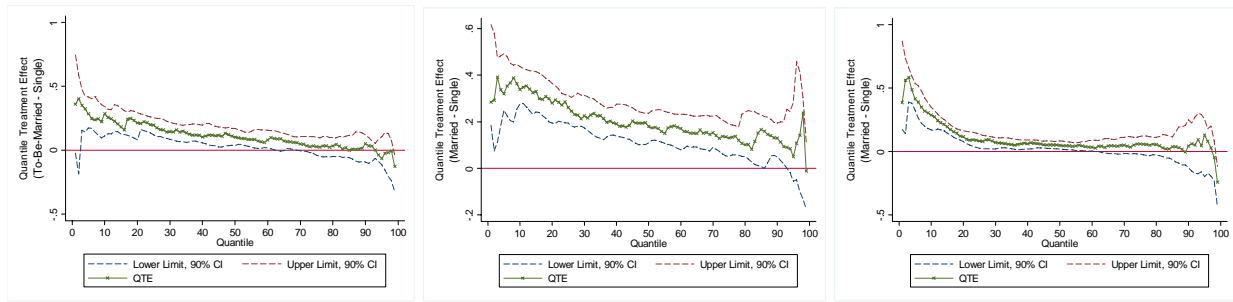
NOTES: All results use appropriate panel weights. Probabilities are obtained via 500 bootstrap repetitions. No observed ranking implies only that the distributions are not rankable in the first- or second-degree stochastic dominance sense. P-values for the test of equality and using the *Equal Boot* approximate the distributions of the tests statistics when the CDFs are equal, which represents the Least Favorable Case when the null is first- or second-order dominance; the *Simple Boot* does not. Instrument #1 is a binary indicator for whether the mother's marital status was married when the individual was born and remained intact till the present. Instrument #2 is a binary indicator for whether the sex ratio in the individual's state of residence is greater than the sample average. Instrument #3 is a binary indicator for whether divorce laws require 'No Fault' for division of property and alimony in the state. Instrument #4 is a binary indicator for whether divorce laws require equitable division of property and assets in the state. See the text for further details.

**Table 6. Distributional Tests of Wages Adjusted for Covariates.**

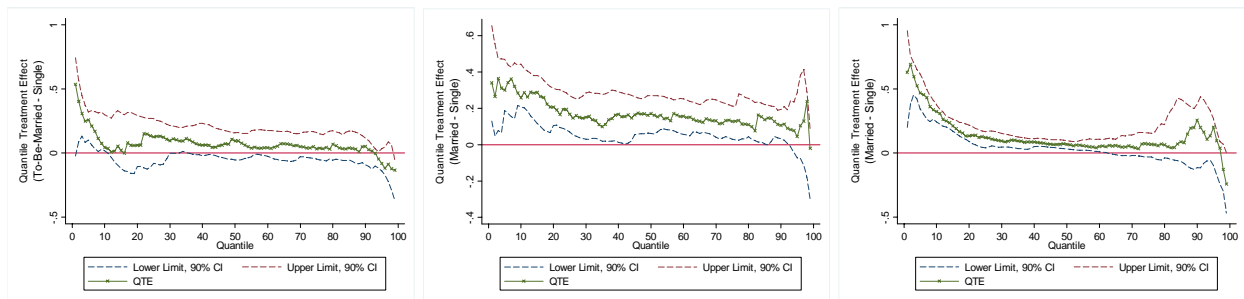
Distributions		Observed Ranking	Test of Equality	First Order Dominance					Second Order Dominance				
X	Y			d <sub>1,MAX</sub>	d <sub>2,MAX</sub>	d	Pr{d≤0} (Simple Boot)	Pr{d*≥d} (Equal Boot)	s <sub>1,MAX</sub>	s <sub>2,MAX</sub>	s	Pr{s≤0} (Simple Boot)	Pr{s*≥s} (Equal Boot)
<b>CPS</b>													
<b>A. Wages in Period One</b>													
Single	To-Be-Married	SSD	p = 0.308	1.324	0.392	0.392	0.000	0.692	132.290	-0.003	-0.003	0.888	0.854
<b>B. Wages in Period Two</b>													
Single	Married	SSD	p = 0.008	2.235	0.164	0.164	0.000	0.940	309.124	0.000	0.000	0.976	0.996
<b>C. First-Difference in Wages</b>													
Single	Married	SSD	p = 0.004	2.494	0.205	0.205	0.000	0.894	175.675	-0.008	-0.008	1.000	0.822
<b>PSID</b>													
<b>A. No Instrument</b>													
Single	Married	None	p = 0.000	2.091	5.511	2.091	0.002	0.000	15.812	228.011	15.812	0.188	0.090
<b>B. IV #1: Parent's Marital Stability</b>													
Single	Married	None	p = 0.144	1.165	0.274	0.274	0.000	0.796	104.420	7.428	7.428	0.070	0.290
<b>C. IV #2: State-Level Sex Ratio</b>													
Single	Married	None	p = 0.000	0.689	1.998	0.689	0.000	0.074	50.298	93.070	50.298	0.068	0.000
<b>D. IV #3: No-Fault Divorce Law</b>													
Single	Married	None	p = 0.002	0.726	2.096	0.726	0.000	0.064	27.624	107.455	27.624	0.050	0.028
<b>E. IV #4: Equitable Property Division Divorce Law</b>													
Single	Married	None	p = 0.202	1.063	0.414	0.414	0.000	0.522	46.929	3.836	3.836	0.124	0.420

NOTES: All results use appropriate panel weights. Probabilities are obtained via 500 bootstrap repetitions. No observed ranking implies only that the distributions are not rankable in the first- or second-degree stochastic dominance sense. P-values for the test of equality and using the *Equal Boot* approximate the distributions of the tests statistics when the CDFs are equal, which represents the Least Favorable Case when the null is first- or second-order dominance; the *Simple Boot* does not. Instrument #1 is a binary indicator for whether the mother's marital status was married when the individual was born and remained intact till the present. Instrument #2 is a binary indicator for whether the sex ratio in the individual's state of residence is greater than the sample average. Instrument #3 is a binary indicator for whether divorce laws require 'No Fault' for division of property and alimony in the state. Instrument #4 is a binary indicator for whether divorce laws require equitable division of property and assets in the state. See the text for further details.

### Panel A. Unconditional



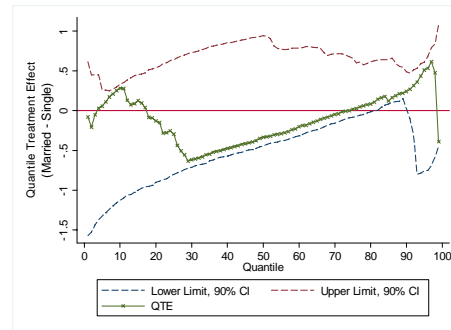
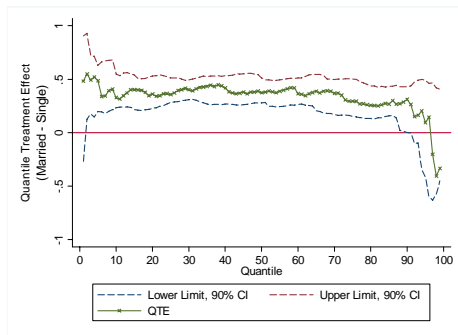
### Panel B. Adjusted for Covariates



**Figure 1. Impact of Marital Status on Wages: CPS Sample.**

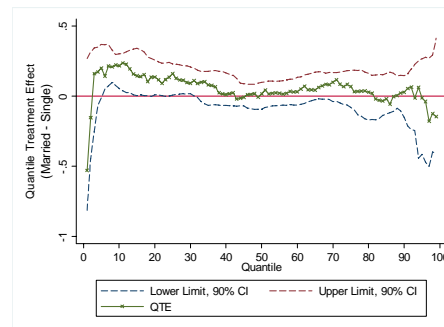
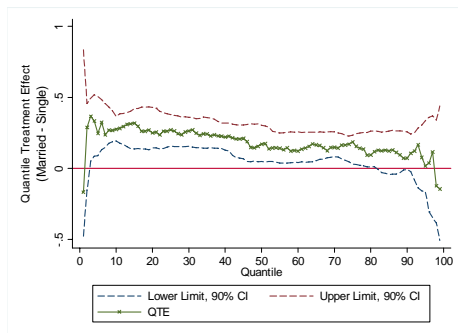
NOTES: Left (middle) [right] column in each panel contains the CDF of To-Be-Married minus the CDF of Single (the CDF of Married minus the CDF of Single) [the CDF of intertemporal wage differential for the Married minus the CDF of intertemporal wage differential for the Single]. CDFs adjusted for covariates use inverse propensity score weighting, adjusting for the covariates in Table 2.

**Panel A. Without Instrument**

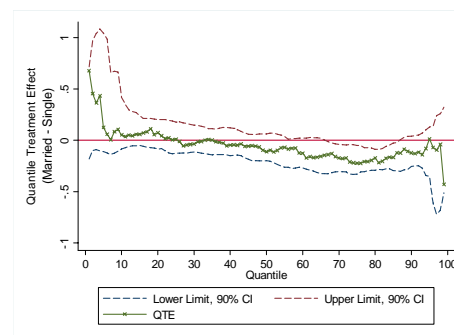
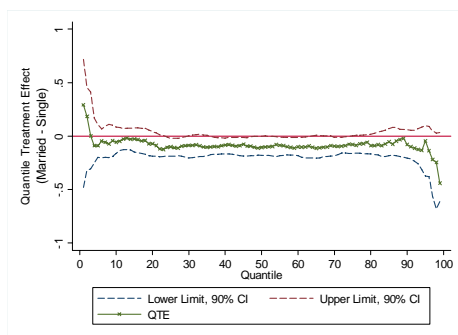


**Panel B. With Instrument**

**B1. IV #1: Parent's Marital Stability**



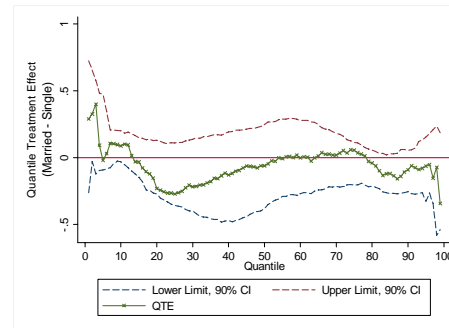
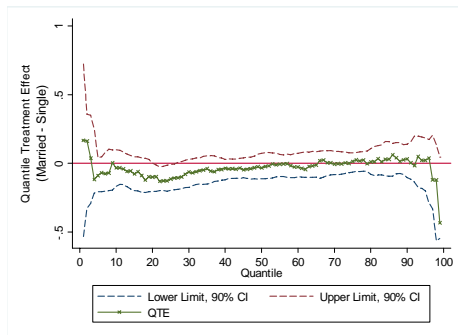
**B2. IV #2: State-Level Sex Ratio**



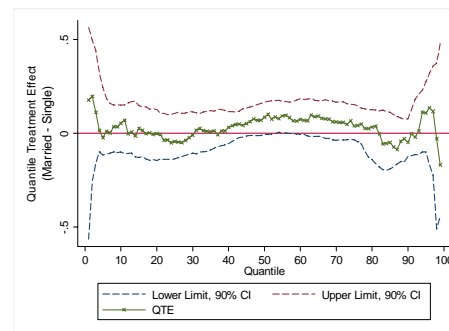
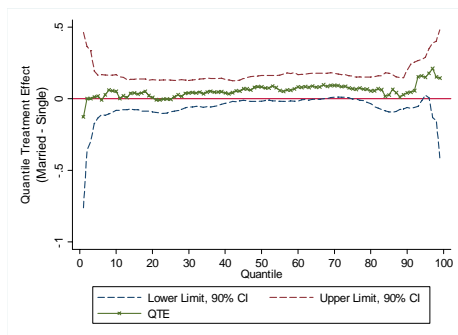
**Figure 2. Impact of Marital Status on Wages: PSID Sample.**

NOTE: Left (right) column in each panel contains the unconditional (adjusted for covariates) CDF of Married minus the CDF of Single. CDFs adjusted for covariates use inverse propensity score weighting, adjusting for the covariates in Table 4.

### B3. IV #3: No Fault Divorce Law



### B4. IV #4: Equitable Property Division Divorce Law



**Figure 2 (cont.). Impact of Marital Status on Wages: PSID Sample.**

NOTE: Left (right) column in each panel contains the unconditional (adjusted for covariates) CDF of Married minus the CDF of Single. CDFs adjusted for covariates use inverse propensity score weighting, adjusting for the covariates in Table 4.