

**SOME MEN EARN MORE, SOME MEN EARN LESS;
WHICH MEN EARN MORE WHEN THEY MARRY?**

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ABSTRACT

This article investigates the effect of marriage on male earnings through an analysis of the National Longitudinal Survey of Youth (1979-2010). Unlike prior research, this article does not assume that marriage affects men who earn a lot the same way that it affects men who earn little. The analysis shows that low-earning men marry around a time in their lives when they do particularly well: their earnings rise before they marry, peak around the fourth year, and then decline. In contrast, among high-earning men, earnings grow after and not before they marry. Recent scholarship questions the direction of causation between marriage and earnings because the average man's earnings begin to rise shortly before marriage. However, the evidence that selection into marriage rather than effects of marriage explain men's marriage premium pertains not to all but a subset of men – those at the bottom of the earnings distribution – a group of men also less likely to marry and remain married. For men higher in the distribution, marriage elevates earnings. Thus, ironically, marriage may have a causal effect on male earnings – just not on the earnings of the poor men on whom social scientists and policymakers focus most of their concern about the retreat from marriage. Marriage reinforces preexisting male earnings inequality, by increasing the distance between men at the bottom and top of the distribution. Thus, decreasing socioeconomic disparities in marriage rates will not decrease male earnings inequality—unless by a process which discourages high-earning men from marrying.

1. INTRODUCTION

Since the 1960s, sociologists and economists have explored how earnings affect whether and when men marry and the male marriage premium – the notion that marriage increases men’s earnings. These questions are relevant to theories about gender specialization in marriage based on either efficiency or social norms about men as breadwinners. Interest also stems from policy makers’ common sense but naïve notion that men’s earnings offer a solution to single mothers’ poverty—and by extension, as single mothers and their children are the majority of poor Americans—to poverty itself. Decades of scholarship focus on either marriage timing, or on the size of men’s marriage premium, yet the question of how men’s earnings relate to marriage remains unresolved. In this article, I conclude that the answer lies in the causal heterogeneity of a marriage effect; I present evidence which suggests that selectivity into marriage based on earnings is more pronounced at the low end of men’s earnings distribution, while the causal effect of marriage on earnings may exist only at the high end.

Scholars long ago established the relevance of selection into both marriage and good jobs (Heckman 1977; Michael & Tuma 1985; Heckman 1993). Potential marriage partners and potential employers both tend to rank men the same way: responsible, nice, and well-educated rank *high* while inattentive, surly, and uneducated rank *low*. In such a world it is next to impossible to separate the effects of marriage from selectivity into marriage on the basis of pay.

But the processes behind selection into marriage, and effects of marriage, may each play larger roles at opposite ends of the distribution. This suggests a solution to the puzzle, which, if true, substantively informs policy and theory. If we could find a marriage premium among low-earning men, this would provide rationale to concentrate policy efforts on the marriage prospects of men in that range of the earnings distribution because of their anti-poverty implications. If, on the other hand, the marriage premium exists only high up the distribution, then whatever its relevance to affluent couples, it is not going to reduce poverty or inequality. Moreover, if the

premium exists only for high earning men, it raises the theoretical puzzle of why marriage would induce behavioral changes or employer favor at the high end without inducing the same changes at the low end.

Past research implicitly assumes that marriage operates in a similar way for all men, and estimates the average effect of marriage on male earnings. However, marriage could affect earnings at the bottom much less, if at all. In this article, I test this assumption that marriage affects all men's earnings in the same way, which underlies decades of analyses.

To preview the results, my analysis reveals heterogeneity in the size and causal direction of the relationship between marriage and earnings across men's earnings distribution. Low-earning men exhibit the largest earnings difference in the early years of marriage, but this is less plausibly a causal effect of marriage. Low-earning men who marry tend to do so at a time in their lives when they do particularly well. Their earnings rise before they marry, peak around the fourth year, and then decline—precipitously. High-earning men earn no more around the time they marry than just previously, but their earnings grow with the length of marriage. Eventually, they exhibit the largest marriage premiums. Marriage, in sum, may have a causal effect on male earnings – just not on the earnings of the poor men on whom social scientists and policymakers focus most of their concerns about the retreat from marriage.

Marriage *reinforces* preexisting male earnings inequality. Decreasing socioeconomic disparities in marriage rates will not decrease male earnings inequality, unless by a process which *discourages* high-earning men from marrying.

Men's earnings play a part in but do not solely determine family poverty and inequality. I return to this point in the discussion. That low-earning men do not earn a marriage premium refines existing theories about the potential causes of a marriage premium, weakens support for the notion that marriage is a solution to poverty, and suggests that high-earning men have more reason to marry and remain married—as they do.

2. BACKGROUND

2.1 CAUSATION

While many authors analyze marriage's effect on earnings, others estimate earnings' effect on marrying. Earnings could affect the probability of marriage (and separation), and marriage could in turn affect earnings, resulting in a feedback loop where marriage reinforces preexisting earnings inequality.

Event history analyses consistently show that earnings predict marriage (Oppenheimer 2003; Xie et al. 2003; Sweeney 2002; Weiss & Willis 1997; Oppenheimer, Kalmijn & Lim 1997). Furthermore, recent analyses show evidence of earnings increases prior to marriage even after adjusting for person-fixed-effects which net out selection on respondents' background characteristics (Dougherty 2006; Ahituv & Lerman 2007; Killewald & Gough 2013). This contrasts with earlier analyses which argued that men's wages increased only with the length of marriage, seen as consistent with a return to enhanced productivity from gender-normative specialization (Becker 1981; Korenman & Neumark 1991).

Estimates of the average marriage premium vary widely, but there is also some consistency across datasets. Ahituv & Lerman's (2007) fixed-effects estimates from the National Longitudinal Surveys panel of men born 1957-64, Korenman & Neumark's (1991) fixed-effect estimates from the NLS panel of men born 1941-52, and Chun & Lee's (2001) switching-equation estimates from men born 1959-81 interviewed in the 1999 Current Population Survey, were 13%, 15%, and 12%, respectively.¹

Though most studies use fixed-effects to estimate a "causal" marriage premium, other studies employ instrumental variables or compare twins (Gray 1997; Chun and Lee 2001; Antonovics & Town 2004). Krashinsky's (2004) analysis of twins is the exception in not finding evidence of a marriage premium, but it includes endogenous controls (Ahituv & Lerman 2007).

In sum, researchers consistently find evidence of a substantial association between marriage and average male earnings. Some evidence suggests that marriage causally affects earnings while other evidence suggests suggest that at least a portion of the marriage premium may be realized through coresidence (such that cohabitation delivers as much premium as marriage) or selection on earnings into marriage (Dougherty 2006; Ahituv & Lerman 2007; Killewald & Gough 2013). This creates ambiguity in whether, or how much of, the association between marriage and earnings is a true “marriage premium” – a causal effect of marriage on earnings.

2.2 HETEROGENEITY IN THE RELATIONSHIP BETWEEN MARRIAGE AND EARNINGS

Although men appear to marry, on average, around a time in their lives when they do particularly well, with an earnings jump that predates the actual event of marriage, this story may not apply to all men. This section reviews theories about why men’s earnings would increase before marriage, and theories about why men’s earnings would increase after marriage, and highlights how these theories vary in their implications for inequality, and whether they suggest selection or causal effects of marriage, based on men’s positions in the male earnings distribution.

2.2.1 SELECTION EFFECTS

Men’s earnings will predict marriage if men and their potential wives believe that starting a family requires a certain standard of living or if men compete within marriage markets at least partially based on their financial resources (Easterlin 1980; Oppenheimer 1988). If men with low earnings are less likely to marry except following an upward shock to earnings, marriage will spuriously appear to increase their earnings even after adjusting for fixed background characteristics. Among men located closer to the top of the male earnings distribution, in contrast, earnings rises may not affect their marriage odds since they are above the normative threshold (Oppenheimer 2003).

Qualitative studies discuss economic “barriers to marriage” among disadvantaged men and women (Edin 2000; Gibson-Davis, Edin & McLanahan 2005; Edin & Reed 2005). Marrying a financially unstable man may be an unattractive and risky prospect for a woman even if he is the father of her children (Gibson-Davis, Edin & McLanahan 2005). Economic barriers to marriage could motivate men with low earnings to change their behaviors with the specific intent that these changes will cause them to become more marriageable. Although this should not be interpreted as an effect of marrying, we could interpret this as an effect of the marriage process. However, observational data would reveal similar patterns if men marry following an exogenous shock to their earnings. This may lead to spuriously large estimates of the marriage premium among low-earning men. Consistent with this possibility, Oppenheimer (2003) finds a “strong threshold effect” in the relationship between earnings and marriage entry; this suggests that shocks to earnings may upwardly bias estimates of men’s marriage premium at the bottom but not the top of men’s earnings distribution.

Oppenheimer (1988) further theorizes that uncertainty over future economic prospects inhibits early assortative mating. This makes marriage more likely when a man realizes his earnings potential. Further, the benefit of waiting may increase, at least for women, relative to assortative mating based on men’s potential earnings, when men’s long-run earnings are less certain (Bergstrom & Bagnoli 1993). Consistent with this, Loughran (2002) and Gould & Paserman (2003) use Census data and find lower marriage rates in areas or periods of higher male wage inequality; this could suggest that delayed or fewer marriages also result from higher levels of uncertainty in any given man’s prospects. Lower male wage inequality may be observed if men at the bottom fair better, men at the top fair worse, or both. However, this conclusion that higher (lower) inequality predicts less (more) marriage does not tell us whether declines among high wage or raises among low wage men would encourage marriage more.

2.2.2 CAUSAL EFFECTS

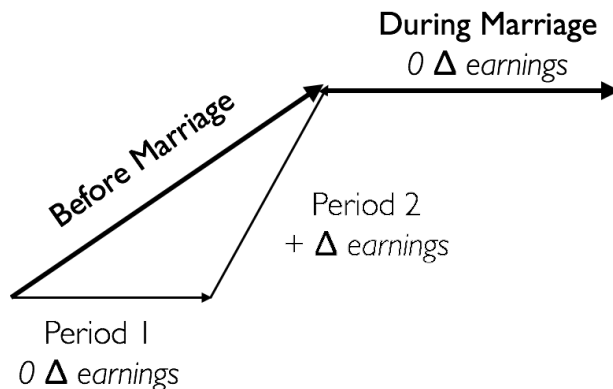
Causal effects of marriage may also vary across men's earnings distribution. Becker's (1981) model of specialization assumes that a man can support his family, as the sole-earning breadwinner (Sweeney 2002). A strict interpretation of specialization posits that it is efficient for a high earner (presumably the husband) to focus on market work, and for a low earner (presumably the wife) to focus on domestic work, so as to maximize the productivity of both in the market and domestic realms, respectively, with the idea that this maximizes total family productivity. Unless the husband earns above a sufficiently high threshold, however, a single-earner household may be infeasible. Even if this is affordable, assortative mating by potential earnings can make specialization a suboptimal economic strategy (Sweeney 2002). Therefore, I will use "specialization" more broadly, to refer to the normative practices within marriages which result from either the efficiency of specialization, or gender norms which emphasize men's market work over women's. Betty Friedan (1963) famously referred to choosing housework as a "holocaust of the mind," but some couples consider financial success a status symbol of an ideal marriage, and a woman choosing not to work for pay is a very visible sign of such success (Cherlin 2004). Another downside to specialization is that it may leave women "economically vulnerable" (Sweeney 2002). These risks are higher in marriages with low-earning men, whose financial stability may be more precarious (Sweeney 2002). Within a marriage with a low-earning husband, therefore, specialization may be irrational or unaffordable, but within a marriage with a high-earning husband, gender-normative specialization may occur even if it is economically irrational. For these reasons, a causal effect of marriage on earnings may be evident among high- but not low-earning men.

Marriages may have a causal effect on male earnings for other reasons. These include discrimination by employers against unmarried men, happiness at home, positive female influences, and men's belief that it is their duty as a married man to be a good breadwinner.

These may vary across the male earnings distribution for a variety of reasons. For example, high-earning men may be in jobs where family matters more, and the psychological characteristics of marriages may vary with men's locations in the earnings distributions, with marriages with high-earning men more likely to result in behaviors that positively reinforce male productivity – or the perception of male productivity – at work. Although it is beyond the scope of this article to examine these characteristics, these possibilities suggest additional reasons to expect a causal effect of marriage – or a larger causal effect of marriage – among men located closer to the top of the male earnings distribution.

2.3 COMPLICATIONS FOR CAUSAL INFERENCE

Figure 1. How a Fixed-Effects Model May Spuriously Estimate a Marriage Premium



The multiplicity of pathways which men may follow through the marriage process lead to complications for causal inference. The coefficient for marriage in a fixed-effects model compares each person's earnings in the periods outside of marriage to those during marriage (Dougherty 2006). For simplicity of exposition, imagine that a person's life consists of three periods and that marriage is the third period, as illustrated in Figure 1. The coefficient for marriage compares third-period earnings to the average earnings of the first two periods, weighted by their respective durations. If earnings increase in the second period, then the coefficient for marriage will be positive even if earnings increase no further in the third period,

because the reference category is not the second period but the average of the first and second periods.

Table 1. Earnings Changes through the Marriage Process

	Positive, Negative, or Neutral Earnings Slopes		Plausibly Causal Marriage Premium
	Before Marriage	After Marriage	
A.	0	+	Yes
B.	+	0	Not an effect of the event of marriage
C.	+	+	Ambiguous
D.	+	-	No

Whether earnings grow before marriage, after marriage, or both, matters for causal inference. Consider the four scenarios enumerated in Table 1.

- A. An earnings rise that occurs within and not before marriage is consistent with a causal effect of marriage.
- B. An earnings rise that occurs before marriage followed by stable earnings within marriage is inconsistent with a causal effect of the event of marriage. Possibilities include a behavioral change related to the desire to marry, or a shock to earnings that led to marriage. The former could be interpreted as related to the marriage process, but not an effect of being married. The latter is neither.
- C. An earnings rise preceding marriage followed by continued earnings rises within marriage may reflect a process that increases earnings, leads to marriage, and continues within marriage. It could also follow from an effect of the marriage process, such as an effect of engagement or cohabitation that continues within the marriage, with earnings continuing to rise. It could also reflect a combination of selection on earnings into marriage and an effect of marriage on earnings. This result would be highly ambiguous.
- D. An earnings rise preceding marriage followed by a decline early on in the marriage may reflect selection, an effect of the marriage process (e.g. an engagement effect), or both,

but one that is short-lived and dissipates within the marriage. It could also follow from a negative effect of marriage on male earnings. It is inconsistent with a lasting positive effect of marriage.

The extant literature leaves us with the third of these scenarios: earnings rise before and during marriage (Dougherty 2006; Ahituv & Lerman 2007; Killewald & Gough 2014). This result is ambiguous. Do rising earnings lead to marriage, which thereafter further increases earnings (Ahituv & Lerman 2007)? Does the anticipation of marriage and marriage itself increase earnings (Killewald & Gough 2013)? Does this solely or largely reflect selection (Dougherty 2006)?

2.4 THE PRESENT STUDY

The mean associations reported in the literature between earnings and the years before and during marriage may reflect interpolations of various pathways that different men follow. One man's earnings may rise before marriage but not after. Another man's earnings may rise during but not before marriage. This will show up as an increase in average male earnings before and during marriage. However, the true underlying process may be that some men select into marriage on earnings while marriage causally affects other men's earnings. This paper investigates whether this is true.

A review of the theoretical and qualitative literatures suggest processes that vary with men's locations in the male earnings distribution. This leads me to expect pronounced evidence of selection among low-earning men, and results most consistent with a causal effect of marriage among high-earning men. However, past research has assumed that all men experience the same "average" marriage premium. I relax this assumption with quantile regression methods, which I discuss further in the *Method* section.

For simplicity of exposition, I refer to high- and low-earning men. To further clarify, note that men may be high- or low-earning, or somewhere in between, at different points in their

lives. These hypotheses relate to how marriage affects a man's earnings at a particular point in time, relative to his counterfactual position in the earnings distribution at that time. For example, a graduate student may have comparatively low earnings, and then in a sudden shift, become a professor with comparatively high earnings. A male graduate student may focus on graduating and earning more if he meets a fiancé, and we may therefore observe selection effects. Later, the same man may be a comparatively high-earning professor, and we may observe causal effects. In a more general scenario, a man may lose a high-paying job or gain success during marriage, and when that man is high-earning, specialization may occur, but when that same man is low-earning, specialization would not occur. These questions, in short, relate not to persons but to person-years. Regardless, as I will show, while men's earnings change, men do not generally switch from being high-to-low-earning or vice-versa.

2.5 ADDITIONAL CONCERNS

Prior research suggests the importance of accounting for the ways race, hours, and union transitions relate to the relationship between marriage and earnings. Men's marriage differential may vary by race, may come in part through hours worked, and may be evident outside the context of marriage, e.g. in nonmarital cohabitation (Ahituv & Lerman 2007; Killewald & Gough 2013).

Race: Analyses of the marriage premium typically do not report separate estimates by race. The exception is the analysis by Ahituv & Lerman (2007) who report larger effects among black men than for the overall population. Black men typically earn less, and failing to take race into account in a distributional analysis could bias estimates; a larger marriage premium among low-earners could stem from the racial composition of locations in the earnings distribution.

Hours Worked: Hours worked may increase just before or after marriage. Men may increase their hours upon marriage, with the result of higher earnings. Or hours worked could

increase prior to marriage, and explain all or a part of men's selection into marriage on earnings. A pre-marriage run-up in hours worked may occur among low-wage but not high-wage workers.

While scholars typically investigate how marriage increases the average man's wages, Ahituv & Lerman (2007) also extend this to earnings: They multiply marriage's effect on wages by its effect on hours worked. However, a low-wage man may work fewer hours and a point along the wage distribution may not correspond to the same point along the earnings distribution. Therefore, I regress earnings quantiles on marriage, and in some models control for hours worked. Since controlling for hours worked imposes a functional form on the relationship between hours, wages and earnings, I also estimate wage regressions as a sensitivity test.

Cohabitation: Analyses of the effects of marriage must distinguish them from the benefits of other types of partnerships, especially unmarried cohabitation. Though cohabitation may not offer the same legal protections as marriage, it does offer the benefits of coresidence, can be seen as a step toward marriage, and men's behaviors may change at some point in the marriage process prior to exchanging wedding rings; prior research shows that cohabitation reduces men's risk behaviors, though less consistently than marriage does (Duncan, Wilkerson & England 2006). Therefore, I estimate whether marriage increases men's earnings not just relative to remaining unmarried, but also relative to non-maritally cohabiting.

3. DATA

I analyze black and white men from the National Longitudinal Survey of Youth. First administered in 1979 to 14-22-year-olds, this survey roughly corresponds to the late baby boom cohort. I use data from 1979 through the latest wave available, collected in 2010 when the respondents reached 45-52. Men were re-interviewed each year through 1994 and every two years subsequently. Because Dougherty (2006) suggests that men who never marry may systematically differ from men who marry at least once, I exclude men who never marry from the dataset. I also exclude students and oversamples which were dropped midway through the

panel. I perform a number of robustness checks with respect to case selection and attrition. I discuss these in the *Sensitivity Analyses* section. The analytic data includes 1,847 white men and 852 black men. Each man averages 16 person-years and the analytic data include 42,168 person-years in all.

I report race-specific means and percentages for the analytic data in Table 2. I also report descriptive statistics by earnings terciles (low, medium, high). I place men into terciles based on their ranks in each wave because the bottom and top terciles would otherwise reflect period and age effects.

I also report these terciles separately by race. Note that these terciles, as with the quantiles in the regressions later on, reflect men's earnings distribution and not race-specific earnings distributions. Hence, there are a similar number of white men in the bottom and top thirds, but far fewer black men in the top third than there are in the bottom third.

Table 1 shows that high-earning men have more education, higher test scores and work longer hours. They spend less time cohabiting and more time married. Differences in union status arise as high-earning men *remain* married (to their first wives) longer. These differences emerge fairly early on after first marrying: There are about twice as many high-earning men as there are low-earning men in their 4th year of first marriage, and 1½ times as many in their 10th.

Appendix Tables 1A and 1B report descriptive statistics by third for white and black men, respectively, for each of three waves: 1990, 2000 and 2010. These show stability in the characteristics of low-, medium-, and high-earners across waves.²

4. METHOD

4.1 QUANTILE REGRESSION

The extant theoretical and qualitative literatures strongly suggest that pathways through the marriage process will vary by men's economic positions, particularly with respect to their ranks in the earnings distribution at each point in their lives (Oppenheimer 1988; Bergstrom & Bagnoli

1993; Edin 2000; Loughran 2002; Gould & Paserman 2003; Gibson-Davis, Edin & McLanahan 2005; Edin & Reed 2005; Schneider 2012). The precise thresholds which delineate these groups are unclear and an advantage of quantile regression is the ability to estimate associations across a continuous distribution without making assumptions pre-regression about where these thresholds are. I adjust for background characteristics as these may relate to earnings rank and pathways through marriage, and because prior research emphasizes substantial differences between black and white men, I interact all covariates with race (Wilson 1987).

To estimate marriage's effect on male earnings, I use the method described by Firpo, Fortin & Lemieux (2009), which they call unconditional quantile regression (hereafter, UQR). Covariates help to net out spurious associations between marriage and earnings, and the "unconditional" in the procedure's name distinguishes it from the more well-known method of Koenker & Bassett (1978), which, in multivariate models, estimates conditional quantiles – that is, earnings measured relative to covariates. I use the UQR procedure because person-fixed-effects are necessary to adjust for selection, but their inclusion in conditional quantile models results in counterfactuals where the quantiles refer to residuals within each man's own distribution rather than whether a man has high or low earnings relative to other men. (For an extensive discussion of this issue, see Killewald & Bearak [2014].) In a simpler example, imagine a model which controls for educational attainment. A high school dropout who earns more than other high school dropouts will be located at a high conditional quantile, even though he earns little. This conflicts with the conceptualization of high- and low-earning men heretofore discussed. In addition, I am interested in the effects of marriage on societal and not residual inequality. The solution is to use UQR. I return to this issue in the *Sensitivity Analyses* section.

UQR can be estimated by way of least squares on a transformed dependent variable, the recentered influence function (hereafter, RIF).

$$RIF(Y, \tau) = q(Y, \tau) + (\tau - \mathbf{1}\{Y \leq q(Y, \tau)\}) / f(Y, q(Y, \tau))$$

The RIF takes two parameters – Y , the outcome variable, and τ , the desired quantile. In the equation above, q is the quantile function, f is the density function, and $\mathbf{1}$ is the indicator function. If, for example, we are interested in the marriage premium for men at the median of the earnings distribution, then $q(\tau)$ is equal to men’s median earnings, $f(Y, q(\tau))$ is the density of men's earnings distribution at the median, and $\mathbf{1}\{Y \leq q(\tau)\}$ is equal to 1 for observations at or above the median and 0 for observations below the median. To estimate men’s median instead of mean earnings, simply estimate a least squares regression but replace the dependent variable, Y , with $RIF(Y, 0.5)$. Similarly, for the 90th percentile, estimate $RIF(Y, 0.9)$, and so forth for any other quantile.

I do not weight my regressions, since weights can cause as well as correct biases (Winship and Radbil 1994), and I provide separate estimates by race (from models fully interacted by race) so that the overrepresentation of blacks in the regressions does not bias coefficients even if effects differ by race.

I bootstrap standard errors to incorporate the uncertainty involved in the estimation of the RIF. Each bootstrap simulation repeats each stage of the procedure, starting from the beginning. For UQR as well as CQR, I resample the design matrix 1000 times. I refer to coefficients or differences in coefficients as significant if $p < .05$ on a two-tailed test.

As with past research, I discuss how effects on quantiles of the male earnings distribution may reflect effects on low-earning and high-earning workers (Firpo et al. 2009). This interpretation relies on the assumption of some degree of rank preservation after adjusting for covariates; that is, that observations at the same quantiles of a pair of counterfactual distributions are conditionally comparable (Djebbari and Smith 2008). I return to this point again, and discuss what this means in the context of the results, in section 6.7.

It may be illustrative to briefly discuss how much variability exists in men's unconditional ranks as they progress through life. Appendix Figure 1 shows the proportion of men whose percentile in the earnings distribution differed before adjusting for covariates by at least one third (33 percentage points) or one sixth (17 percentage points) between 1996 (chosen as a year near the center of the period) and each other year. The asymmetry in the curves reflects the well known fact that earnings among young adults in their first years of employment are often erratic and less predictive of their later wages; this is why high proportions of men deviate at least a sixth or third from their 1996 rank in the early years of the survey (recall that they were 14-21 years of age in 1979, but the analytic data excludes years when they were students). Focusing on the right-hand half of the curves, the "Moved 1/3" curve is at .17 for 2010, showing that only 17% of men moved at least 33 percentage points in rank between 1996 and 2010. Part of the motion through the earnings distribution will reflect, for example, life course transitions including marriage which occur at various ages. This figure highlights that, on the one hand, earnings ranks are comparatively stable during the adult years, but also that earnings are not like a background characteristic by which men may be exogenously grouped.³ A further advantage of quantile regression is that men's ranks at each observation reflect their actual, time-varying locations in the distribution.

4.2 MODELS

The dependent variable in all models is the natural logarithm of weekly earnings, measured in constant dollars (using the consumer price index to adjust for inflation). Consistent with past literature, I use person-fixed-effects to address selection (Korenman & Neumark 1991; Loh 1996; Gray 1997; Dougherty 2006; Ahituv & Lerman 2007; Killewald & Gough 2013). Fixed-effects adjust for constant differences between men in unobserved characteristics related to both marriage and earnings. In addition, I control for age, educational attainment, test scores, geographic area, and wave. Since respondents may also differ in their earnings trajectories

(Killewald & Gough 2013), I include three-way interactions between age, race and educational attainment, and between age, race and test scores. Age and test scores are specified quadratically, and other controls are specified categorically.

The key independent variable in my models is a categorical variable for first marriage. I include indicators for each of the first through the ninth year of marriage and an indicator for all later years of marriage. In addition, I include indicators for each of the four years which precede marriage. This tests for evidence of an earnings bump that occurs prior to the actual event of marriage. I also include indicators for cohabitation, separation and remarriage. These allow for clear definitions of the reference category against which men's marriage differential is measured. For example, married men may be compared to unmarried men who have not yet married and do *not* live with an opposite-sex partner, or to unmarried men who have not yet married and *do* live with an opposite-sex partner. When married couples separate, the year indicators turn off, and the separation indicator turns on, so that any negative effects of separation bias neither the estimates of marriage nor the reference category of having not married. If they remarry, the year indicators remain off, and the remarriage indicator turns on. Thus, first marriage and remarriage are not confounded. The year indicators do not simply reflect time since marriage – they reflect *enduring* marriage. If an effect of marriage exists, it should exist in enduring first marriages.

My main analysis focuses on two models. Model 1 contains these variables, and Model 2 further adjusts for hours worked. The first model shows whether the marriage premium emerges before or after marriage, as would be consistent with selection or causal effects, respectively, and, as a further test to distinguish between these potentialities, whether the marriage premium grows or dissipates with the length of marriage. The second model ascertains how much hours worked statistically explains these relationships.

Following a discussion of the main results, I discuss additional analyses which address children, wages, conditional earnings, the functional form of the marriage premium and case

selection, and I address limitations with respect to causal inference and conditional rank preservation.

4. RESULTS

Studies consistently report a sizeable marriage differential. Recent studies note that male earnings jump before the marriage actually occurs (Dougherty 2006; Ahituv & Lerman 2007; Killewald & Gough 2013). This finding could indicate selection on earnings into marriage, but this does not disprove the existence of a marriage premium. This could also follow from another aspect of the marriage process – whether premarital cohabitation, engagement, or some other phenomenon related to finding a marriage partner or the desire to marry. Tables 3A-B and 4A-B report the associations at various quantiles between men’s earnings and the years preceding and following marriage, as well as the results of significance tests which compare estimates between pairs of quantiles, for white and black men, respectively. These results reveal no evidence of selection among men in the top half of the earnings distribution. High-earning men, in short, exhibit a plausibly causal return to enduring marriage. For low-earning men, in contrast, earnings actually drop – precipitously – after a few years of marriage, relative to what the model predicts someone of their age and education to have. If earnings increased just before marriage, and thereafter remained stable or increased further, as prior studies have found, one could make the case that the low-earners earned more due in part to the marriage process, and in part to the actual marriage. But their earnings do not increase, or remain stable – they dip, substantially and significantly. High-earners might very well earn a marriage premium, but low-earners do not.

Table 3A shows associations at various quantiles with each year of marriage, as well as cohabitation and the run-up to marriage, among white men. As Table 3A shows, low-earning white men exhibit an earnings bump before marriage, but only when they cohabit. Cohabiting is associated with a 0.12 ($p < .05$) increase in log earnings at the 10th percentile of men’s earnings distribution. The positive association with cohabitation could follow from effects of the marriage

process, or this could reflect selection into cohabitation and marriage. Notably, the association between marriage and earnings among low-earning men dissipates early on in a marriage. The marriage differential for men at the 10th percentile peaks in the fourth year of marriage, at 0.23 ($p < .001$), then loses significance by year six, and declines to 0.01 by year 10. Because this model includes indicators for the run-up to marriage, the reference category is more than four years prior to marriage. Table 3B changes the reference category for marriage to cohabiting the year before marriage. None of the associations at the 10th percentile remain significant. This emphasizes that while marriage is associated with higher earnings among low-earning men, they earn no more than they did while cohabiting before the marriage. The negative, though insignificant, coefficient of -0.10 for men at the 10th percentile in their 10th year of marriage is compatible with a return to the earnings level that was evident prior to the start of nonmarital cohabitation. Compared to the peak which occurs around year 4, this represents a drop of -0.21 ($p < .001$). This detail is important, because this is not simply a “negative” finding in the sense of failing to find a statistically significant positive association – *this is a statistically significant drop*. Low-earning men appear to marry around a time in their lives when they do particularly well. Shortly thereafter, their earnings decline, precipitously. These results are wholly incompatible with a causal effect of marriage among low-earners.

A very different pattern is evident among high-earning white men. Log earnings increase 0.2 by the 10th year of marriage among white men at the 90th percentile of the male earnings distribution. In contrast to the pattern of selection evident among low-earners, there is no evidence of an earnings bump in cohabitation or in any of the years preceding marriage. High-earners, unlike low-earners, exhibit a plausibly causal – and sizable – return to enduring marriage.

Figure 2 graphs the return to marriage, for each individual year, relative to cohabiting the year before marriage. This figure includes two curves. The dark curve shows how the low-

earners' ephemeral jump spikes by the fourth year of marriage, and then falls. By a decade of marriage, low-earners earn less than they did when the marriage began. The light curve shows how, among high-earners, the marriage premium grows as the marriage endures.

Figure 3 shows the same associations by year for black men. Among low-earning black men at the 10th percentile of the distribution, these results show growth within the first two years of marriage, followed by a precipitous decline to 0 that begins after year 6. None of these associations are significant, relative to cohabiting the year before marriage (Table 4B), though they are when compared to the period more than four years prior to marriage (Table 4A). This shows that earnings jump, but not because of the marriage, and then, after a few years of marriage, earnings fall. The results for low-earning black men, thus, are broadly consistent with the results for low-earning white men. They marry in the midst of an earnings bump, but the marriage does not sustain this.

In contrast to the results for high-earning white men, black men at the very top of the distribution do not exhibit a return to enduring marriage. The return to enduring marriage is significant among black men at the median, with no evidence of an earnings bump before marriage, but estimates are insignificant at all higher quantiles, and all lower quantiles show evidence of selection.

The results for black men are more difficult to interpret. The results for both races are broadly consistent with the expected finding of selection among low earners, and the results for white men offer clear support for a larger and more plausibly causal effect of marriage among high-earners.

For simplicity of exposition, the preceding discussion focused on two quantiles. Now consider Figure 4, which shows associations at various quantiles with the 10th year of marriage (from . Tables 3B and 4B). These reveal nearly monotonically increasing associations with enduring marriage as earnings go up among white but not black men. Overall, black men exhibit

larger associations with marriage, but this may follow from selection rather than an actual effect of marriage. High-earning white men exhibit the largest returns to enduring marriage, and, as Figure 2 showed, returns consistent with a causal effect of marriage.

Table 5A reports the results of a model which adjusts for hours worked, for white men. This table is otherwise similar to Table 3B – it shows associations at various quantiles with the run-up to marriage, and with enduring marriage, and measures enduring marriage relative to cohabiting a year before marriage. Note that associations with the 10th year of marriage do not meaningfully change from Table 3B. For example, the association among white men at the 90th percentile moves from 0.20 to 0.19, and the negative association among white men at the 10th percentile remains the same -0.10. Statistically adjusting for hours worked affects the run-up to marriage – the associations with cohabitation and the early years of marriage that were evident among white men in the bottom half of the distribution. For example, the coefficient for cohabitation declines by half, from 0.12 to 0.06, and becomes insignificant. Whereas past research argued that hours worked contribute meaningfully to men’s marriage differential (Ahituv & Lerman 2007), these results show that the short-lived earnings bump evident among low-earners around the time they marry reflects increased hours, but effects of enduring marriage do not. Marriage might increase how much high-earning men earn, but marriage does *not* increase how much men work.

In short, white men in the top half of the distribution exhibit a return to marriage and no evidence of an earnings bump prior to marriage. The results for high-earning white men are, as expected, consistent with a causal effect of marriage on earnings. Also consistent with the hypotheses, results for low-earning men, black or white, suggest an earnings bump around the time they marry, and not a return to enduring marriage.

6. SENSITIVITY ANALYSES & LIMITATIONS

This section discusses sensitivity analyses with respect to children, wages, conditional earnings, the functional form of the marriage premium and case selection, and addresses limitations with respect to causal inference and conditional rank preservation.

6.1 CHILDREN

Having children is endogenous to marriage, but I perform supplemental analyses which account for childbearing to address the concern that a growing marriage premium within the later years of marriage really picks up a fatherhood premium. In these models, I adjust for having children with a categorical variable in which the reference category is no children, and the other levels are one child, and two or more children. I interact this with each of the years of marriage. As discussed, high-earning white men exhibit a return to enduring marriage. This return is larger, as Figure 5A shows, among white men in the top tercile, when adjusting for children and showing the return to a marriage with two children, and comparing this to the results previously shown, for a typical marriage, in a model which does not adjust for childbearing. What about a return to childless marriages? Figure 5B shows that the return to marriage, from a model which does not adjust for childbearing, is similar to the return to a marriage with *zero* children, from a model which *does* adjust for childbearing, for years 1-9. Earnings increases are not associated with marriages that have no children a decade or more out, and this suggests a difference not related to effects of fatherhood but to differences over the long-term between marriages which do and do not lead to children. Among high-earning white men, an additional earnings increase is associated with having children, but this does not explain the marriage premium.

6.2 WAGES

So as not to impose a functional form on the relationship between marriage, hours and earnings, I also estimate Model 2W, in which I regress wages (as opposed to earnings as heretofore discussed) on marriage and covariates. Note that earnings, as the product of hours and wages,

allows for more gradation than the hourly rate of pay. As such, the unconditional earnings and wage distributions relate to one another but are distinct, and the wage distribution may miss some variation.

Table 5B shows results which take wages instead of earnings as the dependent variable. Associations with the 10th year of marriage are insignificant and zero or nearly zero among white men located below the median, and 9-10% among white men located at or above the median. A caveat is that the estimate at the 90th percentile is not statistically significant, although it is at least as large as the point estimate at any other quantile, and the estimates are significant at all other quantiles from the median and above. In results not shown (but available in the online supplement), which adjust for fatherhood, associations among high-earners with children are notably larger, ranging from 9-18%, monotonically increasing from the 50th-90th percentiles (all significant, as are all comparisons of quantiles symmetrically distinct from the median).

6.3 CONDITIONAL EARNINGS

There may exist differences, e.g. by class, race, and the cost of living in different areas, which could relate to differences in what couples consider necessary to start a family, given that marriage markets are segmented by these factors (Easterlin 1980; Oppenheimer 1988; Watson & McLanahan 2011). This suggests that conditional earnings, which is to say, men's locations not in the overall male earnings distribution but in within-group distributions, could also relate to the marriage process.

Conditional quantile models pose a challenge for interpretation. In particular, the inclusion of person-fixed-effects results in an idiosyncratic distribution which in turn leads to inappropriate counterfactuals because the earnings quantiles reflect differences within each man's own distribution (Koenker 2004; Killewald & Bearak 2014). With or without person-fixed-effects, such models (available in the online supplement) show evidence of selection at the bottom, and are thus broadly consistent with the idea of relative economic thresholds for

marriage. They contrast with the unconditional quantile models in that the gradient between quantiles is far less pronounced, and the association with enduring marriage is larger at the bottom. Thus, the pattern of plausibly causal effects among high-earning men, and selection among low-earning men, appears to reflect real and not relative earnings.

A limitation of this analysis is that it does not separately analyze men by marriage markets, except by race, and marriage markets might interact with men's real earnings. A much larger dataset, such as the current population surveys, could permit interactions by marriage markets – e.g., by estimating separate results delineated by race, education and area (Watson & McLanahan 2011). Unfortunately, this would not permit including robust controls for selection. In addition, these data do not include earnings during cohabitation and the run-up to marriage, an important limitation as accounting for earnings changes in the run-up to and with respect to each year of marriage is central to testing for evidence consistent with selection and causation.

6.4 THE FUNCTIONAL FORM OF THE MARRIAGE PREMIUM

The online supplement also includes results which specify marriage as a dichotomy. This results in associations which are larger at the bottom. Low-earning men exhibit massive selection effects in the early years of marriage, and their marriages are typically shorter. When specifying marriage as a dichotomy, the estimates for low-earning men are heavily weighted by these early years of marriage when the upticks that selected them into marriage have not yet fully dissipated. The returns for men in the top half also take time to emerge, and, particularly since the panel includes many more observations in the earlier years of life, most married person-years are before the end of the first decade even in the top tercile. This will lead, thus, to the wrong conclusions. Specifying marriage as a dichotomy – i.e., without the distributed fixed effect – misses the pattern of a peak about the time of marriage entry and the precipitous decline shortly thereafter among low-earners, along with the gradual growth among high-earners.

6.5 CASE SELECTION

My analytic data excludes students, oversamples dropped midway through the panel and never-marriers. To see if this led me to understate effects of marriage, in the online supplement, I include the results of otherwise identical analyses that apply none of these restrictions; only excludes students; only excludes “never-marriers;” and only excludes the dropped oversamples. I also include results that drop all waves subsequent to 1996 since several papers have been limited to those years; and results which drop these waves as well as students, the dropped oversamples, and the “never-marriers.” My findings are substantively consistent across these alternative estimation samples. These sensitivity tests also show that my findings are robust to period effects and attrition. When analyzing all men, the penalty and premium at the 10th and 90th percentiles in the 10th or later year of marriage are -0.32 and 0.25, respectively. Recall the estimates from the analytic data, which were -0.1 and 0.2, respectively. The results heretofore discussed, thus, are, if anything conservative.

6.6 CAUSATION

To what extent should these results be interpreted descriptively, and to what extent causally? Including indicators for the run-up to marriage, though used in recent analyses (Dougherty 2006; Killewald & Gough 2013), may be seen as violating the principle that the future should not be used to predict the past. I don’t use these indicators to demonstrate a causal effect of marriage which occurs before marriage, but to confirm that the marriage differential does not occur prior to the actual event of marriage. There is no such evidence among high-earning white men, and their results are thus consistent with a causal return to marriage. Prior research has also suggested that positive associations in the run-up to marriage, or in premarital cohabitation, suggest selection rather than an effect of marriage (Dougherty 2006; Killewald & Gough 2013). However, such results do not disprove an effect of marriage, or, more loosely, of the marriage

process. The precipitous earnings *declines* within enduring marriage among low-earners, however, are wholly inconsistent with a causal effect of marriage.

6.7 CONDITIONAL RANK PRESERVATION

Quantile regression methods estimate effects on the earnings distribution. As previously noted, these may be interpreted as effects on high- and low-earning workers assuming conditional rank preservation; that is, that person-year observations near each quantile are conditionally comparable, after adjusting for covariates. I hypothesized that low-earning men would select into but not experience a causal return to marriage, and I hypothesized the opposite for high-earning men. If these hypotheses are correct, then enduring marriage should push up the top of the male earnings distribution, but not the bottom. Given the results heretofore discussed, if effects at high and low quantiles did *not* correspond to effects on high- and low-earning workers, marriage would not only have to substantially reorder the distribution, but it would have to shift low-earning workers *above* the counterfactual top, and high-earning workers *below* the counterfactual bottom, all the while resulting in a roughly monotonically increasing shift from lower to upper quantiles. The results are consistent with the hypotheses with respect to high- and low-earning workers. Moreover, an alternative explanation in which the effects at high and low quantiles do not roughly correspond to effects on high- and low-earning workers strains credulity.

7. DISCUSSION

Low earning men marry at a time when they do particularly well relative to their own past earnings; indeed, they may marry only should such a time occur. They may well marry because of selectivity on earnings into marriage. This could also indicate that low earning men in serious relationships make a push to conform to the norm of a male breadwinning partner, which they see as part of what marriage means. Their earnings continue to rise for a few years after marriage, which, if a marriage premium, does not last long. Their earnings then typically plummet to a level no higher than their earnings were before the elevation that preceded their

marriage, adjusting for age. They do not appear to experience any marriage premium for enduring marriage, and, indeed, fewer of their marriages endure than high earning men's.

By contrast, high earning men experience no elevation to their earnings before marriage, adjusting for age. This suggests that marriage may truly cause the rising earnings they observe after marriage. Moreover, their marriage premium lasts, and grows, as their marriages endure. They appear to experience a premium for enduring marriage.

Decades of scholarship focus on men's marriage differential. Some scholars focus on whether marriage increases earnings, while others estimate whether earnings increase marriage odds. This fuels a large mainstream political and academic debate spanning demography, economics, sociology – and, with respect to additional outcomes like happiness – psychology, about whether men's marriage differential truly reflects a marriage *premium*. This debate concentrates on the timing of the marriage premium. Does it emerge before or after marriage, or, with ambiguous implications for causal interpretations, both before and after the wedding? As this article shows, the timing of the marriage differential matters a great deal to properly modeling it. But past research ignores distributional implications. In an unequal society, low and high earning men may share very different experiences in a number of processes – including marriage. But attempts to resolve the debate on men's marriage premium have not not addressed this common-sense notion, that things tend to operate differently between the bottom and the top.

7.1 UPDATING RECENT FINDINGS

7.1.1 SPECIALIZATION

Dougherty (2006) and Killewald & Gough (2013) argue that their findings are inconsistent with gender-normative specialization that occurs within marriages, because earnings begin to grow prior to the actual event of marriage. However, my results show that the marriage premium is compatible with specialization for white men in the top half of men's earnings distribution. As specialization predicts, there is a return to years married. That this monotonically increasing

return to an enduring marriage pertains to a (substantial) subset of but not all men has led to the misleading conclusions of prior studies, which employed mean regression methods. Past analyses assumed that all men experienced marriage in the same way, and analyzed the mythical “average” man. However, men who earn a lot and men who earn little experience marriage differently.

Differences in the experiences of high and low earning men could relate to assortative mating or specialization. Knowing the relationship between husband’s and wife’s earnings, and whether this differs between high and low earning men, could offer additional perspective on these questions. For a number of reasons, however, I do not interpret regressions which adjust for spouse’s earnings. First, non-trivial numbers of low earning men skip questions about their spouse’s earnings (while high-earning men nearly always report it). Whether this variable is missing, in short, is associated with men’s earnings quantiles, and it is not clear in which way this would bias estimates. Second, the spouse’s earnings are known during, but not before or after, marriage. As a result, I cannot distinguish between specialization and assortative mating. All this aside, in such regressions, the wife’s earnings negatively predict the husband’s among white and not black men. Lacking good productivity measures, I make no claims about whether these patterns reflect specialization – only that in contrast to conclusions derived from recent mean regression analyses, these findings reopen the door to its possibility. Alternative possibilities include employer discrimination among men in high-paying jobs, and positive behavioral change that results from marital commitment.

7.1.2 INEQUALITY

In the last half-century, male earnings inequality has increased while marriage rates have declined among low-earning men. Studies show that lower earnings inequality is associated with higher marriage odds (Loughran 2002; Gould & Paserman 2003). This suggests that increases in male earnings inequality lead to lower marriage rates among low-earning men, which in turn

further exacerbates male earnings inequality. My findings suggest that the beginning of marriage, but not enduring marriage, is associated with lower inequality; that the only reason that the beginning of marriage is associated with reduced inequality is because of selection; and that when modeling marriage dichotomously and not subtracting cohabitation or the run-up to marriage, this will lead to a spurious association between marriage and lower inequality. Low-earning men marry around a time in their lives when they do particularly well. Their earnings peak and plummet. Marriage will increase inequality regardless of socioeconomic disparities in marriage rates, unless high-earning men – who do exhibit a plausibly causal marriage premium – stop marrying. A caveat in these comparisons is that whereas past findings relate to differences between periods or geographic areas, my findings pertain to perturbations within a cohort.

7.3 SUMMARY & IMPLICATIONS

My analysis shows that the evidence that selection into marriage rather than effects of marriage explain the higher earnings of married men pertains not to all but a subset of men – those at the bottom of the earnings distribution – a group of men who, coincidentally, are less likely to marry and remain married. Whereas past research has argued that such evidence of selection *may* cast doubt on whether marriage increases men's earnings, I show, by weakening the assumption that marriage operates the same way for all men, that shortly after low-earners marry, their earnings precipitously decline.

Marriage may have a causal effect on male earnings – just not on the earnings of the low-earning men on whom social scientists and policymakers have focused the most in their concerns about the retreat from marriage. Marriage *reinforces* preexisting male earnings inequality, by expanding the difference in earnings between men at the bottom and top of the distribution. Decreasing socioeconomic disparities in marriage rates will not decrease male earnings inequality—unless by a process which *discourages* high-earning men from marrying.

Marriage operates very differently from the way that scholars and policymakers have argued. However, scholars and policymakers remain interested in marriage for additional reasons beside male earnings inequality. These include family earnings inequality, and the growth of single-mother families in poverty. Regardless of earnings, others express concern about the potential consequences of multiple union transitions for child development (Wu & Musick 2008). Poverty is a complex issue, with many more factors at play in addition to inequality in men's earnings. With respect to marriage, we must also recall that marrying a low-earning man often exposes women to financial and other risks (Gibson-Davis, Edin & McLanahan 2005). Further, the lack of a return to men's earnings in enduring marriage among low-earners suggests the importance of further inquiry into other kinds of variation in the qualities and quality of marriages. The qualitative literature tends to focus on the disadvantaged and a useful avenue for future research would be to compare high- and low-earning men's marital experiences. The norm of a male breadwinning partner influences men's and women's views of an ideal marriage and family life, but only among high-earners does marriage increase men's earnings.

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Figure 1. How a Fixed-Effects Model May Spuriously Estimate a Marriage Premium

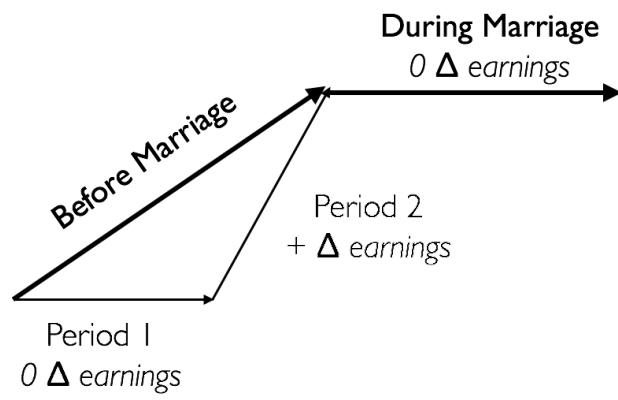
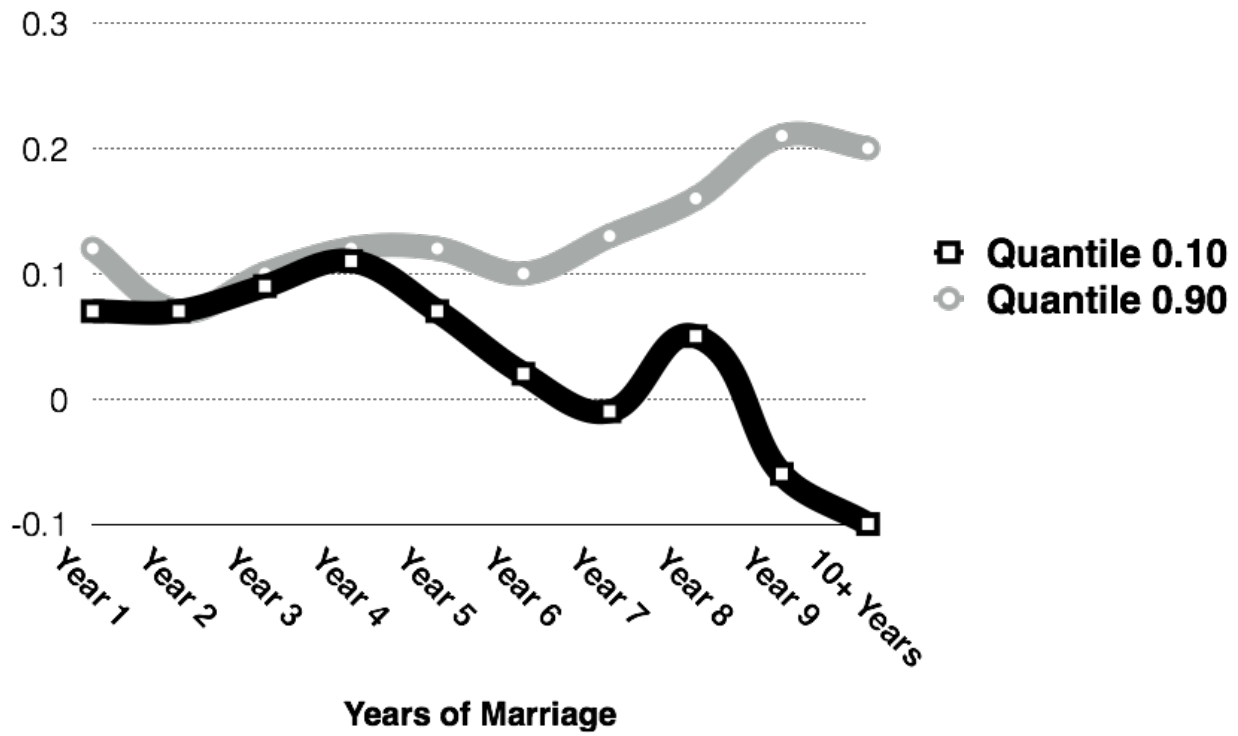
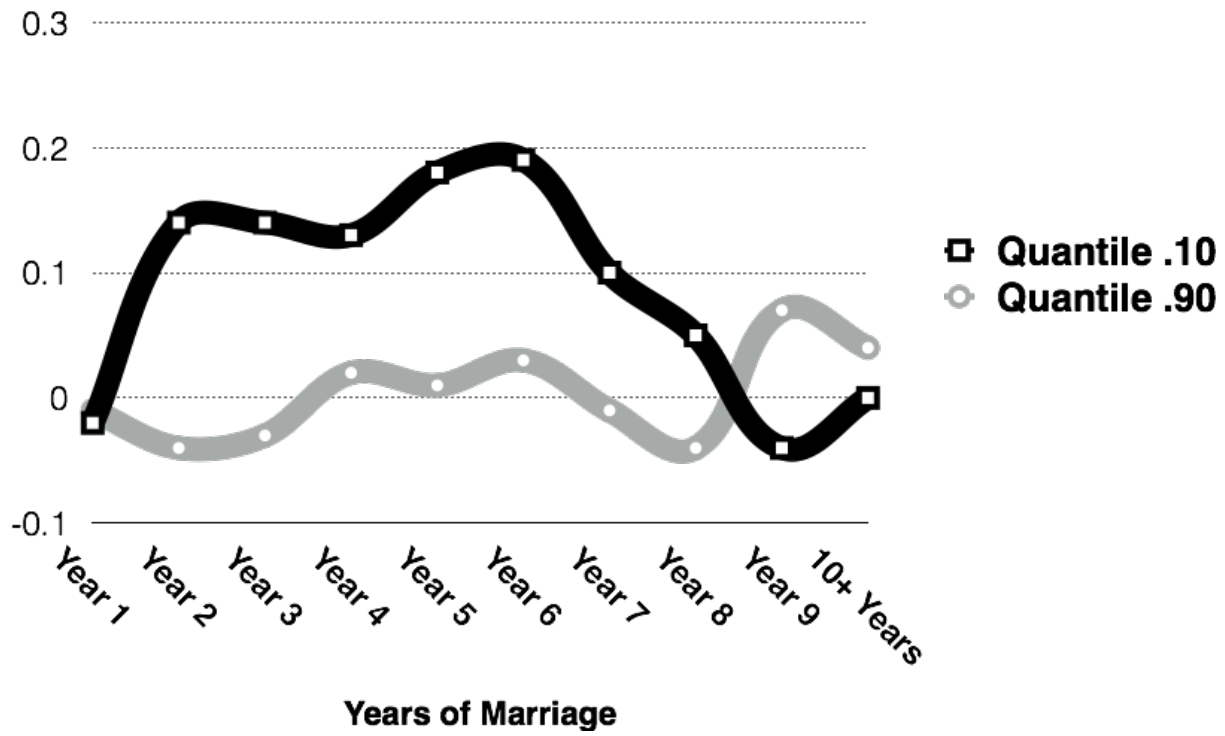


Figure 2. Associations by Year of Marriage, White Men



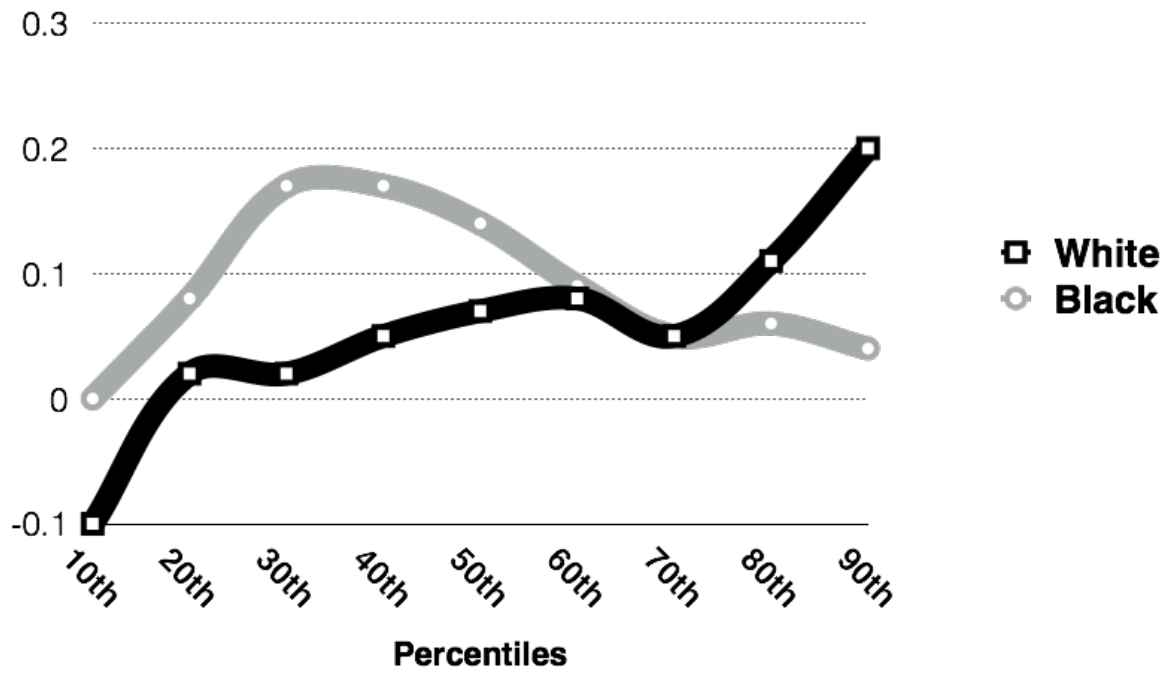
Note: The reference category is nonmarital cohabitation in the year preceding marriage.

Figure 3. Associations by Year of Marriage, Black Men



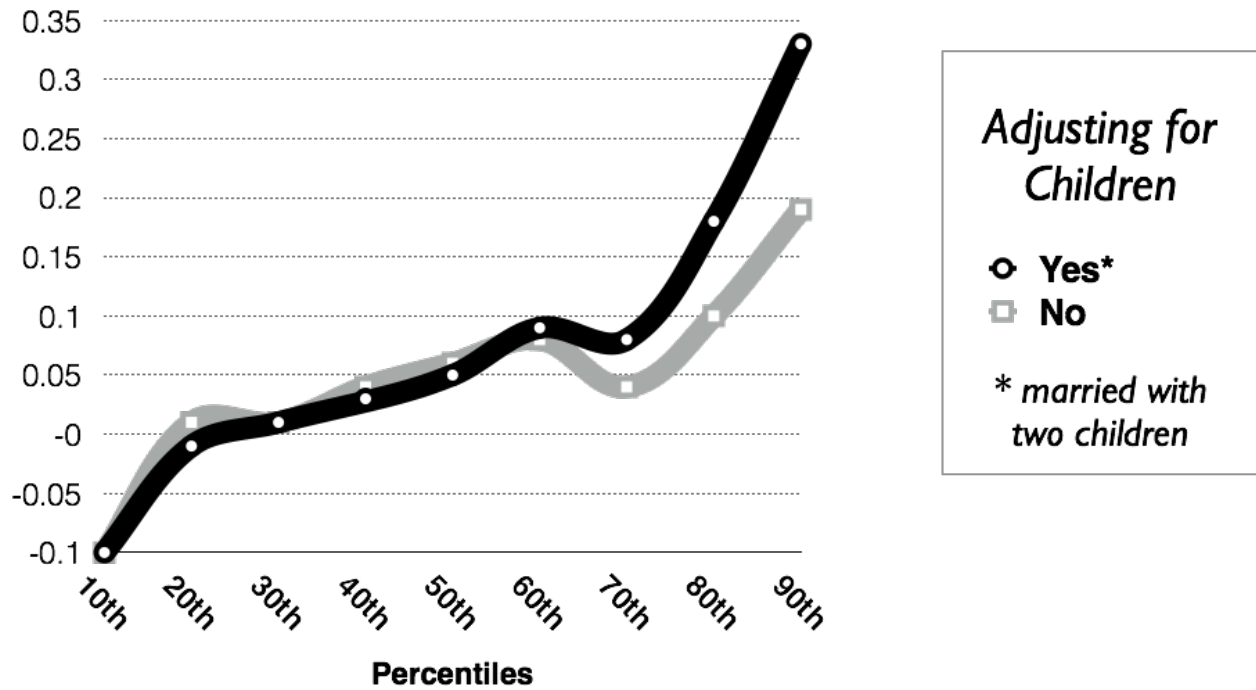
Note: The reference category is nonmarital cohabitation in the year preceding marriage.

Figure 4. Associations at Various Quantiles, with the 10th Year or Later Year of Marriage



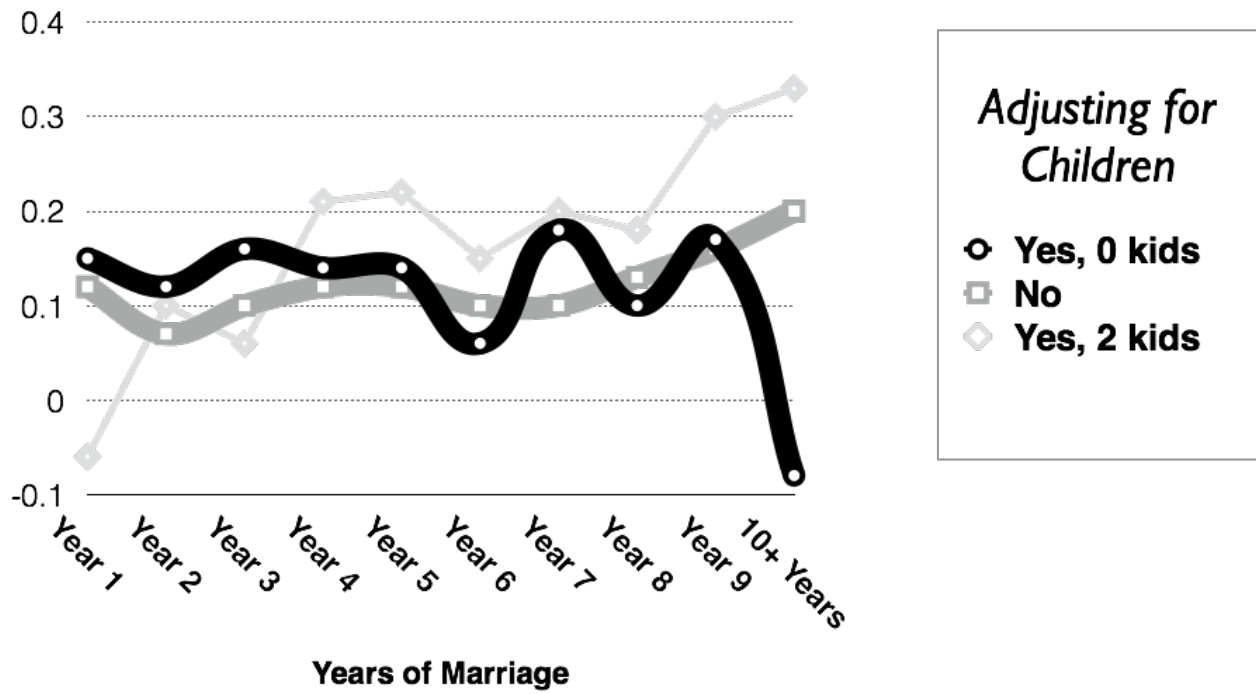
Note: The reference category is nonmarital cohabitation in the year preceding marriage.

Figure 5A. Associations at Various Quantiles, with the 10th Year or Later Year of Marriage, White Men, Before and After Adjusting for Children



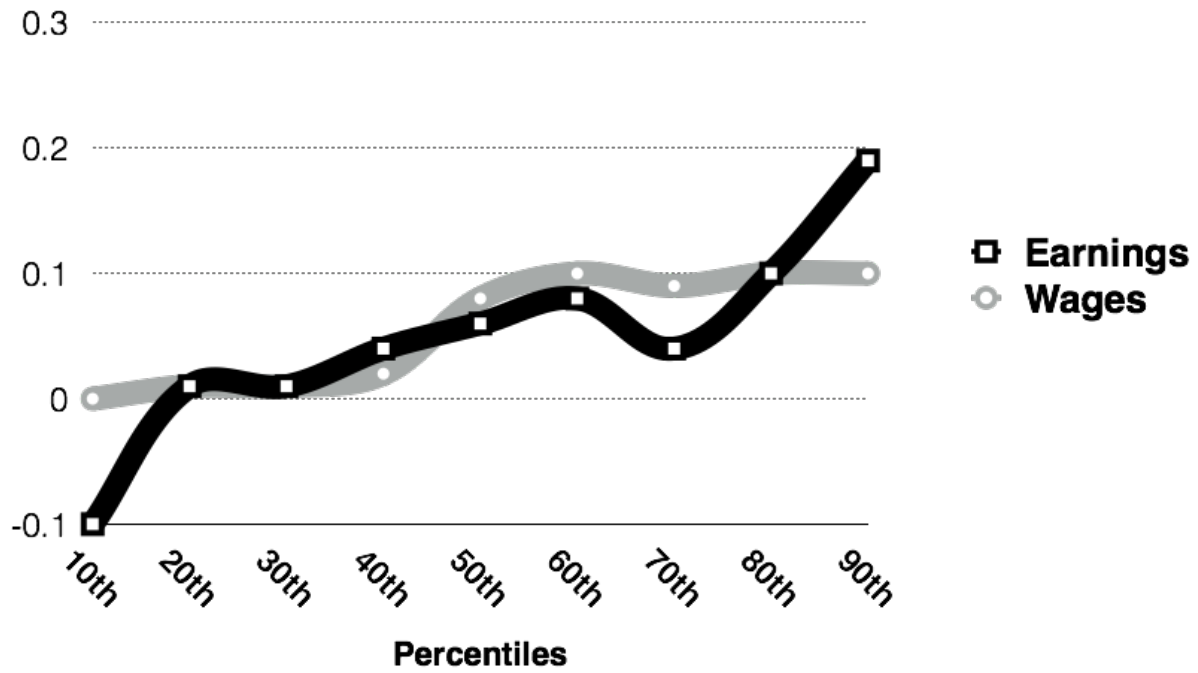
Note: The reference category is nonmarital cohabitation in the year preceding marriage. The dark line graphs the return to a marriage with two children, from a model which adjusts for and interacts marriage with children. The light line graphs the return to marriage from a model which does not adjust for children.

Figure 5B. Associations by Year of Marriage, White Men at the 90th Percentile, Before and After Adjusting for Children



Note: The reference category is nonmarital cohabitation in the year preceding marriage. The thick dark line graphs the return to a marriage with zero children, from a model which adjusts for and interacts marriage with children. The thick light line graphs the return to marriage from a model which does not adjust for children. The thinnest and lightest line graphs the return to a marriage with two children, from the former model.

Figure 6. Associations at Various Quantiles, with the 10th Year or Later Year of Marriage, After Adjusting for Hours Worked



Note: The reference category is nonmarital cohabitation in the year preceding marriage.

Table 1. Earnings Changes through the Marriage Process

	Positive, Negative, or Neutral Earnings Slopes		Plausibly Causal Marriage Premium
	Before Marriage	After Marriage	
A.	0	+	Yes
B.	+	0	Not an effect of the event of marriage
C.	+	+	Ambiguous
D.	+	-	No

Table 2. Descriptives by Race: Overall and by Rank

	White Men				Black Men			
	All	Bottom	Middle	Top	All	Bottom	Middle	Top
Median Weekly Earnings	745	362	702	1222	548	370	684	1146
Median Age	31	30	30	31	30	30	31	30
Union Status								
Cohabits	3%	4%	3%	3%	5%	6%	5%	4%
Married	63%	52%	63%	71%	47%	38%	52%	58%
Separated	13%	19%	13%	9%	20%	24%	18%	15%
Remarried	9%	10%	11%	7%	7%	5%	7%	9%
1 st marriage, year = 4	3%	2%	4%	4%	3%	3%	4%	4%
1 st marriage, year ≥ 10	25%	20%	24%	30%	16%	13%	18%	19%
Median Hours Worked	2112	2080	2080	2340	2080	2035	2080	2181
Children								
Exactly One	20%	20%	22%	19%	25%	23%	25%	28%
Two Or More	37%	33%	35%	40%	44%	45%	46%	41%
Standardized Age-Adjusted AFQT	0.17	-0.18	0.04	0.51	-0.87	-1.04	-0.89	-0.44
Educational Attainment								
College or more	23%	11%	16%	38%	13%	5%	12%	33%
Some College	17%	15%	17%	19%	19%	15%	22%	21%
High School	42%	45%	48%	35%	43%	47%	44%	33%
Below High School	18%	30%	19%	8%	25%	33%	22%	13%
Observations	29189	7740	9977	11472	12979	6212	4115	2652
Persons	1847	1403	1543	1493	852	769	739	531

Note: Ranks are wave-specific. Spouse's earnings are shown for (re)married men.

Table 3A. Associations at Various Quantiles Between Men's Earnings, the Run-Up to Marriage, and Enduring Marriage Relative to More Than Four Years Before Marriage, White Men

	Quantiles									Gradient				
	.1	.2	.3	.4	.5	.6	.7	.8	.9	.9-.1	.8-.2	.7-.3	.6-.4	
Cohabits										**	n.s.	*	n.s.	
The 4th Year Preceding Marriage	0.12*	0.04	0.09**	0.06*	0.02	0.02	0.02	-0.01	-0.05	n.s.	n.s.	n.s.	n.s.	
The 3rd Year Preceding Marriage	-0.06	-0.05	0.01	-0.03	-0.02	-0.02	-0.01	-0.04	-0.02	n.s.	n.s.	n.s.	n.s.	
The 2nd Year Preceding Marriage	0.00	-0.03	0.01	0.03	0.01	0.01	-0.00	-0.04	-0.07*	n.s.	n.s.	n.s.	n.s.	
The Year Preceding Marriage	0.04	-0.01	0.02	-0.02	0.01	0.03	0.04	0.03	0.02	n.s.	n.s.	n.s.	^	
The Year Following Marriage	-0.01	0.03	0.02	0.01	0.01	-0.00	0.03	0.02	-0.02	n.s.	n.s.	n.s.	n.s.	
The 2nd Year of Marriage	0.18***	0.12***	0.10***	0.05^	0.03	0.03	-0.01	0.00	0.05	*	**	**	n.s.	
The 3rd Year of Marriage	0.18**	0.19***	0.17***	0.10**	0.07*	0.03	0.02	0.01	-0.00	*	***	***	*	
The 4th Year of Marriage	0.20**	0.18***	0.19***	0.12***	0.09**	0.07*	0.03	0.03	0.03	*	**	***	^	
The 5th Year of Marriage	0.23**	0.20***	0.21***	0.15***	0.09**	0.09**	0.08*	0.05	0.05	*	**	**	*	
The 6th Year of Marriage	0.18*	0.17***	0.20***	0.16***	0.11***	0.08*	0.08*	0.06	0.05	n.s.	^	**	*	
The 7th Year of Marriage	0.13^	0.15**	0.20***	0.15***	0.10**	0.10**	0.07^	0.06	0.03	n.s.	n.s.	*	n.s.	
The 8th Year of Marriage	0.10	0.14**	0.18***	0.17***	0.12***	0.10**	0.07^	0.06	0.06	n.s.	n.s.	*	^	
The 9th Year of Marriage	0.16^	0.17***	0.17***	0.14***	0.09*	0.08^	0.09*	0.06	0.09	n.s.	^	n.s.	n.s.	
The 10th or Later Year of Marriage	0.05	0.13*	0.15**	0.16***	0.12**	0.12**	0.10*	0.10^	0.14*	n.s.	n.s.	n.s.	n.s.	
Remarriage	0.01	0.09	0.14**	0.12**	0.10*	0.11*	0.09*	0.11*	0.12^	n.s.	n.s.	n.s.	n.s.	
	0.08	0.06^	0.04	0.06*	0.04	0.04	0.05^	0.04	0.07^	n.s.	n.s.	n.s.	n.s.	

*** $p < .001$

** $p < .01$

* $p < .05$

^ $p < .1$

This model estimates earnings as a function of years preceding and during 1st marriage, remarriage, separation, cohabitation, age, educational attainment, wave, arca, age × educational attainment, age × skill, and person-fixed-effects. Coefficients report effects at different points in the outcome distribution. Their significance tests correspond to 1000 clustered design matrix bootstraps.

Table 3B. Associations at Various Quantiles Between Men's Earnings, the Run-Up to Marriage, and Enduring Marriage Relative to Cohabiting the Year Before Marriage, White Men

	Quantiles									Gradient			
	.1	.2	.3	.4	.5	.6	.7	.8	.9	.9-1	.8-2	.7-3	.6-4
Cohabits	0.12*	0.04	0.09**	0.06*	0.02	0.02	0.02	-0.01	-0.05	**	<i>n.s.</i>	*	<i>n.s.</i>
The 4th Year Preceding Marriage	-0.06	-0.05	0.01	-0.03	-0.02	-0.02	-0.01	-0.04	-0.02	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 3rd Year Preceding Marriage	0.00	-0.03	0.01	0.03	0.01	0.01	-0.00	-0.04	-0.07*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 2nd Year Preceding Marriage	0.04	-0.01	0.02	-0.02	0.01	0.03	0.04	0.03	0.02	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	\wedge
The Year Preceding Marriage	-0.01	0.03	0.02	0.01	0.01	-0.00	0.03	0.02	-0.02	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The Year Following Marriage	0.07	0.05	-0.02	-0.02	-0.00	0.00	-0.05	-0.00	0.12*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 2nd Year of Marriage	0.07	0.12**	0.05	0.03	0.03	0.01	-0.02	0.01	0.07	<i>n.s.</i>	*	<i>n.s.</i>	<i>n.s.</i>
The 3rd Year of Marriage	0.09	0.11**	0.07 \wedge	0.06	0.05 \wedge	0.04	-0.01	0.03	0.10 \wedge	<i>n.s.</i>	\wedge	\wedge	<i>n.s.</i>
The 4th Year of Marriage	0.11 \wedge	0.13**	0.09*	0.09*	0.05	0.07*	0.03	0.05	0.12*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 5th Year of Marriage	0.07	0.09*	0.08 \wedge	0.09*	0.07*	0.05	0.03	0.06	0.12*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 6th Year of Marriage	0.02	0.08 \wedge	0.08 \wedge	0.08 \wedge	0.07 \wedge	0.08*	0.03	0.06	0.10 \wedge	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 7th Year of Marriage	-0.01	0.07	0.06	0.10*	0.08*	0.08*	0.02	0.06	0.13*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 8th Year of Marriage	0.05	0.10*	0.05	0.07 \wedge	0.06	0.06	0.04	0.06	0.16*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 9th Year of Marriage	-0.06	0.06	0.03	0.09*	0.09*	0.09*	0.05	0.09*	0.21**	**	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 10th or Later Year of Marriage	-0.10	0.02	0.02	0.05	0.07 \wedge	0.08*	0.05	0.11*	0.20**	**	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
Remarriage	-0.04	-0.02	-0.08 \wedge	-0.01	0.00	0.02	0.00	0.04	0.14*	\wedge	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>

** $p < .01$

* $p < .05$

\wedge $p < .1$

This model estimates earnings as a function of years preceding and during 1st marriage, remarriage, separation, cohabitation, age, educational attainment, wave, arca, age \times educational attainment, age \times skill, and person-fixed-effects. Coefficients report effects at different points in the outcome distribution. Their significance tests correspond to 1000 clustered design matrix bootstraps.

Table 4A. Associations at Various Quantiles Between Men's Earnings, the Run-Up to Marriage, and Enduring Marriage Relative to More Than Four Years Before Marriage, Black Men, for Black Men

	Quantiles										Gradient				
	.1	.2	.3	.4	.5	.6	.7	.8	.9	.9-.1	.8-.2	.7-.3	.6-.4		
Cohabits	0.08	0.10 [^]	0.06	0.02	0.02	0.02	0.03	0.01	0.01	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 4th Year Preceding Marriage	0.27**	0.11 [^]	0.11*	0.10*	-0.00	-0.03	-0.03	-0.04 [^]	-0.02	**	*	**	***		
The 3rd Year Preceding Marriage	0.20*	0.07	0.00	0.02	0.02	0.02	0.00	-0.03	-0.00	[^]	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 2nd Year Preceding Marriage	0.13	0.06	0.06	0.08 [^]	0.04	0.02	-0.01	0.00	-0.02	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The Year Preceding Marriage	0.14	0.06	0.06	0.02	0.02	0.02	0.02	-0.01	-0.01	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The Year Following Marriage	0.19*	0.17**	0.12*	0.15***	0.09**	0.03	0.02	0.02	-0.00	*	**	*	***		
The 2nd Year of Marriage	0.36***	0.24***	0.26***	0.17***	0.12**	0.06	0.01	-0.03	-0.03	***	***	***	**		
The 3rd Year of Marriage	0.35**	0.34***	0.30***	0.17***	0.13***	0.08*	0.07 [^]	0.03	-0.03	**	***	***	*		
The 4th Year of Marriage	0.34**	0.28***	0.27***	0.19***	0.17***	0.09*	0.07	-0.01	0.02	**	***	***	*		
The 5th Year of Marriage	0.40***	0.30***	0.30***	0.15**	0.11**	0.07 [^]	0.02	-0.02	0.02	**	***	***	<i>n.s.</i>		
The 6th Year of Marriage	0.40***	0.30***	0.30***	0.24***	0.17***	0.10*	0.06	0.02	0.04	**	***	***	**		
The 7th Year of Marriage	0.31*	0.28***	0.31***	0.22***	0.18***	0.11*	0.09 [^]	0.01	0.00	*	**	***	*		
The 8th Year of Marriage	0.27 [^]	0.18*	0.25***	0.19**	0.14**	0.07	0.03	-0.01	-0.03	*	*	***	*		
The 9th Year of Marriage	0.18	0.17*	0.25***	0.22***	0.15**	0.13*	0.06	-0.01	0.08	<i>n.s.</i>	*	*	<i>n.s.</i>		
The 10th or Later Year of Marriage	0.21	0.24**	0.29***	0.22***	0.18***	0.13**	0.11*	0.06	0.04	<i>n.s.</i>	*	*	[^]		
Remarriage	0.09	0.11*	0.14**	0.13**	0.09*	0.12**	0.14***	0.13**	0.10*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		

*** $p < .001$ ** $p < .01$ * $p < .05$ [^] $p < .1$

This model estimates earnings as a function of years preceding and during 1st marriage, remarriage, separation, cohabitation, age, educational attainment, wave, area, age × educational attainment, age × skill, and person-fixed-effects. Coefficients report effects at different points in the outcome distribution. Their significance tests correspond to 1000 clustered design matrix bootstraps.

Table 4B. Associations at Various Quantiles Between Men's Earnings, the Run-Up to Marriage, and Enduring Marriage Relative to Cohabiting the Year Before Marriage, Black Men, for Black Men

	Quantiles										Gradient			
	.1	.2	.3	.4	.5	.6	.7	.8	.9	.9-.1	.8-.2	.7-.3	.6-.4	
Cohabits	0.08	0.10 [^]	0.06	0.02	0.02	0.02	0.03	0.01	0.01	n.s.	n.s.	n.s.	n.s.	
The 4th Year Preceding Marriage	0.27***	0.11 [^]	0.11*	0.10*	-0.00	-0.03	-0.03	-0.04 [^]	-0.02	**	*	**	***	
The 3rd Year Preceding Marriage	0.20*	0.07	0.00	0.02	0.02	0.02	0.00	-0.03	-0.00	^	n.s.	n.s.	n.s.	
The 2nd Year Preceding Marriage	0.13	0.06	0.06	0.08 [^]	0.04	0.02	-0.01	0.00	-0.02	n.s.	n.s.	n.s.	n.s.	
The Year Preceding Marriage	0.14	0.06	0.06	0.02	0.02	0.02	-0.01	-0.01	-0.01	n.s.	n.s.	n.s.	n.s.	
The Year Following Marriage	-0.02	0.01	-0.00	0.10*	0.05	-0.01	-0.04	0.02	-0.01	n.s.	n.s.	n.s.	*	
The 2nd Year of Marriage	0.14	0.08	0.14*	0.13**	0.07 [^]	0.01	-0.05 [^]	-0.03	-0.04	n.s.	n.s.	**	**	
The 3rd Year of Marriage	0.14	0.18*	0.18**	0.12*	0.09*	0.04	0.01	0.03	-0.03	n.s.	^	**	^	
The 4th Year of Marriage	0.13	0.11	0.15*	0.14**	0.13**	0.05	0.01	-0.01	0.02	n.s.	n.s.	*	*	
The 5th Year of Marriage	0.18	0.14 [^]	0.18**	0.11*	0.07 [^]	0.03	-0.04	-0.02	0.01	n.s.	^	**	n.s.	
The 6th Year of Marriage	0.19	0.14 [^]	0.18**	0.19***	0.13**	0.06	0.01	0.02	0.03	n.s.	n.s.	*	**	
The 7th Year of Marriage	0.10	0.12	0.20**	0.18**	0.14**	0.07	0.03	0.01	-0.01	n.s.	n.s.	*	*	
The 8th Year of Marriage	0.05	0.02	0.14 [^]	0.14*	0.10*	0.03	-0.03	-0.01	-0.04	n.s.	n.s.	*	*	
The 9th Year of Marriage	-0.04	0.01	0.14 [^]	0.18**	0.11*	0.08	0.00	-0.01	0.07	n.s.	n.s.	n.s.	n.s.	
The 10th or Later Year of Marriage	-0.00	0.08	0.17*	0.17**	0.14**	0.09 [^]	0.05	0.06	0.04	n.s.	n.s.	n.s.	^	
Remarriage	-0.12	-0.05	0.02	0.09	0.05	0.08	0.08	-0.13*	0.10	n.s.	^	n.s.	n.s.	

*** $p < .001$ ** $p < .01$ * $p < .05$ ^ $p < .1$

This model estimates earnings as a function of years preceding and during 1st marriage, remarriage, separation, cohabitation, age, educational attainment, wave, area, age × educational attainment, age × skill, and person-fixed-effects. Coefficients report effects at different points in the outcome distribution. Their significance tests correspond to 1000 clustered design matrix bootstraps.

Table 5A. Associations at Various Quantiles Between Men's Earnings, the Run-Up to Marriage, and Enduring Marriage Relative to Cohabiting the Year Before Marriage, After Adjusting for Hours Worked, White Men

	Quantiles									Gradient			
	.1	.2	.3	.4	.5	.6	.7	.8	.9	.9-.1	.8-.2	.7-.3	.6-.4
Cohabits	0.06	0.01	0.03**	0.05 [^]	0.02	0.02	0.02	-0.01	-0.05	[^]	<i>n.s.</i>	[^]	<i>n.s.</i>
The 4th Year Preceding Marriage	-0.07	-0.05	0.01	-0.03	-0.02	-0.02	-0.02	-0.04	-0.02	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 3rd Year Preceding Marriage	-0.03	-0.04	0.01	0.03	0.01	0.02	-0.00	-0.03	-0.06 [^]	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 2nd Year Preceding Marriage	-0.07	-0.06 [^]	-0.02	-0.05 [^]	-0.01	0.01	0.03	0.02	0.02	<i>n.s.</i>	[^]	<i>n.s.</i>	*
The Year Preceding Marriage	-0.05	0.00	0.00	-0.00	0.00	-0.01	0.03	0.02	-0.02	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The Year Following Marriage	0.05	0.05	-0.02	-0.02	0.00	0.01	-0.05 [^]	0.00	0.13**	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 2nd Year of Marriage	0.00	0.08*	0.02	0.02	0.02	0.01	-0.02	0.01	0.08	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 3rd Year of Marriage	0.01	0.07 [^]	0.03	0.03	0.04	0.04	-0.02	0.02	0.10*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 4th Year of Marriage	0.05	0.09*	0.06	0.06 [^]	0.03	0.05 [^]	0.02	0.04	0.11*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 5th Year of Marriage	0.04	0.07 [^]	0.06	0.08*	0.06*	0.05	0.03	0.06	0.12*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 6th Year of Marriage	-0.01	0.06	0.06	0.06	0.06 [^]	0.07*	0.02	0.05	0.10 [^]	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 7th Year of Marriage	-0.04	0.04	0.04	0.08*	0.07*	0.07*	0.01	0.05	0.13*	*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 8th Year of Marriage	-0.00	0.07 [^]	0.03	0.05	0.04	0.05	0.04	0.05	0.16**	*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 9th Year of Marriage	-0.08	0.05	0.02	0.08 [^]	0.08*	0.09*	0.05	0.09*	0.21**	***	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
The 10th or Later Year of Marriage	-0.10 [^]	0.01	0.01	0.04	0.06 [^]	0.08*	0.04	0.10*	0.19**	***	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>
Remarriage	0.05	0.02	-0.05	0.00	0.01	0.02	0.00	0.04	0.13*	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>

*** $p < .001$ ** $p < .01$ * $p < .05$ [^] $p < .1$

This model estimates earnings as a function of years preceding and during 1st marriage, remarriage, separation, cohabitation, age, educational attainment, wave, area, age × educational attainment, age × skill, worked hours, worked weeks, worked hours × worked weeks, usual weekly hours at the primary or most recent job, and person-fixed-effects. Coefficients report effects at different points in the outcome distribution. Their significance tests correspond to 1000 clustered design matrix bootstraps.

Table 5B. Associations at Various Quantiles Between Men's Wages, the Run-Up to Marriage, and Enduring Marriage Relative to Cohabiting the Year Before Marriage, White Men

	Quantiles										Gradient				
	.1	.2	.3	.4	.5	.6	.7	.8	.9	.9-1	.8-2	.7-3	.6-4		
Cohabits	0.04	0.06*	0.08***	0.06*	0.03	0.01	-0.01	-0.00	0.00	<i>n.s.</i>	<i>n.s.</i>	**	^		
The 4th Year Preceding Marriage	-0.01	-0.03	0.01	-0.02	-0.03	-0.04^	-0.03	-0.01	-0.01	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 3rd Year Preceding Marriage	0.01	-0.04	-0.01	0.01	0.01	0.00	-0.02	-0.02	-0.06	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 2nd Year Preceding Marriage	-0.03	-0.01	-0.01	-0.01	-0.01	0.02	0.02	0.01	0.00	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The Year Preceding Marriage	0.02	-0.00	0.01	0.02	-0.00	0.00	0.01	0.01	-0.05	^	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The Year Following Marriage	0.01	0.00	-0.02	-0.04	0.01	0.00	0.02	-0.01	0.09^	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 2nd Year of Marriage	0.04	0.03	-0.00	0.00	0.05^	0.01	0.00	0.02	0.03	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 3rd Year of Marriage	0.03	0.02	0.01	0.00	0.04	0.03	0.03	0.04	0.06	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 4th Year of Marriage	0.03	0.04	0.04	0.02	0.05^	0.06^	0.06*	0.06	0.06	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 5th Year of Marriage	0.03	0.05	0.06^	0.03	0.08*	0.06^	0.07*	0.06	0.03	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 6th Year of Marriage	0.01	0.05	0.04	0.01	0.08*	0.06^	0.06^	0.03	0.03	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 7th Year of Marriage	0.00	0.03	0.03	0.03	0.09**	0.07^	0.05	0.03	-0.00	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 8th Year of Marriage	-0.01	0.03	0.03	0.01	0.04	0.07^	0.09*	0.05	0.07	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	^		
The 9th Year of Marriage	-0.01	0.03	0.03	0.03	0.10**	0.07^	0.08*	0.08^	0.07	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
The 10th or Later Year of Marriage	-0.00	0.01	0.01	0.02	0.08*	0.10*	0.09*	0.10*	0.10	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>	<i>n.s.</i>		
Remarriage	-0.02	-0.03	-0.07*	-0.06^	0.01	0.04	0.05	0.05	0.11*	^	<i>n.s.</i>	**	**		

*** $p < .001$

** $p < .01$

* $p < .05$

^ $p < .1$

This model estimates wages as a function of years preceding and during 1st marriage, remarriage, separation, cohabitation, age, educational attainment, wave, area, age × educational attainment, age × skill, worked hours, worked weeks, worked hours × worked weeks, usual weekly hours at the primary or most recent job, and person-fixed-effects. Coefficients report effects at different points in the outcome distribution. Their significance tests correspond to 1000 clustered design matrix bootstraps.

Appendix Table 1A. White Descriptives by Wave and Rank

	1990			2000			2010		
	Bottom	Middle	Top	Bottom	Middle	Top	Bottom	Middle	Top
Median Weekly Earnings	426	725	1161	498	910	1627	237	832	1678
Median Age	28	29	29	39	38	39	49	48	48
Union Status									
Cohabits	9%	8%	5%	0%	0%	0%	0%	0%	0%
Married	59%	67%	71%	65%	78%	86%	67%	78%	87%
Separated	17%	15%	8%	30%	19%	12%	32%	22%	13%
Remarried	8%	7%	6%	17%	20%	12%	20%	25%	19%
1 st marriage, year = 4	4%	5%	7%	1%	1%	1%	0%	0%	0%
1 st marriage, year ≥ 10	10%	15%	17%	38%	46%	60%	45%	52%	66%
Median Hours Worked	2080	2185	2340	2080	2288	2600	2080	2080	2340
Children									
Exactly One	23%	30%	22%	23%	21%	18%	21%	22%	14%
Two Or More	31%	28%	31%	53%	59%	67%	61%	63%	74%
Standardized Age-Adjusted AFQT	-0.23	0.07	0.53	-0.24	0.10	0.63	-0.03	0.13	0.69
Educational Attainment									
College or more	11%	18%	41%	11%	20%	49%	20%	15%	55%
Some College	14%	18%	19%	15%	20%	21%	19%	21%	17%
High School	44%	46%	33%	45%	44%	24%	45%	53%	25%
Below High School	30%	19%	7%	29%	16%	6%	16%	12%	3%
Observations	356	493	553	350	486	567	316	365	447
Persons	356	493	553	350	486	567	316	365	447

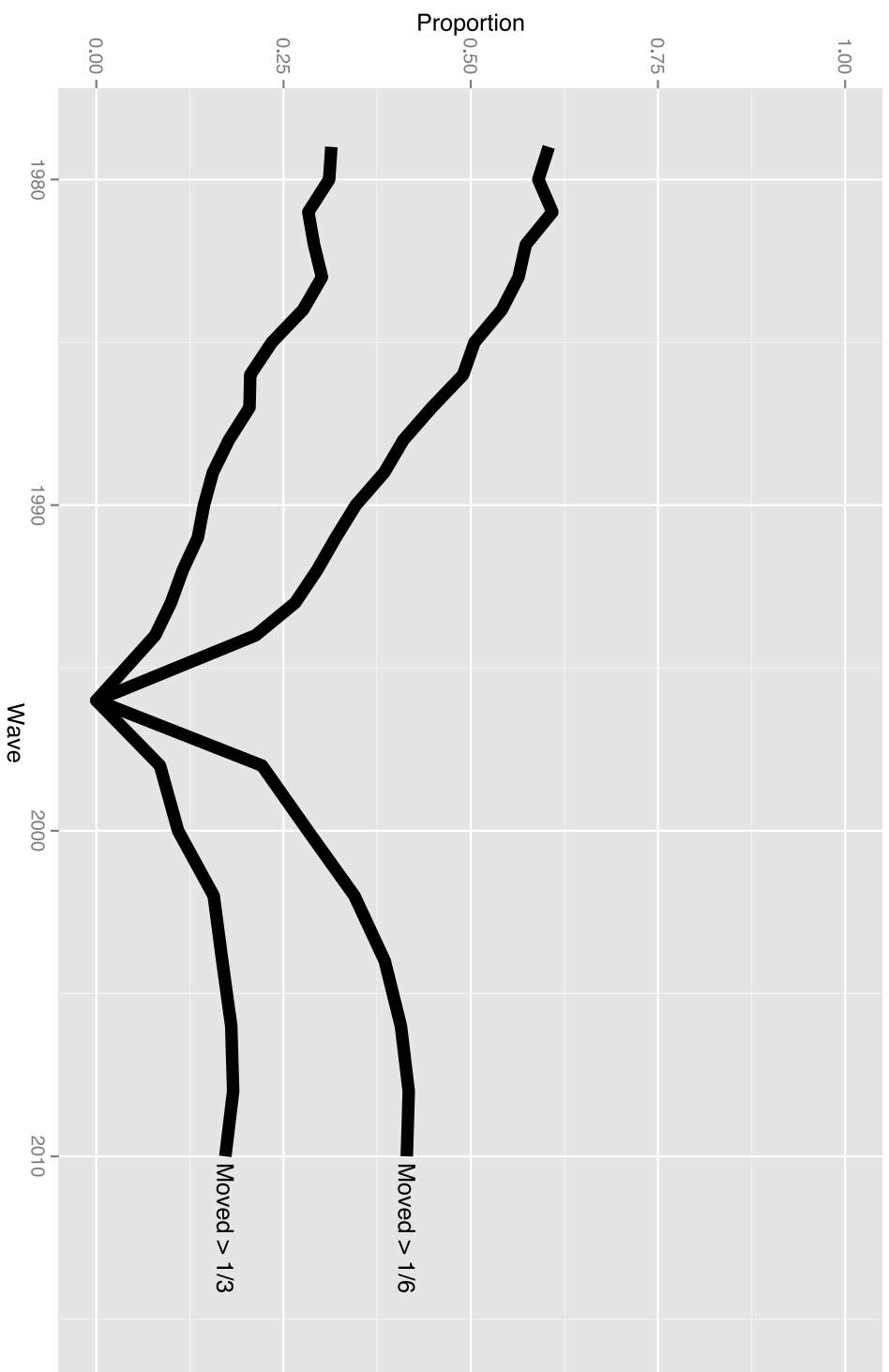
Note: Ranks are wave-specific. Spouse's earnings are shown for (re)married men.

Appendix Table 1B. Black Descriptives by Wave and Rank

	1990			2000			2010		
	Bottom	Middle	Top	Bottom	Middle	Top	Bottom	Middle	Top
Median Weekly Earnings	394	689	1104	494	893	1494	370	772	1429
Median Age	29	29	29	39	38	39	48	48	49
Union Status									
Cohabits	15%	8%	9%	0%	1%	0%	1%	0%	0%
Married	40%	54%	62%	56%	65%	75%	50%	73%	81%
Separated	22%	17%	12%	35%	31%	19%	47%	27%	19%
Remarried	3%	3%	3%	10%	12%	18%	12%	24%	28%
1 st marriage, year = 4	3%	5%	5%	1%	2%	0%	0%	1%	0%
1 st marriage, year ≥ 10	3%	10%	8%	30%	36%	45%	35%	43%	51%
Median Hours Worked	2080	2080	2225	2080	2080	2600	2080	2080	2152
Children									
Exactly One	26%	30%	28%	17%	25%	30%	22%	17%	21%
Two Or More	47%	40%	39%	66%	67%	59%	66%	72%	72%
Standardized Age-Adjusted AFQT	-1.04	-0.87	-0.37	-1.04	-0.83	-0.22	-1.00	-0.90	-0.30
Educational Attainment									
College or more	4%	17%	34%	6%	17%	47%	7%	14%	37%
Some College	15%	21%	21%	18%	28%	19%	20%	18%	30%
High School	46%	39%	34%	46%	38%	26%	51%	53%	31%
Below High School	35%	22%	11%	30%	17%	8%	23%	15%	2%
Observations	311	201	128	325	185	113	225	176	94
Persons	311	201	128	325	185	113	225	176	94

Note: Ranks are wave-specific. Spouse's earnings are shown for (re)married men.

Appendix Figure 1. Proportion of Men Whose Ranks Change by One-Third or One-Sixth, Between 1996 and Other Waves



ENDNOTES

¹ Killewald & Gough (2013) report a lower estimate of the marriage premium, 7%. In supplemental analyses (available in the online supplement), I find that excluding interactions with children raises the estimate to 8%, and excluding age-education and age-test scores interactions further raises the estimate to 12%, in line with previous estimates. I discuss covariate selection in the *Models* section.

² An exception is that by the end of the panel, the earnings of men in the bottom third plummet, perhaps due to the recession.

³ It may be tempting to consider grouping men by their earnings just before marriage, but this is inappropriate since this would group men into categories based on a future event, and furthermore, since earnings changes relate to the marriage process.