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Does Specialization Explain Marriage Penalties and Premiums?

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Abstract

Married men's wage premium is often attributed to within-household specialization: men can devote more effort to wage-earning when their wives assume responsibility for household labor. We provide a comprehensive evaluation of the specialization hypothesis, arguing that, if specialization causes the male marriage premium, married women should experience wage losses. Furthermore, specialization by married parents should augment the motherhood penalty and the fatherhood premium for married as compared to unmarried parents. Using fixed-effects models and data from the NLSY79, we estimate within-gender differences in wages according to marital status and between-gender differences in the associations between marital status and wages. We then test whether specialization on time use, job traits, and tenure accounts for the observed associations. Results for women do not support the specialization hypothesis. Childless men and women both receive a marriage premium. Marriage augments the fatherhood premium but not the motherhood penalty. Changes in own and spousal employment hours, job traits, and tenure appear to benefit both married men and women, although men benefit more. Marriage changes men's labor market behavior in ways that augment wages, but these changes do not appear to occur at the expense of women's wages.

Keywords

marriage, parenthood, earnings, family

Household formation typically leads to specialization, with each partner increasing time in certain tasks and reducing time in others. Under traditional household specialization, married women invest and specialize in home production, and married men specialize in labor market activities (Becker 1981). Consistent with this perspective, women's housework time tends to rise following marriage, while men's housework time falls (Gupta 1999; Hersch and Stratton 2000; Nock 1998).

Specialization is the dominant causal explanation in the literature for the higher wages earned by married men as compared to unmarried men (Chun and Lee 2001; Gray 1997), and it has also been offered as a possible explanation for married men's fatherhood premium (Glauber 2008). However, the relationship between marriage and men's wages is only half of the specialization story. Studies of the motherhood penalty frequently

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find a marriage premium for women (Budig and England 2001; Glauber 2007; Taniguchi 1999; Waldfogel 1997). If household specialization is truly the source of the male marriage premium, why do women also receive a marriage premium? It is clearly not possible for specialization to explain wage gains at marriage for *both* spouses. Furthermore, predictions of specialization have been almost exclusively tested in single-gender models of men's behavior. Specialization is, by its nature, a theory of how partnership differentially affects men and women. A comprehensive evaluation of specialization predictions should employ between-gender as well as within-gender comparisons.

Additionally, previous research has failed to carefully examine the implications of specialization for how marriage moderates the association between parenthood and wages, despite evidence of variation by marital status in the association between parenthood and wages for both men and women (Budig and England 2001; Glauber 2007, 2008; Killewald 2013). Children vastly increase the demand for unpaid labor in the household, potentially increasing the demand for specialization. Specialization may therefore be more extensive, and its effects on wages larger, for married parents as compared to childless couples. We know of no prior work that tests whether measures of household specialization explain the moderating role of marriage in the association between motherhood and wages.

These are important omissions because they limit our understanding of the distribution of marriage's costs and benefits by gender and parental status. If gender differences in marriage's financial costs and benefits are due primarily to a gendered division of labor within the household, this suggests that the couple dyad is responsible for the disproportionate costs of family responsibilities borne by women. On the other hand, if specialization is not the source of men's marriage premium, then we need to consider alternative explanations.

Specialization is not the only theoretical model to explain the association between marriage and wages. Individuals' family status may affect their wages through employers' perceptions and discrimination. Budig and England (2001) considered employer discrimination as one possible source of the motherhood wage penalty, and Correll, Benard, and Paik (2007) found evidence suggesting discrimination against mothers during the hiring process. Hodges and Budig (2010) challenged the specialization explanation for the fatherhood premium. Instead, they argued that the fatherhood premium may reflect employer discrimination that occurs within the context of organizational hegemonic masculinity. Notably, these studies all investigate the association between *parenthood* and wages. By comparison, sociologists have conducted little work on the *marriage* premium for men or women.

This article fills the gap in the existing literature by providing a comprehensive evaluation of the predictions of household specialization for both wage differences between married and unmarried individuals of the same gender and the difference between genders in the association between marriage and wages. We do not assume that household specialization is responsible for the wage changes associated with marriage for men and women; rather, we identify the empirical implications of the specialization hypothesis for these associations. We assert that specialization predicts (1) a marriage premium for men and a marriage penalty for women, leading to a gender difference in the association between marriage and wages; and (2) a larger motherhood penalty for married than unmarried mothers, and a larger fatherhood premium for married than unmarried fathers, leading to a gender difference in the moderating role of marriage in the association between parenthood and wages. We develop these hypotheses in greater detail in a later section.

We then test these hypotheses, estimating fixed-effects models using data from the 1979 cohort of the National Longitudinal Survey of Youth (NLSY79). Unlike previous work, we explicitly test for gender differences in the associations between marriage and wages. Our analysis thus provides a more comprehensive test of household specialization than has been accomplished in single-gender models or models that ignore the possibility that marriage moderates the association between parenthood and wages.

Having established the associations among gender, marriage, parental status, and wages, we then examine whether indicators of household specialization mediate the observed relationships. Previous work typically focuses on specialization in terms of time use—employment status and housework time (Chun and Lee 2001; Gray 1997; Hersch and Stratton 2000; Loh 1996); we broaden the conceptualization to include specialization on job traits and job tenure. This theoretical expansion is especially important because women's and mothers' average time in paid labor increased considerably over the past several decades, narrowing the gender gap in paid labor time (Sayer 2005). By 1990, both spouses were employed full-time in over 40 percent of married couples (Waite and Nielsen 2001). For these couples, seeking to balance work and family life may lead to specialization in areas other than time, such as allocating one spouse a *career,* while the other spouse holds a *job*—that is, more flexible work that can take a backseat to the needs of the household and the spouse's career (Becker and Moen 1999). Specialization of this kind, although rarely considered in previous work attempting to explain the male marriage premium, may contribute to gender differences in the wage gains to marriage and married parenthood.

Like all observational studies, our analyses have limitations that make causal conclusions challenging. We cannot rule out all forms of selection that may bias our results, but we follow the main analysis with a discussion of results from supplemental models that address various forms of selection not accounted for in standard fixed-effects models. We found no evidence that our conclusions with respect to specialization were biased by these forms of selection.

HOUSEHOLD SPECIALIZATION

The economic model of household specialization implies that couples pursue a joint, household-level strategy in which they divide labor to maximize household well-being, with each partner spending more time in the activities in which she holds the comparative advantage (Becker 1981). Thus, the spouse with the comparative advantage in the labor market will invest more heavily in paid labor, while the other spouse invests more heavily in domestic production. Most often, due to the gender wage gap, husbands will increase, and wives reduce, paid labor effort, while husbands reduce, and wives increase, household labor effort. Specialization thus implies that spouses engage in activities that differ from what they would undertake as single individuals. To evaluate the predictions of specialization, it is therefore appropriate to compare outcomes of individuals of the same gender who differ by marital status.

The specialization model has been criticized for ignoring the role of gender in shaping specialization decisions, regardless of economic rationality, and for ignoring spouses' individual interests when making household employment decisions (Berk 1985). In this article, we evaluate whether specialization can explain observed associations between marriage, parenthood, and wages, for both men and women, rather than how it arises.

Within-household specialization may clearly affect individuals' earnings by affecting hours spent in the labor market. However, it may also affect wages by altering productivity on the job or traits of jobs or employers. For example, the spouse with reduced household responsibilities may arrive at work better-rested and have less need to interrupt paid labor when household matters arise, such as waiting for a repair person or picking up a sick child from school. We consider hourly wages, rather than annual earnings, as our outcome of interest for several reasons. First, wages indicate the financial returns that individuals are able to command for each hour they spend in the labor market, which is a stronger indicator of advantage than earnings that depend in part on individuals' preferences for employment hours. Second, spouses' bargaining positions within marriage and their well-being in the event of divorce are hypothesized to depend on hourly wages, not earnings (Pollak 2005). Wages may thus be an indicator of marital power. Finally, the literature on the association between family and labor market outcomes predominantly examines hourly wages (Budig and England 2001; Chun and Lee 2001; Hersch and Stratton 2000; Loh 1996; Loughran and Zissimopoulos 2009; Taniguchi 1999; Waldfogel 1997). Focusing on hourly wages thus allows us to engage more directly with prior studies.

Our approach differs substantially from that of Light (2004), who examined the association between union formation (cohabitation or marriage) and changes in individuals' total family income, family income adjusted for household composition, and own income. She found that transitions to marriage were associated with slight, nonsignificant increases in men's income and moderate declines in women's income; transitions to marriage and cohabitation were associated with large gains for both men and women in total family income and gains for women in family income adjusted for household size. This is an important point: much of individuals' financial gain from marriage is due to combining financial resources with a partner and generating economies of scale, not changes in individual incomes of each member of the couple. Light's (2004) analysis does not address, however, whether individual income changes associated with marriage are truly attributable to household specialization. We address this question here.

Specialization and Gender Differences in the Marriage Premium

Specialization predicts that marriage will lead to increases in women's time in household labor and reductions in their labor market productivity, leading to a wage penalty. Consistent with this hypothesis, time in female-typed household labor is negatively associated with wages (Noonan 2001), suggesting a trade-off between efforts in home production and the labor market. A within-couple division of labor that gives women primary responsibility for household labor, and childcare in particular, may also increase women's likelihood of part-time labor, which is associated with lower hourly wages (Waldfogel 1997). Furthermore, given the positive association between women's labor market experience and wages (Budig and England 2001), work interruptions around child-bearing may have long-term negative effects on married women's wages. By contrast, married men's reduced household labor responsibilities, compared to when unmarried (Gupta 1999; Hersch and Stratton 2000), should lead to wage gains.

One might argue that childless couples engage in little specialization and thus there is no reason to expect a marriage penalty for women. This may be true, but, if so, it has implications for our understanding of the male marriage premium: if childless couples do not specialize sufficiently to generate wage losses for wives, then specialization cannot be the source of the male marriage premium, despite its dominant place in the literature.

Prior research on whether specialization explains the male marriage premium has found mixed results. Chun and Lee (2001) and Gray (1997) found that specialization augments the male marriage premium, but other work shows less support for the specialization hypothesis (Dougherty 2006; Hersch and Stratton 2000; Loh 1996). With the exception of Dougherty (2006), these assessments of specialization were all based on the addition of relevant variables to models of *men's* wages. This ignores a key expectation of the hypothesis: women's increasing specialization in nonmarket work following marriage should lead to a marriage penalty. However, most existing fixed-effects estimates instead find a female marriage premium of about 3 to 7 percent (Budig and England 2001; Glauber 2007; Taniguchi 1999; Waldfogel 1997).

These prior estimates of the female marriage premium are derived from studies whose primary goal is to estimate the motherhood penalty, and each controls for at least one measure of specialization—either labor market experience or current employment hours. This is appropriate for these authors' research questions, but it means it is not possible, from these earlier results, to determine whether married women's wages truly rise compared to unmarried women, or if they merely do better than would be expected, given their reduced employment hours. It is possible that, when measures of specialization are excluded, marriage will be associated with wage losses for women.

We test this hypothesis directly. If marriage leads women to reduce their employment hours, leading to less labor market experience and, as a result, lower wages, this is still a real effect of marriage on wages. Because controlling for labor market experience will obscure this relationship, we first present results from models that do not control for specialization measures. In later models, we add these measures to test whether specialization explains the observed relationships between marriage and wages.

Although the discussion of household specialization traditionally focuses on labor market and household labor time, couples may also make joint job decisions based on factors other than work hours. Dual-income couples may choose to prioritize the career of one partner over the job of another (Becker and Moen 1999). Although it is not possible to measure all employment attributes that distinguish jobs from careers, we can use indicators of job characteristics, such as occupation, industry, and sector, to capture some features of jobs that are also correlated with wages. For example, even before accounting for the establishments in which individuals work, Petersen and Morgan (1995) found that occupational gender segregation explained more than half of the gender gap in wages in a sample of workers in manufacturing and service industries, indicating that occupations differ by gender in patterns that tend to advantage men's wages. Marriage may contribute to this gender variation if wives make sacrifices in their jobs to accommodate their husbands' careers.

We also consider job tenure as a measure of whether individuals are pursuing a job or a career. After controlling for measures of current employment hours and prior labor market experience, job tenure reflects not work interruptions but job mobility. Spouses' job tenures may be positively correlated (e.g., if a geographic move leads to new jobs for both spouses), but it is also possible that partnership will lead to divergence in spouses' job tenures. Couples may assign one partner responsibility for maximizing wages by moving up the career ladder at a single firm, while the other partner has responsibility for changing jobs when necessary to accommodate changing household responsibilities. For example, a firm that is a good fit because it has an onsite childcare center may not be a good fit as children age, if the firm is located far away from the children's school.

If spouses engage in specialization on job traits and tenure, we expect marriage will lead women to move into jobs with lower average wages, and the reverse will be true for men. Consistent with the latter expectation, Gorman (1999) found that marriage is associated with job-shift patterns for men that facilitate greater wage growth.

We list three hypotheses for childless adults that follow from expectations of the specialization model. We discuss hypotheses for parents in the next section. For each hypothesis, we note predictions for differences within each gender (e.g., married versus unmarried men) and between genders (e.g., the relative size of the marriage premium or penalty for men versus women).

> *Hypothesis 1a:* Among childless adults, men will experience a marriage wage premium, and women will experience a marriage penalty, resulting in a gender difference in the marriage premium.

If marriage disproportionately benefits men's wages and depresses women's wages because of household specialization, controlling for measures of specialization should reduce gender disparities in the marriage premium, diminishing men's marriage premium and women's marriage penalty.

Hypothesis 1b: Controlling for measures of time-use specialization among childless adults will reduce the marriage premium for men and the marriage penalty for women, thus reducing the gender gap in the marriage premium.

Hypothesis 1c: Controlling for measures of job traits and job tenure will also reduce the marriage premium for men and ameliorate the marriage penalty for women, further narrowing the gender gap in the marriage premium.

Specialization and Parenthood

The birth of a child to a married couple is expected to lead to increased specialization. Because children generate significant new demands on parents' time, a strategy of two partners employed full-time may no longer be feasible, either because childcare is perceived as prohibitively expensive or because the couple has a preference for childcare by a parent rather than a paid provider or other family member.

Specialization cannot be tested by comparing wages of childless individuals to those of parents, because parenthood may alter wages for reasons other than household specialization. Instead, specialization—the ways in which couples' joint behavior differs from the behavior that each partner would adopt if living singly—can be tested by comparing wages of parents who are either married or unmarried. To measure the effect of specialization, the key contrast is what individuals' outcomes would be *without the marriage*. It is the marriage (or, more broadly, partnership) that indicates the possibility for specialization. As an extreme example, imagine a father who has no involvement with his child at all, while the mother takes all responsibility for caring for the child *and* providing financial support. Certainly, this indicates gender inequality in the costs of parenthood. However, there is no sense in which these parents are specializing: they are not enacting a joint strategy that divides responsibilities for paid and unpaid labor. Gender inequality in the costs of parenthood is therefore not prima facie evidence that household specialization is responsible for the inequality.

Instead, it is the interaction between marriage and parenthood that provides insight into the role of specialization, as this measures the moderating effect of marriage on the experience of parenthood for each gender. Unpartnered parents provide a comparison group of persons who experience the gendered costs and benefits of parenthood but do not experience the effects of couple-based household specialization.

Consistent with predictions of specialization, research shows larger motherhood wage penalties for married mothers than for unmarried mothers (Budig and England 2001; Glauber 2007), and fatherhood advantages married fathers more than unmarried fathers (Glauber 2008; Killewald 2013). Glauber (2008) and Killewald (2013) both consider the role of wife's employment in shaping the fatherhood premium for married men, but we know of no research that provides a similar analysis for women, or considers other forms of household specialization. As for marriage, the gendered nature of household specialization may occur for any number of reasons, including men's higher average wages, women's biological role in birth and nursing, or gendered expectations of motherhood and fatherhood.

Below, we extend our hypotheses for childless adults to parents. If married parents specialize, we expect mothers and fathers will be pushed further toward a traditional division of labor than childless couples.

Hypothesis 2a: The fatherhood premium will be larger for married than for unmarried fathers. The motherhood penalty will be larger for married than for unmarried mothers. As a result, the moderating effect of marriage on the association between parenthood and wages will differ by gender.

If marriage augments men's wage gains from parenthood and exacerbates the motherhood penalty because married couples adopt a more specialized division of labor at the transition to parenthood, controlling for measures of specialization should reduce the moderating role of marriage in the association between parenthood and wages for both men and women.

> *Hypothesis 2b:* Controlling for measures of time-use specialization will reduce the additional wage bonus that married fathers experience compared to unmarried fathers, and the additional wage penalty that married mothers experience compared to unmarried mothers. As a result, the gender difference in the moderating effect of marriage on the association between parenthood and wages will shrink.

> *Hypothesis 2c:* Controlling for specialization on the basis of job traits and job tenure will further reduce the wage premium that married fathers receive compared to unmarried fathers, and the wage penalty that married mothers experience compared to unmarried mothers. As a result, the gender difference in the moderating effect of marriage on the association between parenthood and wages will further shrink.

DATA AND METHODS

We analyzed data from the 1979 to 2008 waves of the NLSY79 (Bureau of Labor Statistics 2008), which is the dataset of choice for many researchers interested in assessing wage effects of family status transitions (Budig and England 2001; Dougherty 2006; Glauber 2007, 2008; Loughran and Zissimopoulos 2009). The NLSY79 was initiated in 1979 with a sample of men and women age 14 to 22 years. By 2008, respondents were age 43 to 51 years, so most transitions to first marriage and parenthood had occurred.

The panel nature of the NLSY79 allowed us to use fixed-effects models, which control for selection into family forms that is correlated with fixed but unobserved individual traits that are also correlated with wages. Given evidence that earnings are positively associated with entry into marriage for both men and women in the NLSY79 (Sweeney 2002), models that do not account for this positive selection into marriage may give upwardly biased estimates of the marriage premium for both genders.

We present three models. The first model estimates the total relationship between family status changes and wages (Total Effect). It thus excludes potentially endogenous covariates, such as labor market experience. Results from the Total Effect model address Hypotheses 1a and 2a, which concern the gross association between marriage and wages. The second model (Employment Hours) includes controls for an individual's labor market experience and the current employment hours of both the individual and her spouse, testing the mediating role of employment hours and addressing the predictions of Hypotheses 1b and 2b. The third model (Employment Hours + Job Traits and Tenure) tests the mediating role of job traits and tenure, addressing Hypotheses 1c and 2c.

Variables

Wages. The dependent variable is the log of hourly wages in the respondent's most recent job since the last interview. We adjusted all wages to 2008 dollars using the Bureau of Labor Statistics Consumer Price Index. We recoded wages above the 99th percentile or below the 1st percentile of the weighted distribution to the 99th and 1st percentile, respectively.

Union and parenthood status. We categorized parental status into three mutually exclusive groups: no children (the reference group), one child, and two or more children. We defined number of children as the number of surviving biological children under the age of 18 years, regardless of whether the child lives with the parent.[1] We categorized observations into four mutually exclusive union statuses: never-married living singly (the reference group, referred to as single hereafter), unmarried and cohabiting, currently married, and divorced but not cohabiting.[2] Distinguishing cohabitors from other unmarried individuals is particularly important because cohabitation is associated with a wage premium for men compared to living singly (Cohen 2002; Loh 1996). We identified cohabitors based on whether respondents listed a partner in the household roster. Individuals may have experienced multiple union status transitions while in the sample, such as moving from single to cohabiting to married.

Control variables. Individuals are likely to earn higher wages in the years following a transition to marriage simply because they are older. To make wages before and after entry into marriage comparable, we adjusted for potential experience, defined as a respondent's age minus her years of schooling, minus five. We adjusted for potential rather than actual experience because actual experience may be endogenous with marital status.[3] As stated previously, our Total Effect model was designed to measure the total relationship between marriage and wages, rather than the residual difference that cannot be explained by observed indicators of specialization, such as job characteristics or labor market experience. We considered the role of actual work experience in shaping individuals' wages in subsequent models.

We included a quadratic in potential experience in the model. We allowed an individual's current education level to interact with her potential experience because education moderates the association between experience and earnings (Heckman, Lochner, and Todd 2003). For similar reasons, we allowed the quartile of an individual's score on the Armed Forces Qualification Test (AFQT), a measure of cognitive ability, to interact with a quadratic in her potential experience. We measured education in four mutually exclusive categories: less than a 12th-grade education, exactly a 12th-grade education, at least one year of college but fewer than four, and at least four years of college.

We controlled for region of residence using dummy variables that capture four regions: Northeast, North Central, South, and West. We controlled for health using a dummy variable that indicates whether a respondent reported that her health limits the amount or kind of work she can perform. Finally, we included a series of dummy variables for the current calendar year.

Employment hours. In models testing the mediating role of specialization behaviors, we controlled for employment hours with dummy variables indicating whether an individual was working part-time or full-time and whether her spouse worked full-time or less than full-time (including individuals not working for pay). We classified a respondent as working part-time if she reported usually working fewer than 35 hours per week. We defined a spouse as working less than full-time if he worked fewer than 1,500 hours in the previous calendar year. We further controlled for the number of hours a respondent worked above 35 hours per week, for those working full-time. To adjust for previous household specialization decisions, we controlled for a quadratic in an individual's total hours of labor market experience to date, interacted with her education and AFQT score, analogous to the interactions for potential experience. All continuous variables were top-coded at the 99th percentile.

Job traits and tenure. We measured job traits with dummy variables for the occupation, industry, and sector of a respondent's job. We divided occupation into 38 categories and industry into 19 categories (coding schemes available from the authors upon request). Sector was classified by whether an individual was employed by the government. Finally, we controlled for a quadratic in a respondent's tenure (in years) with her current employer, top-coded at the 99th percentile. These are only coarse indicators of the types of jobs individuals held, but they move beyond conceptualizing specialization as occurring only on the basis of employment hours.

Missing data. Because health limitations were quite rare in our sample, we assumed that respondents with missing data on this variable did not have health limitations. For all other covariates, we created a dummy variable and set it equal to one for any observation missing a valid value for that covariate.

Sample

We dropped NLSY79 subsamples that were not followed throughout the entire survey period. We censored all individuals 18 years after the birth of their first child. We thus avoided the assumption that the empty-nest experience (no children under the age of 18 at home) is similar to the pre-child experience.

We excluded 794 women and 177 men who experienced a marriage or birth prior to age 18 because this group did not have reliable wage observations prior to marriage or childbearing. However, results were very similar when this group was included (see Tables S3 and S4 of the online supplement). Similarly, we excluded observations for which wage reports were poor indicators of individuals' long-term earnings potential: those that occurred when a respondent was under age 18, a student, on active military duty or excluded from the labor force questions for some other reason, self-employed, or working at a job without pay. We excluded the self-employed because for this group the division between labor income and business income is measured with substantial error (Fairlie 2005).

None of the previous sample restrictions were due to missing data; they were designed to improve estimates of the relationships of interest. We also employed listwise deletion, which collectively dropped less than 1 percent of the sample for women or men. We excluded observations from widows and widowers, observations lacking data on union or parental status, observations in which potential experience was calculated to be less than zero, and respondents who never reported their education.[4] Because we employed fixed-effects models, each individual must contribute at least two observations to the wage equation to contribute to the estimates. We dropped observations from 172 women and 158 men who did not satisfy this condition.

Finally, 17.7 percent of observations from women in our sample and 7.9 percent of observations from men did not include a wage report, nearly always because the individual did not report working any regular jobs since the last interview. In the main analysis, we excluded observations without wage data, following the dominant tradition in the literature (Budig and England 2001; Hersch and Stratton 2000; Loughran and Zissimopoulos 2009; Taniguchi 1999). It is possible that individuals who experience the largest penalties for parenthood and marriage disproportionately drop out of the labor market. We considered this possibility in supplemental analyses discussed in a later section.

After exclusions, our sample included 46,240 observations from 3,915 women and 56,404 observations from 4,411 men. The median number of observations per individual was 12 for women and 14 for men, with more than 75 percent of individuals of both genders observed eight or more times and more than 90 percent observed five or more times.

Analytic Approach

We estimated fixed-effects models, which allowed us to net out the influence of individuals' fixed, unmeasured traits that may be associated with both wages and marital status. For example, individuals who possess strong social skills may be more likely to be hired and promoted and also more likely to marry. Fixed-effects models avoid the bias due to this selectivity by estimating the marriage premium by comparing wages of the same individual when she is in different union statuses. We discuss the robustness of our results to forms of selection not accounted for by fixed-effects models following presentation of the main results. All standard errors were clustered at the individual level, and all analyses were weighted.

To estimate the marriage premium separately for individuals of different parental statuses, our models include interactions between indicators of union status and parental status. Because of the interaction terms, care must be taken in interpreting the coefficients. Coefficients on the union status variables (married, cohabiting, and divorced) indicate the difference between a childless individual's average wages when she is in the given union group, as compared to when single, net of changes in the control variables. Likewise, coefficients on the parenthood variables indicate the wage changes associated with parenthood for unmarried respondents. Coefficients on the interactions between union and parenthood variables indicate how the parenthood wage premium or penalty is moderated by marriage.

For Hypotheses 1b, 1c, 2b, and 2c, we tested whether changes in coefficients across models are statistically significant using the results of seemingly unrelated regressions (SUR). SUR allows for a correlated error structure between the two models, which is appropriate in our case given that both models use the same sample (full results available upon request).[5]

RESULTS

Table 1 shows weighted descriptive statistics. During the observation period, 88 percent of women and 80 percent of men married. Right-censoring and sample attrition influence these and all sample statistics: some individuals left the sample before marrying, and some married after 2008. Of those who married, about one-quarter cohabited before marriage and 43 to 45 percent divorced. Based on respondents' subsample classification in 1979, about 80 percent of respondents were White, 13 to 14 percent were African American, and 6 percent were Hispanic.

Marriage was the most common union status for men and women. Women were married in 54 percent of observations and men in 49 percent. Women were divorced in 11 percent of observations and men in 8 percent; for each gender, 6 percent were cohabiting. Because of the smaller samples of cohabiting and divorced individuals (compared to married and single), we are less confident about results for these groups. We therefore focus our discussion on comparisons between married and single respondents.

About half of observations for both women and men were when childless, about 20 percent were when parents of one child, and the rest were when parents of two or more children. Descriptive statistics for sector, occupation, and industry categories are found in Table S6 of the online supplement.

To illustrate the potential for specialization for childless married couples and married parents, we examined whether an individual worked part-time. Among unmarried childless adults, women were somewhat more likely than men to work part-time: 16 percent of women compared to 11 percent of men. Among married childless couples, the fraction of women working part-time was the same, but only 4 percent of employed, married, childless men worked part-time. Thus, even for childless couples, marriage had different associations with employment hours for men versus women. Mothers were more likely to be employed part-time than non-mothers, no matter their union status. However, the gap was larger for married than for unmarried women. Among married women, mothers were 21 percentage points more likely than non-mothers to work part-time (37 versus 16 percent). For unmarried women, the difference was only 4 percentage points (20 versus 16 percent). We might therefore expect a larger motherhood penalty for married versus unmarried women because married women are more likely to change to part-time work when they become mothers. For men, the pattern is reversed: fathers were less likely than childless men of the same union-status to work part-time. Although married fathers were the least likely to work part-time (3 percent), full-time work was so common for married men of all parental statuses that married fathers did not experience a large decline in part-time work compared to married childless men.

These results suggest that married couples may specialize on time use, even when childless, and that married parents may specialize more than childless married couples. It is

Table 1. Sample Statistics

	Women Mean (SD)	Men Mean (SD)
Panel A: Individuals		
Ever Marry	.88	.80
Age at first marriage	23.87 (5.29)	25.39 (5.34)
Cohabit before marriage	.26	.26
Ever divorce	.45	.43
Ever Parent	.78	.72
Age at entry to parenthood	25.49 (5.22)	26.79 (5.48)
Race		
White	.81	.80
African American	.13	.14
Hispanic	.06	.06
N (Individuals)	3,915	4,411
Panel B: Person-Years		
Hourly Wage	$15.29 ($9.58)	$19.00 ($11.62)
Union Status		
Cohabiting	.06	.06
Married	.54	.49
Divorced	.11	.08
Number of Children		
Zero	.51	.53
One	.21	.19
Two or more	.28	.27
Work Part-Time (among employed)		
Unmarried childless	.16	.11
Married childless	.16	.04
Unmarried parent	.20	.08
Married parent	.37	.03
Health Limitation	.05	.03
Education		
Less than 12th grade	.05	.13
Exactly 12th grade	.46	.48
1 to 3 years college	.24	.18
4+ years college	.25	.22
N (Person-Years)	46,240	56,404

therefore reasonable to ask whether this specialization contributes to gender differences in the marriage premium for childless adults and to even larger differences for married parents.

Total Effect

In preliminary models, we allowed the full set of interactions between union status (four categories) and parenthood status (three categories) (see Table A1 in the Appendix). We found no evidence that parenthood is differentially associated with wages for cohabitors or divorced persons as compared to single persons, for either women or men. In subsequent models, we therefore specified only an additive association between cohabitation and parenthood and between divorce and parenthood, reducing the number of groups and increasing statistical power. This does not mean we prohibited an association between wages and either cohabitation or divorce. We merely constrained the model such that there

Table 2. Associations between Hourly Wages (ln), Union Status, and Parenthood from Fixed-Effects Models, Total Effect Model

	Women	Men	*P*-Value of Difference
Married	.037**	.073***	.019
	(.011)	(.010)	
Cohabiting	.036**	.058***	.179
	(.012)	(.011)	
Divorced	.045**	.020	.255
	(.016)	(.014)	
1 Child	–.051**	.010	.006
	(.017)	(.014)	
X Married	–.012	.009	.362
	(.018)	(.015)	
2+ Children	–.137***	–.034	<.001
	(.023)	(.018)	
X Married	–.016	.082***	<.001
	(.021)	(.017)	
Person-Year Observations	46,240	56,404	
Individuals	3,915	4,411	
Overall R^2	.25	.28	

Note: Results presented are coefficients with clustered standard errors in parentheses. Childless, single women and men are the excluded categories. Models control for a respondent's region of residence, whether her health limits her work, her potential experience, her education, the interaction between her education and her potential experience, the interaction between her AFQT score and her potential experience, and the year. It is not possible to reject the joint null hypothesis of no interaction between marriage and parenthood for women ($F(2, 3914) = .40$, $p = .67$) but it is for men ($F(2, 4410) = 11.88$, $p < .001$). It is also possible to reject the joint null hypothesis that the interaction between marriage and parenthood is the same for men and women ($F(2, 8325) = 6.78$, $p = .001$).
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

was no interaction between these union statuses and parenthood. As a result, we estimated parenthood penalties or premiums for only two groups of parents: unmarried (including single, cohabiting, and divorced) and married.

Table 2 shows multivariate results for the resulting Total Effect model. Results from separate gender-specific models that evaluate the within-gender components of each hypothesis are shown in the first two columns. To assess between-gender components of the hypotheses, we used a single model that fully interacts each variable with gender. This model is statistically equivalent to the two gender-specific models but facilitates a comparison of coefficients between genders. The third column of Table 2 presents *p*-values of the tests of these gender differences.

Specialization suggests that a transition to marriage should cause an increase in paid labor effort for men and a decrease for women. Hypothesis 1a thus predicted a marriage premium for men, a marriage penalty for women, and a gender gap in the association between marriage and wages. These predictions are supported for men but not for women. We found that marriage is associated with higher wages for both men (7.3 percent) and women (3.7 percent), although the gain is significantly larger for men.

Compared to being single, men and women also experience higher wages when cohabiting (5.8 versus 3.6 percent, respectively), and the gender difference is not statistically significant. Compared to when single, divorce is associated with wage gains of 4.5 percent for women and a nonsignificant wage increase of 2.0 percent for men. The difference between genders is not statistically significant.

Partnered parents can specialize, but not unpartnered parents. Hypothesis 2a predicted that marriage would moderate the association between parenthood and wages for both genders, but in opposite directions, leading to a larger motherhood penalty for married than for unmarried mothers, a larger fatherhood premium for married than for unmarried fathers, and, therefore, a gender gap in the moderating effect of marriage. These predictions are again supported only for men. For men with two or more children, the fatherhood premium is 8.2 percent larger for married than for unmarried men, a statistically significant difference.

Interactions can be interpreted in the following way: to calculate the estimate of the parenthood premium or penalty for a married individual, simply add the coefficient on the parenthood variable to the interaction term. Compared to when married and childless, married fathers of two or more children have wages 4.8 percent higher (–3.4 percent + 8.2 percent). Unmarried fatherhood is not associated with significant wage gains at either parity. Marriage thus moderates the association between fatherhood and wages.

For women, the motherhood penalty is slightly but not significantly larger for married than for unmarried women. Thus, marriage does not appear to moderate the association between motherhood and wages. As a result of these patterns, there is a significant gender difference in the interaction between marriage and parenthood.

Although not the focus of our analysis, our results also demonstrate the large gender gap in the association between parenthood and wages for unmarried men and women. As previously noted, there is no statistically significant association between parenthood and wages for unmarried men. For unmarried women, a first child is associated with a wage penalty of 5.1 percent, and two or more children are associated with a penalty of 13.7 percent, both of which are statistically significant. Gender differences in the experience of unmarried parenthood are large and statistically significant. These results highlight why simple gender differences in the association between parenthood and wages cannot be used as a test of household specialization. Specialization within marriage cannot explain the pronounced gender difference in the labor market costs of parenthood that exists even for unmarried parents. To understand effects of specialization, we must examine how marriage moderates the association between wages and parenthood for men and women. For this reason, we focus on interactions between marriage and parenthood for men and women, rather than examining gender differences in the effects of unmarried parenthood on men's and women's wages.

Employment Hours Model

Results of the Employment Hours model are presented in the first three columns of Table 3. As described in the previous section, we included an indicator variable for married individuals with spouses employed less than full-time. This variable was set to zero for all unmarried individuals and all married individuals with spouses employed full-time. Thus, the coefficient for the *married* variable estimates the wage difference between single individuals and respondents who were married and did *not* have a spouse who specialized in home production. In this way, we test whether the marriage premium is due to the fact that marriage provides a partner who can specialize in home production, or if the marriage premium persists even among married individuals who lack this resource.

Consistent with expectations, we found that individuals whose spouses were employed less than full-time earned a larger marriage premium than those whose spouses were employed full-time, although the difference was only statistically significant for men.[6] Hypothesis 1b proposed that, if wage changes associated with marriage are due to changes in spouses' employment hours and labor market experience, controlling for these measures should reduce the marriage premium for childless men, reduce the marriage penalty for childless women, and narrow the gender gap in the association between marriage and wages. Again, the predictions are supported

Table 3. Associations between Hourly Wages (ln), Union Status, and Parenthood from Fixed-Effects Models, Employment Hours and Employment Hours + Job Traits and Tenure Models

	Employment Hours			Employment Hours + Job Traits and Tenure		
	Women	Men	*P*-Value of Difference	Women	Men	*P*-Value of Difference
Married	.028*	.054***	.086	.025*	.039***	.318
	(.011)	(.010)		(.010)	(.010)	
Cohabiting	.028*	.053***	.128	.028*	.045***	.268
	(.012)	(.011)		(.011)	(.011)	
Divorced	.035*	.016	.349	.036*	.013	.247
	(.016)	(.013)		(.015)	(.013)	
1 Child	–.038*	.006	.040	–.039*	.009	.016
	(.017)	(.013)		(.016)	(.013)	
X Married	–.004	.002	.811	–.003	–.003	.988
	(.018)	(.014)		(.016)	(.014)	
2+ Children	–.089***	–.034	.054	–.082***	–.022	.024
	(.022)	(.018)		(.021)	(.017)	
X Married	–.010	.065***	.005	–.011	.054**	.008
	(.020)	(.017)		(.019)	(.016)	
Work Part-Time	–.108***	–.085***	.173	–.049***	–.033*	.289
	(.009)	(.014)		(.009)	(.013)	
Hours above 35	–.007***	–.006***	.152	–.007***	–.006***	.209
	(.001)	(.000)		(.001)	(.000)	
Spouse Works <FT	.011	.042***	.012	.015	.042***	.024
	(.010)	(.007)		(.010)	(.007)	
Person-Year Observations	46,240	56,404		46,240	56,404	
Individuals	3,915	4,411		3,915	4,411	
Overall R^2	.31	.30		.42	.40	

Note: Results presented are coefficients with clustered standard errors in parentheses. Childless, single women and men are the excluded categories. In addition to the controls in Table 2, the Employment Hours model controls for work hours of the individual and her spouse, her work experience to date, the interaction between her education and her experience, and the interaction between her AFQT score and her experience. The Employment Hours + Job Traits and Tenure model further controls for the occupation and industry of the respondent's job, whether she is employed by the government, and her tenure with the current employer. In the Employment Hours model, it is not possible to reject the joint null hypothesis of no interaction between marriage and parenthood for women ($F(2, 3914) = .12$, $p = .89$) but it is for men ($F(2, 4410) = 8.43$, $p < .001$). It is also possible to reject the joint null hypothesis that the interaction between marriage and parenthood is the same for men and women ($F(2, 8325) = 4.53$, $p = .01$). The marriage premium for individuals with spouses working less than full-time can be found by summing the coefficients for *married* and *spouse works less than full-time* and is significant for both women ($F(1, 3914) = 7.65$, $p = .006$) and men ($F(1, 4410) = 81.21$, $p < .001$). The gender difference in the marriage premium within this group is statistically significant ($F(1, 8325) = 10.70$, $p = .001$). In the Employment Hours + Job Traits and Tenure model, it is not possible to reject the joint null hypothesis of no interaction between marriage and parenthood for women ($F(2, 3914) = .19$, $p = .83$) but it is for men ($F(2, 4410) = 7.17$, $p < .001$). It is possible to reject the joint null hypothesis that the interaction between marriage and parenthood is the same for men and women ($F(2, 8325) = 4.29$, $p = .01$). The marriage premium for individuals with spouses working less than full-time is significant for women ($F(1, 3914) = 9.66$, $p = .002$) and men ($F(1, 4410) = 65.65$, $p < .001$), and the gender difference is statistically significant ($F(1, 8325) = 6.22$, $p = .01$).
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

for men but not for women. The male marriage premium significantly decreases from 7.3 percent in the Total Effect model to 5.4 percent in the Employment Hours model, but, contrary to expectations, the female marriage premium also falls (from 3.7 to 2.8 percent). The gender gap in the marriage premium for childless adults is significantly reduced from 3.6 to 2.6 percent, and the gender gap becomes nonsignificant. It thus appears that wage-beneficial changes in one's own employment hours and the potential for a partner specializing in home production explain a portion of the marriage premium for childless men *and* women, although the effect is larger for men.

Hypothesis 2b stated that, if the moderating effect of marriage on the association between parenthood and wages is due to specialization, controlling for measures of employment hours should reduce this effect, narrowing the variation by marital status in the fatherhood premium and the motherhood penalty, and thus narrowing the gender gap in marriage's moderating effect. Again, these predictions are supported only for men. Controlling for measures of time-use specialization significantly reduces the additional fatherhood bonus received by married as compared to unmarried fathers. For fathers of at least two children, the additional advantage for married fathers compared to unmarried fathers falls from 8.2 to 6.5 percent. As before, variation in the motherhood penalty by marital status is not statistically significant.

Controlling for employment hours significantly reduces the gender gap in the moderating effect of marriage on the association between parenthood and wages. For respondents with two or more children, the gender gap in marriage's moderating effect declines from 9.8 percent (8.2 percent + 1.6 percent) in the Total Effect model to 7.5 percent (6.5 percent + 1.0 percent). Thus, the tendency for marriage to augment the wage benefits of fatherhood, but not motherhood, is partially explained by gender differences in the way that marriage conditions employment responses to parenthood. However, both the gender gap in marriage's moderating effect on parenthood and the within-gender gap between married and unmarried fathers remain statistically significant.

For both parities and both marital statuses, controlling for labor market experience and employment hours reduces the motherhood penalty by 25 to 35 percent. Clearly, one important way motherhood leads to reduced wages is through reduced employment hours and experience. However, the mediating role of lost employment time is similar, regardless of marital status. We thus found no evidence that household specialization is responsible for the motherhood penalty; rather, gender inequality in the labor market costs of parenthood affects *all* mothers, regardless of marital status.

Employment Hours + Job Traits and Tenure

The three right-hand columns of Table 3 present results for the Employment Hours + Job Traits and Tenure model.[7] Hypothesis 1c predicted that, if couples specialize on job characteristics other than employment hours, controlling for measures of job traits and tenure should reduce the male marriage premium and the female marriage penalty, narrowing the gender gap in the association between marriage and wages. Compared to the Employment Hours model, controlling for job traits and tenure reduces the male marriage premium for childless men from 5.4 to 3.9 percent, which is a statistically significant decline. For women, the marriage premium again falls slightly (contrary to expectations) but not significantly. Again, marriage appears to benefit both men and women through encouraging job stability or moves into better jobs, but the advantage is larger for men. Controlling for job traits and tenure further reduces the gender gap in the marriage premium for childless adults from 2.6 to 1.4 percent, and the change is statistically significant. Together, employment hours, job traits, and tenure explain about 60 percent of the gender gap in the marriage premium for childless adults, and the gap is no longer statistically significant. Contrary to expectations, however, these attributes explain a portion of the marriage *advantage* for women, rather than explaining a marriage penalty.

Hypothesis 2c predicted that, if partnered parents specialize on job traits and tenure as well as employment hours, the moderating effect of marriage on the association between parenthood and wages would decline for both genders after controlling for these measures, further reducing the gender gap in marriage's moderating effect. The gap between married and unmarried fatherhood premiums for men with at least two children is significantly reduced after controlling for job traits and tenure. However, for fathers with at least two children, married fathers still receive a fatherhood premium that is 5.4 percent (and statistically significantly) larger than the premium for unmarried fathers. Married men appear to experience a larger fatherhood premium than unmarried men in part because they are more likely to move into more lucrative jobs (as measured by occupation, industry, and sector) after they become fathers, or because they reduce job mobility. For women, the interaction between marriage and parenthood remains nonsignificant. The gender difference in the interaction between marriage and parenthood for parents of two or more children is further reduced from 7.5 to 6.5 percent, although the change is not statistically significant. Together, employment hours, job traits, and tenure explain about one-third of the gender difference in the interaction between marriage and parenthood for parents of at least two children.

In supplemental models, we attempted to isolate whether occupation, industry, sector, or tenure played the largest role in reducing the male marriage premium and the moderating influence of marriage on the fatherhood premium by entering these traits one at a time in our wage models. For childless married men, industry differences had the largest role. Compared to unmarried fathers, married fathers' wages benefit primarily from larger increases in tenure with their current employer. For women, there are no significant interactions between marriage and parenthood, and the marriage premium did not change significantly with inclusion of additional controls, so it is not meaningful to ask which job traits are responsible for changing coefficients. However, we did find that occupation controls are most successful in reducing the motherhood penalty, suggesting that a portion of the motherhood penalty for women of any marital status is due to their placement in more poorly paid occupations.

We also considered models that tested for variation in the marriage premium according to whether the spouse was in a managerial/professional occupation. We found no evidence that individuals' marriage premiums are lower when their spouse is in a professional occupation.[8] Net of individuals' own employment hours and job traits and the employment status of their spouse, spousal occupation class does not appear to moderate the marriage premium. Spousal occupation may of course have an indirect effect that operates through a respondent's own employment hours and job traits.[9]

Selection

In this section, we discuss results of several models designed to test the robustness of results in Table 2 to various forms of selection not accounted for by fixed-effects models. As previously noted, not all individuals reported a wage. If changes in the probability of employment following union status or parenthood transitions are correlated with the magnitude of the wage change that an individual would experience were she to remain continuously employed, then resulting estimates will be biased regarding the average wage penalties or premiums experienced by all individuals, were they to remain continuously employed. To give one example, married women who become mothers and anticipate a large motherhood wage penalty may be most likely to exit the labor force. Unmarried mothers may not have this option. As a result, the motherhood penalty might be underestimated for married mothers, narrowing the gap in the motherhood penalty between married and unmarried mothers.

In supplemental models (see Table A2 in the Appendix), we imputed for missing wage data the value of an individual's hourly wage from the next year it was available, provided it was within the next three years. Failing that, we used the most recent available wage for the individual within the past three years. Results were similar to those presented in the main analysis: we still found a marriage premium for men and women, but it is larger for men. We also found a negative interaction between marriage and parenthood for women and a positive interaction for men, although only the latter is statistically significant.

Loughran and Zissimopoulos (2009) raised the possibility that unobserved heterogeneity in not only wage levels, but wage *growth,* is correlated with selection into marriage. If individual-specific wage growth rates are correlated with age at marriage, then conventional fixed-effects estimates of the marriage premium will be biased. Using fixed-effects models where the outcome was the first difference in wages across subsequent years, they found that, in the NLSY79 cohort, marriage lowered women's wages 4 percent in the year in which the marriage occurred and slowed future wage growth by about 4 percent. Marriage did not affect men's wages in the year of marriage but slowed future wage growth by 2 percent (Loughran and Zissimopoulos 2009). Our own first-difference fixed-effects models had very little explanatory power, so we used the conventional fixed-effects approach. However, we addressed the concern raised by Loughran and Zissimopoulos by allowing heterogeneity in the rate of wage growth by age at marriage. These models did not change how the marriage and parenthood premiums were estimated—we were still comparing wages before and after entry into these statuses—but they allowed further flexibility in the rate of wage growth. We still found a positive and significant marriage premium for both men and women that was slightly larger for men. The gender gap in the marriage premium for childless adults is not statistically significant. If correct, this finding further undermines the specialization hypothesis for childless adults. If marriage has the same positive association with individuals' wages regardless of gender, specialization cannot be responsible for the male marriage premium. We also found a positive interaction between marriage and parenthood for men, but a negative interaction for women, although only the former is statistically significant (see Table A3 in the Appendix).

Finally, we tested for the possibility that individuals may select into marriage or parenthood at a particular time due to time-*varying* traits that are also associated with their wages. For example, unmarried couples report wanting to achieve a certain level of financial sufficiency and stability prior to marriage (Edin and Kefalas 2005; Smock, Manning, and Porter 2005). If wage growth facilitates marriage, and higher wages persist after marriage, it will appear in fixed-effects models that marriage is associated with wage increases, even if the wage increase occurred prior to marriage. In support of this concern, Dougherty (2006) found that both men and women experience a wage premium beginning several years before marriage.

We tested whether individuals' wages were different in the year immediately preceding marriage, as compared to other pre-marriage years, and in the year immediately prior to parenthood, compared to other childless years (see Table A4 in the Appendix). For men and women who were not cohabiting, wages were statistically significantly higher in the year immediately prior to marriage than in other premarital years, although not as high as they were following marriage. For women, cohabitation did not moderate this association; for men, cohabitation depressed the anticipatory effect of marriage. These results suggest that some of the returns to marriage may not be due to the marital union itself, but instead to the benefits of romantic partnerships more generally, or to time-varying selection into unions following wage increases. Estimates from Tables 2 and 3 may therefore overestimate the causal relationship between marriage and wages. Nonetheless, we found no evidence that our conclusions with regard to specialization change after accounting for a premarital wage increase: marriage generates a wage premium for women as well as for men.

Wage changes just prior to entry into parenthood do not threaten our conclusions with regard to the predictions of specialization. None of the indicators of anticipated parenthood are statistically significant. Furthermore, the interaction between marriage and impending parenthood is *opposite* the interaction between marriage and parenthood, for both men and women. Whatever the cause of these slight pre-parenthood wage changes, selection of this form clearly does not drive the observed interactions between marriage and parenthood, as it operates in the opposite direction.

Limitations

Our analyses have several limitations. Because the NLSY79 lacks data on time spent in housework or childcare, couples' household specialization must be proxied with information on their time in the labor market, without complementary information on time in domestic labor. We did capture the indirect effect of household labor that operates through more proximal determinants of wages, including employment hours and job traits.

The NLSY79 also includes only relatively coarse measures of job traits. Future research with more detailed data on job traits would allow a better identification of whether couples specialize on factors other than work hours, such as adopting a one job–one career approach. Our own preliminary attempts suggest that married men move to higher-paying industries and married fathers benefit from longer job tenure. Our measures of employment hours are similarly coarse, as we did not consider work schedule. Married women may be more likely to move into jobs with employment schedules that allow them to accommodate household labor and childcare obligations. For example, part-year employment is the norm in teaching, and nonstandard employment hours are common in the service sector and health professions. If these professions are associated with lower wages (perhaps in part because of their feminized nature), household specialization based on the schedule, rather than amount, of employment hours may also contribute to the gender gap in the association between marriage and wages. Further research is needed to assess this possibility.

In the NLSY79 more extensive information was collected on respondents than on their spouses. This is a particularly relevant limitation for our analysis, which is focused on specialization—a joint process between partners. We made use of the available information on spouses' employment time and occupations, finding that—net of controls for individuals' own job traits, labor market hours, and human capital—spousal employment status does moderate the marriage premium, especially for men, but the role of spousal occupation is negligible. We believe our measures of individuals' job traits and employment hours capture many of the indirect paths by which spouses' job characteristics affect marriage premiums. Further research is needed, however, to test the mediating and moderating roles of spousal employment and household labor in shaping the marriage premium.

CONCLUSIONS

What do our results tell us about support for the specialization hypothesis? Table 4 summarizes results for each of our hypotheses. We refer to a hypothesis as *supported* if coefficients (Hypotheses 1a and 2a) or changes in coefficients (Hypotheses 1b, 1c, 2b, and 2c) were in the expected direction and statistically significant.

For men, the empirical pattern of wage premiums was consistent with specialization. Marriage and cohabitation were associated with wage gains for men, and married fathers had a larger fatherhood premium than unmarried fathers. Changes in men's employment hours, job traits, and tenure associated with marriage and married fatherhood explain a

Table 4. Summary of Results

Hypothesis	Result
Hypothesis 1a: Among childless adults, men will experience a marriage wage premium, and women will experience a marriage penalty, resulting in a gender difference in the marriage premium.	Men: Supported Women: Not supported Between: Supported
Hypothesis 1b: Controlling for measures of time-use specialization among childless adults will reduce the marriage premium for men and the marriage penalty for women, thus reducing the gender gap in the marriage premium.	Men: Supported Women: Not supported Between: Supported
Hypothesis 1c: Controlling for measures of job traits and job tenure will also reduce the marriage premium for men and ameliorate the marriage penalty for women, further narrowing the gender gap in the marriage premium.	Men: Supported Women: Not supported Between: Supported
Hypothesis 2a: The fatherhood premium will be larger for married than for unmarried fathers. The motherhood penalty will be larger for married than for unmarried mothers. As a result, the moderating effect of marriage on the association between parenthood and wages will differ by gender.	Men: Supported Women: Not supported[a] Between: Supported
Hypothesis 2b: Controlling for measures of time-use specialization will reduce the additional wage bonus that married fathers experience compared to unmarried fathers, and the additional wage penalty that married mothers experience compared to unmarried mothers. As a result, the gender difference in the moderating effect of marriage on the association between parenthood and wages will shrink.	Men: Supported Women: Not supported[a] Between: Supported
Hypothesis 2c: Controlling for specialization on the basis of job traits and job tenure will further reduce the wage premium that married fathers receive compared to unmarried fathers, and the wage penalty that married mothers experience compared to unmarried mothers. As a result, the gender difference in the moderating effect of marriage on the association between parenthood and wages will further shrink.	Men: Supported (2+ children) Women: Not supported Between: Not supported[a]

[a]Point estimates were in the correct direction but not statistically significant.

portion of these wage gains, as does the potential to have a spouse specializing in home production. For parents of at least two children, about one-third of the larger fatherhood premium for married as compared to unmarried men can be explained by the combination of these factors. For childless married men, changes in these characteristics explain almost half of the wage premium. Among childless men, changes in job traits and tenure were almost as important to explaining the marriage premium as were changes in employment hours and the opportunity to have a partner specializing in home production. Prior research has focused almost exclusively on specialization as measured by time use (Chun and Lee 2001; Gray 1997; Hersch and Stratton 2000; Loh 1996), but our results highlight the need to consider the effect of marriage on career strategies beyond employment hours.

Our results are consistent with prior research that has found a marriage premium for both men (Chun and Lee 2001; Gray 1997; Hersch and Stratton 2000; Loh 1996) and women (Budig and England 2001; Glauber 2007; Taniguchi 1999; Waldfogel 1997). Our analyses are also consistent with findings that wives' employment moderates the association between marriage and men's wages (Chun and Lee 2001; Gray 1997). It is not surprising that these patterns have led researchers studying only men's wages to conclude that household specialization is responsible for the marriage premium. Our study, therefore, challenges not the empirical findings of prior research, but the interpretation.

Household specialization is inherently a two-gender theory. It should explain the relationship between marriage and wages for women as well as for men. Yet our results for women are generally inconsistent with the predictions of specialization. Marriage and cohabitation are associated with wage gains for childless women, not wage losses as predicted by specialization, and marriage does not significantly moderate the association between motherhood and wages. Furthermore, if anything, marriage alters women's employment hours, job traits, and tenure in ways beneficial to their wages. Although effects are more modest for women, results suggest that marriage benefits men's *and* women's wages through similar processes.

Neither employment hours nor job traits and tenure fully explain the marriage premium for childless adults or the moderating effect of marriage on the fatherhood premium. Our results thus indicate the need for future research that considers alternative causal mechanisms linking marriage to individuals' wages. We highlight several possibilities. First, marriage may alter individuals' preferences for financial resources. Gorman (2000) found that married individuals of both genders rank pay as a more important job characteristic than do unmarried individuals. Marriage may motivate both men and women to devote more effort to paid labor, perhaps with the goal of accumulating assets for future joint investments, such as children or owning a home. Second, marriage may provide individuals with benefits that are positively associated with wages, such as better health (Waite and Gallagher 2000) and access to each other's human capital (Loh 1996). Finally, employers may discriminate in favor of married workers of both genders, particularly if marriage generates a positively biased assessment of a worker's reliability.

Our results also highlight that marriage plays a larger role in men's labor market outcomes than in women's. Childless men and women both experience a marriage premium, but gains are larger for men. The motherhood penalty is large but varies little by marital status. Although we see profound gender differences in the labor market costs of parenthood, specialization does not appear to be the main cause of the motherhood penalty. Conversely, marriage leads to substantial increases in men's wages, and these gains are further augmented when married men become fathers. A portion of the marriage premium for both men and women appears to be due to changes in employment hours, job traits, and tenure, but men gain more from these changes. In fact, accounting for these factors explains the gender gap in the marriage premium for childless adults.

A satisfactory explanation for these results should explain why men and women both experience wage gains at marriage, and also why men's wages rise more. It is possible that specialization within couples explains the larger wage gains for men, and other processes explain the wage gains that both men and women experience at marriage. We propose two alternative possibilities. Transitions to marriage and married parenthood may encourage men's sense of responsibility, particularly financial responsibility (Townsend 2002), and encourage their involvement in stabilizing communities, such as religious organizations (Knoester and Eggebeen 2006; Nock 1998). Changes of this kind may be more modest for women, either because single women already possess these positive traits or because gendered norms of family behavior place less emphasis on financial providership for women. It would be inappropriate to conclude that specialization explains the association between family and wages for men but not for women. Because specialization is a two-gender theory, failure to explain outcomes for both genders indicates a lack of empirical support for the theory. We encourage future work to consider alternative explanations for the gendered associations between partnership and employment outcomes for childless adults and parents.

APPENDIX

Table A1. Associations between Hourly Wages (ln), Union Status, and Parenthood from Fixed-Effects Models, Total Effect Model, Fully Interacted Union Status and Parenthood

	Women	Men	*P*-Value of Difference
Married	.039**	.074***	.024
	(.012)	(.010)	
Cohabiting	.031*	.063***	.106
	(.014)	(.014)	
Divorced	.061**	.023	.188
	(.021)	(.019)	
1 Child	–.034	.025	.051
	(.026)	(.016)	
X Married	–.029	–.007	.477
	(.027)	(.018)	
X Cohabiting	.005	–.023	.477
	(.031)	(.024)	
X Divorced	–.047	–.027	.690
	(.038)	(.030)	
2+ Children	–.136***	–.040	.018
	(.032)	(.025)	
X Married	–.017	.088**	.009
	(.031)	(.025)	
X Cohabiting	.015	–.003	.701
	(.037)	(.029)	
X Divorced	–.016	.009	.625
	(.038)	(.035)	
Person-Year Observations	46,240	56,404	
Individuals	3,915	4,411	
Overall R^2	.25	.28	

Note: Results presented are coefficients with clustered standard errors in parentheses. Childless, single women and men are the excluded categories. Models control for a respondent's region of residence, whether her health limits her work, her potential experience, her education, the interaction between her education and her potential experience, the interaction between her AFQT score and her potential experience, and the year. It is not possible to reject the joint null hypothesis of no interaction between divorce and parenthood or between cohabitation and parenthood for either women (F(4, 3914) = .66, p = .62) or men (F(4, 4410) = .43, p = .79).
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

Table A2. Associations between Hourly Wages (ln), Union Status, and Parenthood from Fixed-Effects Models, Total Effect Model, Imputed Wages for Non-employed Individuals

	Women	Men	*P*-Value of Difference
Married	.041***	.081***	.010
	(.011)	(.010)	
Cohabiting	.038**	.062***	.146
	(.013)	(.011)	
Divorced	.050**	.022	.186
	(.016)	(.014)	
1 Child	–.064***	.015	<.001
	(.017)	(.014)	
X Married	–.008	.008	.485
	(.017)	(.015)	
2+ Children	–.138***	–.026	<.001
	(.021)	(.019)	
X Married	–.033	.076***	<.001
	(.019)	(.018)	
Person-Year Observations	56,393	62,067	
Individuals	3,915	4,411	
Overall R^2	.27	.28	

Note: Results presented are coefficients with clustered standard errors in parentheses. Childless, single women and men are the excluded categories. Models control for a respondent's region of residence, whether her health limits her work, her potential experience, her education, the interaction between her education and her potential experience, the interaction between her AFQT score and her potential experience, and the year. We imputed wages for non-employed individuals using an individual's subsequent or most recent wage. It is not possible to reject the joint null hypothesis of no interaction between marriage and parenthood for women ($F(2, 3914) = 1.51$, $p = .22$) but it is for men ($F(2, 4410) = 10.22$, $p < .001$). It is also possible to reject the joint null hypothesis that the interaction between marriage and parenthood is the same for men and women ($F(2, 8325) = 9.38$, $p < .001$).
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

Table A3. Associations between Hourly Wages (ln), Union Status, and Parenthood from Fixed-Effects Models, Total Effect Model, Flexible Wage Growth by Age at Marriage

	Women	Men	*P*-Value of Difference
Married	.031**	.056***	.103
	(.012)	(.010)	
Cohabiting	.032**	.051***	.273
	(.012)	(.011)	
Divorced	.042*	–.002	.047
	(.017)	(.014)	
1 Child	–.050**	.005	.013
	(.017)	(.014)	
X Married	–.010	.006	.493
	(.018)	(.015)	
2+ Children	–.131***	–.043*	.003
	(.023)	(.018)	
X Married	–.014	.076***	.001
	(.021)	(.018)	
Person-Year Observations	46,240	56,404	
Individuals	3,915	4,411	
Overall R^2	.25	.28	

Note: Results presented are coefficients with clustered standard errors in parentheses. Childless, single women and men are the excluded categories. Models control for a respondent's region of residence, whether her health limits her work, her potential experience, her education, the interaction between her education and her potential experience, the interaction between her AFQT score and her potential experience, the interaction between her age at marriage and her potential experience, and the year. It is not possible to reject the joint null hypothesis of no interaction between marriage and parenthood for women ($F(2, 3914) = .27$, $p = .76$) but it is for men ($F(2, 4410) = 10.65$, $p < .001$). It is also possible to reject the joint null hypothesis that the interaction between marriage and parenthood is the same for men and women ($F(2, 8325) = 5.80$, $p = .003$).
*$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed tests).

Table A4. Associations between Hourly Wages (ln), Union Status, and Parenthood from Fixed-Effects Models, Total Effect Model, Anticipation

	Anticipation of Marriage			Anticipation of Parenthood		
	Women	Men	*P*-Value of Difference	Women	Men	*P*-Value of Difference
Marry Next Year	.036** (.012)	.046*** (.011)	.520			
X Cohabiting	.002 (.025)	–.047* (.023)	.154			
Parent Next Year				–.020 (.019)	.029 (.018)	.061
X Married				.034 (.022)	–.032 (.021)	.030
Married	.046*** (.013)	.082*** (.011)	.029	.034** (.012)	.075*** (.011)	.011
Cohabiting	.034* (.013)	.064*** (.013)	.102	.037** (.012)	.057*** (.011)	.216
Divorced	.051** (.017)	.029* (.014)	.312	.045** (.016)	.019 (.014)	.230
1 Child	–.048** (.017)	.011 (.014)	.007	–.053** (.018)	.014 (.014)	.004
X Married	–.014 (.018)	.009 (.015)	.329	–.007 (.019)	.005 (.015)	.614
2+ Children	–.132*** (.023)	–.030 (.019)	.001	–.139*** (.024)	–.030 (.019)	<.001
X Married	–.019 (.021)	.080*** (.018)	<.001	–.012 (.021)	.078*** (.018)	.001
Person-Year Observations	46,240	56,404		46,240	56,404	
Individuals	3,915	4,411		3,915	4,411	
Overall R^2	.25	.28		.25	.28	

Note: Results presented are coefficients with clustered standard errors in parentheses. Childless, single women and men are the excluded categories. Models control for a respondent's region of residence, whether her health limits her work, her potential experience, her education, the interaction between her education and her potential experience, the interaction between her AFQT score and her potential experience, and the year. It is not possible to reject the joint null hypothesis of no interaction between marriage and parenthood for women in either the Anticipation of Marriage (F(2, 3914) = .53, p = .59) or the Anticipation of Parenthood (F(2, 3914) = .16, p = .85) model, but it is for men in both models (F(2, 4410) = 11.35, p < .001, and F(2, 4410) = 11.14, p < .001, respectively). It is also possible to reject the joint null hypothesis that the interaction between marriage and parenthood is the same for men and women in both models (F(2, 8325) = 6.90, p = .001, and F(2, 8325) = 5.77, p = .003, respectively).
*p < .05; **p < .01; ***p < .001 (two-tailed tests).

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Notes

1. Some error in reporting births is likely, particularly for men. Joyner and colleagues (2012) found that the NLSY79 undercounts births to young men by about 11 percent, compared to less than 3 percent for young women, and that men's underreporting is especially likely for nonmarital births. Unmarried fathers in our sample are thus likely to be a positively selected subset.
2. We found no evidence that the cohabitation premium differs for individuals who are single versus those who are divorced.
3. Tables S1 and S2 in the online supplement show results of alternative models that used age, rather than potential experience, as a control variable (http://asr.sagepub.com/supplemental).
4. Table S5 in the online supplement shows the number of person-year observations excluded for each reason.
5. To estimate these models, we first centered all variables at the individual level and then applied standard SUR estimation procedures. As a result, the standard errors associated with the SUR models do not account for the estimation involved in the centering process. This slightly overstates the statistical significance of differences across models, thereby *overstating* the evidence for specialization.
6. Parenthood does not significantly moderate the association between spousal employment status and wages.
7. Coefficients for job traits and tenure are shown in Table S7 in the online supplement.
8. Spousal occupational class also does not moderate the association between marriage and annual earnings. For married individuals, we also tested whether spousal occupation, measured using the same set of occupational categories we used for respondents, mediates the association between parenthood and wages. We found no evidence that it does, for either gender.
9. We considered models that used the log of annual earnings as the dependent variable (Tables S8 and S9 in the online supplement). For men, marriage and cohabitation are associated with larger earnings gains than wage gains, suggesting that union formation is associated with both increased wages and increased employment hours. For women, neither marriage nor cohabitation is associated with earnings gains, suggesting that union formation leads to higher wages but reduced paid labor time. The interaction between marriage and motherhood is strong in these models. Although married mothers do not experience significantly larger wage penalties than unmarried mothers, their earnings losses are significantly larger, indicating that they experience greater reductions in employment hours.

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