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HOUSEHOLD SPECIALIZATION AND THE MALE MARRIAGE WAGE PREMIUM

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Empirical research has consistently shown that married men have substantially higher wages, on average, than otherwise similar unmarried men. One commonly cited hypothesis to explain this pattern is that marriage allows one spouse to specialize in market production and the other to specialize in home production, enabling the former—usually the husband—to acquire more market-specific human capital and, ultimately, earn higher wages. The authors test this hypothesis using panel data from the National Survey of Families and Households. The data reveal that married men spent virtually the same amount of time on home production as did single men, albeit on different types of housework. Estimates from a fixed effects wage equation indicate that the male marriage wage premium is not substantially affected by controls for home production activities. Household specialization, the authors conclude, does not appear to have been responsible for the marriage premium in this sample.

Virtually all wage regressions including an indicator of marital status find that married men have substantially higher wages than do not-married men, even after controls are added for observable human capital and job characteristics. The magnitude varies but is quite large, with typical values indicating that married men receive a wage premium of 10–30%. While the empirical evidence of a marriage premium for men is incontrovertible, the precise nature of the relation has not yet been convincingly explained. A leading theory is that specialization within the household results in genuine productivity differences between married men who have the opportunity to specialize and unmarried men who do not. Lacking direct measures of specialization and time allocation, studies have used wife's employment status as a proxy for specialization, and ensuing results have been inconsistent.

An important contribution of this paper is the use of more direct measures of time allocation to control for specialization

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A data appendix with additional results and copies of the computer programs used to generate the results presented in the paper are available from Leslie Stratton at the Department of Economics, Virginia Commonwealth University, Richmond, VA 23284–4000.

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within the household. We use panel data from the National Survey of Families and Households that include information on time allocated to nine different home production activities. With these data we investigate whether the observed marriage premium is due to specialization within the household.

Background

Two primary explanations have been advanced regarding the wage premium for married men.[1] One is that more productive men marry, and the other is that marriage makes men more productive.[2] If more productive men marry, then men who marry should be more productive and receive higher wages throughout their lifetime. In this case, marriage is serving as a proxy for unobserved characteristics that are correlated with productivity, and the marital wage differential is attributable to selection. If, on the other hand, marriage makes men more productive, then wages should increase following marriage.

A substantial literature examines these competing hypotheses and the source of the underlying productivity differential. If more productive men are selected into marriage, then marriage and wages are jointly endogenous. Using instrumental variables estimation on a cross-section of data to control for this possible joint endogeneity, Nakosteen and Zimmer (1987) found that the magnitude of the marriage premium was unchanged but that it became statistically insignificant, a finding they interpreted as evidence that the marriage premium is due to selection. However, it is difficult to find suitable instruments for marriage, and findings based on weak or invalid instruments are suspect. Recent investigations have used panel data to estimate fixed effects models, thereby netting out any unobserved individual-specific fixed effect that may be correlated with both marriage and wages. Using this approach, Korenman and Neumark (1991) concluded that at most 20% of the observed premium is due to selection. Other researchers (Cornwell and Rupert 1997; Daniel 1991; Gray 1997) also have reported finding a marriage premium in fixed effects estimates, though often of a smaller magnitude than that found by Korenman and Neumark. An alternative selection mechanism, for which fixed effects models would not correct, posits that men with more rapid wage growth (rather than men with higher wage levels) may be more likely to marry. Both Korenman and Neumark (1991) and Gray (1997) tested for but found no evidence of such selection.

If marriage somehow makes men more productive, one possible implication is that men who have been married longer receive higher wages. In fact, Kenny (1983), Korenman and Neumark (1991), and Daniel (1991) found faster wage growth for married men, particularly early in marriage. Cornwell and Rupert (1997) contested these findings, but overall the evidence tends to support the productivity hypothesis.

Although the empirical evidence supports the hypothesis that marriage enhances productivity, the causal mechanism is less clear. The theory most often referenced is Becker's theory of the family (1991). According to this theory, it is efficient for one spouse to specialize in market production and the other to specialize in home production. The spouse (usually the husband) who devotes more time to market production will acquire more market-specific human capital, which will lead to an increase in market productivity and thereby to higher wages.

A number of other arguments can also be made linking marriage, specialization, and wages. Becker's (1985) model of effort posits that total effort is limited and any

[1]Excellent surveys of the literature specifically examining the premium appear in Korenman and Neumark (1991) and Loh (1996).

[2]An alternative explanation is that married men are more likely to choose jobs with undesirable and hence wage-compensating characteristics (Reed and Harford 1989). However, Hersch (1991) reported a statistically significant marital wage differential even after controlling for job characteristics.

effort allocated to housework reduces the effort available for market work. If increased effort means increased productivity and increased wages, then men may benefit from specialization because they are able to expend more effort on-the-job following marriage. Alternatively, married men's wages may increase via specialization in particular types of home production activity. Even if marriage does not affect either total market hours or total time on home production, certain household activities may cause intermittent disruptions to the workday. For instance, those who need to stay at home for emergency home repairs or emergency child care obligations may find their productivity is affected by such disruptions to their schedule. Specialization could reduce these disruptions to market work for men if after marriage they are less likely to be called upon to interrupt their workday.

Efforts to test the specialization hypothesis have used measures of the wife's employment as a proxy for specialization. The argument is that if marriage enhances market productivity by allowing men to specialize, then married men whose wives do not work in the market (or whose wives work fewer hours) will have higher wages than will either unmarried men or men with employed wives. Using cross-sectional data, Loh (1996) found that married men whose wives worked in the market received higher wages. Daniel (1991) and Gray (1997), on the other hand, found the expected inverse relation between the husband's wage and his wife's market hours—a relation that persists in fixed effects and instrumental variables estimates accounting for the possible endogeneity of the wife's market hours with her husband's wage.

These conflicting results suggest that wives' market hours are a weak indicator of household specialization. Indeed, theoretically the effect on the husband's time of his wife's employment status could go either way, as there are competing income and substitution effects. Married men with employed wives may spend less time on housework than men whose wives are not employed because household income is greater, or they may spend more time because the value of their spouse's time may be greater. The net effect will depend on the magnitude of these two components. Use of spousal employment as a proxy for household specialization implicitly assumes that the substitution effect dominates the income effect. However, research by South and Spitze (1994) indicates that the time allocated to home production by husbands whose wives are employed is not substantially different from the time allocated by husbands whose wives are not employed.

The goal of this study is to use data on time allocated to housework to directly examine whether the marriage premium can be attributed to specialization within the household. Compared to the wife's employment status, which has a theoretically ambiguous relation to the husband's housework time or effort and therefore to his investments in market capital, own housework time provides a more direct indicator of the extent of specialization within the household. If marriage is merely a proxy for specialization, then inclusion of home production time should reduce or eliminate the marriage premium.

Data

We use data from the National Survey of Families and Households (NSFH—see Sweet and Bumpass [1995] and Sweet, Bumpass, and Call [1988] for a more complete description). This is a national sample of 13,008 households, with a double sample of minorities, single parents, stepparents, cohabitors, and recently married persons who were interviewed once in 1987–88 and again in 1992–94. This data set has two key attributes. First, the survey includes measures of time spent on nine types of household activities. Second, it provides panel data that allow us to estimate fixed effects equations on housework time and wages. Since we have two years of data for this sample, this amounts to estimation in first differences.

We restrict our analysis to employed,

white, non-Hispanic[3] men, age 18–59 at the time of the first survey, who were not students and who reported valid information on all the variables used in the wage analysis for both years of the survey. These restrictions result in a sample size of 1,373. While this sample is substantially smaller than the original data set, most observations are lost due to gender, race, and age restrictions. By sample design, within households with more than one adult, one adult was randomly chosen as the primary respondent. Although information was also elicited from other household members, primary respondents were asked to provide the more extensive information used in this paper. Almost 60% of the primary sample respondents were women, almost 28% were non-white or Hispanic, and at least 21% were over the age of 64 at the time of the second survey; white, non-Hispanic men of working age comprised less than a quarter of the sample. Sample attrition between surveys for this group ran about 19%, somewhat less than that for the entire sample, bringing the potential sample size below 2,500. Just over 10% of this sample were in school or in the military, and another 10% were not employed, at the time of at least one of the surveys. Excluding those individuals who did not report a wage or reported one below $2.50 or above $75 per hour in 1992 dollars, and those with incomplete work or marital histories, results in a final sample of 1,373.[4]

Primary respondents were asked to provide extensive demographic information including age, education, marital status and history, family composition, number of children in the household in each of three age groups (under age 6, between age 6 and 12, and between age 13 and 18), and whether they had a disability that affected their ability to work. Labor market information included wages and method of payment (for example, hourly or salaried), number of hours worked per week, years of work experience and tenure, and industry/occupation of employment.[5] Hourly wage is calculated in the usual manner from information on earnings, method of payment, and hours worked, and is converted to 1992 constant dollars using the Consumer Price Index.

Table 1 provides descriptive statistics for the sample based on data from the first survey, stratified by marital status as of the first survey date. We consider three marital status categories: currently married; divorced, separated, or widowed (henceforth previously married); and never married. Sixty-six percent of the men were married, and 14.6% had previously been married but were unmarried at the time of the survey. As the values indicate, married men had hourly earnings about 7% higher than previously married men and about 35% higher than never-married men. Although there are statistically significant differences by marital status in the means of many of the variables (indicated in the last column of Table 1), the most substantial differentials are in age, experience, tenure, and children. The differentials in experience

[3]Hispanic men are excluded for three reasons. First, the NSFH does not separately inquire about race and ethnicity. Thus, the racial composition of the Hispanic sample is uncertain, and all the studies cited herein regarding the marital wage differential restrict analysis to white men. Second, the only study to discuss Hispanics (Daniel 1991) explicitly excluded them on the grounds that a dummy variable alone was not sufficient to explain ethnic wage differences. Finally, regression estimates using the NSFH sample of Hispanic men find no statistically significant marital wage differential.

[4]In addition, for eleven of the respondents, computed wage differs by more than a factor of five between the first and second survey. At least one of these wage measures was invariably calculated based on a measure of usual annual or monthly earnings and is likely incorrect. Panel estimates are especially sensitive to such measurement error; hence these observations were excluded. Also excluded were a handful of observations lacking information on other variables used in the regression analysis, such as education.

[5]Consistent information on union status is unavailable from this survey. The estimated returns to marriage are unaffected by exclusion of the available measures; hence these measures are excluded from the specifications presented in this paper.

Table 1. Descriptive Statistics by First-Wave Marital Status. (Standard Deviations in Parentheses)

Variable	*Married*	*Divorced, Separated, Widowed*	*Never Married*	*Significant Differences*[a]
Hourly Wage (1992$)	15.75	14.72	11.62	b,c
	(8.43)	(7.26)	(6.59)	
Education	13.87	13.48	13.79	
	(2.62)	(2.60)	(2.46)	
Work Experience	15.73	18.73	7.54	a,b,c
	(8.61)	(8.90)	(6.33)	
Tenure	7.88	9.17	3.72	b,c
	(7.20)	(8.09)	(4.06)	
Hours Worked per Week	46.47	45.61	44.29	b
	(9.68)	(10.69)	(10.15)	
Age	35.95	38.41	27.52	a,b,c
	(8.53)	(9.09)	(7.13)	
Years Married	11.87	10.75	0	a,b,c
	(8.61)	(7.74)		
Lives in South	0.32	0.31	0.27	
	(0.47)	(0.46)	(0.44)	
Lives in SMSA	0.70	0.71	0.78	b
	(0.46)	(0.45)	(0.42)	
Disability Affecting Work	0.01	0.04	0.02	
	(0.11)	(0.18)	(0.12)	
Children under Age 6	0.57	0.12	0.05	a,b
	(0.80)	(0.41)	(0.26)	
Children Age 6–12	0.44	0.26	0.06	a,b,c
	(0.74)	(0.57)	(0.32)	
Children Age 13–18	0.35	0.24	0.16	b
	(0.68)	(0.55)	(0.43)	
Sample Size	909	200	264	
% of Total	66.2%	14.6%	19.2%	

[a]Statistically significant differences in means at $p < 0.01$, where a = married vs. divorced, separated, widowed; b = married vs. never married; and c = divorced, separated, widowed vs. never married.

and tenure are important because these variables are typically found to be important determinants of wages. We explore the role of children separately in the wage analysis. The lower average wage for never-married men is certainly due in part to the fact that they had lower experience and tenure than ever-married men.

Since those changing marital status are the primary force behind the fixed effects estimates, it is important to examine the frequency of marital status changes within the sample. Within the almost six years between survey periods, 22% of the respondents in the panel data sample changed marital status. Eleven percent of the men who were married as of the first wave were no longer married by the second wave, and 45% of the men classified as previously married at the time of the first wave had remarried by the second wave. Of the men who had never been married as of the first wave, 39% were classified as married and another 6% as previously married by the time of the second wave.

The detailed information on home production essential to this study was elicited by asking respondents to report the time spent by themselves on nine activities: "meal preparation" (MEALS), "washing dishes and cleaning up after meals" (DISHES), "house cleaning" (CLEANING), "outdoor and other

household maintenance tasks" (OUTDOOR & MAINTENANCE), "shopping for groceries and other household goods" (SHOPPING), "washing, ironing and mending" (LAUNDRY), "paying bills and keeping other financial records" (BILLS), "auto maintenance and repair" (AUTO), and "driving other household members to work, school, or other activities" (DRIVING OTHERS).[6] A number of respondents failed to provide responses to questions about one or more household tasks, particularly during the first wave of interviews. NSFH personnel indicate that many respondents to this wave left blanks instead of filling in zeros as the directions requested. Interviewers were instructed to check for this during the second wave, thus leading to substantially improved response rates. Some non-responses from the first wave can reasonably be assigned zero values based on answers to other questions. For instance, driving time is set to zero for those not answering the question about time spent driving who reported that they had not driven a car for over six months. Other missing responses from the first wave are coded zero if the respondent answered at least six of the nine questions.[7] We consider reported housework time to be unreliable for those who failed to report housework time in more than three categories at the time of the first survey, and for those in either survey who reported "some" time in an activity rather than a magnitude or who reported an implausible 70 hours or more of housework activity per week. We exclude the 263 respondents with unreliable housework measures from the housework time analyses, but include them in the wage analyses with all housework measures set to zero and two dummy variables to indicate which survey contained the unreliable housework measure. Since these missing data were not entirely random but more likely to arise for married and for disabled men in the first wave, we perform a variety of sensitivity tests, which are discussed later.

As the focus of this paper is on housework time, it is important to consider the accuracy of the reported data. Juster and Stafford (1991) provided an excellent discussion of time allocation studies. They reported that survey data like those used here tend to overstate true time spent on most activities. Only in the case of activities that are sporadic in nature, such as home repairs, did they recommend use of survey data to supplement diary-based measures. Indeed, a comparison of diary-based data from the Time Use Survey (TUS) with the NSFH data employed here indicates substantial differences. Married men working full-time reported an average of 11.01 hours of housework per week in the TUS and 17.86 hours per week in this sample of white, non-Hispanics from the NSFH. In part, the difference may be due to an increase in housework done by men over time between the TUS survey period of 1975–76 and the NSFH survey period of 1987–88, but the magnitude of the difference suggests that housework time is likely to be overstated in the NSFH.[8] We investigate in the empirical section whether measurement error alters the findings of this paper by using, among other variables, the wife's report of her husband's housework time as an instrument for the husband's own report.

Also of some concern is the lack of specific information on child care activities in

[6]The introductory wording for this series of questions was: "The questions on this page concern household tasks and who in your household normally spends time doing those tasks. Write in the APPROXIMATE number of hours per week that you, your husband or wife, or others in the household normally spend doing the following things. If no time is spent doing the household task, write in '0.'"

[7]The activities most often affected by this recoding are OUTDOOR & MAINTENANCE and AUTO. There were 54 cases recoded with one missing housework measure, 22 with two, and 21 with three for the panel-based sample not missing information from the second-wave survey. We examine the robustness of the estimates with respect to this imputation procedure and to other sample selection criteria later.

[8]We use the NSFH rather than the TUS primarily because the NSFH has substantially more observations. Only 1,519 individuals of any age, race, or gender were interviewed for the first wave of the TUS.

Table 2. Time Spent by Men on Home Production per Week: First Wave of Panel Data. Mean (Percent of Own Total Home Production Time)

Variable	*Married*	*Divorced, Separated, Widowed*	*Never Married*	*Significant Differences*[a]
		Panel A		
TOTAL	**17.95** (100.0%)	**20.86** (100.0%)	**18.31** (100.0%)	a —
Meals	2.44 (13.0%)	4.25 (19.5%)	3.66 (19.5%)	a,b (a,b)
Dishes	2.07 (11.1%)	2.66 (12.1%)	2.28 (11.7%)	a —
Cleaning	1.82 (9.1%)	3.01 (13.7%)	2.32 (12.4%)	a,b (a,b)
Shopping	1.39 (8.1%)	1.67 (8.6%)	1.51 (8.9%)	a —
Laundry	0.77 (3.8%)	1.74 (8.8%)	1.49 (8.9%)	a,b (a,b)
Outdoor & Maintenance	5.49 (31.1%)	3.42 (16.4%)	2.86 (15.9%)	a,b (a,b)
Auto	1.68 (10.6%)	1.43 (7.3%)	2.04 (9.9%)	— (a)
Bills	1.28 (7.8%)	1.62 (9.3%)	1.63 (10.5%)	— (b)
Driving Others	0.99 (5.4%)	1.06 (4.1%)	0.52 (2.4%)	b,c (b)
		Panel B[b]		
Traditionally Female Tasks	8.50 (45.1%)	13.33 (62.8%)	11.27 (61.3%)	a,b (a,b)
Traditionally Male Tasks	7.18 (41.7%)	4.85 (23.7%)	4.89 (25.8%)	a,b (a,b)
Neutral Tasks	2.27 (13.2%)	2.68 (13.5%)	2.15 (12.9%)	— —

[a]Statistically significant differences in means (or percent of total) at $p < 0.01$, where a = married vs. divorced, separated, widowed; b = married vs. never married; and c = divorced, separated, widowed vs. never married.

[b]Traditionally female tasks are meals, dishes, cleaning, shopping, and laundry. Traditionally male tasks are outdoor & maintenance and auto. Neutral tasks are bills and driving others.

the NSFH. Fortunately, many of the activities associated with children—like additional cleaning, cooking, and driving—appear to be incorporated in the housework measures reported here. Married men with children spent over one hour more per week on housework than married men without children, while married women with children spent over eight hours more per week on housework than married women without children. The types of child care activities least likely to be included are activities such as playing and reading, which may be more accurately denoted leisure activities. Nevertheless, to address concerns regarding the measurement of child care activities, we estimate wage equations that explicitly control for the number of children in the household in our sensitivity analysis.

Household Specialization

Table 2 provides descriptive statistics by marital status for the housework-related measures. The measures reported are based on the sample of 1,110 observations with valid housework data from the first wave of

our panel data set. Descriptive statistics for the second wave are similar and are available upon request. The first row indicates that on average married men spent about the same amount of time on housework as did never-married men. Both spent, on average, about 18 hours a week on household activities, while previously married men spent a statistically significant 3 hours more per week. Further calculations (not reported in the table) show that, on average, men with employed wives spent more time on housework than men whose wives were not employed, but the difference is only marginally significant (18.3 versus 16.6 hours: p-value = 0.09).

The statistics in Table 2 indicating that married men spent about the same amount of time on home production as did never-married men should not, however, be interpreted to mean that *holding all else constant* marital status and housework time are uncorrelated. These unconditional means do not control for differences in individual circumstances or for differences in individual preferences that may influence time spent on housework. Some changes in lifestyle occur as individuals age and as their assets increase, even in the absence of marital status changes. Alternatively, it could be that men who spend more time on housework are more likely to marry, but marriage reduces their housework time. Thus, even in the absence of specialization attributable to marriage, we may expect differences in time spent on household activities according to individual and household characteristics.

To isolate the effect of marital status on housework time from individual preferences and unobserved life-cycle factors that are linear with age, we estimate fixed effects, reduced form housework equations of the form

$$(1) \qquad \text{HOUSEWORK}_{it} = Z_{it}\delta + \text{MARITSTAT}_{it}\gamma + C_i + \mu_{it},$$

where HOUSEWORK_{it} is time spent on housework by individual i at time t, Z is a vector of observable characteristics expected to affect time spent on housework, and MARITSTAT is a vector of indicator variables for marital status. The term C represents an unobserved individual fixed effect, such as taste for home production or for domestic comforts. First-difference estimation of this equation nets out this unobserved individual fixed effect.

Estimates of housework equations are presented in Table 3. The dependent variable in column (1) is total housework time per week. The observable time-varying factors for which we control include marital status, number of children under age 6, number of children age 6–12, number of children age 13–18, car ownership, and an indicator of wife's employment status. Fully 1,066 men provide complete information on these variables for both survey dates.

Most of the explanatory power is provided by the fixed effects, C_i, which jointly explain 70% of the variation in reported housework time. Each additional child under age 6 adds about 80 minutes per week to housework time. Older children have a much smaller impact that is not statistically significant. Buying a car increases housework time by over 2.5 hours per week, but the effect is significant only at the 10% level.

Although the raw means reported in Table 2 reveal little difference in housework time between married and never-married men, the estimates in Table 3 provide evidence that, conditional on other factors influencing housework time and net of unobserved individual fixed effects, marriage can allow men to specialize by decreasing their time on housework. In particular, conditional on other factors including a fixed individual-specific effect, those men who married for the first time and whose wives were not employed reported a three-hour reduction in housework as compared to those whose marital status was unchanged.[9] Wife's employment status

[9]By reducing time on housework, marriage may allow men to increase their time on market work. If so, the marital wage differential could be explained by increased investment in job-related human capital. In fact, the correlation between changes in housework time and changes in market hours is a statistically insignificant –0.025 for the sample that married between waves.

Table 3. Fixed Effects Estimates of Housework Hours per Week.[a] Coefficient (Standard Error)

Variable	*Total Housework*	*Traditionally Female*[b]	*Traditionally Male*[c]	*Neutral*[d]
Married	-2.960**	-1.782	-1.107	-0.071
	(1.413)	(0.972)	(0.783)	(0.382)
Divorced, Separated, Widowed	1.705	3.391***	-2.369***	0.683
	(1.477)	(1.016)	(0.818)	(0.400)
Children under Age 6	1.377***	0.409	0.687***	0.280***
	(0.460)	(0.316)	(0.255)	(0.124)
Children Age 6–12	0.464	-0.089	0.012	0.541***
	(0.449)	(0.309)	(0.249)	(0.121)
Children Age 13–18	0.503	-0.440	0.304	0.639***
	(0.466)	(0.321)	(0.258)	(0.126)
Own Car	2.696	1.765	0.768	0.162
	(1.566)	(1.078)	(0.868)	(0.424)
Married, Wife Employed	1.309	0.909	0.304	0.096
	(0.817)	(0.562)	(0.453)	(0.221)
Adjusted R^2	.41	.45	.39	.26
R^2 within	.02	.05	.02	.04

[a]Fixed effects equations estimated by first differences. Estimates are based on the sample of 1,066 observations who reported valid data for each variable in the analysis.

[b]Traditionally female tasks are meals, dishes, cleaning, shopping, and laundry.

[c]Traditionally male tasks are outdoor & maintenance and auto repair.

[d]Neutral tasks are bills and driving others.

**Statistically significant at the .05 level; ***at the .01 level (two-tailed tests).

mitigates this marital effect, with married men whose wives became employed reporting more time spent on housework, but this spousal employment effect is only marginally significant (p-value = 0.11). Any time saved during marriage, moreover, ended when that marriage ended. Indeed, men whose marriages ended reported spending more time on housework than did never-married men with similar household characteristics, though the difference is not statistically significant.

The results of pooled cross-sectional estimates are fairly similar. Men married to women who were not employed reported spending 2.9 fewer hours per week on housework than did never-married men (versus 3.0 in fixed effects). Men married to employed women also reported spending fewer hours per week than did never-married men, but the differential is smaller (1.0 versus 1.7 hours in fixed effects), and previously married men reported spending significantly more time on housework than did never-married men (3.4 hours versus 1.7 in fixed effects).

Clearly marital status influences time spent on housework, controlling for various personal characteristics. However, if the link between marital status and wages depends on the total amount of time, the relatively small housework time difference between married and never-married men, regardless of wives' employment status, suggests that specialization does not explain the substantial marriage premium in hourly wages observed throughout the literature. Specialization may, however, explain the premium if different types of housework have different effects on wages. Some types of household activity may be more easily postponed or scheduled so as not to interfere with market activities, and some may almost complement market-related activities. If married men are generally less involved in the types that reduce wages, the observed marriage premium may simply reflect this reduced involvement and could

disappear once adequate controls for home production are added to the wage equation.

Indeed, the allocation of total time among household chores is quite different by marital status. The descriptive statistics in Table 2 indicate that married men spent a significantly smaller share of their time on meal preparation, cleaning, and laundry, and a significantly larger share of their household time on outdoor work/home maintenance, than did men who were not married. It is interesting to note by comparison that previously married men spent more time on housework than did never-married men, but allocated it similarly.

This differential allocation of housework time can be seen even more clearly when home production activities are grouped into three activities using a classification method employed by sociologists (for example, South and Spitze 1994). One purpose of the classification scheme is to distinguish among tasks that, within households, are disproportionately performed by either the husband or the wife, or are performed in nearly equal proportions by husbands and wives. This classification scheme simultaneously distinguishes between tasks that must be done fairly frequently, often on a daily basis, and those that can be attended to less often and are easier to postpone. Thus, meal preparation, dishes, cleaning, shopping, and laundry are categorized as "traditionally female" type tasks, auto repair and outdoor/maintenance as "traditionally male" type tasks, and bill paying and driving others as "neutral" tasks. Meal preparation and dishes must typically be tackled daily, while outdoor work and home maintenance can vary widely with the season and with household or individual preferences. Outdoor work and home maintenance can often be delayed, avoided, or contracted out.

Panel B of Table 2 shows the breakdown of housework time using this classification scheme. Married men spent about 45% of their total housework time on traditionally female tasks, compared to 63% for previously married men and 61% for never-married men. Calculations using our data (not reported in the table) show that within married households, the gender division of tasks was quite pronounced. By the husbands' own estimates, their wives' share of the total time spent on traditionally female tasks was 76%. In contrast, the wives' share of traditionally male tasks within the household was 18%, while the neutral tasks were shared more equally, with wives performing 56% of the household total. If it is primarily the daily tasks categorized here as traditionally female that negatively influence productivity and wages, then married men may earn a wage premium attributable to specialization as they relinquish tasks in this category.

Estimates examining whether these different categories of housework were affected differently by marital status are presented in columns (2)–(4) of Table 3. The dependent variables in columns (2)–(4), respectively, are time spent on traditionally female tasks, traditionally male tasks, and neutral tasks. Marriage had its greatest effect on traditionally female tasks, reducing the time men spent per week on these tasks by 1.8 hours (p-value of 0.067). Time spent on traditionally male tasks was also reduced, though the impact is not statistically significant, while time spent on neutral tasks was essentially unchanged. Again the wife's employment status acted to mitigate changes in housework time. Finally, while men whose marriages had ended reported spending about the same amount of time on housework as never-married men, the allocation is different. Consistent with the unconditional sample means in Panel B of Table 2, men whose marriages ended between interviews spent significantly more time on traditionally female tasks and less time on traditionally male tasks than did never-married men.

We also estimated housework equations incorporating a measure of the hours worked by the wife rather than simply an indicator of her employment status. The total time spent on housework was significantly affected by wife's market hours, primarily via its impact on traditionally female activities. A husband whose wife worked 40 hours per week, for example, on average

spent a statistically significant additional 1.8 hours per week on housework, 1.6 of which were devoted to traditionally female housework. However, since wife's market hours were missing substantially more often than wife's employment status, we report in the text the results with the dummy variable for employment status. Overall, these findings indicate that the employment status of the wife does provide evidence of specialization as assumed in earlier studies, though a more direct measure of housework would provide a richer measure.

Household Specialization and the Marriage Wage Premium

To estimate the degree to which the marriage wage premium is attributable to household specialization, we begin by estimating the marriage wage premium itself using a wage equation of the form

$$(2) \quad \ln W_{it} = X_{it}\beta + \text{MARITSTAT}_{it}\gamma + A_i + \varepsilon_{it},$$

where $\ln W_{it}$ is the logarithm of the real hourly wage of individual i at time t; X is a vector of observable individual, human capital, and job characteristics expected to affect the wage; MARITSTAT is a vector of marital characteristics including indicator variables for marital status and a quadratic in years married; and A represents an unobserved individual fixed and time-invariant effect, such as market ability. If men with more ability are more likely to marry, omitting A imparts an upward bias to the estimate of the return to marriage. If this unobserved characteristic is genuinely time-invariant, fixed effects estimation eliminates the unobserved fixed effect and provides an unbiased estimate of the return to marital status. A comparison of fixed effects to cross-sectional estimates provides information on the importance of selection.

We augment equation (2) by including measures of time spent on home production to examine the impact of specialization on the marriage premium:

$$(3) \quad \ln W_{it} = X_{it}\beta + \text{MARITSTAT}_{it}\gamma + \text{HOUSEWORK}_{it}\delta + A_i + \varepsilon_{it}.$$

HOUSEWORK is a vector of home production or specialization measures, measured as either total time spent on housework or the time spent on various types of housework. This vector also includes two dummy variables to identify those 263 respondents whose housework measures have been replaced by zeros because of unreliable values in either of the two waves. This approach controls for at least some forms of sample selection bias. If it is specialization that explains the marital wage differential, then including HOUSEWORK_{it} should drive γ to zero.

Table 4 presents the key coefficients for wage equations (2) and (3). (Complete results are available upon request.) In each equation, the vector X_{it} includes controls for education, quadratics in work experience and in tenure, and indicators for residence in the South, residence in an SMSA, a job-related disability, eleven 1-digit industries, and seven 1-digit occupations. In every case, the results indicate that wages increase with education, and with experience and tenure at a decreasing rate.

The marriage premium is substantial and statistically significant. The coefficients on married and previously married are 0.090 and 0.104, indicating wage advantages of 9.4% and 11.0%, respectively, relative to men who had never been married. A quadratic in years married is included in order to permit wages to grow more rapidly during marriage, as would be the case if men invest more in job-related human capital while married. Neither the coefficient on years married nor the coefficient on its square is individually significant, although a test of their joint significance yields a p-value of 0.10.

Since tenure, industry, and occupation may be affected by marital status, we also estimate fixed effects models excluding these variables. The results yield slightly smaller coefficient estimates for both marital status indicators. This suggests that the marital status effect is robust with respect to any correlation between marital status and work characteristics. (Results available upon request.)

These fixed effects estimates of the mari-

Table 4. Specialization and Fixed Effects Estimates of the Marriage Wage Premium.[a] Coefficient (Standard Error)

Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
Married	0.090***	0.087**	0.088**	0.078**	0.074**
	(0.036)	(0.036)	(0.036)	(0.037)	(0.037)
Divorced, Separated, Widowed	0.104**	0.104**	0.104**	0.107**	0.107**
	(0.043)	(0.043)	(0.043)	(0.044)	(0.043)
Years Married	0.003	0.003	0.003	0.0004	0.0001
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Years Married Squared/100	0.017	0.017	0.016	0.026	0.026
	(0.017)	(0.017)	(0.017)	(0.018)	(0.018)
Total Housework/100		−0.151			−0.166
		(0.086)			(0.086)
Traditionally Female Housework/100			−0.098		
			(0.127)		
Traditionally Male Housework/100			−0.397		
			(0.306)		
Neutral Housework/100			−0.143		
			(0.159)		
Children under Age 6				0.033***	0.034***
				(0.012)	(0.013)
Children Age 6–12				0.017	0.017
				(0.013)	(0.013)
Children Age 13–18				0.009	0.009
				(0.013)	(0.013)
Adjusted R^2	.76	.77	.77	.77	.77
R^2 within	.15	.15	.15	.15	.15

[a]Fixed effects estimates obtained by first differencing. Dependent variable is the log of hourly wage. Additional variables included in each equation are a constant, education, experience, experience squared, tenure, tenure squared, indicators for missing housework measures in each wave as well as for SMSA, South, disability, eleven 1-digit industries, and seven 1-digit occupations.

**Statistically significant at the .05 level; ***at the .01 level (two-tailed tests).

tal wage differential correct for some but not all marriage selection concerns. Fixed effects estimation should control for unobserved time-invariant individual-specific characteristics that influence the probability of marriage. Cross-sectional estimates of the specification in column (1) indicate that married men earned about 11% more than did never-married men. The smaller impact of marriage in the fixed effects estimates (9.4% versus 11.0%) provides evidence that selection matters, but it explains less than half of the marriage premium.

Turning to the estimates including housework time in columns (2) and (3), we find in column (2) that housework has a negative effect on wages that is statistically significant at the 7% level, with ten additional hours of housework time reducing wages by about 1.7%.[10] However, inclusion of this measure has essentially no impact on the magnitude of the marriage premium. The coefficient on the married indicator variable drops slightly from 0.090 to 0.087. Breaking down total housework into three categories (traditionally female, traditionally male, neutral) in column (3) yields the

[10]These results are corroborated by Hersch and Stratton (1997), who found that housework time has a statistically significant negative effect on wages, especially for married women, and the inclusion of housework time in the wage equation increases the explained component of the gender wage gap by about 30%.

same marital wage differential. None of these categories individually has a statistically significant effect. Further analysis (not reported here) including all nine types of housework indicates that much of the negative effect of total household tasks on men's wages appears to be driven by time spent on meal preparation and, to a lesser extent, on outdoor/maintenance activities and driving other household members, but once again, the marriage premium declines by less than 5%.[11] Sensitivity tests indicate that these results are robust with respect to a variety of alternative specifications of the marriage vector.[12]

In order to address concerns about measurement of child care activities, we re-estimate the wage equations reported in columns (1) and (2), adding variables indicating the total number of children in each of the three age groups. These results are reported in columns (4) and (5) of Table 3. The marital wage differential shown in column (4) is clearly smaller than that observed in column (1), the coefficient having fallen from 0.090 to 0.078. Children of all ages tend to increase wages, but the effect is statistically significant only for children under the age of six. However, controls for housework time continue to have no impact on the marital wage differential, as indicated by a comparison of columns (4) and (5). The results are similar when we replace total housework with the three types of housework or with the nine types of housework.

The marital wage differential observed in this study differs somewhat from that observed in other studies. Korenman and Neumark (1991), for example, found that dummy variables for marital status had significant coefficients in a fixed effects wage specification that did not control for marital duration, but that these variables had no statistically significant effect on wages once controls for marital duration were included. Gray (1997) reported no statistically significant marital wage effect at all in a fixed effects model using data similar to Korenman and Neumark's but from a later time period.[13] All of the previously cited works on the marital wage differential, however, relied on samples in which the oldest respondent was only 36 years old. The average age of our sample is 35 in wave 1. Both Korenman and Neumark (1991) and Gray (1997) controlled for a quadratic in experience but not for tenure, possibly because of multicollinearity problems within their young samples. In our sample, experience and years married are highly correlated. Excluding experience increases the statistical significance of the marriage duration coefficients but does not change the basic results.

For comparison to earlier studies, we estimated the wage equations restricting the sample to those under the age of 35 in the first wave (results available upon request). This age restriction results in substantial multicollinearity between experience, tenure, and years married. Given our substantially smaller sample size in this age group, multicollinearity problems make the results difficult to analyze when we include even two of these three variables. Excluding both years married and tenure yields a coefficient of 0.03 for married men and 0.09 for previously married men. Korenman

[11]The dummy variables for missing housework information are discussed later, in the section devoted to missing housework data.

[12]Among the specifications tested were one without any controls for time married; one with controls for time married and time separated, divorced, or widowed; and one in which marital status was interacted with time married. In the latter specification, wages appeared to rise less rapidly during marriage for those whose marriages had since ended, but the differential was not statistically significant at even the 10% level. Also estimated with robust findings were fixed effects estimates excluding tenure, industry, and occupation and pooled cross-section models. These results are available upon request.

[13]Korenman and Neumark used data from the National Longitudinal Survey, Young Men's Cohort (NLSYM) for the years 1976, 1978, and 1980. Gray used NLSYM data for the same years and National Longitudinal Survey of Youth (NLSY) data for the years 1989, 1991, and 1993.

and Neumark reported estimates of 0.06 and 0.04 using the same specification. These results are not dissimilar, particularly given the high standard errors associated with each. Most important, inclusion of time spent on housework in a wage equation for this young sample does not affect the estimated marriage premium either.

We examined three additional hypotheses regarding the effect of housework on wages. First, if time spent on housework today influences the amount of on-the-job investment today and hence wages tomorrow, then controls for the complete history of housework time may influence wages or wage growth. As a partial control, we include the only available earlier measure, housework at the time of the first survey, in the wage difference estimates. Introduction of this variable indicates that those spending more time on traditionally female tasks in the first wave had slower wage growth between waves. However, inclusion of the level of housework time in the first wave in any form leaves unchanged the magnitude of the marital wage differential. Second, the effect of housework time on wage growth may differ by marital status. Indeed, there is some indication that the effect of housework was larger for those previously married, but the terms interacting housework time with marital status are not statistically significant either individually or jointly. Finally, the wage-enhancing effects of specialization may be due to the housework time spent by the wife rather than that spent by the husband. Wage equations including total or disaggregated measures of wives' housework time indicate that wives' housework time was not a statistically significant determinant of men's wages.

In summary, the marital wage differential is remarkably persistent. It remains even after we control for selection by allowing for fixed individual-specific effects. Although there are substantial differences by marital status in conditional hours of housework, the marriage premium is unaffected by the inclusion of housework time in the wage equation. If marriage makes men more productive, it does not appear to do so because of specialization within the household.

Missing Housework Data

As discussed earlier, data on housework time are missing far more often from the first survey than from the second, and our results could be attributed to the peculiarities of our housework data. We employed an imputation procedure to assign zeros to missing housework time in the first survey for those individuals who provided valid information on at least six of the nine types of housework, and included dummy variables to identify the remaining observations with missing or invalid values in the wage analysis. The coefficient on the dummy variable indicating missing housework data from wave two is consistently statistically insignificant, suggesting that missing housework data from this wave are uncorrelated with wages. The coefficient on the dummy variable indicating missing housework data from the first wave, however, is consistently positive, of the same magnitude in all wage equations, and statistically significant at about the 7% level. Since our first difference estimates subtract period 1 values from period 2 values, the positive coefficient on the dummy variable implies that those failing to report housework in wave one had significantly lower wage growth between interviews. To check the sensitivity of the wage equation estimates to the imputation procedure, we re-estimated the wage equations allowing between zero and five missing values to be imputed. The results were not sensitive to this assignment. We also restricted the analysis to only those observations that did not require any imputation of housework time and obtained similar results.

Endogeneity and Measurement Error

While sample selection and data definition problems regarding housework time do not appear to influence the basic results reported here, there are two other possible sources of bias relating to housework. First, housework time may be determined endogenously with the wage. Endogeneity driven

by individual-specific time-invariant characteristics is remedied by the fixed effects estimation technique employed above. However, fixed effects estimation will not eliminate other sources of endogeneity. For instance, if men with faster wage growth do less housework, the fixed effects estimates will themselves be biased.

Second, housework time may be measured with error. One possible form measurement error could take is through respondents' systematic over- or understatement of their housework time by a constant amount in each wave. This source of measurement error would be eliminated in the fixed effect estimates. But random measurement error will bias the estimated coefficient on housework toward zero, and this bias is likely to be exacerbated in panel data estimates.[14]

In the presence of either endogeneity bias or measurement error, the coefficient on housework, and on any other variable correlated with housework (such as marital status), will be biased. A possible solution to both problems is instrumental variables estimation. The data set provides a wide array of plausible instruments for housework time. Since many of these potential instruments are constant over the panel and since the explanatory power of the time-varying covariates in the panel data housework equations is low, we use only the first wave cross-section reporting valid data on housework so that we can include a broader set of instruments. In particular, we use as instruments car ownership; eight measures of the number of other adults in the household (distinguishing between male and female children age 19 and older, male and female parents, male and female other relatives, and male and female nonrelatives); the respondents' parents' education; three variables indicating whether the respondent's mother worked outside the home when the respondent was under age 6, age 6–11, or age 12–17; and five missing value indicators for these latter five variables. (Results available upon request.)

A Hausman test fails to reject the hypothesis that the OLS estimates are consistent, indicating that neither endogeneity nor measurement error is a substantial problem. Coefficient estimates for the housework measure are statistically insignificant in both OLS and IV estimates. A test of the power of the instruments not also in the wage equation indicates that these instruments are jointly significant. In addition, a Lagrange Multiplier test confirms that the instruments do not themselves belong in the wage equation; the exclusion restrictions are valid.[15] Varying the instrument set to test the exogeneity of the instruments with respect to wages yields similar results: the instruments appear to be exogenous.

Although technically these instruments are valid, the overall fit of the housework equation is weak, with an adjusted R^2 of 0.03. Our failure to find evidence of endogeneity or of measurement error may be due to our weak instrument set. At least for the case of measurement error, we can test this hypothesis on a subset of observations by introducing what is certainly a powerful instrument: the wife's report of her husband's housework time. This instrument is necessarily available only for

[14]The primary concern in other studies examining the marriage premium has been the endogeneity of marriage. As discussed earlier, to the extent that men who marry are more productive, the marital wage effect will be biased upward in cross-sectional estimates. Fixed effects estimation removes this type of endogeneity bias, but not, for example, selection based on wage growth. As mentioned earlier, neither Korenman and Neumark (1991) nor Gray (1997) found evidence of such selection.

[15]The value of the Hausman test statistic is 1.72. This statistic is distributed chi-squared with 30 degrees of freedom and soundly fails to reject the null hypothesis that housework is exogenous. The test statistic for the power of the instruments is 1.91. It is distributed F with 19 and 1133 degrees of freedom. The Lagrange Multiplier test statistic, distributed chi-squared with 19 degrees of freedom, is 16.2.

the subsample of married men whose wives provided such a report.

Within this sample of 465 married men, the inclusion of wife's report increases the adjusted R^2 of the housework equation from 0.011 to 0.086; the wife's report is clearly highly correlated with the husband's report, making it a good instrument. A Lagrange Multiplier test confirms that the exclusion restrictions are valid; the wife's report does not itself belong in the wage equation. For this sample of married men, the coefficient on housework in the IV specification is –0.27 (p-value = 0.56), while the corresponding coefficient in OLS estimates is –0.07 (p-value = 0.68). The difference in magnitude provides some evidence of measurement error, but neither estimate provides statistical evidence strong enough to warrant much attention. The IV and OLS coefficients in the wage equation are not statistically significantly different: a Hausman test fails to reject the hypothesis that housework is exogenous with respect to wages. As this sample has been restricted to married men, no estimate of the marital wage differential can be obtained using these data, but these findings suggest that measurement error is not a serious problem and that our earlier failure to reject exogeneity is not simply due to weak instruments.

Conclusion

From the perspective of economists, an important benefit of marriage is that it allows spouses to specialize in either market or home production, creating a bigger "pie" to be shared by all members of the household. Historically, husbands have been more likely to specialize in the market. The commonly observed marriage premium for men is frequently attributed either to selection of more productive men into marriage or to enhanced productivity resulting from the specialization possible within a joint household. In this paper, we examine the specialization hypothesis. We present evidence on the amount of time spent in home production by men according to their marital status. We then examine the impact that controlling for home production activities and selection has on the estimated marriage premium.

Our results indicate that the marriage premium is not primarily due to the selection of more productive men into marriage. In the sample we examine, marriage does seem to have made men more productive in the market. However, this enhanced productivity does not seem to have resulted from household specialization. There is little difference by marital status in the total amount of time men spent on home production, although there are differences in the type of home production activities. Not surprisingly, married men spent less time than unmarried men on tasks such as cooking and cleaning. With little difference in the total time spent on housework, the only way specialization can explain the premium is if different types of housework have different effects on wages. While we find evidence that own time spent on home production negatively affected wages, controlling for housework time does not have a substantial impact on the measured marriage premium.

If specialization does not make married men more productive, what could explain the marriage premium? Married men may get preferential treatment from employers, such as more training or promotions. Or men may become better workers because of the stability induced by marriage. These explanations cannot be modeled using an individual fixed effect, since they suggest an actual change in behavior resulting from marriage or the decision to marry. If neither selection nor specialization explains the differential, then more attention should be paid to alternative explanations such as these.

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