THE MALE MARITAL WAGE PREMIUM: SORTING VS. DIFFERENTIAL PAY

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The authors examine whether male marital and parenthood premia arise due to differential pay by employers or from differential sorting of employees on occupations and establishments. They investigate these premia using matched employee-employer data from the period 1979–96 in Norway, a country with increased pressures on men to be more active in the family sphere and in which public policy has aimed at remaking the family institution. We find that the effect of marriage, and to a lesser extent of children, occurs mostly through sorting on occupations and occupation-establishment units. The role of differential pay from employers is marginal in explaining the marital and parenthood premia. Results assessing within-individual changes in wages suggest that about 80% of the marital premium is due to selection. The men who eventually marry and/or have children sort into the higher-paying occupations and occupation-establishment units even prior to marriage and parenthood.

There are strongly divergent career effects of family, beneficial to men and detrimental to women. Men earn a substantial wage bonus from marriage, and a smaller premium from fatherhood. Women, however, typically experience small wage differentials from marriage, but substantial penalties from motherhood. The male marital premium is well documented, though not yet fully understood (Rodgers and Stratton 2009). Conversely, the female parenthood penalty is less documented, but probably better understood (Budig and England 2001).

Why are these facts significant? They command our attention because the processes that unfold in the family are among the core obstacles for achieving gender equality in the workplace today. In this paper, we investigate the better documented but less well understood marital and parenthood premia for men. Of central interest is the potential

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role of employer discrimination in generating the premia. Two questions then arise. When employing single and married men in same occupation and same workplace, do employers pay more for the married than for the single men? Or, alternatively, do married men work for different and higher-paying employers without there being a pay differential once employed in the same occupation and workplace?

We attempt to shed light on these conundra using matched employer-employee data from Norway during the period 1979–96. Given the potential salience of employers in this context, such data are important since they allow us to ascertain whether single versus married employees are paid differently in the same occupation and firm, the level at which differential treatment in wage-setting occurs. Our analysis thus provides a crucial and entirely novel empirical angle by assessing whether marriage and fatherhood premia arise because employers pay men differently according to their marital and parental status, or whether these premia are due to sorting on occupations and establishments. In addition, we report the role of these processes for wage growth. Apart from the novelty of the matched employer-employee data, our data are also noteworthy in that they include annual observations of employees and establishments over an 18-year period and practically error-free recording of wage rates and occupations. The one significant weakness of the data set is that it does not provide information on cohabitation status. Accordingly, we assess the biases this may introduce.

Although the empirical and attendant conceptual questions raised on the subject of wage premia and wage inequality in the workplace are relevant across the entire spectrum of rich countries, the national setting itself is of particular interest. Scandinavia, along with the United States, is at the forefront of gender equality policies, and both nations have progressive values toward gender equality. Scandinavia leads in the area of family policies (Gornick and Meyers 2003: 15); the United States leads in affirmative action and workplace regulation. Most Scandinavian family policies are gender neutral. Their first-order impact is, however, primarily on mothers, making it easier for them to combine family and careers. The second-order impact is on the adjustments fathers make. In passing Norwegian family legislation, one goal explicitly raised during parliamentary debates was to redefine familial norms (Leira 2002: 95). We therefore provide evidence on the marital premium in an economy in which public policy has done much to change the internal organization of the family by trying to create a more equal division of household labor between the sexes.

Selection, Treatment, and Discrimination

Researchers have posited three basic hypotheses—selection, treatment, and discrimination—to explain the premia (e.g., Chiodo and Owyang 2002). Below, we review these and then discuss our core errand, the role of differential pay within vs. sorting on establishments, occupations, and occupation-establishment units.

Selection

According to the *selection hypothesis,* the factors that cause married men to be productive and to receive high wages are the same factors that cause them to get married—conscientiousness, industriousness, and other traits valued both by prospective partners and prospective employers.[1] Marriage, as such, does nothing to increase their productivity. Men who marry and become fathers are more productive than men who do not, even before entrance into marriage and fatherhood. The observed relationship between marital status and productivity and wages is thus spurious, arising from insufficient measurement of the underlying causal factors, or, in technical terms, as a result of an omitted variable bias. The

[1] Mueller and Plug (2006) reported wage bonuses for a variety of personality traits that may or may not be related to productivity, such as when men are rewarded for being antagonistic.

causality is thus in the other direction: productivity causes marriage.

Several implications follow from this hypothesis. To the extent that productivity can be observed and hence rewarded by employers, there should be no differences at the individual level as a result of getting married. That is, as individuals move between marital states, there should be no wage changes. Productivity is high both before and after marriage. This also means that men who eventually marry earn a wage premium relative to men who remain single even prior to entrance into marriage.

Treatment

According to the *treatment* hypothesis, getting married and becoming a father induce men to change their behavior, for example, by paying more attention to work or by working harder, Marriage thus causes higher productivity. Researchers have put forth three mechanisms by which men could increase their productivity. The first mechanism points to the benefits of household specialization. With a wife to help run the household, the husband can expend more effort in the workplace (Becker 1985). A second mechanism points to the possibility of increased human capital accumulation during marriage. Men may spend more time developing their skills while married (Kenny 1983), and this will eventually result in higher wages. A third mechanism, similar to the first, proposes that marriage leads to higher work effort. This is not due to household specialization but rather due to a more well ordered life style or the need for more money when married, and especially when having children. This point was raised 100 years ago by Weber (1908[1924]: 174) and more recently by Hersch and Stratton (2000: 93).

All three mechanisms imply higher wages for married than for single men, but they have different implications for post-marriage outcomes: separation, divorce, or widowhood (e.g., Ribar 2004). The first mechanism implies that these advantages disappear in post-marital states, since the time savings from the household division of labor then disappear. The second mechanism implies that the premium stays with the employee for the remainder of his career, regardless of whether he remains married, since skills once acquired do not disappear. The third mechanism has no clear implications for post-marriage outcomes.

The first mechanism has an additional implication: the marital premium should decline over time as the household division of labor becomes more equal and thus increases the demands on married men's hours, leaving fewer hours for market work in the present than it had in the past.[2] The other two mechanisms have no implications for trends in the premium over time.

Discrimination

Common to both the selection and treatment hypotheses is the claim that married men in fact are more productive than the average single man. The two hypotheses differ, however, in their view on the causal direction. The selection hypothesis holds that being productive causes both high wages and marriage whereas the treatment hypothesis holds that getting married causes high productivity and wages. The *discrimination* hypothesis, in contrast, does not rest on the claim that married men are more productive than single men. Rather, it posits that employers consciously or unconsciously discriminate in favor married men, either as a reflection of societal norms, which stress the value of marriage, or as a result of statistical discrimination (or error discrimination), which suggests that married men correctly (or erroneously) are perceived on average to be better employees, but with no attempt made to assess which married and which single men are more productive (England

[2] In the United States, average household work for married women decreased from 34 to 19.5 hours per week between 1965 and 1995 whereas among married men it increased from 5 to 10.5 hours (Bianchi, Milkie, Sayer, and Robinson 2000, Appendix A). In Norway, the average daily household work for mothers dropped from 7.5 in 1971 to 5.5 hours in 2000 whereas for fathers they increased from 2.3 in 1971 to 3.4 in 2000, a narrowing of the gap between mothers and fathers from 5.2 to 2.1 hours per day, with fathers doing 23.5% of the work in 1971 and 38.2% in 2000 (Kitterød 2002: 17).

Table 1 . The Implications of the Three Hypotheses for the Wages of Men who Eventually Become Married: Relative to Themselves while Single, Relative to Men Who Stay Single, and Change in the Marital Premium Over Time

		Treatment			*Discrimination*	
	Selection	*Household Specialization*	*Human Capital Accumulation*	*More Work Effort*	*Animus*	*Statistical*
Single	High Wage	Low Wage	Low Wage	Low Wage	Low Wage	Low Wage
Married	High Wage	High Wage	High Wage	High Wage	High Wage	High Wage
Post-Married	High Wage	Low Wage	High Wage	Unclear	High Wage	High Wage
Change Over Time	Zero	Decline	Zero	Decline	Decline	Zero

Note: In the 24 cells, for the four rows, the empirical implications differ in five cells. In Row 1, only cell 1 gives a different implication. In Row 2, all the cells are equal. In Row 3, cell 2 gives a different implication and cell 4 gives no implication. In Row 4, cells 1, 3, and 6 give different implications from cells 2, 4, and 5.

1992: 56–60). Regarding the discrimination vs. treatment hypothesis, Hersch and Stratton (2000: 93) write that "married men may get preferential treatment from employers, such as more training and promotions. Or men may become better workers because of the stability induced by marriage."

In its pure form, the discrimination hypothesis suggests that the differential treatment arises due to societal norms that favor marriage, family, and stable relationships, historically related to norms around the male breadwinner model (Hill 1979: 592; Bartlett and Callahan 1984). It is an instance of animus (or taste) discrimination, comparable to when an employer is willing to pay more for certain demographic groups, such as hiring more and paying more for white than black employees, even the in absence of objective reasons for doing so (England 1992: 54–56).

In a less pure form, the discrimination hypothesis suggests that married men may in fact be on average more productive than single men—be it due to selection or treatment—but without each married man being more productive than each single man, net of other characteristics (Hill 1979: 592). When productivity is costly to observe and measure, employers may act as if the group average applies to each group member and will pay more for married than single men. In this case, the marital premium would be an instance of statistical discrimination, and would, if costs of measuring productivity are high, be economically rational behavior.

These two discrimination mechanisms have various implications. According to the animus mechanism, the marital premium should decline over time because marriage has become less important among younger people, average age at first marriage has gone up, and cohabitation and other family forms have gained broader acceptance (Chiodo and Owyang 2002). No such decline follows from the statistical discrimination mechanism.

In summary, we have three separate hypotheses—selection, treatment, and discrimination—the second and third of which have three and two separate mechanisms, respectively, each of which produce different outcomes at different levels. Table 1 summarizes the implications of these hypotheses.

The Role of Sorting versus Differential Pay

Regardless of the precise mechanisms producing the premia, it is instructive to ask, Where do these premia arise? Do they arise at the level of employers, when single and married men work in same occupation and establishment? Or do they arise in the sorting of employees on occupations and establishments, so that married men are hired and promoted into the higher paying establishments, occupations, and occupation-establishment units? If the premia arise due to sorting, does the sorting come from the employees' choosing establishments and occupations, or do they come from the employers' choosing to favor married over single men? The two mechanisms are clearly difficult to disentangle empirically.

A subtle implication arises, however, that allows us to gain some insight into the role of employee choices and productivity vs. employer discrimination. If the single and childless men who eventually get married and have children are more likely to be in the better-paying occupations and occupation-establishment units than the men who remain single and childless, then some of the sorting must occur due to choice or assessed higher productivity. The reason is simply that employers have no opportunity to discriminate among single men on the basis of their future marital status, and if sorting still occurs, it is unrelated to employer preferences for married rather than single men and is thus not caused by discrimination.

Summary of Research Evidence

There are three excellent summaries of the relevant empirical evidence (Ribar 2004; Bardasi and Taylor 2008; Rodgers and Stratton 2009). Since our empirical aims are different from what has been addressed by research to date, below we discuss only the central findings.

For assessing selection effects, one should ideally use panel data, so that men who marry are observed while both single and married. Several studies have done this. For the United States, estimates of the degree to which the marital wage premium (of up to 15%) is due to selection vary from 10 to 20% in research from the early 1990s, to more recent estimates of 40–6 0% and even 80–100% (see Rodgers and Stratton 2009). In Britain, the selection effect accounts for 75% of the marital premium (Bardasi and Taylor 2008), and in Denmark, it accounts for about 80% (Datta Gupta, Smith, and Stratton 2007).[3]

If the selection effect cannot account for the entire marital premium, the remainder is by default attributed to treatment. Some researchers further investigate the possible sources of such treatment effects. Given the difficulty of measuring productivity directly, a variety of indirect strategies are used. With respect to the household specialization mechanism, the results are mixed (see Rodgers and Stratton 2009) and the issue may ultimately be very difficult to settle. However, married and single men's household hours are similar (Hersch and Stratton 2000, 2002). This provides *prima facie* evidence against the household specialization mechanism. Household hours, as such, reduce wages but do not change the coefficient for the marital premium itself (Hersch and Stratton 2000). Blackburn and Korenman (1994) reported a decline in the male marital premium over time, consistent with a more equal distribution of household labor over time, but they did not find any separate effect of aggregate measures of household specialization.

As for the human capital acquisition mechanism, there are only a limited number of investigations. Recent evidence suggests that married men receive more on-the-job training, but the marital premium does not decrease when adjusting for training (Rodgers and Stratton 2009). There is also partial support for the human capital mechanism and against the household specialization thesis when there are premia even for post-marital states.

Regarding the third mechanism—that being married leads to higher work effort—without this being due to gains from household specialization, there are no studies yet that are directly relevant. To the extent that one finds limited evidence for the household specialization mechanism, but still a

[3] Three studies use data on identical twins to assess selection vs. treatment effects. One study found that within pairs of twins, the twin who marries receives a wage premium of 30% (Antonovics and Town 2004) whereas another study found a difference of less than 1% (Krashinsky 2004). A third study, using a large panel-data sample from Sweden, found that the premium disappears once individual-level fixed-effects are introduced (Isacsson 2007). This research thus yields no coherent results.

treatment effect, it seems plausible to infer that married men might work harder.

The discrimination hypothesis is clearly the most difficult to investigate. Ideally, it requires matched employer-employee, applicant pool, or employment audit data. Korenman and Neumark (1991) have provided perhaps the best evidence to date. Using company-level data, they found that married men receive higher performance ratings than single men, and that once performance evaluations are controlled for, the marital premium is sharply reduced. This finding could be interpreted as higher productivity for married men or as discrimination in the performance rating. Jacobsen and Rayack (1996) argued that since there is an even larger marital wage premium among self-employed than employed men, the evidence favors the claim that married men are more productive rather than the claim that employers discriminate in favor of them. Premia disappear or are vastly reduced once individual fixed-effects are included: the marital differentials are zero percent among self-employed while still being 5 percent among employees.[4]

A few studies have addressed Scandinavia—Denmark and Sweden—but none have yet addressed Norway. Datta Gupta, Smith, and Stratton (2007) reported a low male marital premium in Denmark, a gross premium of about 6% and a net premium of about 2% and even 1% in some of their fixed-effects analyses, with similar results in Datta Gupta and Smith (2002). They attributed the low premia to the more equal division of household labor in Scandinavia. For Sweden, Richardson (2002) showed a marital premium that declined from about 23% to 8% between 1968 and 1991, and a cohabitation premium that declined from 16% to 3.5% in same period. Isacsson (2007) reported large selection effects in the longitudinal component of a large-sample Swedish twin study.

[4] With respect to hiring, in their study of an applicant pool data from a Norwegian bank, Petersen and Togstad (2006) found that married men were more likely to receive job offers than single men.

Some studies (e.g., Hundley (2000)) have addressed the male premia to having children, and these are generally low. Recent research shows premia ranging from zero to 6%: of 2%, 5%, and 6% for 1, 2, or 3+ children from one data set but smaller and insignificant premia in another data set (Hundley 2000). Stratton (2002) reported a miniscule, positive, and insignificant coefficient for presence of children in a cross-sectional analysis, which turns negative at about 3% in a fixed-effects analysis.

For Scandinavia, Datta Gupta, Smith, and Stratton (2007) reported fixed-effects estimates from Denmark of having children 0–2, 3–9, and 10–17 years old of 0.3%, −1.0% and −1.2%. Datta Gupta and Smith (2002) found similarly small premia in both a cross-sectional and a fixed-effects analysis. The wage differential for having children is thus, on average, quite low in Denmark.[5]

No study has used matched employer-employee data to analyze these premia.[6] These data are required for ascertaining whether there is different pay for the same work for the same employer, that is, whether productivity differences or discrimination could have arisen at that level. Nor has any study addressed the role of sorting on occupations and occupation-establishment units, due to a lack of suitable data.

Setting and Data

National Setting

Norwegian family policies have been considerably more elaborate than in most other countries, though not at the level of Swedish policies. They include paid parental leave, with some portion reserved for fathers, so as to strengthen the bond between fathers and children, thereby creating entirely new norms for fatherhood (Leira 2002: 95). They also include cash benefits for families with children, and most important, they make

[5] For Britain, Bardasi and Taylor (2008) reported small effects of about 1.5% per child, but in fixed-effects analysis, this drops to zero and is insignificant.

[6] A partial exception is the organizational case study in Korenman and Neumark (1991).

high-quality publicly supported childcare available at a relatively low cost.

Parental leave has been in place for decades. In 1977, it was available for 18 weeks; in 1987, for 20 weeks; and in 1988, for 22 weeks—with 100% pay since 1978. Since 1977, fathers could share the leave except for the first 6 weeks, which were reserved for mothers. By 1993, parental leave was given for 52 weeks at 80% pay or for 42 weeks at 100% pay (up to a maximum amount), and 4 of those weeks were reserved for the father whereas 6 were reserved for the mother (Leira 2002: 89, 95). In 1996, 69% of fathers took paid parental leave; about 7% of all parental leave days were taken by fathers (Leira 2002: 86, 91).

Like parental leave, publicly funded childcare support has risen steadily since the early 1970s: 5% of preschoolers had access to publicly funded child care in 1973, 25% in 1983, and 32% in 1988. By 1995, 22% of 0–3 and 61% of 3–6-year-olds attended publicly supported childcare facilities in Norway (Leira 2002: 62). Though the policies on average affect women more than men by making it easier to combine family and career, their impact on men's behavior can also be substantial. With mothers more likely to be employed, and with cultural pressure on fathers to become more involved in household activities, the potential benefits to men from household specialization are likely lower than in other countries.

Norway is also noteworthy for its highly centralized system of wage determination, which is thought to compress wage inequalities and reduce returns to various individual or productivity-related characteristics. Centralization also increased from the late 1980s to the mid-1990s, a fact that may have limited the ability of local units to deviate from central agreements (Kahn 1998: 604–605), which could have induced a decline in premia for marital status and children. Not all workers in Norway, however, are covered by central agreements. Centralization foremost affects low-ranking occupations and blue-collar workers in the private industries, including banking, retail and wholesale trade, hotel and restaurants, and transportation. During most of the period of our data, wage determination of public-sector employees was also highly centralized. For those occupations and workers, the coverage of central wage agreements was, and still is, high, even for non-unionized workers (Sheuer 1997). White-collar workers in the private sector, and especially employees in high-ranking occupations, are, in contrast, less affected by the centralized wage setting. For example, among professional employees in the manufacturing sector, wages are determined at the firm level, in large part based on individual merits and contributions. Likewise, employees are remunerated based on individual merits within parts of the insurance industry.

Our focus is therefore on white-collar employees, for whom there is a significant component of merit and individually determined pay compared to blue-collar workers who are covered by national collective agreements. Among the latter, the wage gap between men and women working in same occupation and establishment is low (Petersen, Snartland, Becken, Olsen 1997), and there is little to analyze when it comes to unequal pay.[7] The employees we study are covered by wage-setting systems comparable to what is found among many white-collar employees elsewhere in Europe and in the United States.

Data

We use matched employee-employer data on entire populations of white-collar employees in central sectors of the Norwegian economy during the period 1979–96. These allow us to (1) compare employees working in the same occupation for the same employer, and to make those comparisons between single, married, previously married, and those with and without children; (2) assess the role of sorting; and (3) analyze wage growth between years. We can follow the establishments and their employees from year to year, and we have information on about

[7] For example, in 1990, in the manufacturing industries the within-job wage gap was 3 percent among blue-collar workers, but 6 percent among white-collar employees.

2.6 million person-years. We restricted the analysis to men 20–50 years old, yielding about 1.9 million person-years. On an annual basis, information is available and used on 101,734 to 122,123 men 20–50 years old, from 9,547 to 15,419 establishments, and from 41,254 to 54,339 occupation-establishment units (see the note to Table 2), with the exception of the three years—1979, 1980, and 1982—when there were fewer employees and establishments as a result of incomplete data collection. For each employee, we have information on sex, occupation, age, part- vs. full-time status, contractual hours worked, and monthly earnings from work on contracted hours, which excludes wages on overtime hours. Additionally, the data have been matched to register data from the Central Bureau of Statistics on educational attainment (length and type, 4-digit code), family or civil status (5 statuses), number and ages of children, and adoptions. This information provides annual educational, marital, and parental histories up to 2005, which is 9 years beyond the last year for which we have employment data.

The data were collected from individual-level records kept by the establishments and compiled by the Norwegian Central Bureau of Statistics. Norwegian employers are bound by law to collect and report the data (e.g., Central Bureau of Statistics 1991: 120–123). They are used in wage bargaining and economic planning and should be reliable compared to information from sample surveys with personal reports of pay rates, hours worked, and occupation or position. These data on white-collar employees cover all occupational groups with a few exceptions, the main one being CEOs.

The following five broad sectors of the Norwegian economy are included: (1) manufacturing, oil extraction, mining, quarrying, transportation, storage, communication, and various other industries; (2) business services; (3) retail and wholesale trade; (4) banking; and (5) insurance. The data cover white-collar employees, such as technical, professional, administrative, and managerial employees.

From the contractual monthly earnings and contractual hours worked, we computed the hourly wage, which refers to hourly wages paid on regular work hours, and does not mix pay on regular and over-time hours. Since one of our key goals is to assess whether there is differential pay by employers, we focus on the rate of pay and avoid mixing the different rates of pay on regular and overtime hours.

We distinguished five marital statuses: single, married, divorced, separated, and widowed. We coded three dummy variables for number of children aged 20 or younger: 1, 2, or 3 or more children. We experimented with a number of different codings for the children variables, such as number of children below age 6, between 6 and 15, and so forth. The alternative codings make no substantive difference for the conclusions arrived at in the analysis.

The occupational code is quite detailed, with 474, 500, and 558 occupations in 1981, 1989 and 1996, respectively. We use data on employees in all of these occupations.

Labor force experience is imputed as age minus 16 minus years of education beyond age 16. Initially, we controlled for 21 educational groups, based on length and type. Our final analysis uses a simplification with five educational groups, but with only small differences in results.

Table 2 provides descriptive statistics for our key variables, with annual averages reported separately for each of three periods: 1979–84, 1985–89, and 1990–96. Wages are 19–28% higher for married and previously married men compared to single and childless men. On average, employees are observed for 5 years (see the note to Table 2). About 20% of employees leave the sample every year.

Our data suffer from one significant weakness: we do not know which men are cohabiting. For the men who are recorded as single, some are truly single, others are cohabiting. Cohabitation is common in Norway and increased over the period 1980–2000, and especially among the younger cohorts (Noack 2001). In 1990, about one in seven of unmarried Norwegian men aged 20–66 were cohabiting. In our data, 29% percent are recorded as single in the period 1990–96, suggesting that the correct number

Table 2. Descriptive Statistics: Computed Separately by Years, but Averaged Across Years within Each of Three Periods (1979–1984, 1985–1989, and 1990–1996)

	1979–1984	*1985–1989*	*1990–1996*
Percent			
Single	21.6	27.8	29.4
Married	73.4	66.1	63.0
Divorced	3.0	4.0	5.2
Widowed	0.2	0.2	0.2
Separated	1.7	1.9	2.1
Ever married	91.6	87.8	83.9
No children	29.4	34.1	33.8
One child	21.1	22.1	23.5
Two children	34.9	31.7	30.1
Three+ children	14.6	12.1	12.7
Ever kids	88.9	87.6	85.8
Basic education	70.5	67.7	60.3
College/bachelor	7.2	8.3	11.4
Graduate	1.6	2.3	3.7
Professional	20.7	21.7	24.7
Wage relative to singles/childless			
Married	1.28	1.27	1.24
Divorced	1.26	1.24	1.19
Widowed	1.27	1.25	1.23
Separated	1.28	1.26	1.20
One child	1.13	1.14	1.10
Two children	1.23	1.21	1.17
Three+ children	1.24	1.23	1.20
Experience			
Mean	15.61	15.77	16.80
Std. Dev.	8.34	8.50	8.49
N person-years	509176	593723	802027
N individuals	175209	198401	213721
N occupations	545	540	629
N establishments	23053	24922	25353
N occ-est	113469	124092	133297

Notes: The statistics above have been computed separately for individual-years within each of three periods (1979–1984, 1985–1989, and 1990–1996). We computed the distributions (in percent) on marital status, parenthood status, educational attainment, and also means and standard deviations for experience. We also computed the average wage for each marital and parenthood status as proportion of average wage of single and childless employees. The last five lines of the table give for each of the three periods (1) the number of individual-years, (2) the number of distinct individuals, (3) the number of occupations, (4) the number of establishments, and (5) the number of occupation-establishment units. The total number of individual-years is 1,904,926; total number of occupation-years is 8,898; total number of establishment-years is 228,127; and the total number of occupation-establishment years is 827,273. Excluding the years 1979, 1980, and 1982—the three years when our data are not complete—the average, minimum, and maximum number of observations per year are for individuals (Mean = 114,583, Min = 101,734, Max = 122,123); occupations (Mean = 529, Min = 474, Max = 587); establishments (Mean = 13,766, Min = 9,547, Max = 15,419); and occupation-establishments (Mean = 49,500, Min = 41,254, Max = 54,339). Over the 18-year period 1979–1986, individuals are observed on average for five years.

of single persons is around 25%. Though we are not aware of any Norwegian studies investigating wage premia for cohabitors, there are such premia in Sweden (3%) and Denmark (2%) (Richardson 2002; Datta Gupta and Smith 2002).[8]

Some biases arise from this misclassification, as Cohen (2002) has documented, using U.S. data. If cohabitors enjoy wage premia similar to those of married men, the cross-sectional analysis will overestimate the wages of single men while still correctly estimating the wages of married men, thus underestimating the wage differential, that is, the marital premium. To the extent that cohabiting men are more like single men in their economic success, there is no problem. In the within-individual analysis, some men will be misclassified as single while cohabiting, and if there are treatment effects of leaving singlehood, a similar underestimation occurs, but none if the entire premium is due to selection. Cohabitation is often a prelude to marriage, and if the premium to cohabitation and marriage are similar, the underestimation can be substantial. Since the marital premium for men is substantial, about 10% in the cross-section (see below), both the absolute and relative magnitude of the bias may be sizeable. For example, if among men recorded as single one in three is cohabiting, and if they earn the same premium as married men, then we would estimate the marital premium to be 10% rather than the correct 15.8%, whereas with one in seven cohabiting, the correct premium would be 11.9%.

Methods

The data have a unique multilevel structure. One level arises from the across-time dimension and the other level, at a given time point, arises from the nesting of employees within occupations and establishments. Most individuals are observed at several points in time, and some even every year during the period 1979–96. This structure yields a standard panel data set-up (e.g., Hsiao 1985; Petersen 2004). Similarly, each establishment is observed at several points in time, as much as every year during the period 1979–96. In a given year, we can use fixed-effects procedures to account for the clustering of employees into establishments, occupations, and occupation-establishment units. Across years, we can exploit the panel-nature of the data, taking into account that some employees are observed at more than one point in time, also using fixed-effects procedures; additionally, we can account for the fact that some employees remain in the same establishment, occupation, or occupation-establishment unit.

For each of the two dependent variables, wage level and wage growth, we report results from a sequence of four regression equations. Each equation includes independent variables for education and imputed labor force experience, plus dummy variables for both marital status and the number of children below age 20. The first regression equation does not take into account where the employees work or their occupations, the second controls for the establishment (workplace), the third controls for the occupation, and the fourth controls for the occupation-establishment unit. The second, third, and fourth specifications are estimated using fixed-effects procedures. The four specifications are referred to as the *Population, Establishment, Occupation*, and *Occupation-Establishment* estimators.

Each of the coefficients for being married on wage levels is significantly different from zero usually at a high level, often with z- or t-statistics of 40–50 and significance levels of .000001 or better. No point is served in reporting these significance levels since the sizes of the z-statistics reflect the large number of observations each year, not superior model specification. For several of the other variables, the number of observations are often small (such as for widowed), or the coefficients are very small in absolute magnitude and are neither substantively nor statistically significant (such as for wage changes). We indicate in notes to the tables the variables for which and the number of annual coefficients that fail to reach statistical significance at the .05 or .10 level.

[8] In our data, the percentages of the men who were married for the first time by ages 20, 25, 30, 35, 40, and 45 were 0.2, 8.5, 30.6, 49.1, 60.9, and 67.7, respectively.

The estimated equations and technical details are given in the Appendix. Below, we provide a non-technical explanation of our methods.

Methods for Analyzing Total Effects on Wage Levels

The baseline analysis reports how wages depend on marital status and children, controlling for education and imputed labor force experience, at each of the four levels: population, establishment, occupation, and occupation-establishment. From the multilevel structure of the data we can assess how the employee outcomes within establishments and occupations differ from those occurring across establishments and occupations. The estimates from the occupation-establishment analysis will address whether the marital and parenthood premia in wages are present when the same work is done for the same employer.

The equations are estimated separately for each of the 18 years in the data, which allows us to assess the possible changes over time implied by two of the hypotheses. To simplify the presentation, we report the averages of the coefficients for three time periods: 1979–84, 1985–89, and 1990–96.

The main dependent variable is the natural logarithm of the hourly wage. When small (e.g., less than .10 in absolute value), a coefficient can be interpreted as giving the relative change in the unlogged dependent variable from a one-unit increase in the independent variable, holding the other variables constant. We implicitly interpret this as the relative change in the mean of the unlogged wages, but correctly interpreted, it gives the absolute change in the mean of the logarithms of wages or the relative change in the geometric mean of unlogged wages.

Accounting for Selection Effects

The analyses outlined above do not account for possible selection processes. Men who are married and who have children may differ from those who do not in ways relevant for wages. The next set of analyses thus addresses this concern.

In the first analysis, we selected only employees who in a given year are single and childless. The variables for current marital and parenthood status are then excluded. However, we introduce two new dummy variables, one for whether the employee at some time in the future got married and another for whether the employee eventually had children, called "Ever married" and "Ever children," each coded 1 for employees who ever were married or ever had children by 2005 and 0 for everyone else. Otherwise, the analysis is identical to the one described above. This provides an estimate of the selection effect—whether future marital and parenthood statuses can predict the wages while single and childless. As discussed above, we have data on marital and fatherhood histories through 2005, which is 9 years beyond the last year for which we have employment data.

In the second analysis, we use information on all employees and introduce the same two dummy variables for "Ever married" and "Ever children," entered in each year the employee was present in the data. In addition, as in the analyses of total effects, we enter dummy variables for current marital status and current number of children. The dummy variables for "Ever married" and "Ever children" estimate the selection effects whereas the dummy variables for current marital and parenthood status estimate treatment effects. The sum of the two dummy variables gives the total effect of marriage and children, comparable to the analyses in which we do not separate the selection and treatment effects.

These two analyses address the question of selection effects most directly, assessing whether these are present before the entrance into the state of marriage or parenthood has occurred. The part of the marriage and children effects not due to selection is then due to treatment, according to the interpretation given here.

Accounting for Treatment Effects

Above, the treatment effect was primarily identified as the residual—the premium left over after having subtracted out the selection effect. We can estimate the treatment effect more directly by utilizing the longitudinal structure of the data. We add a fixed effect for the individual employee in addition to fixed effects for establishment, occupation, and occupation-establishment. We then assess whether individuals, as they transition between statuses—from single to married to separated and so on, and from childless to having 1, 2, or 3+ children—experience within-individual changes in wages (premia or penalties) following such transitions. We use the individual-level data from multiple years, observing employees before and after family transitions. We can account for all time-constant variables, measured and unmeasured, plus for measured time-varying variables. Biases may still arise from the exclusion of unmeasured time-varying variables.

Accounting for only individual or for only occupation-establishment fixed effects is straightforward. Accounting for both at the same time is difficult. With two sets of fixed effects there is no estimator in which all the dummy variables "vanish" from the estimation procedure. Further, with an average of 114,000 male employees each year, and with occupation-establishment units numbering from 41,254 to 54,339 (see the note to Table 2), estimating the effects of all the dummy variables may be impossible. No computer software known to us can handle this. Our solution has been to adapt a simple procedure from Goux, Dominique, and Maurin (1999) in which we create a fixed effect specific to the individual and the establishment, the occupation, and the occupation-establishment unit, respectively, in which he works. Thus, if the individual changes occupation-establishment unit, a new dummy variable pertaining to him and the new occupation-establishment unit is created. This analysis addresses the question of treatment effects most directly since it estimates the effects at the individual level of getting married and becoming a parent. As above, the part of the total effect of marriage not due to treatment is due to selection.

The two sets of analyses, selection and treatment, may yield somewhat different results regarding their relative importance. When estimating selection effects, we compare the above individuals with those who stayed single and childless. When estimating treatment effects, the comparison is intra-individual, before and after the person enters into marriage and parenthood.

Methods for Analyzing Individual Career Dynamics

We analyze changes in wages from one year to the next among those employees who stayed in the sector for two consecutive years and who remained in the same establishment, simply because our concern is primarily with what occurs within firms. The dependent variable is the change in logarithm of wages from one year to the next. We estimated the same set of models as we did for the total effects on wage levels.

How to Think about the Various Sets of Coefficients

A fruitful way to think about the various estimators is that they report on different aspects of the data. Comparing changes in coefficients as we go from the population-level estimator to the occupation- and then to the occupation-establishment-level estimators enables us to assess the levels at which differences between groups arise—from differential wages at the occupation-establishment level, or from differential sorting of the groups on occupations and occupation-establishment units.

Similarly, when we take into account individual-level fixed effects, then we assess how an individual's transition from being single to married (or from having 0 to 1 child) on average affects the individual's wages. We no longer compare, for example, married men to other men who are single; rather, we compare the wages of married men to the wages they earned when they were single. Both types of comparisons are relevant to make. For answering questions about the effects of marriage at the individual level, the fixed-effects estimator is preferable.

The Wage Gap by Marital Status and Children

Total Effects on Wage Levels

In Table 3 we report the coefficients on wages of marital status and number of children below age 20 adjusted for education and imputed labor force experience. At the population level—the level that provides the analyses comparable to those reported in the literature—the effects of marital status are similar across the four time periods: wages are 10–12% higher for married vs. single men, and with a somewhat lower though still substantial wage bonus for previously married men, which can be taken as partial evidence against the household specialization mechanism. The effects of children are similarly consistent across the time periods: negligible effects for 1 child, and about 1–2% for 2 or 3+ children, a fairly minimal differential.

What happens to these effects as we successively control for establishment, occupation, and the occupation-establishment unit? The effects are again consistently similar across the three time periods, at each of the three levels of controls. Controlling for establishment reduces the effects of marital status to about 8%. The children effects increase slightly, meaning that fathers tend to work in somewhat lower-paying establishments than non-fathers.

Controlling for either occupation or occupation-establishment, however, results in large reductions in coefficients. For marital status, the premia are down to 4% at the occupation level and down to 2–3% at the occupation-establishment level, a reduction of 70–80%. At the occupation-establishment level, there are small positive effects of having children, ranging from 0.4% to 1.1%.

What can be concluded from this? The effects of marital status, and to a lesser extent those of children, work mostly through the sorting of employees on occupations and occupation-establishment units: 70–80% of the marital premia are due to sorting. Married or previously married men work in different and better-paying occupations and occupation-establishment units than single men. But once these groups work side-by-side, they receive practically the same pay.[9] Employers do not pay men with family obligations more for the same work.[10] The marital premia did not decline over time, suggesting that the premia are not due to animus from employers against single men, since any animus likely declined over the period.

Are Men Who Marry or Have Children Different from Men Who Remain Single or Childless?

Are the men who marry and become fathers different from those who remain single and childless, so that the former group would earn more even in the absence of marriage or parenthood, and even prior to these states? Or are the effects due to changes in behavior, such as increased work effort and occupational aspirations, induced from marriage and parenthood?

In Table 4 the question about selection effects is addressed using two different types of analyses. In Panel A, we select the set of men who in a given year are single and childless and then examine the effects of eventually marrying and/or becoming a parent. Recall first that the effects of having children were small. At the population level during the period 1979–1984, the effects were 2.4% and 1.9% for 2 and 3+ children, and they were even smaller in later years and at other levels. There is thus not much of an effect to partition, and these results command less attention.

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[9] We also estimated models including an interaction term between being married and years married (see Kenny 1983; Korenman and Neumark 1991). At the population and establishment levels, the interaction effect is positive in each of the three periods and ranges from 0.3% to 0.6% per year of marriage. At the occupation and occupation-establishment levels, the interaction effect disappears. Our main results are unaffected by inclusion of the interaction effect.

[10] Differential treatment from employers can, of course, occur through assignment to occupations either at the point of hire or in subsequent promotion. We have addressed the latter possibility in the wage growth analyses below, and we rule it out there.

Table 3. Effects of Marital Status and Children Age 20 or Below on the Logarithm of Hourly Wage in Three Time Periods and for Four Different Levels: Population, Establishment, Occupation, and Occupation-Establishment

	1979–1984				*1985–1989*				*1990–1996*			
	Pop	*Est*	*Occ*	*Occ-Est*	*Pop*	*Est*	*Occ*	*Occ-Est*	*Pop*	*Est*	*Occ*	*Occ-Est*
Married	.103	.075	.041	.028	.118	.080	.043	.026	.112	.075	.036	.022
Divorced	.075	.033	.039	.018	.088	.039	.039	.016	.080	.040	.029	.012
Widowed	.080	.053	.032	.020	.099	.062	.043	.020	.104	.068	.031	.022
Separated	.092	.045	.042	.018	.109	.057	.045	.018	.093	.052	.036	.015
One child	–.001	.013	.001	.005	–.004	.009	.000	.004	.000	.011	.002	.005
Two children	.024	.039	.006	.011	.009	.023	.000	.008	.009	.020	.003	.008
Three+ children	.019	.042	–.001	.011	.003	.024	–.007	.008	.012	.025	.002	.010

Note: These results control for experience, as experience and experience squared, and for five educational groups represented by dummy variables. The dummy variables for children are for having one child age 20 or below, two children age 20 or below, or three or more children age 20 or below. In the column denoted "Pop," no further controls are introduced. In the columns denoted "Est," "Occ," and "Occ-Est," we include dummy variables (fixed effects) for the establishment in which the employee worked, for the occupation in which he worked, and for the occupation-establishment unit in which he worked. The estimates are obtained separately for each of the 18 years in the period 1979–1996. The analysis is restricted to employees 20–50 years old. The table reports the average of the yearly coefficients for three subperiods: 1979–1984, 1985–1989, and 1990–1996. Most of the yearly coefficients are statistically significantly different from zero at a very high significance level, usually with z- or t-statistics in the 40–50 range. Regarding marital status, 72 of 72 annual coefficients are statistically significantly different from zero at the .05 level or better, and for post-marital states, 12 of the 216 annual coefficients are not statistically significant at the .05 level. For children, 52 out of the 216 annual coefficients are not statistically significant at the .10 level, with 50 of 52 non-significant coefficients occurring at the population and occupation level, primarily for one and two children.

Table 4. Effects of Current Marital Status and Current Children as Well as Future Marriage and Children on the Logarithm of Hourly Wage in Three Time Periods and for Four Different Levels: Population, Establishment, Occupation, and Occupation-Establishment

	1979–1984				*1985–1989*				*1990–1996*			
	Pop	*Est*	*Occ*	*Occ-Est*	*Pop*	*Est*	*Occ*	*Occ-Est*	*Pop*	*Est*	*Occ*	*Occ-Est*
Panel A: Childless singles												
Ever married	.047	.029	.020	.009	.054	.028	.020	.008	.043	.018	.013	.006
Ever children	–.019	–.003	–.007	–.001	–.004	.013	.003	.002	–.002	.018	.007	.005
Panel B: Overall population												
Married	.089	.066	.037	.028	.097	.068	.036	.023	.094	.063	.030	.019
Divorced	.060	.024	.035	.017	.066	.026	.032	.013	.061	.028	.024	.010
Widowed	.065	.043	.027	.020	.077	.049	.036	.017	.085	.056	.026	.019
Separated	.077	.036	.038	.018	.088	.045	.038	.015	.075	.040	.030	.013
One child	–.001	.009	.002	.006	–.006	.003	–.001	.004	–.003	.002	.001	.004
Two children	.024	.035	.008	.012	.006	.016	.000	.008	.006	.011	.003	.007
Three+ children	.018	.038	.001	.013	.000	.017	–.007	.008	.010	.016	.002	.009
Ever married	.033	.019	.012	.001	.040	.019	.015	.004	.029	.013	.009	.003
Ever children	–.002	.008	–.006	–.004	.005	.018	–.001	.000	.005	.023	.000	.002

Notes: Panel A, "Childless singles," displays regression coefficients estimated for employees who in the given year are singles and have no children. The variable for "Ever married" indicates whether the employee eventually got married (= 1) or not (= 0) within the time frame of the data (up to 2005 for marriage and children). The variable for "Ever children" indicates whether the employee had children (= 1) or not (= 0) within the time frame of the data. For "Ever married," 68 of the 72 annual coefficients are statistically significantly different from zero at the .05 level or better, with all four of the non-significant coefficients occurring at the occupation-establishment level. For "Ever children," 34 of the 72 annual coefficients are not statistically significant at the .05 level.

Panel B, "Overall population," pertains to everyone in the data. The variables "Ever married" and "Ever children" are coded as noted above—they are equal to 1 in every year for employees who were observed as married and potentially having children age 20 or below in at least one year in the time frame of the data (up until 2005 for marriage and children). The variables for marital status and for 1, 2, or 3+ children are coded with their actual values in the year, as 1 for married if the employee is married in the given year. In the columns denoted "Est," "Occ," and "Occ-est," we include dummy variables (fixed effects) for the establishment in which the employee worked, in which occupation he worked, and in which occupation-establishment unit he worked. The estimates are obtained separately for each of the 18 years in the period 1979–1996. The tables report the average of the yearly coefficients for three subperiods: 1979–1984, 1985–1989, and 1990–1996. For "Ever married," 59 of the 72 annual coefficients are statistically significantly different from zero at the .05 level or better, with all 13 of the non-significant coefficients occurring at the occupation-establishment level. For "Ever children," 31 of the 72 annual coefficients are statistically significant at the .05 level or better. For marital status, for being married, 72 of the 72 annual coefficients are statistically significant at the .05 level or better, and for post-marital states, 13 of the 216 annual coefficients are not statistically significant at the .05 level. Regarding children, for one child, 37 out of the 72 annual coefficients are not statistically significant at the .10 level, and for two or three-plus children, 29 of 144 annual coefficients are not statistically significant at the .10 level.

At the population level, the effects are clear: in the period 1979–89, even among single men, the men who eventually married earned higher wages than those who did not, the premium being about 4–5%. At the occupation level, there was a small advantage to eventually getting married, of about 2% in 1979–84, and of 1% at the occupation-establishment level, with comparable effects in later periods. With respect to becoming a parent, on balance the selection effects are small and the treatments effects dominate. At the population level, the selection effect is even negative. But as noted above, the premia for children are small to begin with.

The conclusion to draw from Panel A is that almost 50% of the marital premium at the population level is due to selection. Men who eventually marry are different from men who do not, even before they get married. The selection effects work through sorting into the higher-paying occupations and occupation-establishment units, and this sorting occurs even prior to marriage. But at the occupation-, and especially occupation-establishment level, men who eventually get married do not reap any significant wage advantages. Employers do not recognize the productivity advantages of the men who eventually marry with higher wages.[11]

The finding that sorting into the higher-paying occupations and occupation-establishment units occurs even prior to marriage is important. It indicates that the marital premium is due in part to employees' choice or to observable productivity, not from differential treatment by employers, simply because employers cannot sort employees on the basis of their future marital status.

A variant of this analysis is presented in Panel B. Here, we select all employees—single, married, previously married, fathers, and non-fathers—and examine the effects of "Ever married" and "Ever children" and of current marital and parenthood status. This allows us to distinguish the effects of being someone who eventually gets married and/or has children (selection) from the additional effects of actually being married and/or having children (treatment).

At the population level, the marital status selection effect is about one-third of the treatment effect; the coefficient for married is almost 3 times as large as the coefficient for "Ever married." Men who are single in a given year but who eventually marry earn a wage premium of 3%. Once they actually marry, they earn an additional premium of 9% for a total premium of 12%. This squares well with Table 3, which shows the premium for being married at around 11%.[12] For the variable "Ever children," the selection effects are again small and practically absent at the occupation-establishment level. The treatment effect dominates.

What are the results at the occupation and occupation-establishment levels? Especially at the latter level, there are practically no selection effects of eventually marrying. There is a treatment effect of being married (1.9–2.8%), constituting almost the entire marital effect at that level. At the occupation-establishment level, employers do not recognize the presumed productivity advantages of men who eventually marry with higher wages, but men who actually are married receive higher pay. Being marriageable does not help, but being married does. If married men really are more productive, then the

[11] It should be noted that the effect of "Ever married" may partly reflect the effect of currently cohabiting, assuming that being married and cohabiting affect wages in similar ways. Marriage is often preceded by cohabitation. One should expect that many of the men who marry soon in our data currently are cohabiting. If the wage effects of marriage and cohabitation are similar, then the effect of "Ever married" should be larger in our data for those who enter marriage in the near future. To explore this possibility, we performed separate analyses in which we also controlled for the years until marriage occurs. This specification leads to a larger coefficient for "Ever married" of an entire 7.5% during the period 1979–1984, and a negative coefficient for the variable years until marriage of 0.5% per year. This means that the premium to "Ever married" is larger for those marry soon than for those who wait for a longer period of time. The result is consistent with a conjecture that cohabitation is similar to marriage in its impact on wages.

[12] Note that among the men who eventually marry, the higher earners may marry earlier than the lower earners. If so, we overstate the treatment effect of marriage in this analysis. This would be consistent with the supplemental analysis reported in footnote 11, above.

Table 5. Effects of Marital Status and Children Age 20 or Below on the Logarithm of Hourly Wage at Four Different Levels: Individual, and Interaction of Individual and Establishment, Occupation, and Occupation-Establishment

		Individual ×		
	Individual	*Est*	*Occ*	*Occ-Est*
Married	.023	.015	.023	.016
Divorced	.011	.005	.012	.007
Widowed	.018	.010	.024	.012
Separated	.017	.011	.017	.011
One child	.005	.003	.005	.004
Two children	.008	.004	.006	.004
Three+ children	.006	.003	.002	.001

Note: In these analyses, an individual-level fixed effect is included in each column. When establishment-level (or occupation-level) fixed effects are also included, this is done by interacting the establishment- (or occupation-) and the individual-specific dummy variable. As long as an individual remains in the same establishment (or occupation), the fixed effect remains the same. When the person changes establishments (or occupation), the fixed effect also changes. The same is the case for the occupation-establishment-level fixed effect. It obtains as the interaction of the occupation-establishment- and the individual-specific dummy variables. This procedure of interacting the individual- and establishment-level (or occupation or occupation-establishment-level) fixed effects is adapted from Goux and Maurin (1999). Controlling separately for the individual- and establishment-level (or occupation or occupation-establishment-level) fixed effects would have led to equations not estimable by current software; there would have been too many dummy variables to take into account. See the section on methods and the Appendix for further explanation. Each of the coefficients in the table is statistically significantly different from zero at the .05 level or better.

premium at that level must be interpreted as a treatment effect. The men change their behavior upon marriage, but this premium is small relative to that which arises through sorting.

Do Men Become More Productive upon Marriage and Parenthood?

We analyze within-individual dynamics using models with fixed-effects for individuals, which describe how wages evolve as men move from one marital status to another and from being childless to having children. The results are presented in Table 5.

At the individual level, there are small effects of marital status. As men move from being single to being married, their wages increase 2.3%, but they fall back to being 1.1–1.8% higher than singles when their status is previously married. At the individual-occupation-establishment level, these effects are smaller, with a 1.6% increase for getting married and effects ranging from 0.7–1.2% for being previously married. The effects of children are even smaller. This analysis shows that the treatment effects of marital status and children are small. An individual's wages increase by only 2% upon marriage.

One may ask why the within-individual comparisons are different from the comparisons between individuals. The reason is that when making within-individual comparisons, we adjust for a much larger set of variables, insofar as an individual is compared to himself in two or more years, thereby accounting for all time-constant variables that pertain to that individual between years.

What are the implications of these findings? Selection is important in explaining the marital premium. Treatment—that is, behavior that has changed upon marriage—appears to be less important. The selection effect works through sorting. The men who eventually marry, and the married men, sort into different occupations and occupation-establishment units than the men who remain single. Employers do not pay men differently according to marital status once they work in the same occupation-establishment unit; differences range from 0.7 to 1.6% at this level.

Wage Growth

The clear marital premium and the smaller parenthood premium have little to do with employers paying married and unmarried men or fathers and non-fathers different wages for the same work. To the extent that the observed premia can be attributed to actions by employers, they must arise either at the point of hire or in subsequent promotions. We have no information on applicant pools and thus cannot address hiring, but we can investigate wage growth processes.

In addressing wage growth we are restricted to looking at employees who remained in the data set in two adjacent years. Table 6 presents the coefficients for marital status and number of children below age 20 on growth in wages between two consecutive years among employees who remained in the same establishment. Each regression is estimated separately by year for 17 years, but not for 1996–1997, since we do not know wages in 1997. As before, we have averaged the coefficients across years within three separate periods: 1979–84, 1985–89, and 1990–96.

The coefficients for marital status and children are, in substantive terms, equal to zero, in all years and at all levels, and less than one-third of the annual coefficients reach statistical significance at the .10 or better level, leading us to conclude that the marital premium does not arise due to promotion differentials within establishments, and that the differential must arise at the point of hire through differential allocation to occupations and establishments.

Conclusion and Discussion

We have investigated whether the male marital and parenthood premia arise as a result of differential pay by employers or from differential sorting of employees on occupations and establishments. We investigated these premia in Norway over the period 1979–96, when public policy made it easier to combine family and career, with the clearest first-order impact on women, but with possibly attendant increased pressures on men to be more active in the family sphere. To the extent that the premia arise from household specialization, this should in itself lead to lower male premia than in other countries and to its decline over time.

Our research has led us to six conclusions. First, the effect of marriage, and to a lesser extent of children, occurs mostly through sorting on occupations and occupation-establishment units. Once the same work is performed for the same employer, wages vary only minimally by marital and parental status. This indicates an absence of productivity differences and discrimination at that level and answers a question previously not addressed. The role of differential pay from employers is thus marginal in explaining the marital and parenthood premia.

Second, the premia to marital status and parenthood remain stable over time. Neither the changing division of household labor, nor possibly declining animus against single men, nor the increased wage compression over the period of the study (Kahn 1998), resulted in a decline of the premia.

Third, men who marry experience wage advantages even prior to marriage. According to this analysis, about 50% of the marital premium at the population level is due to selection. This pre-marriage premium is earned almost entirely through sorting on occupations and occupation-establishment units. The fact that men who marry sort into the higher-paying occupations and occupation-establishment units even prior to marriage is *prima facie* evidence that sorting occurs due to choice or due to observed productivity, not due to employer discrimination with respect to marital status, since sorting of employees by employers on the basis of future marital status is not feasible.

Fourth, an examination of within-individual wage changes as men in the study moved from being single to married reveals that wages change only marginally upon such transitions. This analysis, particularly designed for assessing treatment effects, shows that only 20% of the marital effect is due to treatment and the remainder 80% is due to selection. It assigns a higher weight to selection than the analysis based on the effect of future marital status does. Since it is based on within-individual level wage

Table 6. Effects of Marital Status and Children Age 20 or Below on Change in Logarithm of Hourly Wage Between Two Adjacent Years in Three Time Periods and for Four Different Levels: Population, Establishment, Occupation, and Occupation-Establishment

	1979–1984				*1985–1989*				*1990–1996*			
	Pop	*Est*	*Occ*	*Occ-Est*	*Pop*	*Est*	*Occ*	*Occ-Est*	*Pop*	*Est*	*Occ*	*Occ-Est*
Married	.002	.001	.002	.002	.000	.001	.001	.003	.002	.002	.001	.002
Divorced	.000	–.002	.000	.000	–.002	–.001	–.001	–.001	.002	.001	.001	.001
Widowed	–.004	–.003	–.004	–.001	–.001	.001	.000	.003	.003	.001	.002	–.002
Separated	–.001	–.001	–.001	–.001	–.002	–.002	–.001	–.001	–.001	–.002	–.002	–.001
One child	–.002	–.001	–.002	–.001	–.001	–.001	–.001	.000	–.001	.000	.000	.000
Two children	–.002	–.001	–.001	.000	–.001	.000	.000	.000	.000	.000	.000	.000
Three+ children	–.003	–.001	–.002	.000	–.001	.000	.000	.000	.000	.000	.000	.000

Note: These results control for experience, as experience and experience squared, and for five educational groups represented by dummy variables. The dummy variables for children are for having one child age 20 or below, two children age 20 or below, or three or more children age 20 or below. In the column denoted "Pop," no further controls are introduced. In the columns denoted "Est," "Occ," and "Occ-est," we include dummy variables (fixed effects) for the establishment the employee worked in, for the occupation he worked in, and for the occupation-establishment unit he worked in. The estimates are obtained separately for each of the 17 pairs of adjacent years from 1979–1996. Since 1996 is the last year with employment data, the last year with analysis of wage changes is from 1995 to 1996. The table reports the average of the yearly coefficients for three subperiods, 1979–1984, 1985–1989, and 1990–1996. The wage change analyses were restricted to employees who stayed in the same establishment between two adjacent years. For marital status, for being married, 37 of the 72 annual coefficients are not statistically significantly different from zero at the .10 level or better, and for post-marital states, 181 of the 216 coefficients are not statistically significant at the .10 level. For children, 174 of the 216 annual coefficients are not statistically significant at the .10 level.

changes, it should be taken as the more authoritative estimate of the role of selection.

Fifth, the premia for children are small, accounting for about 1–2% at the population level. Given their size, attempting to partition what is due to selection and what is due to treatment makes little sense.

Sixth, there were no marital or parenthood premia for the wage growth of employees who remained in the same establishment. The marital premia are thus not due to differences in promotion rates; rather they must arise due to the differential allocation to occupations and establishments that occur at the point of hire.

The central conclusion, then, is that the marital wage premium has little to do with employers paying married and unmarried men different wages. It is due to sorting of married and non-married men into different occupations and occupation-establishment units. Moreover, the men who eventually marry (or eventually get children) sort into the high-paying occupations and establishments even prior to entering these states, which is, as stated above, evidence that the sorting is not due to discrimination from employers but is the effect of employees' choices.

A caveat must be pointed out with respect to the interpretation of the selection vs. treatment effects. It is possible that the men who eventually marry and have children act preemptively, seeking high-paying jobs in anticipation that they will get married and become fathers, thus expecting that they will need the money in the future. In that case, the marital and parenthood premia are, after all, treatment effects. What we observe may be adaptive behavior at a given point in time to expected future events. This is consistent with the within-individual analysis: employees' wages do not increase by much upon marriage and parenthood. Alternatively, it could be that earning high wages makes these men more marriageable. In that case, it is a selection effect. We cannot separate these processes. What is clear, however, is that the premia to marriage and parenthood occur even before entrance into marriage and having children, and that these premia on balance are stronger than the premia for actually entering into marriage and fatherhood when using the results from within-individual changes in wages. The empirical fact is solid. How this is interpreted, however, is still open.

Appendix: Methods

Methods for analyzing total effects on wage levels

The subscripts used are as follows: i for individuals, o for occupations, e for establishments, and t for years. The dependent variable is the logarithm of wages ($\ln w_{it}$) for individual i in year t, and the independent variables are collected in the vector x_{it}, which includes the constant 1.

In a cross-sectional analysis, separately for each year t we regress the logarithm of wages $\ln w_{it}$ on explanatory variables x_{it}, using four different specifications:

$$\text{(A1)} \qquad \ln w_{it} = \alpha_{P,t}x_{it} + \varepsilon_{it}$$

$$\text{(A2)} \qquad \ln w_{it} = \alpha_{E,t}x_{it} + \eta_{et} + \varepsilon_{iet}$$

$$\text{(A3)} \qquad \ln w_{it} = \alpha_{O,t}x_{it} + \eta_{ot} + \varepsilon_{iot}$$

$$\text{(A4)} \qquad \ln w_{it} = \alpha_{OE,t}x_{it} + \eta_{oet} + \varepsilon_{ioet},$$

where η_{et}, η_{ot}, and η_{oet} are fixed effects (or dummy variables) capturing establishment e, occupation o, and occupation-establishment unit oe; ε_{it}, ε_{iet}, ε_{iot}, and ε_{ioet} are error terms. The subscripts to the α parameters indicate that these are different coefficients, pertaining to different levels, population, establishment, and so on.

Accounting for selection effects

These analyses are described in sufficient detail in the text. The same set of equations as above are estimated, but two new variables are entered: "Ever married" and "Ever children." The sample restrictions differ in one of the analyses reported.

Accounting for treatment effects

Define four sets of dummy variables: $D_i = 1$ for individual i (0 otherwise); $D_e = 1$ for establishment e (0 otherwise); $D_o = 1$ for occupation o (0 otherwise); and $D_{oe} = 1$ for occupation-establishment unit oe (0 otherwise). Further, define a dummy variable for the year D_t. For the four levels, we conduct the following sets of analyses:

$$\text{(A5)} \qquad \ln w_{it} = \alpha_I x_{it} + \delta_{I,i}D_i + \gamma_t D_t + \varepsilon_{it},$$

$$\text{(A6)} \qquad \ln w_{it} = \alpha_{IE} x_{it} + \delta_{IE,ie} D_i \cdot D_e + \gamma_{E,t} D_t + \varepsilon_{iet},$$

$$\text{(A7)} \qquad \ln w_{it} = \alpha_{IO} x_{it} + \delta_{IO,io} D_i \cdot D_o + \gamma_{O,t} D_t + \varepsilon_{iot},$$

$$\text{(A8)} \qquad \ln w_{it} = \alpha_{IOE} x_{it} + \delta_{IOE,ioe} D_i \cdot D_{oe} + \gamma_{OE,t} D_t + \varepsilon_{ioet},$$

where $\delta_{I,i}$, $\delta_{IE,ie}$, $\delta_{IO,io}$, and $\delta_{IOE,ioe}$ are fixed effects, comparable to the regressions in (A1)–(A4), while the γs are fixed effects for the dummy variables for year. The subscripts *I, IE, IO* and *IOE* to the α and δ coefficients denote that these are fixed effects at the individual-population, individual-establishment, individual-occupation, and individual occupation-establishment levels.

In the first of these specifications (A5), we include only the individual-level fixed effects, as in standard panel data analysis.

In the second specification (A6), we include the interaction term between the individual- and establishment-level dummy variables. As long as an individual stays within the same establishment, the fixed effect remains $\delta_{IE,ie}$, but when the establishment is changed, from say $e = 1$ to $e = 2$, the fixed effect changes from $\delta_{IE,i1}$ to $\delta_{IE,i2}$.

In the third specification (A7), we include the interaction term between the individual- and occupation-level dummy variables. As long as an individual stays within the same occupation, the fixed effect remains $\delta_{IO,io}$, but when the occupation is changed, for example from $o = 1$ to $o = 2$, the fixed effect changes from $\delta_{IO,i1}$ to $\delta_{IO,i2}$.

The fourth specification (A8) corresponds to the second, except that here we include the interaction term between the individual- and occupation-establishment level dummy variables. As long as a person stays in the same occupation-establishment unit, we account for the fixed effect specific to that person and occupation-establishment unit. If a change occurs in occupation, or in establishment, or in both, then an entirely new fixed effect is defined for the same person but now for a different occupation-establishment unit.

The procedures are cumbersome, but they still provide the simplest way to account simultaneously for the individual fixed effects on the one hand, and the establishment, occupation, or occupation-establishment-level fixed effects on the other. For example, it would have been computationally infeasible to estimate the fixed effects of separate dummy variables for persons and establishments, which amount to several tens of thousands of the former and several thousands of the latter.[13]

[13] Abowd, Kramarz, and Margolis (1999) have developed a creative estimator that does this, but it is computationally more complex to implement and relies on the specific assumption of independence of the dummy variables for the various fixed effects and the other measured variables, x_{it}. It appears to work well for the data they analyze.

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