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Abstract

We investigate whether the male marital and parenthood premia arise due to differential pay by employers or from differential sorting of employees on occupations and establishments. We investigate these premia in Norway using matched employee-employer data in the period 1980–97, a country where public policy has 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. 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. We also find that about 50–75% 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 no marital premia on wage growth within establishments, but marital premia on promotions. Part of the marital wage premium is thus due to higher promotion rates for married men.

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TITLE

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ABSTRACT

We investigate whether the male marital and parenthood premia arise due to differential pay by employers or from differential sorting of employees on occupations and establishments. We investigate these premia in Norway using matched employee-employer data in the period 1980–97, a country where public policy has 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. 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. We also find that about 50–75% 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 no marital premia on wage growth within establishments, but marital premia on promotions. Part of the marital wage premium is thus due to higher promotion rates for married men.

1 INTRODUCTION

Men earn a wage bonus from family: substantial from marriage, smaller from fatherhood. The opposite is the case for women, who typically experience small wage differentials from marriage but substantial penalties from motherhood. The male marital premium is well documented, though yet not fully understood (Rodgers and Stratton 2005), whereas the female parenthood penalty is less documented but probably better understood (Budich and England 2001).

Why are these facts significant? They command our attention simply because the processes that unfold in the family are a core, if not the core, obstacle for achieving gender equality in the workplace today. There are strongly divergent career effects of family, beneficial to one sex, detrimental to the other. For men, the premia appear to arise mostly from marriage itself, less so from children, whereas for women, the penalties arise not so much from marriage but from having children. The present paper concerns the better documented but less well understood marital and parenthood premia for men.¹

Three basic hypotheses have been put forth to explain the premia: the selection, the treatment, and the discrimination hypotheses (e.g., Chiodo and Owyang 2002).

According to the *selection* hypothesis, men who marry and become fathers are more productive than men who don't, even before entrance into marriage and fatherhood. The same factors that make these men more productive and highly valued by employers also make them more attractive as marital partners. Marriage as such does nothing to increase their productivity. The observed relationship between marital status and productivity and wages is thus spurious.

According to the *treatment* hypothesis, getting married and becoming a father induce men to change their behavior by paying more attention to work, by working harder, and so on. Marriage thus causes higher productivity. The proposed mechanisms are varied, including freeing up more time for market work due to the benefits from household specialization, increased investments in skills and human capital, and pressures to earn money due to costs of homes and children.

Common to the selection and treatment hypotheses is the claim that married men are more productive than the average single man. The two hypotheses differ in their view on the causal direction. Under selection, being productive causes both high

¹A separate paper addresses the female family penalty, which has a different structure, and probably other causes. Many of the issues are similar, but given the different empirical patterns and the complexity of data and results, separating the findings facilitates presentation of results. As Chiodo and Owyang (2003, p. 9) write: "the three theories often used to explain the phenomenon between men's marital status and wages offer little insight into the situation for women."

wages and marriage. Under treatment, 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. It puts forth that employers consciously or unconsciously favor married men, either as a reflection of societal norms, which stress the value of marriage, or due to statistical discrimination, where married men correctly or erroneously are seen on average to be better employees, but where no attempt is made to assess which married and which single men are more productive. As Hersch and Stratton (2000, p. 93) comment on the discrimination versus treatment hypothesis: “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.”

Although there are both similarities and differences in the empirical implications from the three hypotheses, each posits that the relevant processes occur at the level of employers. After all, it is employers who pay the wages of married, previously married, and single men. Two questions thus arise. Do employers—when employing single and married men in same occupation and same workplace—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 same occupation and workplace? If the answer to the first question is no—employers pay same wages for married and non-married men—and to the second question is yes—married men work in the higher-paying establishments and occupations—then the marital premium arises from differences in hiring and promotions, not from unequal pay once hired and promoted.

This paper makes an attempt to shed light on these conundra, using matched employer-employee data from Norway in the period 1980–1997. Given the salience of employers in this context, such data are important as 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 wages occurs, or whether they work in different occupations and firms. We thus address first whether employers pay men differently according to marital and parenthood status, second the role of sorting of employees on occupations and establishments for the size of the premia, and finally the extent to which the premia arise from promotion and wage growth differentials. Our analysis will thus provide an entirely novel and crucial empirical angle, by assessing where the premia arise, at the level of the employer in how they pay married and single men, or in how employees are sorted on employers and occupations. We also report the role for these processes of parenthood status. Apart

from the novelty of the matched employer-employee data, there are several noteworthy features of the data, making them in some respects better than data previously used. These include annual observations of employees and establishments over an 18-year period and practically error-free recording of wage rates and occupation.

While the empirical and attendant conceptual questions raised are relevant across the entire spectrum of rich countries, the national setting itself is of particular intrinsic interest. Scandinavia, along with the U.S., is in the forefront in regards to gender equality policies and both have progressive values toward gender equality. Scandinavia leads in the area of family policies, the U.S. in affirmative action and workplace regulation. While most Scandinavian family policies are gender neutral, their first-order impact is primarily on mothers, making it easier to combine family and careers, where female labor-force participation rates now are close to male rates, though with higher rates of part-time work for women. The second-order impact is however on the adjustments fathers make. In passing Norwegian family legislation an explicit goal expressed during parliamentary debates was to redefine the family institution, by shifting the culture around how families operate. We thus provide evidence on the marital premium in an economy where 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.

2 SELECTION, TREATMENT, AND DISCRIMINATION

We first review the three hypotheses. Then we discuss our core errand, the role of differential pay within versus sorting on establishments, occupations, and occupation-establishment units for how the premia arise. Next we summarize the empirical implications of the hypotheses. Finally we summarize existing evidence.

2.1 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. These include conscientiousness, industriousness, and other traits valued both by prospective partners and prospective employers.² Marriage as such does nothing to increase their productivity. Men who marry and become fathers are more productive than men who don't even before entrance into marriage and fatherhood. The observed relationship between marital status and productivity and wages is thus

²Mueller and Plug (2006) report wage bonuses for a variety of personality traits which may or may not be related to productivity, for example, that men are rewarded for being antagonistic.

spurious, arising from insufficient measurement of the underlying causal factors; in technical terms, due to an omitted variable bias. The causality is the opposite: Productivity causes marriage, not the other way around.

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 of getting married: as individuals move between marital states, there should be no wage changes. Productivity is high both before and after marriage, and the act of marrying does nothing to increase or decrease it. This also means that men who eventually marry even prior to entrance into marriage earn a wage premium relative to men who remain single.

2.2 TREATMENT

According to the *treatment* hypothesis, getting married induces men to change their behavior by paying more attention to work, by working harder, and so on. Marriage thus causes higher productivity. Three mechanisms that could produce this result have been put forth.

One 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 is similar to the first. It proposes that marriage leads to higher work effort, but not due to household specialization, but to a more well-ordered life style or the need for more money when married and especially when having children, a point raised already a 100 years ago by Weber (1908[1924], p. 174).

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

The first mechanism has an additional implication: the marital premium should have declined over time as the household division of labor has become more equal and thus increased the demands on married men's time, leaving less time for market

work today than earlier. In the U.S., average household work for married women decreased from 34 to 19.5 hours per week between 1965 and 1995, while among married men it increased from 5 to 10.5 hours (Bianchi, Milkie, Sayer, and Robinson 2000, App. A). The other two mechanisms have no implications for trends in the premium over time.

2.3 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. In the selection hypothesis they are more productive than the subset of single men who do not eventually marry or do not have the characteristics conducive to getting married. The relationship is only correlational, since it exists prior to, during, and post marriage. In the treatment hypothesis married men are *ceteris paribus* more productive than all single men, simply because marriage makes them more productive. The relationship is causal: Marriage causes increased productivity.

The *discrimination* hypothesis, in contrast, does not rest on the claim that married men are more productive than single men, at least not in its pure form. It argues that employers discriminate in favor of married men. In its pure form the hypothesis puts forth 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, p. 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 from and paying more for white than black employees, even in absence of objective reasons for doing so (England 1992, chap. 3). In a less pure form, married men may in fact on average be 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, p. 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. It would be an instance of statistical discrimination (England 1992, chap. 3), and would, if costs of measuring productivity are high, be economically rational behavior.

These two mechanisms have various implications. According to the animus mechanism the marital premium should decline over time, as 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).

The statistical mechanism also has a subtle but interesting implication. If true, and the premium is earned also at the occupation-establishment level—not only at point of hire and promotion—then the men who eventually marry should reap a wage premium at that level even before marriage, as their productivity at that level is observable to the employer.

2.4 THE ROLE OF SORTING

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?

And if the premia arise due to sorting, does the sorting come from employee choice in which establishments and occupations to work in, or does it come from employer choices favoring married over single men? The two mechanisms are clearly difficult to disentangle.

But a subtle implication does arise here that allows us to gain some insight into the role of employee choices and productivity versus employer discrimination. If the men who eventually get married and have children, while they still are single and childless, sort into the better-paying occupations and occupation-establishment units to a higher degree 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 among single men to discriminate on the basis of their future marital status, and if sorting still occurs, it is unrelated to employer preferences for married over single men, and thus not caused by discrimination.

2.5 SUMMARY OF MAIN IMPLICATIONS

It is useful to summarize the main empirical implications of the hypotheses. All three hypotheses agree that married men earn a premium over single men. They differ in the mechanisms proposed for the premium and with respect to what happens with it upon separation, divorce, and widowhood.

According to the selection hypothesis, the men who eventually marry will earn high wages also prior to marriage, will not increase their wages upon marriage, and will not decrease wages upon separation, divorce, or widowhood, unless there are treatment effects on productivity of ceasing to be married.

According to the treatment hypothesis, men who marry will increase their wages upon marriage. If the treatment effect arises from household specialization, then the premium should disappear upon separation, divorce, and widowhood. If it arises from human capital accumulation, no changes in wages should occur post marriage. If it arises from more effort at market work, then there is no clear implication for post-marriage wages.

In the discrimination hypothesis, the animus mechanism does not give a clear implication for post-marriage wages. It would depend on whether there is animus against divorced men, and whether the animus is larger or smaller than against single men. Under the mechanism of statistical discrimination, there should be no decrease in wages in post-marital states, provided an employer can verify that the employee has been married, and hence has the characteristics that lead both to productivity and marriage, unless of course men who divorce have characteristics that are correlated with lower productivity.

Two of the hypotheses also have specific implications for the trend in the premium over time. Both the household specialization and the animus mechanism would imply lower premia over time, as the distribution of household work has become more equal and the amount of animus against single men probably has declined.

Additionally, according to the selection hypothesis, to the extent that the premium arises from sorting rather than from differential pay for same work for same employer, and this sorting is due to employee choices or to observable productivity, we should observe that the sorting occurs even prior to marriage. The high-productivity employees should sort themselves into the high-paying occupations and occupation-establishment units even before they get married. This would be evidence against discrimination from employers based on animus.

In summary, we have three separate hypotheses, and the second and third have respectively three and two separate mechanisms, producing different outcomes at different levels. This can become intricate, and to help focus ideas Table 1 summarizes the implications of the hypotheses.

(Table 1 about here)

2.6 SUMMARY OF RESEARCH EVIDENCE

There are three recent and excellent summaries of the relevant empirical evidence (Ribar 2004; Bardasi and Taylor 2005; Rodgers and Stratton 2005). Since our empirical aims are different from what has been addressed by research to date, only the central findings are discussed below.

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.

From a marital premium of as much as 15% in the U.S., net of other variables, the estimate of the percent of this premium attributable to selection varies, from 10–20% in research from early 1990s, to more recent estimates of 40–60% and even 80–100% (see Rodgers and Stratton 2005). For Britain, the selection effect accounts for 75% of the marital premium (Bardasi and Taylor 2005), and about 80% in Denmark (Datta Gupta, Smith, and Stratton 2005).³

If the selection effect cannot account for the entire marital premium, the remainder is by default attributed to treatment. Some papers investigate further 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 2005) and the issue is far from settled and may in the end be very difficult to settle. Married and single men’s household hours are however similar (Hersch and Stratton 2000, 2002). This provides *prima facie* evidence against the household specialization mechanism. Household hours as such reduce wages, but does not change the coefficient for the marital premium itself (Hersch and Stratton 2000). Blackburn and Korenman (1994) report a decline in the male marital premium over time, consistent with a more equal distribution of household labor over time, but 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. The most recent study reports that married men receive more on-the-job training, but that the marital premium does not decrease when adjusting for training (Rodgers and Stratton 2005). Partial evidence in favor of it, and against the household specialization thesis, is obtained when there are premia even to post-marital states.

As for 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. But to the extent that one finds limited evidence for the household specialization mechanism, but still a treatment effect, it seems congruent to infer that married men just work harder.

³Three studies use data on identical twins to assess selection versus treatment effects. One study finds that within pairs of twins, the twin who marries receives a wage premium of an entire 30% (Antonovics and Town 2004), while another study finds a difference of less than 1% (Krashinsky 2004), and a third study, using a large panel-data sample from Sweden, finds that the premium disappears once individual-level fixed-effects are introduced (Isacsson 2006). This intriguing research hence yields no coherent results.

The discrimination hypothesis is clearly the most difficult to investigate. Ideally it requires matched employer-employee, applicant pool, or employment audit data. Petersen and Togstad (2006) in a study of applicant pool data from a Norwegian bank find that married men receive job offers at a higher rate than single men. Korenman and Neumark (1991) provide perhaps the best evidence to date. Using company-level data they find that married men receive higher performance ratings than single men, and that once control is made for performance evaluation, the marital premium gets sharply reduced. This could be interpreted as higher productivity for married men or as discrimination in the performance rating. Jacobsen and Rayack (1996) argue that since there is an even larger marital wage premium among self-employed than employed men, the evidence is in favor of the claim that married men are more productive and against the claim that employers discriminate in favor of them. These premia disappear once individual fixed-effects are included: the marital differentials are zero percent among self-employed and five percent among employees.

A few studies address Scandinavia—Denmark and Sweden—but not yet Norway. Datta Gupta, Smith, and Stratton (2005) report 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 attribute the low premia to the more equal division of household labor in Scandinavia. For Sweden, Richardson (2002) shows 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 (2006) reports large selection effects in the longitudinal component of a large-sample Swedish twin study.

Some studies also address the male premia to having children. These premia are generally low. The most recent research shows premia of 0–6%: of 2%, 5%, and 6% for 1, 2, or 3+ children in Hundley (2000) from one data set but then smaller and insignificant premia in another data set. Stratton (2002) reports a miniscule, positive, and insignificant coefficient for presence of children in a cross-sectional analysis (Tab. A1), which turns negative at about 3% in a fixed-effects analysis (Tab. A2).

For Scandinavia, Datta Gupta, Smith, and Stratton (2005) report fixed-effects estimates from Denmark of having children of about 0.5%, and of having children 3–9 and 10–17 years old of negative 1%, with similarly small results in Datta Gupta and Smith (2002) in both a cross-sectional and fixed-effects analysis. The wage differential for having children is thus on average quite low in Denmark.⁴

No study has used matched employer-employee data to analyze the premia. These

⁴For Britain, Bardasi and Taylor (2005) report small effects of about 1.5% per child, but in fixed-effects analysis this drops to zero and insignificance.

are required for ascertaining whether there is different pay for the same work for the same employer, that is, whether productivity differences and/or discrimination could have arisen at that level. A partial exception is the organizational case study in Korenman and Neumark (1991). Nor has any study addressed the role of sorting on occupations and occupation-establishment units, due to lack of suitable data.

3 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, chap. 4). They include cash benefits for families with children. And most important, there is publicly supported childcare at relatively low cost and high quality.

With respect to parental leave, it was available for 18 weeks in 1977, 20 weeks in 1987, and 22 weeks in 1988, with 100% pay since 1978. Since 1977 fathers could share the leave except for the first six weeks which were reserved for the mother. 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 four of those weeks are reserved for the father, whereas six weeks are reserved for the mother (Leira 2002, pp. 89, 95). Among fathers, 69% took paid parental leave in 1996, and about 7% of parental leave days were taken by fathers (Leira 2002, pp. 86, 91).

With respect to child care, 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 in Norway (Leira 2002, p. 62). Single parents pay lower fees.

While the policies on average impact women more than men, by making it easier to combine family and career, their impact on male behavior can also be substantial. With mothers more likely to be employed, and with cultural pressures on fathers to become more involved in household activities, the benefits to men from potential household specialization are potentially lower than in other countries.

Data

We use matched employee-employer data on entire populations of white-collar employees in central sectors of the Norwegian economy in the period 1980–97. These allow us (1) to 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) to assess the role of sorting, and (3) to analyze wage growth and promotions between years. Information is available on about 100,000 employees (70,000 men) and 3,000 establishments each year. We can follow the establishments and their employees from year-to-year, about 1.2 million person-years for the men. We restricted the analysis to men 20–50 years old, yielding about 900,000 person-years. For each employee we have information on sex, occupation, rank in occupational hierarchy, age, part- versus 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 detailed educational attainment (length and type, 4 digit code), family or civil status (8 statuses), number and ages of children and adoptions. This gives annual educational, marital, and parental histories up to year 2000.

The data were collected from individual-level records kept by the establishments and compiled by the Norwegian Central Bureau of Statistics and the main employer's association in Norway, the Confederation of Business and Employers (NHO). Norwegian employers are bound by law to collect and report the data (e.g., Central Bureau of Statistics 1991, pp. 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.⁵

These data on white-collar employees cover all occupational groups with a few exceptions: CEOs, working supervisors, top editors of newspapers, secretary to the editor of newspapers, and journalists. While working supervisors are excluded, supervisors in administrative positions are included.

The data come from a variety of industries: manufacturing, oil extraction, mining, quarrying, transportation, storage, communication, and various other industries. Most of the industries outside the manufacturing sector are relatively small, but the hotel and research sectors count 2,201 and 4,771 employees respectively in our data in 1990. This grouping of industries is used by the Norwegian Central Bureau of Statistics. It is the first sector to carry out wage negotiations and is thus central for wage setting in other sectors, and is typical of other major sectors in the economy. For our purposes it is a strategic sector. Of the seven sectors from which gender wage gaps were computed for 1990, it had the largest gaps at all levels, also the occupation-establishment level (Petersen, Snartland, Becken, and Olsen 1997). This ensures

⁵The data are quite complete. For example, for the year 1992 we have complete data on 84% of the establishments and 94% of their white-collar employees.

variation in the dependent variable especially at the occupation-establishment level, which could also show up in marital and parenthood premia.

From the contractual monthly earnings and contractual hours worked we computed the hourly wage, which then refers to hourly wages paid on regular work hours, hence not mixing pay on regular and over-time hours. Five marital statuses are distinguished: single, married, separated, divorced, and widower. Among the married, separated, and divorced, we include a few hundred employees in same-sex unions that were still intact (“married”), “separated”, and “divorced”; these are legal categories in Norway. We coded three dummy variables for number of children aged 20 or younger: for one, two, or three or more such 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 analyses.

The occupational code is quite detailed, with 201, 210, and 209 occupations in 1980, 1990, and 1997. We use data on employees in 155 of these occupations, for the simple reason that for those occupations a simple aggregation of 21 occupations exists, an aggregation which allows us to investigate promotions between years. It makes no substantive difference for the results whether we use 21, 155, or 210 occupations. The gaps between groups are slightly reduced when more occupations are used, but not the pattern of results. We used the coarser grouping of 21 occupations so as to avoid the large loss of number of observations when computing fixed-effects estimators at the occupation-establishment level. We need variation at the occupation-establishment level not only in marital status, but also in whether employees have 1, 2, or 3+ children aged 20 or younger.

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. This choice followed from the same logic as when using only 21 occupational groups.⁶

In the promotion analysis we utilize the information on 21 occupational groups. These are divided into five career ladders with respectively 9, 3, 5, 2, and 2 steps. For employees who remain in same career ladder between two adjacent years, and who are employed below the top step in the ladder, we can analyze promotions.

Table 2 provides descriptive statistics for our key variables, with annual averages reported separately for each of four periods, 1980–84, 1985–89, 1990–94, and 1995–

⁶Results using the full set of 155 or 210 occupations and the fuller set of 21 educational groups are available from the authors upon request.

97. Wages are 18–19% higher for married and previously married men compared to single and childless men. Single men and childless men are however promoted at higher rates, but no control is here made for labor force experience. On average employees are observed for nine years.

(Table 2 About Here)

Our data suffer from one significant weakness. We do not know which men are cohabitating. For the men who are recorded as single, some are truly single, others are cohabitating. Cohabitation is important in Norway, increased over the period 1980–2000, and is more common in younger cohorts (Noack 2001). In 1990, about 58% of Norwegian men aged 20–66 were married and another 6% were cohabitating, with the remaining 36% being single. In our data, 25% percent are recorded as single in 1995–97, but about one in six of the singles are probably cohabitating, which would yield a correct percent single around 21. While we are not aware of any Norwegian studies investigating wage premia for cohabitators, there are such premia in Sweden of about 3% and Denmark of 2% (Richardson 2002; Datta Gupta and Smith 2002).

Which biases arise from this misclassification? To the extent that cohabitators enjoy wage premia similar to married men, we will in the cross-sectional analysis overestimate the wages of single men, while still correctly estimating the wages of married men, and thus underestimating the wage differential between the two groups. The marital premium then gets systematically underestimated. To the extent that cohabitating men are more like single men in their economic success, there is no problem. Cohen (2002) documents this type of bias using U.S. data.

The misclassification of cohabitating men as single men makes less of a difference in the intra-individual analysis, when we compare men’s wages before and after marriage. Some of the men will then be compared to themselves after transitioning from cohabitating to marrying, but we will also have data on them while they were single. The problem is not entirely solved, but is probably not as strong as in the cross-sectional analysis.

4 METHODS

The data have a unique multilevel structure. One level arises from the across-time dimension, 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 in 1980–1997. This gives a standard panel data set-up (e.g. Hsiao 1985; Petersen 2004). Similarly, each establishment is ob-

served at several points in time, as much as every year in 1980–1997. In a given year, we can account for the clustering of employees into establishments, occupations, and occupation-establishment units, using standard fixed-effects procedures. 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, and additionally we can account for the fact that some employees remain in the same establishment, occupation, or occupation-establishment unit.

For each of three dependent variables, we report a sequence of four regression equations. Each equation includes independent variables for education and imputed labor force experience plus dummy variables for marital status and dummy variables for the number of children below age 20. The first regression equation does not take into account where the employees work nor their occupations, the second controls for the establishment (workplace), the third for the occupation, and the fourth for the occupation-establishment unit. The second, third and fourth specifications are estimated using fixed-effects procedures. The four specifications will be referred as the *Population, Establishment, Occupation, and Occupation-Establishment* estimators.

Each coefficient estimated is significantly different from zero usually at a high level, often with z- or t-statistics of 40–50 and significance levels typically of .000001 or better. No point is served in reporting these significance levels. The gigantic z-statistics reflect the large number of observations each year, not superior model specification.

The estimated equations and technical details are given in Appendix. Below we give a verbal account.

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 bonuses or penalties in wages are present when same work is done for the same employer.

The equations are estimated separately for each of the 18 years in the data. This allows us to assess possible changes over time, as implied by two of the hypotheses. To simplify presentation, we report the averages of the coefficients within each of four time periods, 1980–84, 1985–89, 1990–94, and 1995–97.

The 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 (Joshi and Paci 1998, p. 160).

Accounting for Selection Effects

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

In a first analysis we selected only employees who in a given year are single and childless. The variables for current marital and children status are then excluded. But we introduce two new dummy variables, one for whether the employee some time in the future got married and another for whether the employee eventually had children, called “ever married” and “ever children”, each of them coded 1 for employees who ever were married or ever had children during the period 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.

In a second variant we used information on all employees, but for the employees who eventually got married and/or eventually had children, we introduced 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 give the total effect of marriage and children, comparable to the analyses where 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 estimate next 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 etc., 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.

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 where all the dummy variables “vanish” from the estimation procedure. And with about 60,000–80,000 individuals each year, and some 20,000 occupation-establishment units, estimating the effects of all the dummy variables may be impossible. No computer software known to us can handle this. Our solution was to adapt a simple procedure proposed by Goux, Dominique, and Maurin (1999). We create an interaction term between the individual-level dummy variable and subsequently for the establishment, occupation, and occupation-establishment dummy variables. This creates a fixed effect specific to the individual and say the occupation-establishment unit in which he works. If the individual changes occupation-establishment unit, a new dummy variable pertaining to that individual 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 then due to selection.

The two sets of analyses, of selection and treatment, may give somewhat different results regarding their relative importance. When estimating selection effects, we make comparisons to individuals 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 first analyze changes in wages from one year to the next among those employees who stayed in the sector in two adjacent years and remained in the same establishment, simply because we now primarily are concerned with what occurs within firms.

The dependent variable is the change in logarithm of wages from one year to the next. The same set of models as for the wage levels are estimated.

Next we analyze promotions in occupational rank, for employees who remained in (a) the sector between two adjacent years, (b) the same establishment, and (c) the same career ladder, provided they had not reached the top of the ladder. Our data allow us to investigate promotions within career ladders, where a hierarchy of occupations is defined, but not between ladders. In order to facilitate communication of results, we estimate a linear probability model for promotion, where coefficients can be interpreted directly as giving the difference in proportion promoted adjusting for the other variables. Computationally it is also easier to estimate than a logit model with fixed effects, given the large number of occupation-establishment units. We report the same set of models as in the case of wage changes. The difference is that the dependent variable now is binary, equal to 1 if a promotion occurred in two adjacent years and equal to 0 if not.

How to Think About the Various Sets of Coefficients

How should one then think about the various estimates we report? It is tempting to assume that the estimates including the most detailed set of fixed effects are the better ones.

We underline instead that it is not necessarily the case that one estimator is better than another. A more fruitful way to think about the estimators is that they report on different aspects of the data. No estimator is then necessarily better, they just answer different questions. The population-level estimator reports what on average is the case when all individuals are compared, without making distinctions about where they work and what type of work they do, both of which, in contrast, are taken into account in the occupation-establishment estimator. It reports what on average is the case at the occupation-establishment level. For example, one may find in the population-level estimator that there is a big positive effect on wages of being married, whereas at the occupation-establishment level there is no such effect. This would correctly indicate that married men earn higher wages than single men, but that once employed—married or single—in the same occupation in the same workplace, then there are no differences in wages. The reason for the premia in the population-level estimator is that married men tend to work in higher-paying occupation-establishment units than single men.

By comparing changes in coefficients as one goes from the population-level estimator to the occupation- to the occupation-establishment-level estimators one will be able to assess at what levels differences between groups arise: From differential

wages at say 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 transitions at the individual level from being single to married, from having 0 to 1 child, etc., on average impact the individual's wages. We no longer make comparisons between individuals, comparing say single to married, we rather make comparisons of wages at the within-individual level between when they were single and when they were married. Both types of comparisons are relevant to make, and none is better than the other. They just address different questions, and we need to focus on the estimator that best answers the corresponding question.

5 THE WAGE GAP BY MARITAL STATUS AND CHILDREN

Total Effects on Wage Levels

Table 3 reports the coefficients on wages of marital status and number of children below age 20, adjusting for education and imputed labor force experience. At the population level, with analyses comparable to results already reported in the literature, the effects of marital status are similar across the four time periods: Wages are 6–7% higher for married than 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: About 0% for 1 child, and about 1–2% for 2 or 3+ children, a fairly minimal differential.

(Table 3 about here)

What happen to these differentials as one successively controls for establishment, occupation, and the occupation-establishment unit? The effects are again consistently similar across the four time periods, at each of the three levels of controls. Controlling for establishment only slightly reduces the effects of marital status. The children effects even increase a bit, meaning that fathers tend to work in somewhat lower-paying establishments than non-fathers.

Controlling however for either occupation or occupation-establishment results in large reductions in coefficients. For marital status the premia are down to 2–3% at the occupation level and to 1–2% at the occupation-establishment level, a reduction of 70–85%. At the occupation-establishment level there are no effects of having children, or at most small positive effects of 0.5–1.0%, with the exception of the period 1980–85 when the effects were 1.3–1.4%.

What can we conclude from this? The effects of marital status, and to a lesser extent of children, work mostly through the sorting of employees on occupations and occupation-establishment units: 60–90% of the premia are due to sorting. Married or previously married men, and men with children, work in different and better-paying occupations and occupation-establishment units than single and childless men. But once these groups work side-by-side, they receive practically the same pay. Employers do not pay men with family obligations more.

The marital premia did not decline over time. This is evidence that they are not due to animus from employers against single men, since any animus almost surely must have declined over the period.

Are Men Who Marry and Have Children Different?

Are the men who marry and/or have children different from those who remain single and/or without children, so that the former group would earn more even in absence of marriage or parenthood and even prior to these? Or are the effects due to changes in behavior, such as increased work effort and occupational aspirations, induced from marriage and parenthood?

Table 4 answers the question about selection effects from 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. We focus on the results for 1980–84 and 1985–89, since the window for eventually getting married and becoming a parent is short from 1990 and later years, which would lead us incorrectly to classify many single and childless men as always being single and childless.

(Table 4 about here)

At the population level the effects are clear: In 1980–89, even among single men, the men who eventually marry earn higher wages than those who don't, a premium of about a 5%. The effect is smaller in later years. But this has probably nothing to do with changes in the magnitude of the selection effect, but rather reflects the fact that many of the men who eventually marry are misclassified as never marrying in the later period, thus diluting the estimated wage advantage.

At the occupation level there is a small advantage to eventually getting married, of about 2% in 1980–84, and of less than 1% at the occupation-establishment level.

There are practically no or very small selection effects of ever becoming a parent.

The conclusion is that at least 50% of the marital premium at the population level is due to selection. Men who eventually marry are different from men who don't 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 wage advantages. Employers do not recognize with higher wages the productivity advantages the men who eventually marry supposedly have.

The finding that sorting into the higher-paying occupations and occupation-establishment units occurs even prior to marriage is important. This is clear evidence that the marital premium is due to choice from employees 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 have children (i.e., selection) from the additional effects of actually being married and/or having children (i.e., treatment).

At the population level, focusing on the periods 1980–84 and 1985–89, for marital status the selection effect is about as large as the treatment effect; the coefficients for “ever married” and married are of same size. Men who in a given year are single, but who eventually marry, earn a wage premium of 3.5%. Once they actually marry, an additional premium of 3.5% is earned. The total premium is 7%. This squares well with Table 3, where the premium for being married is 6.4% [=exp(.062)].

For children there are mostly selection effects, in each of the four periods. But they are small.

But what are the results at the occupation and occupation-establishment levels? At especially the latter level there are practically no selection effects of eventually marrying. There is a small treatment effect of being married (1.0–1.5%), constituting about 80% of the total but very small marital effect at that level. At the occupation-establishment level employers do not recognize with higher wages the presumed productivity advantages of men who eventually marry, but men who actually are married receive higher pay: Being marriageable does not help, being married does. If married men really are more productive, then the 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 finally report an analysis of within-individual dynamics. This describes how wages evolve as the men move from one marital status to another and from being childless to having children. We perform a fixed-effects analysis for the individual. The results are given in Table 5.

(Table 5 about here)

At the population level, there are small effects of marital status: As men move from being single to being married to being separated the wages, at the within-individual level, increase with 0.5% and then with another 1.1% ($=1.6-0.5$). At the occupation and occupation-establishment levels these effects are even smaller, close to 0%. The effects of children are similarly small.

This analysis shows that the treatment effects of marital status and children are small: An individual's wages increase with only 0.5% upon marriage.

One may ask why the intra-individual comparisons are different from the comparisons between individuals. The reason is simply that when making intra-individual comparisons we adjust for a much larger set of variables, in that an individual gets compared to himself in two or more years, and we thus account 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, changed behavior upon marriage, appears to be less important. The selection effect works through sorting: The men who eventually marry, or 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 same occupation-establishment unit. Differences amount to at most 1.0–1.5% at the occupation-establishment level.

6 WAGE GROWTH AND PROMOTIONS

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 can not address hiring, but we can investigate wage growth and promotion processes.

In addressing wage growth and promotions we are restricted to looking at employees who remained in the sector in two adjacent years. We use the subset of employees

who also remained in the same establishment, and for the promotion analysis we restrict the sample further to those who also remained in the same career ladder; the latter pertains to 71.1% of the employees. The restrictions were made since promotion essentially is a firm-internal process. Wage growth and increases in occupational rank across establishments involve both a departure and a hire.

Panel A of Table 6 gives the coefficients for marital status and number of children below age 20 on growth in wages between two adjacent years. Each regression is estimated separately by year, for 17 years, but not for 1997 to 1998, since we don't know wages and occupations in 1998. As before, we have averaged the coefficients across years within four separate periods: 1980–84, 1985–89, 1990–94, and 1995–97.

(Table 6 about here)

The coefficients for marital status are in substantive terms equal to zero, in all years and at all levels. There are premia for parenthood of about 1% for 2 and 3+ children in 1980–89, but these premia had disappeared by 1990–97.

Panel B of Table 5 gives the corresponding results for promotions. Married and previously married men are promoted at a higher rate than single men, especially so in 1980–89. Even in 1990–97 there are promotion advantages of 1% at the occupation-establishment level. Recall that without control for education and imputed labor force experience this was not the case at the population level (see Table 2). There are no promotion premia to parenthood.

To put the promotion results into context, among employees who remained in the sector between two adjacent years, and who stayed in same establishment and career ladder, on average 6.0% were promoted. At the occupation-establishment level, comparing married to single men the former were promoted at a rate that was 2–3% higher in 1980–89 and about 1% higher in 1990–97. A difference of 1% in promotion rates is not large, but not trivial either, given the overall promotion rate. If promotion rates are 7% and 6% for married and single men, the odds of promotion are 17% higher for married men (odds ratio of 1.17); the logit coefficient for being married would be .165. The drop over time in the marital promotion advantage is consistent with a decline in preferences for married over single men, but also with a decline in the institution of marriage and increased importance of cohabitation.

In summary, there are no wage growth premia to marital status but a small one to parenthood status in 1980–89. There are however clear promotion premia to being married or previously married, even at the occupation-establishment level, though none for parenthood. The promotion premium to marriage has gone down over the period.

There is evidence then of promotion differences by marital status at the occupation-establishment level. These could be due to discrimination or to treatment. The promotion premium goes a long way toward explaining the marital wage premium found in the population-level estimators and the lack of premium at the occupation-establishment level. The married men end up in the higher-paying occupation-establishment units in part through higher promotion rates.

7 CONCLUSION AND DISCUSSION

We have investigated whether the male marital and parenthood premia arise due to differential pay by employers or from differential sorting of employees on occupations and establishments. We investigated these premia in Norway in the period 1980–97, a country where public policy has 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. The marital premium is lower than what often is found in the U.S, but with no decline over time.

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

Second, 50–75% of the marital premium is due to selection. Men who get married experience wage advantages even prior to marriage. This pre-marriage premium is earned in its entirety through sorting on occupations and occupation-establishment units. The fact that men who get married sort into the higher-paying occupations and occupation-establishment units even prior to marriage is prima facia 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 basis of future marital status is not feasible.

Third, according to the within-individual fixed-effects analysis, getting married and becoming a father changes wages only marginally. From this analysis, particularly designed for assessing treatment effects, only 10–15% of the marital effect is due to treatment, and the remainder to selection.

Fourth, almost the entire premium for children is a selection effect, but it was so small to begin with that further partitioning of it makes limited sense.

Fifth, there are no marital premia for wage growth within establishments. There are however substantial promotion premia to marital status early in the period, and still noticeable premia in 1990–97. This leads one to the conclusion that the wage premia between single men and married or previously married men observed in the population-level analyses to a large extent are attributable to higher within-establishment promotion rates for married men.

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. The fact that this sorting occurs even prior to marriage (and parenthood) is evidence that the premia arise due to choice or due to observable higher productivity: 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 in favor of married men. To the extent that these observed premia, prior to control for sorting processes, can be attributed to actions by employers, they must arise either at the point of hire or in subsequent promotions, so that married men and/or fathers have an advantage in those processes. We have no information on applicant pools, and could thus not address hiring, but our analyses of wage growth and promotion processes show that part of the marital premium arises due to higher promotion rates for married men, especially so earlier in the period analyzed.

A caveat must be pointed out with respect to the interpretation of the selection versus 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 have children, 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 in the regressions is adaptive behavior to expected future events. This is consistent with the the intra-individual analyses: employees do not increase by much their wages 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 however clear 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. The empirical fact is solid. Its exact interpretation 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:

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

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

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

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

where η_{et} , η_{ot} , and η_{oet} are fixed effects (i.e., of dummy variables) capturing establishment e , occupation o , and occupation-establishment unit oe , and 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, etc.

Accounting for Selection Effects

These analyses are described in sufficient detail in 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 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). Also define a dummy variable for the year D_t . For the four levels, we conduct the following sets of analyses:

$$\ln w_{it} = \alpha_{IP}x_{it} + \delta_i D_i + \gamma_t D_t + \varepsilon_{it}, \quad (\text{A5})$$

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

$$\ln w_{it} = \alpha_{IO}x_{it} + \delta_o D_o + \delta_{i|o} D_i + \gamma_{O,t} D_t + \varepsilon_{iot}, \quad (\text{A7})$$

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

where δ_i , δ_{ie} , δ_o , $\delta_{i|o}$, and δ_{ioe} are fixed effects, comparable to the regressions in (A1)–(A4), while the γ 's are fixed effects for the dummy variables for year. The subscripts

IP, *IE*, *IO* and *IOE* to the main α 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 δ_{ie} , but when the establishment is changed, from say $e=1$ to $e=2$, the fixed effect changes from δ_{i1} to δ_{i2} .

In the third specification (A7), we include separate sets of dummy variables for occupation and for the person. We can do this, since in our analysis we decided to settle on reporting the results using only 21 occupational groups. With so few occupational groups, we can estimate occupation effects plus account for the fixed effects for the tens of thousands of employees.

The fourth specification (A8) corresponds to the second, except that we here 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. This is cumbersome, but is currently probably the simplest way simultaneously to take account of the individual- on the one hand and the establishment-, occupation-, or occupation-establishment-level fixed effects on the other hand. For example, it would have been computationally infeasible to estimate the fixed effects of separate dummy variables for persons and establishments, several tens of thousands of the former and several thousands of the latter.⁷

Methods for Analyzing Individual Career Dynamics

These analyses are described in sufficient detail in text. The same set of analyses as in (A1)–(A4) are reported. The dependent variables differ and the sample is restricted to employees present in at last two adjacent years.

⁷Abowd, 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. It appears to work well for the data they analyze.

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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

	Selection	Treatment			Discrimination	
		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, the empirical implications differ in 5 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 2 and 5 give same implications, but for different reasons: more equal distribution of household labor in Cell 2, less animus against single men in Cell 5.

Table 2
 Descriptive Statistics: Computed Separately by Years, But Averaged Across Years within
 Each of Four Periods (1980-1984, etc.)

	1980-1984	1985-1989	1990-1994	1995-1997
Percent:				
Single	15.53	20.07	22.51	26.13
Married	79.26	73.52	69.79	65.62
Widowed	.25	.25	.27	.28
Divorced	3.15	4.19	5.21	5.96
Separated	1.80	1.97	2.22	2.02
No children	25.35	28.71	29.33	29.83
First child	19.06	20.43	22.20	21.19
Second child	37.81	35.64	33.60	32.54
Third+ child	17.78	15.22	14.86	16.44
Basic education	60.85	58.00	53.22	49.94
College	7.46	8.06	9.95	11.18
Graduate	2.03	3.28	4.87	5.87
Professional	24.34	25.43	27.63	29.48
Unknown	5.32	5.23	4.33	3.54
Percent Promoted Among:				
Single	12.89	9.87	9.48	10.70
Married	9.35	7.37	7.06	7.28
Widowed	6.05	4.21	5.64	7.95
Divorced	7.95	5.99	5.56	6.36
Separated	8.18	6.90	5.70	6.75
No children	12.27	9.68	8.56	9.67
First child	9.71	7.50	7.39	7.26
Second child	9.16	7.16	6.81	7.63
Third+ child	8.16	6.88	7.14	7.23
Wage relative to singles/childless:				
Married	1.20	1.20	1.19	1.20
Widowed	1.17	1.19	1.16	1.17
Divorced	1.18	1.16	1.14	1.13
Separated	1.18	1.19	1.15	1.15
First child	1.08	1.09	1.08	1.07
Second child	1.16	1.16	1.14	1.15
Third+ child	1.17	1.17	1.17	1.19
Experience				
mean	16.44	16.71	17.43	17.52
sd	7.83	7.94	7.80	7.73
N person-years	241277	257754	271703	155867
N individuals	80371	88795	86384	66941
N occupations	21	21	21	21
N establishments	3810	4057	4293	3468
N occ-est	23076	23746	33292	17738

Note: The statistics above have been computed separately for individual-years within each of four periods (1980-84, 1985-89, 1990-94, 1995-97). We computed the distributions (in percent) on marital status, parenthood status, educational attainment, and also means and standard deviations for experience. For employees who were present in data in at least two adjacent years, we computed the percent promoted between two years for each marital status and for each parenthood status. 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 four 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-establishments units. Employees are observed one average for a period of 9 years.

Table 3

Effects of Marital Status and Children Under Age 20 on Logarithm of Hourly Wage in Four Time Periods and for Four Different Levels: Population, Establishment, Occupation, and Occupation-Establishment.

	1980-1984				1985-1989				1990-1994				1995-1997			
	Pop	Est	Occ	Occ- Est	Occ	Est	Occ	Occ- Est	Pop	Est	Occ	Occ- Est	Pop	Est	Occ	Occ- Est
Married	.062	.053	.027	.020	.070	.058	.027	.019	.075	.059	.025	.015	.073	.059	.022	.016
Divorced	.039	.018	.028	.010	.045	.020	.027	.008	.046	.028	.024	.009	.041	.026	.019	.008
Widowed	.035	.034	.017	.008	.061	.043	.035	.012	.051	.046	.020	.020	.046	.042	.002	.002
Separated	.054	.032	.027	.010	.067	.044	.031	.013	.062	.043	.031	.017	.053	.042	.019	.013
First child	.001	.012	-.001	.006	-.003	.006	-.005	.002	.001	.010	-.002	.002	.003	.010	-.001	.004
Second child	.022	.034	.005	.013	.008	.019	-.002	.006	.008	.019	.000	.006	.019	.023	.003	.007
Third child	.020	.035	.000	.014	.009	.022	-.003	.010	.015	.024	.001	.009	.028	.029	.003	.009

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 1 child under age 20, 2 children under 20, or 3 or more children under 20. In the column denoted “Pop”, no further controls are introduced. In the columns denoted “Est”, “Occ”, and “Occ-Est”, we control by dummy variables, as so-called fixed effects, for the establishment the employee worked in, for the occupation worked in, and for the occupation-establishment unit worked in. The estimates are obtained separately for each of 18 years in the period 1980-1997. The table reports the average of the yearly coefficients for four subperiods, 1980-1984, 1985-1989, 1990-1995, and 1995-1997. Each yearly coefficient, with some minor exceptions, is statistically significantly different from zero at a very high significance level, usually with z- or t-statistics in the 40-50 range. The analysis is restricted to employees 20-50 years old.

Table 4

The Effect of Current Marital Status and Current Children as Well as Future Marriage and Children on the Logarithm of Hourly Wage.

	1980-1984				1985-1989				1990-1994				1995-1997			
	Pop	Est	Occ	Occ-Est	Pop	Est	Occ	Occ-Est	Pop	Est	Occ	Occ-Est	Pop	Est	Occ	Occ-Est
Childless singles																
Ever married	.046	.022	.026	.008	.044	.028	.020	.009	.030	.013	.009	.003	.027	.012	.011	.011
Ever children	.003	.004	-.003	.001	.014	.013	.006	.005	.021	.020	.010	.011	.023	.022	.002	.010
Overall population																
Married	.036	.039	.015	.017	.040	.039	.014	.014	.046	.042	.015	.012	.040	.036	.019	.014
Divorced	.014	.005	.015	.007	.014	.001	.014	.004	.018	.011	.014	.006	.007	.003	.016	.007
Widowed	.009	.020	.004	.005	.030	.025	.022	.007	.022	.029	.011	.017	.019	.019	-.002	.000
Separated	.028	.018	.014	.008	.036	.025	.018	.009	.034	.026	.021	.013	.020	.029	.016	.011
First child	-.008	.005	-.004	.004	-.014	-.004	-.009	-.001	-.010	-.002	-.005	-.001	-.009	-.009	-.003	-.003
Second child	.012	.027	.001	.011	-.004	.009	-.007	.003	-.003	.007	-.003	.003	.007	.004	.001	.000
Third child	.009	.028	-.004	.012	-.004	.012	-.008	.006	.004	.012	-.001	.005	.016	.010	.001	.002
Ever married	.035	.017	.019	.003	.037	.021	.017	.005	.030	.014	.011	.003	.032	.019	.003	.000
Ever has child	.018	.013	.006	.003	.024	.020	.009	.007	.020	.023	.004	.007	.017	.027	.003	.010

Note: The top panel (labeled “childless singles”) reports regression coefficients estimated for employees who in the given year are single and have no children. The dummy variable for Ever Married indicates whether the employee eventually got married (=1) or not (=0), within the time frame of the data. The dummy variable for Ever Children indicates whether the employee eventually had children (=1) or not (=0) within the time frame of the data.

This second panel (labeled “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/or having children under age 20 in at least one year in the time-frame of the data. 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 control by dummy variables, as so-called fixed effects, for the establishment the employee worked in, for the occupation worked in, and for the occupation-establishment unit worked in. The estimates are obtained separately for each of 18 years in the period 1980-1997. The table reports the average of the yearly coefficients for four subperiods, 1980-1984, 1985-1989, 1990-1995, and 1995-1997.

Table 5
 Effects of Marital Status and Children Under Age 20 Controlling for Individual-Level Fixed Effects, Controlling for Experience, at four Different Levels, Population, Establishment, Occupation, Occupation-Establishment.

	Individual	Est	Occ	Occ-Est
Married	.019	.014	.013	.011
Divorced	.010	.005	.007	.003
Widowed	.007	.005	.006	.005
Separated	.016	.012	.011	.009
First child	.002	.000	.000	-.001
Second child	.004	-.001	-.001	-.002
Third child	.004	-.002	-.002	-.004

Note: In these analyses an individual-level fixed effect is included in each column. Where establishment-level fixed effects additionally are included, this is done by interacting the establishment- and the individual-specific dummy variables. As long as an individual remains in the same establishment, the fixed effect remains the same. When the person changes establishment, then 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-establishment level) fixed effects is adapted from Goux and Maurin (1999). Controlling separately for the individual- and establishment-level fixed effects would have led to equations not estimable by current software; there would be too many dummy variables to take into account. In the analysis controlling for occupation-level fixed effects, the dummy variables for occupation have been controlled in addition to the dummy variable for the individual, yielding one set of dummy variables for the individual and another for the occupation, without interacting the two.

Table 6

Effects of Marital Status and Children Under Age 20 on Change in Logarithm of Hourly Wage and Promotion in Occupational Rank Between Two Adjacent Years in Four Time Periods and for Four Different Levels: Population, Establishment, Occupation, and Occupation-Establishment Controlling for Education and Experience.

	1980-1984				1985-1989				1990-1994				1995-1997			
	Pop	Est	Occ	Occ-Est	Pop	Est	Occ	Occ-Est	Pop	Est	Occ	Occ-Est	Pop	Est	Occ	Occ-Est
Wage Change																
Married	-.0021	-.0019	-.0021	-.0023	-.0010	-.0004	-.0006	-.0001	.0001	-.0007	.0001	-.0008	-.0012	-.0015	-.0015	-.0018
Divorced	-.0014	-.0016	-.0012	-.0019	-.0007	-.0004	-.0005	-.0002	-.0005	-.0007	-.0004	-.0012	-.0005	-.0017	-.0003	-.0005
Widowed	-.0021	-.0017	-.0023	-.0032	.0003	.0016	.0010	.0023	.0026	.0016	.0005	-.0016	-.0026	-.0016	-.0027	-.0049
Separated	-.0010	-.0018	-.0006	-.0015	.0004	-.0010	.0000	-.0010	.0001	-.0005	-.0003	-.0021	.0007	.0009	-.0006	-.0001
First child	.0002	.0011	.0001	.0013	.0013	.0008	.0011	.0008	-.0002	-.0006	-.0002	-.0002	-.0020	-.0012	-.0019	-.0007
Second child	.0091	.0096	.0090	.0095	.0081	.0073	.0077	.0074	.0032	.0029	.0028	.0027	.0026	.0030	.0025	.0023
Third child	.0079	.0097	.0083	.0094	.0120	.0101	.0099	.0094	.0035	.0031	.0029	.0036	.0016	.0027	.0008	.0013
Promotions																
Married	.0172	.0158	.0265	.0296	.0151	.0130	.0229	.0230	.0097	.0087	.0173	.0182	.0040	.0005	.0135	.0119
Divorced	.0138	.0103	.0204	.0166	.0120	.0074	.0163	.0077	.0063	.0061	.0110	.0125	.0089	.0067	.0154	.0119
Widowed	.0135	.0219	.0254	.0400	-.0015	-.0032	.0057	.0097	.0107	.0039	.0170	.0012	.0217	.0154	.0291	.0138
Separated	.0118	.0076	.0199	.0154	.0162	.0132	.0221	.0213	.0010	.0001	.0065	.0086	.0040	.0006	.0132	.0081
First child	-.0047	-.0034	-.0008	.0007	-.0026	-.0011	-.0009	.0007	.0041	.0037	.0053	.0055	-.0024	.0006	-.0004	.0049
Second child	-.0027	-.0020	.0041	.0057	-.0018	-.0003	.0017	.0029	.0010	.0002	.0042	.0055	.0041	.0055	.0080	.0121
Third child	-.0025	-.0009	.0070	.0090	-.0011	-.0011	.0030	.0026	.0056	.0020	.0090	.0092	.0025	.0009	.0072	.0067

Note: The regressions also 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 1 child under age 20, 2 children under 20, or 3 or more children under 20. In the column denoted "Pop", no further controls are introduced. In the columns denoted "Est", "Occ", and "Occ-Est", we control by dummy variables, as so-called fixed effects, for the establishment the employee worked in, for the occupation worked in, and for the occupation-establishment unit worked in. The estimates are obtained separately for each of 18 years in the period 1980-1997. The table reports the average of the yearly coefficients for four subperiods, 1980-1984, 1985-1989, 1990-1995, and 1995-1997. The wage change and promotion analyses were restricted to employees who stayed in the same establishment between two adjacent years. The promotion analysis was additionally restricted to those who were in the same career ladder between two adjacent years.