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TITLE

The Motherhood Wage Penalty: Sorting Versus Differential Pay

ABSTRACT

The motherhood wage penalty is today probably the largest obstacle to progress in gender equality at work. Using matched employer-employee data from Norway (1980–97), a country with public policies that promote combining family and career, we investigate (a) whether the penalty arises from differential pay by employers or from sorting of employees on occupations and establishments, and (b) changes in the penalties over time in a period with major changes in family policies. The findings are as follows. (1) There are major wage penalties to motherhood, but these declined strongly over the 18-year period, likely caused by changes in family policies and in how families operate. (2) The penalty to motherhood is mostly due to sorting on occupations and occupation-establishment units. By 1995–97, mothers and non-mothers working in the same occupation-establishment unit were paid same wages. (3) Women who become mothers are wage wise positively selected, but the premia are wiped out by the negative effects of actual motherhood. (4) For wage growth, there were premia to motherhood in 1980–89, but none by 1990–97. In conclusion, the motherhood penalty is not due to employers paying mothers lower wages and its size appears sensitive to changes in family policies, with large reductions in penalties over time.

1 INTRODUCTION

The movement for gender equality in the workplace addressed equal pay for equal work, equality in hiring and promotion, and in some countries equal pay for work of equal value. By the end of the twentieth century it was widely recognized that while major progress has been made in the first two of those three domains, significant obstacles to further progress arises today from a different source: the processes that occur in the family and their interrelationships with work. There is a sense of a serious roadblock, even a stalled revolution.

For men, marriage and to some extent children have positive effects on wages and careers. For women, the opposite is the case, there are small differentials for marital status but large penalties for having children. As the economic historian Eric Hobsbawm (2000, p. 136) reflects in a retrospective on the twentieth century: “There can be no doubt that the emancipation of women has been one of the great historical events of the twentieth century. The problem for the twenty-first is to establish what still has to be done, and what will probably happen.” He continues (p. 136): “There is, however, a serious problem, and it has become increasingly serious: the extraordinary difficulties for women of combining high professional posts with being mothers.”

Two key questions concerning this stalled revolution arise. Did it get stalled by employers, through differential treatment, possibly favoring married men and fathers, while discriminating against mothers? Or was it stalled by women and men from the adaptations they make between the family sphere and work, from differential household-division of labor and preferences for different lifestyles? This paper explores competing explanations for the most significant part of this puzzle: the motherhood wage penalty as well as the female marital premium. We address the role of employer discrimination against mothers and the extent to which the penalties can be ameliorated by family policies.¹

Three central hypotheses have been put forth to explain the penalty to motherhood and the bonus to marriage (Chiodo and Owyang 2003; Budig and England 2001, p. 204): The selection, treatment, and discrimination hypotheses.

The *selection* hypothesis argues that women who have children are less productive than women who don't, sometimes referred to as the unobserved heterogeneity hypothesis. The *treatment* hypothesis proposes that upon parenthood women become less productive: from lower work effort and/or loss of human capital accumulation. The *discrimination* hypothesis claims that employers treat women with children dif-

¹A separate paper addresses the male marital and parenthood premia, which have different structures, and probably different causes. Many of the issues are similar, but given the different empirical patterns and complexity of results, separating the findings facilitates their presentation.

ferently, due to taste and/or statistical discrimination and even nonconscious biases.

These processes and their effects are however amenable to change. Large-scale cultural transformations over the last 30 years have certainly affected family values and work-family priorities. A central goal of the women's movement was to change how families operate, by having men do more work in the home and more caring for children, thereby hopefully freeing up time for women to be active on an equal footing with men in the marketplace. But employers too have been affected by the cultural changes, and are probably more willing today to accommodate constraints arising from family obligations than 30 years ago. The processes can also be changed through family policies, many of which aim at facilitating combining family and career. These can shift the incentives and the feasibility for being active in market work for women, and hence also the bargains that are struck within the family.

Against this background we address several issues. The *first* objective is to assess the extent to which the motherhood penalty (and secondarily the marriage premium) arises from differential treatment by employers: (a) whether employers pay women differently according to motherhood and marital status, (b) the role of sorting of employees on occupations and establishments for the size of penalties (and premia), and (c) whether the penalties (and premia) are due to promotion and wage growth differentials. We cannot address discrimination in hiring, but we can assess the role of pay and promotion penalties to motherhood, two important aspects of potential differential treatment in the workplace. The *second* objective is to assess how these penalties have evolved over time during a period when significant family policies have been unrolled. To this end we use matched employer-employee data from Norway in the period 1980–1997. Our data enable us to provide entirely novel and crucial empirical angles, by documenting where the penalties arise, at the level of the employer in how they pay and promote mothers and non-mothers, or in how employees are sorted on establishments and occupations. The longitudinal aspect of the data allows us to investigate the change over time in the penalties.

Why are these questions important? They are important because if the roadblocks arise at the level of employers, or fail to arise there, then there are significant public policy implications. Employers are easier to regulate than families, and if gains can be had by further regulation of employers, then this should be done. If employers are not a culprit, an increased emphasis on family policies is warranted. The question then arises as to whether such policies have had effects in settings where they have been tried on a large scale. Nowhere have family policies been more extensive than in Scandinavia. Major policies to reinvent the family were rolled out over the last 20–30 years. But have they worked? Have they led to one of their goals, to facilitate

employment and careers for mothers? Scandinavia, including Norway, may now be the place where the revolution is unstalling.

The national setting is thus significant and of broad interest. Scandinavia along with the U.S. are in the forefront in regards to gender equality policies and both have progressive values toward gender equality. The U.S. led the world in regulation of the workplace, being the first country to pass extensive equality legislation. Scandinavia is the vanguard in family policies, having adapted policies often identified as central (Waldfogel 1998, pp. 141–145; Dex and Joshi 1999, pp. 655–656): paid parental leave with a portion reserved for fathers, cash and tax benefits for children, publicly provided child care at high quality and low cost, and employment policies allowing flexible hours and practically universal access to part-time work. 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 motherhood penalty in a country where public policy has done much first to make it easier to combine family and career and second to change the internal organization of the family by trying to create a more equal division of household labor between the sexes, with internationally now one of the most equal such divisions (Hook 2006, Fig. 1, p. 650).

A caveat is in order. Ultimately it is probably impossible to discern the precise effects of family policies on the motherhood penalty, simply because policies work out over many years and come bundled with other changes. Policies impact fertility, the work-family interface, and employer adaptations, each of which adjusts slowly over several years. But then there have been concurrent changes in family culture and decline in discrimination against women more generally. Empirically one thus faces an entire constellation of changes. But to the extent that declines have occurred in the motherhood penalty over the last 20–30 years, family policies likely have been a major contributing cause.

2 SELECTION, TREATMENT, AND DISCRIMINATION

In four subsections we review the three hypotheses (2.1), discuss our core errand, the role of differential pay within versus sorting on establishments, occupations, and occupation-establishment units for how the penalties arise (2.2), summarize the

implications of the hypotheses (2.3), and summarize existing evidence (2.4).

2.1 WOMEN AND FAMILY

Three central hypotheses have been put forth to explain the motherhood penalty and the female marital premium (Chiodo and Owyang 2003; Budig and England 2001, p. 204). Since the marital premium is modest for women, but the motherhood penalty is substantial, the focus will be on the mechanisms behind the latter.

The *selection hypothesis* works best for the marital premium. The factors that cause married women to earn more are the same factors that cause them to get married: conscientiousness, industriousness, and other traits valued both by employers and prospective partners.² Marriage as such does nothing to increase their productivity. But women who get married also tend to get children, and there is a clear children penalty. The selection hypothesis may explain the marital wage premium, but not, without further elaboration, the children penalty. For the latter, it may be that less productive women tend to have children at younger ages, in which case the penalty in part is a selection effect (Taniguchi 1999; Amuedo-Dorantes and Kimmel 2005).

Several implications follow from this hypothesis. To the extent that productivity can be observed and hence rewarded by employers, there should be no wage differences at the individual level of getting married or having children: as individuals move between marital and motherhood statuses, there should be no wage changes. This also means that women who eventually marry or have children even prior to entrance into marriage or motherhood earn a wage premium or penalty relative to women who remain single and childless.

According to the *treatment hypothesis*, getting married and having children induce women to change their behavior. Productivity goes up with marriage and down with children. Two mechanisms that could produce this result have been put forth.

One mechanism posits that mothers exert less effort at work than non-mothers. This could arise simply from the time constraints children give rise to, even when mothers and fathers share equally in householdwork and in taking care of children. But with household specialization this effect gets exacerbated, with further reduction of effort in the market for women, while often the opposite for men (Becker 1985). The lower effort in market work can occur in the same workplace, possibly leading to a wage penalty in the same job. It can also occur through changing to less demanding workplaces, in which case the children penalty manifests itself not at the workpla-

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, while women for openness to experience.

ce level, but in occupational segregation. The policy implications to be drawn with respect to removing the motherhood penalty varies by the source of lower effort. If there is equal distribution of work in the household, and the only added burden is having children, better family-work policies are needed, for example, by providing high-quality child care at low cost. If there additionally is unequal distribution of work within the family, one need also to change how families operate, through cultural campaigns, perhaps the tax system, and more.

A second mechanism in the treatment hypothesis points to less human capital accumulation during marriage and motherhood. There is loss of experience and on-the-job training as a result of part-time work and career breaks.

Under both mechanisms the motherhood penalty should have declined over time, in part because public provision of child care has increased and in part because the household division of labor and caring for children have become more equal, providing women with more time for market work today than earlier. For example, in the U.S., average household work for married mothers decreased from 34.5 to 19.4 hours per week between 1965 and 2000, while among married fathers it increased from 4.4 to 9.7 hours, an increase in the share done by men from 13 to 33% (Bianchi, Milkie, Robinson, and Milkie 2006, p. 93, Table 5.1).

The *discrimination* hypothesis, in contrast, does not rest on the claim that mothers are less productive than non-mothers, at least not in its pure form. It argues that employers discriminate against mothers. In its pure form the hypothesis puts forth that the differential treatment arises from animus (or taste) discrimination, where an employer has a distaste for working mothers, 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, mothers may in fact on average be less productive than childless women—be it due to selection or treatment—but without each mother being less productive than each childless woman, 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 childless women than for mothers. 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. This relates to an older historical phenomenon. Before and at the turn of the twentieth century, and especially up through the 1920s and 1930s, many organizations practiced a so-called marriage bar, under which married women were not hired and women upon marriage

or childbirth often were fired.³ Additionally there may be nonconscious sources of discrimination, as stressed in much recent psychological, legal, and sociological scholarship (e.g., Greenwald and Krieger 2006), with same effects as the conscious taste and statistical discrimination.

While these three hypotheses typically are used to explain the relationship between family and wages for both men and women, it needs to be added that the issues for women are considerably more complex. They involve a set of sequential decisions made under significant uncertainty and partial knowledge about future personal, organizational, and societal conditions, such as timing of pregnancy, duration of career breaks, public policy, and more. But a tractable analysis needs to set more limited goals than analyzing the entire set of life-cycle decisions.

2.2 THE ROLE OF SORTING

Regardless of the precise mechanisms producing the children penalties (and secondarily the marital premia), it is instructive to ask, Where do these penalties arise? Do they arise at the level of employers, when mothers and non-mothers work in same occupation and establishment? Or do they arise in the sorting of employees on occupations and establishments, so that non-mothers are hired and promoted into the higher-paying establishments, occupations, and occupation-establishment units?

And if the penalties 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 childless women over mothers? A subtle implication arises here that allows us to gain some insight into the role of employee choices and productivity versus employer discrimination. If the women who eventually get married and have children, while they still are single and childless, sort into the better- or lower-paying occupations and occupation-establishment units to a higher degree than the women who remain single and childless, then some of the sorting must occur due to choice or assessed productivity. The reason is simply that employers have no opportunity among single and childless women to discriminate on the basis of their future marital and motherhood status, and if sorting still occurs, it is unrelated to employer preferences for single over married women or childless women over mothers, and thus not caused by discrimination.

³For the U.S. see Goldin (1991) and for Norway see Hagemann (1994).

2.3 SUMMARY OF MAIN IMPLICATIONS

It is useful to summarize the main empirical implications of the hypotheses. All three hypotheses agree that mothers suffer a wage penalty relative to non-mothers. They differ in the mechanisms proposed for the penalty.

According to the selection hypothesis, more relevant for the marital premium than for the motherhood penalty, the women 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. Women who become mothers will suffer a wage penalty also prior to entrance into motherhood.

According to the treatment hypothesis, women who are mothers suffer a wage penalty relative to non-mothers, simply because of their decreased effort in market work upon motherhood, either in same occupation in same workplace or through change of occupation and workplace. Over time there will also be loss in human capital accumulation which induces additional penalties.

In the discrimination hypothesis, there will be low wages induced from being a mother, regardless of whether this is due to animus against working mothers or statistical discrimination. Under statistical discrimination, however, as mothers gain seniority with the same employer, productivity gets revealed and high-productivity mothers may be rewarded accordingly and no longer as being representative of the average mothers.

Two of the hypotheses also have specific implications for the historical trend in the penalties. Both the treatment hypothesis and the animus mechanism under the discrimination hypothesis would imply a decline in penalties over time, as the distribution of household work has become more equal, with less loss in human capital accumulation, and the amount of animus against working mothers 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 and motherhood. This would be evidence against discrimination from employers based on animus.

The processes implied by the three hypotheses also interact. For example, as the household division of labor has become more equal over time, some employers will observe that some mothers have more time or effort left for market work, which in turn may lead them to revise their statistical estimates and perhaps engage less in statistical discrimination.

In summary, we have three separate hypotheses, and the second and third each

has two separate mechanisms, producing different outcomes at different levels. This can become intricate, and to focus ideas Table 1 summarizes the implications.

(Table 1 about here)

2.4 SUMMARY OF RESEARCH EVIDENCE

Since our empirical aims are different from what has been addressed by research to date, only the central findings are discussed below. We start with the U.S. evidence.

With respect to marriage, Waldfogel (1997) in a cross-sectional analysis finds a positive effect of about 6%, which goes down to 4% with controls for individual-level fixed effects. Hundley (2000) reports non-significant marital wage differentials from -2.0 to $+3.0\%$ for employed women in one data set but significant premia of 5–8% in another data set. Studies using earlier data find similar results, marital premia of 1–5% in Korenman and Neumark (1992), of 3.6–4.8% in Hersch (1991), and of 4.2 and 6.7% for white and black women respectively in Hill (1979).

For children, Waldfogel (1997) reports cross-sectional penalties of 4 and 9.6% for 1 and 2+ children, and even larger within-individual penalties of 5.6 and 14.7%, from comparing an individual's wages before and after motherhood. Budig and England (2001) report similar penalties: in cross-sectional analysis of 3.9, 7.1, and 5.3% for 1, 2, and 3+ children, which increase to 4.5, 11.2, and 15.1% in within-individual analyses. Anderson, Binder, and Krause (2003) show cross-sectional penalties of 5–9% and 7–13% of 1 and 2+ children, but these drop to 3 and 5.5% in within-individual analyses, with somewhat larger penalties in Anderson, Binder, and Krause (2002). Amuedo-Dorantes and Kimmel (2005) find cross-sectional penalties of 4.5 and 8.8% for 1 and 2+ children, which increase to 4.9 and 10.3% in within-individual analyses. Hundley (2000) reports wage penalties of 8, 15, and 23% for 1, 2, and 3+ children, and substantially larger children penalties among self employed women. But from another data set, he shows no penalties to 1 and 2 children but a 6% penalty for 3+ children. Lundberg and Rose (2000) find that women who stay continuously employed after childbirth suffer no wage penalties whereas women who take career breaks suffer a 5% wage penalty. Taniguchi (1999) finds smaller children penalties for women who delay childbirth and Amuedo-Dorantes and Kimmel (2005) even find children bonuses among the highly-educated women who do so.

As for earlier studies, Korenman and Neumark (1992) report penalties of about 15% for 2+ children in both cross-sectional and within-individual analyses. From more complex econometric analyses they conclude that with continuous employment there is no children penalty. Hill (1979) finds a 6.9% penalty per child 18 years or

younger among white women, but no penalty for children among black women.

Two studies do find positive effects of children. From a sample of 217 women employed in 18 firms Hersch (1991) find premia of 2.8–4.1% per child, while Hersch and Stratton (1997) report positive effects of up to 4 younger children. The small sample size in the first study limits its generality.

Regarding the role of household specialization Hersch and Stratton (2002) find that both married and unmarried women suffer a wage penalty from housework, among married women of about 4% for a ten hour increase in weekly housework hours. The penalty is incurred when they perform typically female tasks in the household, but not for male or sex neutral tasks. Hersch and Stratton (1997) report a negative effect of housework hours: wages decrease with 5.5% for a 10 hour increase in housework, but with a very small effect of housework hours once within-individual analyses are done.

On balance, the studies show relatively small marital premia of 4–6%, but substantial children penalties, up to 15–20% for 2 or 3+ children. Three of the studies show large penalties within women’s lives as they transition between having 1, 2, and 3+ children, larger than the penalties found between mothers and non-mothers in the cross section. This suggests major treatment effects on wages for women.

Several comparative studies investigate marital premia and children penalties and some studies address these specifically in the Scandinavian context, though research on Norway is limited.

With respect to marriage, Harkness and Waldfogel (2003) find premia of 2 and 3% in Finland and Sweden, while Albrect, Edin, Sundström, and Vroman (1999) report premia in Sweden of about 1%. Isacsson (2006) in large-scale longitudinal study of Swedish twins, finds that among identical twins there is 11% marital wage penalty, which gets reduced to 7.2% when within-twin analysis is conducted, and to 0.5% and non-significance when within-individual analysis is done. This analysis did not include controls for children.

Harkness and Waldfogel (2003) report the largest children penalties in the U.S., UK, and Germany, and the lowest in the Nordic countries. The country-specific Scandinavian studies report small penalties to motherhood. For Denmark they are 1% for 1 child and 2% for 2+ children in cross-sectional analysis, but when within-individual analyses are done, then there are even small positive effects of having children of less than 0.5%, controlling for other variables, but effects that do not reach statistical significance (Datta Gupta and Smith 2002, Table 2, p. 618). Using the same data, Nielsen, Simonsen, and Verner (2004) refine these findings. They demonstrate that in the family-friendly public sector of the economy there are negative effects

of being a mother, a penalty of about 2.4%, but that in the private sector there are no such effects, a non-significant penalty of about 0.7%. They also investigate whether women upon childbirth self-select into the public sector. For example, 52% of women employed in the private sector have no children, while this is the case for only 38.5% of those in the public sector (Table 1, p. 727). Rosholm and Smith (1996) find similarly small effects of motherhood in Denmark in 1980–1990. Davies and Pierre (2005) report children penalties in Denmark, of 0, 3, and 3% for 1, 2, and 3+ children, but then substantially larger penalties in within-individual analyses of 3, 8, and 13%. Albrecht, Edin, Sundström, and Vroman (1999) report similar types of analyses for Sweden, finding that women are penalized neither in the public nor private sector for taking maternity leave. As in Denmark, there is, after controlling for career interruptions, even a premium for having children among the highly educated. The single study of Norway does however find wage penalties to motherhood, but no within-individual analysis is reported. As in the Danish study, the penalties are higher in the private than public sector (Hardoy and Schøne 2004).

The evidence is clear that Denmark and Sweden have lower marital premia and lower children penalties than the U.S. Norway has yet to be studied in detail.

Regardless of country, no study has used matched employer-employee data to analyze the premia and penalties. These 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. Nor has any study addressed the role of sorting on occupations and occupation-establishment units. And there is no documentation of the evolution of the premia and penalties over time, of the extent to which they have changed as family policies have been unrolled.

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 and thereby creating entirely new norms for fatherhood (Leira 2002, chap. 4). They include tax and cash benefits for families with children. Most important, there is publicly supported child care at relatively low cost and high quality. Additionally, part-time and work at flexible hours are almost universally available, and with no wage penalty to being part-time employed. These are all institutional arrangements central to lessening the family gap in pay and careers

(Waldfogel 1998).

With respect to parental leave, it was available for 18, 20, 22 weeks in 1977, 1987, and 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. Between 1988 and 1993, parental leave was increased with a few weeks every year from 22 to 52 weeks at 80% pay or for 42 weeks at 100% pay up to a maximum amount (Rønsen and Sundström 2002). Four of those weeks are reserved for the father, whereas six weeks are reserved for the mother (Leira 2002, pp. 89, 95). In 1996, 69% of fathers took paid parental leave and about 7% of the parental leave days (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 child care (Leira 2002, p. 62). Single parents pay lower fees. The cost of child care as percent of female earnings is 13 in Norway, while an entire 22 in the US (Waldfogel 1998, Table 2).⁴

While the policies on average impact women more than men, by making it easier to combine family and career, their impact on how both members of a couple behave can be substantial. With mothers more likely to be employed, and with cultural pressures on fathers to become more involved in household activities, the gender division of labor in the household is likely to be more egalitarian than in other countries. This further facilitates career success for mothers through getting more help from men in running the household and caring for children. Internationally, Norway—along with Sweden, Canada and the U.S.—has one of the most equal divisions of household labor (Hook 2006, Fig. 1, p. 650), and along with Sweden scores at the top of the Gender Empowerment Measure of the *Human Development Report* (United Nations Development Program 1998).

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 110,000 employees (about 40,000 women from 1990 on) and 3,000 establish-

⁴Esping-Andersen (1999, p. 66, Table 4.4) in contrast argues and documents that net costs for child care in the U.S. are among the lowest internationally, stating that even in the absence of publicly provided child care “the United States offers a superior cost-subsidy mix”; as a percent of family income with costs equal to those in Denmark and France and lower than in Sweden.

ments each year. We can follow establishments and their employees from year-to-year, about 800,000 person-years for the women. We restrict the analysis to women 20–50 years old, leaving about 600,000 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. The data were matched to register data from the Central Bureau of Statistics on education (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 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 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 variation in the dependent variable especially at the occupation-establishment level, which also could show up in marital premia and parenthood penalties. The restriction of analyses to these white-collar employees probably leads to results with somewhat larger penalties than if additional employees had been included.

From the contractual monthly earnings and contractual hours worked we compu-

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.

ted the hourly wage, which then refers to hourly wages paid on regular work hours, hence not mixing pay on regular and over-time hours. This is important since a central goal of the analysis is to assess whether employers pay mothers and non-mothers differently, in which case we need to measure the pay rate, not mixings the different rates from regular and overtime 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. Including these cases does not affect the results. 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. The coarser grouping of 21 occupations also helps avoid the otherwise 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, minus one year per child. It makes no difference for results whether we subtract a year per child or not.

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.

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.

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 1995–97. Wages are 18–19% higher for married and previously married women compared to single and childless women. Single women and childless women 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 women are cohabitating. For the women who are recorded as single (i.e. not yet married), 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 women aged 20–66 were married and another 6% were cohabitating, with the remaining 36% being single. In our data, 26% percent are recorded as single in 1995–97, but about one in six of the singles were 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 Denmark of about 1% (Datta Gupta and Smith 2002).

Some biases arise from this misclassification, as documented by Cohen (2002) for men using U.S. data. If cohabitators enjoy wage premia similar to married women, the cross-sectional analysis will overestimate the wages of single women, while still correctly estimating the wages of married women, and thus underestimating the wage differential, that is, the marital premium. To the extent that cohabitating women are more like single women in their economic success, there is no problem. In the within-individual analysis some women will be misclassified as single while cohabitating, and if there are treatment effects of leaving singlehood, a similar underestimation occurs, but none if the entire premium is due to selection. Since the marital premium for women is so small to begin with, about 2% (see below), the absolute size of the bias will however be small. If one in three of single women are cohabitating, and they earn the same premium as married women, the bias will be 1%: We will estimate the marital premium to be 2% rather than the correct 3%.

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 observed 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 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 of .000001 or better. No point is served in reporting these significance levels. The large 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. These are referred to as total effects.

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 premia and parenthood 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 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. Women 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 covered in our data (up to 2000 for marital status and children), while 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 gives 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 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

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 40,000 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 she 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 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.

A clarification of terminology should be noted. We uniquely can identify the selection effect. But what we refer to as the treatment effect really consists of two parts: The treatment effect, from possible employee adaptations to family situation, plus the treatment effect of possible employer discrimination, from employers reacting to changes in the family circumstances of their employees. These two cannot be

separated further with our data. But to the extent we find no penalties at one of the levels, say the occupation-establishment level, then a reasonable inference is to assume that there are neither true treatment nor discrimination effects at that level.

Methods for Analyzing Individual Career Dynamics

In addition to estimating wage differentials, we also analyze 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? A fruitful way to think about the estimators is that they report on different aspects of the data, that they 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 positive effect on wages of being married, whereas at the occupation-establishment level there is no such effect. This would correctly indicate that married women earn higher wages than single women, 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 women tend to work in higher-paying occupation-establishment units than single women.

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

Differential Pay Versus Sorting

Table 3 gives the coefficients on wages of marital status and number of children below age 20. Each regression is estimated separately by year, for 18 years. But to make presentation more compact, we have averaged the coefficients across years within four separate periods: 1980–84, 1985–89, 1990–94, and 1995–97.

(Table 3 about here)

At the population level, with analyses comparable to results already reported in the literature, the effect of marital status are similar across the four time periods: Wages are 2% higher for married than single women, with a somewhat lower wage bonus for previously married women. With controls for establishment, occupation, and occupation-establishment these effects are cut in about half: The premia are about 1% for being married and much smaller for post-marital states. The marital premia are thus small, and close to zero at the occupation-establishment level.

The wage penalties for children are however substantial, especially in 1980–89. In 1980–84 the penalties were 3.5, 7.6, and 10.7% for 1, 2, and 3+ children. But by 1990–97 the penalties were cut in half, to 2.3, 4.0, and 4.9% (in 1995–97).

As one successively controls for establishment, occupation, and the occupation-establishment unit the penalties change. Controlling for establishment reduces the

children effects by a small 10%. But at the occupation-establishment level, the children effects are dramatically reduced with about 50% in 1980–89, about 75% in 1990–95, and 90–100% in 1995–97: The penalties were 0.5–1.6% in 1990–95 and 0.1–0.5% in 1995–97, with almost as strong reductions at the occupation level. By the mid-1990s the children penalties at the occupation-establishment level had thus disappeared, and almost disappeared at the occupation level.

What can we then conclude? There are small premia for marital status, but sizeable penalties for children. For women, as for men there is a premium to being married, but it is much smaller than typical male premia (in Norway of 6–7%). For women, unlike men, there is a penalty to having children. The effects of marital status and especially of children work mostly through sorting on occupations and occupation-establishment units: In 1980–84, about 50% of the children penalties are due to sorting, and in 1995–97 a massive 90% is. Women with children work in different and lower-paying occupations and occupation-establishment units than childless women. In 1995–97, once mothers and non-mothers work side-by-side, they receive the same pay. Employers do not pay women with family obligations less. To the extent there is discrimination then, it is not in pay, but in hiring or promotion.

The strong decline in the children penalties over time at all levels constitutes a remarkable historical change over a short period. Though difficult to ascertain, this probably reflects (1) major changes in family culture and the interrelationship between family and work, including less demands on mothers' time in the household, due to the gradual impact of family policies, and (2) changes either in employer behavior through lower animus against mothers or increased productivity of mothers.

Are Women Who Marry and Have Children Different?

Are the women who marry and/or have children different from those who remain single and/or without children, so that the former group would earn different wages even in absence of marriage or parenthood and even prior to these? Or are the effects due to changes in behavior, such as changes in 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 women 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 women as always being single and childless.

(Table 4 about here)

At the population level, there is among single women a wage advantage of 2.3% for those who eventually marry, identical to the total effect of being married from Table 3 (1980–84). The differential is larger in later years, but these effects are more open to question, as discussed above. The entire marital premium for women thus appears to be due to selection.

Women who eventually have children earn higher wages while childless than women who don't, of about 1.5% in early years, but less later. There is thus positive selection not only of wives but also of future mothers. Once the women actually have children, there is however a penalty, as shown above, and again below.

What happen to these differences as adjustments are made for establishment, occupation, and occupation-establishment? At the establishment level there is a clear reduction in both premia, at the occupation level a smaller reduction, and then the premia practically disappear at the occupation-establishment level. In summary, at the population level, there are small but clear premia for eventually getting married and having children, but very small premia at the occupation and occupation-establishment levels, that is, after sorting has occurred.

The pre-marital premia provide *prima facie* evidence that the premia to actually being married is due either to choice from employees or to higher productivity, not from differential treatment by employers, since employers cannot sort employees on the basis of their future marital statuses. The evidence is hence against the claim that the marital wage premium is due to discrimination in hiring on the basis of marital status. The pre-motherhood premium also shows that the motherhood penalty is not about selection. Mothers are earnings wise in fact positively selected.

A variant of this analysis is presented in Panel B. Here we select all employees—single, married, previously married, mothers, and non-mothers—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 larger than the treatment effect; the coefficients for “ever married” is large relative to the coefficient for actually being married. Women who in a given year are single, but who eventually marry, earn a wage premium of 1.8% in 1980–84. Once they actually marry, an additional premium of 0.8% is earned. The total premium is 2.6%. This corresponds to results in Table 3, where the premium

for being married is 2.3%.

The selection effects at the establishment and occupation levels are smaller, and are absent at the occupation-establishment level. At that level, there is a small treatment effect of about 0.5%.

For children there are mostly treatment effects, in each of the four periods. In the earlier years these are also present at the occupation and occupation-establishment levels, but not in later years.

Note that at the population level, the negative treatment effects of children are larger than those found for the total effect in Table 3. The reason is simply that the total effect is the sum of the selection and treatments effects, the former being positive, the latter negative, and their sum is then in between. For example, in 1980–84, for the population effect for 3+ children, the sum of the selection (.011) and treatment (−.115) effects is −.104 in Panel B of Table 4, corresponding to the total effect of −.107 in Table 3.

In summary then, for women, the marital premium is entirely about selection. Women who marry receive a premium even prior to getting married. These premia are primarily self made, through seeking better opportunities or higher productivity. There is also a small selection premium of about 1% for the women who eventually become mothers. They earn higher wages before parenthood. But that premium is lost once they actually become mothers. For women who have three children, there is a 1.1% pre-children premium, but then they incur a three-children penalty of 11.5%, yielding a net differential vis-a-vis women who remain childless of 10.4%. The cost of having children is thus larger than the cross-sectional cost, consisting of the loss of the selection premium plus the penalty relative to childless women. The motherhood penalty is entirely a treatment effect, though as noted above, this includes both employee and employer adaptations to motherhood. However, the empirical fact remains: Being motherly pays, being a mother does not.

Do Women's Wages Change Upon Marriage and Motherhood?

We finally report an analysis of within-individual dynamics, of how wage levels evolve as the women 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 premia of post-single states of about 0.7–1.0%. As women move from being single to being married the wages, at the

within-individual level, increase with 0.7%. These premia are similar at all levels.

For children, we know from Table 3 that the premia declined over time, so we have included an interaction term between the children dummy variables and number of years elapsed since 1980 (0–17 years).⁷ Early in the period (1980) there are sizeable negative treatment effects of 2.5, 6.8, and 9.2% for 1, 2, and 3+ children at the population level, with somewhat smaller effects at the other levels. But these penalties erode with time for having 2 or 3+ children. In 1997, at the population and occupation-establishment levels the treatment penalties for 2 children are 3.4 and 2.4% and for 3+ children they are 4.1 and 4.1%.

What are the implications of these findings? For women, selection explains most of the marital premium. There are also clear but small selection premia for women who eventually become mothers. But once a mother, there are strong treatment penalties, which account for the entire children penalties. These penalties arise mostly through sorting on occupations and occupation-establishment units. There may be some discrimination against mothers early in the period of 0.5–1.6%, but by 1995–97 it had disappeared, down to less than 0.5% at the occupation-establishment level. These results make sense. Being a mother is taxing, often results in lower work effort, and thus may extract a wage premium, which wipes out the selection effects for the women who become mothers.

6 WAGE GROWTH AND PROMOTIONS

There is a small marital wage premium, and a relatively large motherhood penalty. Early in the period there was even unequal pay for equal work by motherhood status, but by the end there were no differentials, that is, when mothers and non-mothers work in same occupation in same establishment they were paid the same wages. To the extent that the observed penalties can be attributed to actions by employers they must toward the end of the period 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. Promotions involving shift of career lad-

⁷Since we include individual-level fixed effects, we cannot estimate coefficients separately by year and instead need to pool individuals across years.

der cannot be analyzed. These 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 wage changes between two adjacent years. Each regression is estimated separately by year, for 17 years, but not for 1997 (last year in data), since we know neither wages nor occupations in 1998. As above, we have averaged the coefficients across years within four periods: 1980–84, 1985–89, 1990–94, and 1995–97.

(Table 6 about here)

The results are unusually simple: There are no wage growth premia for marital status at any of the four levels in any of the four periods. The estimates are in size and substance precisely equal to zero: differentials ranging mostly from -0.01% to $+0.01\%$ (maximum of 1%).

For children, however, there are small wage growth premia for 2 and 3+ children of about 0.8% at all levels in 1980–89. By 1990–97, these premia had disappeared, then being in the range of 0.1 – 0.3% .

In summary, for wage growth there are no marital premia, but small premia for children in 1980–89 and none by 1990–97. The lack of marital premia in wage growth is not surprising. The marital premia for wage levels were small themselves. This corresponds to small wage growth premia.

The children premia for wage growth can help make sense of the reduction in children penalties for wage levels between 1980 and 1990. Mothers who remained in the sector during this decade caught up with non-mothers in wages by receiving higher wage increases. The wage penalty to 3+ children was 10.1% in 1980–84 but down to 6.5% in 1990–94. With annual wage growth being 0.78% higher for mothers with 3+ children than for non-mothers, wages should catch up with 7.8% from 1980 to 1990. The higher wage increases for mothers partly explains the narrowing of the children penalty in wage levels.

Panel B of Table 6 gives the corresponding results for promotions. For marriage, there are small positive effects in 1980–89, of 0.5 – 1.0% for being married at all levels (population, etc.), but by 1990–97 these effects had disappeared, with the exception of a somewhat larger effect for the small group of widowers.

For children, there are positive population-level effects on promotion of 1.0 – 1.5% in 1980–89, but again, these had disappeared by 1990–97. At the occupation level, there are no effects of children in 1980–84 but negative effects in 1985–97. The same is the case at occupation-establishment level; in 1995–97, with effects of -0.8 , -1.0 ,

and -1.4% for 1, 2, and 3+ children.

An instructive comparison can be made. In 1980–89 there are at the population level positive effects on promotions of being a mother, while negative effects at the occupation-establishment level. This means that within an occupation-establishment unit women with children on average are promoted at a lower rate than childless women. At the population level there is however promotion advantage for mothers. This can only come about if mothers to a larger extent than non-mothers are employed in occupation-establishment units with higher promotion rates. It is an error to infer that processes observed at the population level also occur at the workplace level.

A question arises, why are there, at the occupation-establishment level, negative effects of motherhood status on promotions but either positive or zero effects on wage growth? The puzzle is why lower promotions rates for mothers at that level do not translate into lower wage growth, that in fact rather the opposite occurs. The reason is that very few employees experience promotions, only 6–7% per year, with negative effects for mothers, whereas about 90% of employees experience wage growth every year, with positive effects for mothers. Mothers are thus somewhat disadvantaged in the promotion process affecting few employees, but are advantaged or have no disadvantage in the wage growth process affecting almost all employees. On average, the wage increases associated with promotions affecting few employees wash out in the wage growth process affecting almost all employees. It is also possible that mothers experience promotions through change of career ladder to a higher extent than non-mothers, an issue our data do not allow us to address.

7 CONCLUSION AND DISCUSSION

The processes that occur in the family are today probably the largest obstacle to continued progress in gender equality, with women suffering significant workplace penalties from motherhood. For understanding how to ameliorate these penalties, one needs to identify both where they arise and the potential role of public policies.

We investigated first whether the motherhood penalty, as well as the female marital premium, arise due to differential pay by employers or from differential sorting of employees on occupations and establishments, that is, the extent to which the penalties possibly arise from wage discrimination in the workplace. We next investigated wage increases and promotions between years. We assessed how the penalties changed over time during a period where significant family policies were unrolled. Data came from 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 motherhood penalties arise from household specialization, this should in itself lead to lower penalties than in other countries and to its decline over time.

We have four conclusions. First, there are major wage penalties to motherhood, but these declined strongly over an 18-year period: from 3.5, 7.6, and 10.7% for 1, 2, and 3+ children aged 20 or younger in 1980–1984 to roughly half that level in 1995–97 (i.e., 2.3, 4.0, and 4.9%). The marital wage premia are small, about 2%. The decline in motherhood penalties is likely caused by changes in family policies and in how families operate.

Second, the penalty to motherhood (and premium to marriage) is mostly due to sorting on occupations and occupation-establishment units, and the role of sorting increased over the period. Women with children work in different and lower-paying occupations and occupation-establishment units than childless women. By 1995–97, once mothers and non-mothers work in the same occupation in the same establishment, they receive the same pay. This indicates absence of discrimination and productivity differences at that level. The results answer a question previously not addressed. The role of differential pay from employers is marginal in explaining the motherhood penalty and marital premium.

Third, mothers are wagewise positively selected: Among childless women, those who subsequently become mothers earn higher wages than those who remain childless. But once a mother, major negative treatment effects of motherhood wipe out the positive selection effects. The motherhood penalty is thus in its entirety a treatment effect. The marital premium in contrast is almost exclusively about selection.

Fourth, with respect to wage growth, there is no impact of marital status, but premia to motherhood early in the period (1980–89), but these had vanished by 1990–97. For promotions within career ladder, there was however a motherhood penalty at the occupation-establishment level. But this did not result in lower wage growth for mothers at that level.

What are the central implications? One is that the motherhood penalty does not arise from employers paying mothers less than non-mothers. At that level there appears to be neither discrimination nor productivity differences by motherhood status. The penalties may arise however from differentials in hiring, wage increases, or promotions. We could not investigate hiring. But we find that mothers receive higher wage increases than non-mothers. There are some differences in promotions at the occupation-establishment level, but promotions are relatively rare, and the significant motherhood penalty can thus not arise there. This leaves differentials in

hiring as the major culprit (see Correll and Bernard 2007), arising either from choice or discrimination. The single case study done in Norway reports no motherhood penalty in getting hired (Petersen and Togstad 2006). On balance it is thus possible, perhaps even likely, that in Scandinavia little can be gained for reducing the motherhood penalty from further regulation of employers, at least not in wage setting and wage growth. Returning to our opening quotes from Hobsbawm (2000, p. 136), he concludes on the difficulty for mothers to reach high posts: “This has nothing to do with discrimination, but with the natural law that women are the ones who give birth.”

Another important implication follows from the strong decline in the motherhood penalties over time, which is nothing but a remarkable historical change over a short period. Though difficult to ascertain, this likely reflects (1) major changes in family culture and the interrelationship between family and work, including less demands on mothers’ time in the household, due to the gradual impact of family policies, and (2) changes either in employer behavior through lower animus against working mothers or increased productivity of mothers. Nothing can be done about the natural law that women give birth, but its social and economic consequences are obviously amenable to modification as shown in the Norwegian case.

What are then the prospects for solving the workplace disadvantages caused by the organization of the family and its interrelationship to work? The gain to be had in Scandinavia from further equality and opportunity legislation directed at employers is in all likelihood limited. Judging from the changes over time in the motherhood penalty, there are still gains to be had from cultural transformations and better family policies. On the policy side, Norway and Scandinavia have been in the forefront in the four domains often identified as pivotal (Waldfogel 1998, pp. 151–154): paid maternity and importantly paternity leave, cash and tax benefits for children, subsidized high-quality child care at low cost, and universal access to flexible hours, the latter through how employment law works and how social benefits are financed. From the available evidence, though difficult to ascertain, it appears that the policies have had one of their intended effects, to partially remove the penalties from “the greatest barrier to economic equality [...] children” (Fuchs 1988, p. 147).

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 (adapted from Goux and Maurin 1999):

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

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

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

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

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.⁸

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.

⁸Abowd, 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 Women Who Eventually Become Married and/or Eventually Have Children: Relative to Themselves While Single and/or Childless, Relative to Women Who Stay Single and/or Childless, and Change in the Marital Premium Children Penalty over Time

	Selection	Treatment			Discrimination	
		Household Specialization	Human Capital Accumulation	Changed Work Effort	Animus	Statistical
Marital States						
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	Decline
Motherhood States						
No children	Low	High	High	High	High	High
Children	Low	Low	Low	Low	Low	High
Change over Time	Zero	Decline	Decline	Decline	Decline	Decline

Note: In the 42 cells, for the 7 rows, the empirical implications differ in only 8 cells. For the 24 cells for marital status, they 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 1 and 3 give different implications than the other cells. For the 18 cells for motherhood status, the implications differ in 3 cells. In Row 5, only cell 1 gives a different implication. In Row 6, only cell 6 gives a different implication. In Row 7, cell 1 gives a different implication.

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	22	22	22	22
N establishments	3810	4057	4293	3468
N occ-est	23076	23746	33292	17738

Note: For each year in the data 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 statistics above have been computed separately for each year, but have then been averaged across years within each of four time periods. The five last lines of the table we give (1) the number of individual-years in each of the four time periods, (2) the number of distinct individuals in each of the four periods, (3) the number of occupations each year, (4) the average number of establishments each year within each of the four time periods, (5) the average number of occupation-establishments units each year within each of the four time periods. Note that on average, employees are observed for a period of 9 years in the data.

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	.023	.016	.012	.007	.026	.019	.011	.007	.021	.014	.011	.006	.027	.017	.014	.009
Divorced	.025	.005	.017	.004	.026	.007	.017	.001	.021	.006	.008	-.001	.020	.004	.007	-.001
Widowed	.019	.007	.006	-.011	.026	.014	.009	.004	.023	.012	.016	.003	.025	.002	.027	.009
Separated	.012	.002	.011	.001	.024	.005	.015	.002	.028	.008	.018	.002	.021	.011	.008	.001
First child	-.035	-.026	-.023	-.017	-.027	-.019	-.016	-.012	-.025	-.017	-.010	-.005	-.023	-.017	-.004	-.003
Second child	-.076	-.059	-.050	-.041	-.065	-.050	-.036	-.027	-.043	-.031	-.014	-.008	-.040	-.027	-.005	-.001
Third child	-.107	-.090	-.063	-.055	-.103	-.089	-.054	-.046	-.063	-.050	-.016	-.016	-.049	-.036	-.005	-.005

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																
Got married	.023	.006	.014	.000	.026	.013	.015	.006	.033	.016	.021	.009	.039	.017	.020	.010
Had children	.015	.006	.014	.007	.007	.002	.004	.002	.008	.003	.005	.002	.022	.012	.012	.010
Overall population																
Married	.008	.013	.003	.007	.009	.015	.002	.006	-.003	.005	-.005	.002	-.001	.004	.001	.005
Divorced	.009	.001	.008	.004	.008	.002	.007	.001	-.003	-.003	-.008	-.006	-.007	-.008	-.006	-.006
Widowed	.003	.004	-.003	-.010	.009	.009	-.001	.003	-.001	.003	.000	-.002	-.002	-.010	.015	.007
Separated	-.003	-.002	.002	.001	.007	.000	.005	.001	.004	-.001	.002	-.002	-.009	-.002	-.006	-.004
First child	-.042	-.028	-.030	-.020	-.027	-.018	-.016	-.012	-.024	-.017	-.009	-.004	-.010	-.011	.001	.000
Second child	-.084	-.061	-.058	-.044	-.066	-.050	-.037	-.028	-.042	-.031	-.013	-.007	-.026	-.021	.000	.002
Third child	-.115	-.092	-.071	-.058	-.105	-.088	-.056	-.047	-.062	-.050	-.016	-.015	-.035	-.030	.000	-.003
Ever married	.018	.004	.009	-.002	.022	.007	.013	.001	.029	.011	.019	.006	.031	.015	.014	.005
Ever has child	.011	.002	.012	.005	.001	-.001	.001	.001	-.002	-.002	-.001	-.002	-.019	-.009	-.007	-.004

Note: The top panel (labeled “childless singles”) reports regression coefficients estimated for employees who in the given year are single (never married) and have no children. The dummy variable for Got Married indicates whether the employee eventually got married (=1) or not (=0), within the time frame of the data. The dummy variable for Had 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 Got Married and Had 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	.007	.007	.007	.007
Divorced	.007	.009	.009	.010
Widowed	.007	.008	.007	.007
Separated	.007	.009	.007	.008
First child	-.025	-.022	-.017	-.016
Second child	-.068	-.054	-.048	-.041
Third child	-.092	-.076	-.068	-.058
First child x Year	.000	.001	.000	.000
Second child x Year	.002	.002	.001	.001
Third child x Year	.003	.002	.002	.001

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	-.0022	-.0021	-.0027	-.0011	-.0008	-.0008	-.0008	-.0001	-.0008	-.0001	-.0009	-.0013	-.0017	-.0017	-.0017
Divorced	-.0010	-.0012	-.0008	-.0012	-.0006	-.0005	-.0005	-.0005	-.0004	-.0007	-.0002	-.0013	.0000	-.0013	.0001	-.0006
Widowed	-.0017	-.0019	-.0016	-.0036	-.0004	.0018	.0000	.0014	.0027	.0012	.0015	-.0019	-.0025	-.0019	-.0031	-.0059
Separated	.0005	-.0013	.0009	-.0015	.0000	-.0008	-.0004	-.0012	.0003	-.0001	.0000	-.0018	.0010	.0006	-.0005	-.0013
First child	.0003	.0014	.0002	.0016	.0012	.0010	.0011	.0014	-.0002	-.0006	-.0001	-.0001	-.0022	-.0016	-.0021	-.0009
Second child	.0094	.0102	.0094	.0102	.0080	.0073	.0075	.0073	.0034	.0031	.0031	.0029	.0025	.0030	.0025	.0026
Third child	.0078	.0101	.0083	.0101	.0112	.0099	.0095	.0090	.0041	.0035	.0037	.0042	.0015	.0024	.0011	.0016
Promotions																
Married	.0046	.0045	.0084	.0060	.0115	.0089	.0161	.0115	.0043	.0026	.0059	.0023	.0038	.0012	.0053	.0031
Divorced	.0054	.0042	.0063	.0017	.0141	.0092	.0157	.0071	-.0008	-.0022	.0027	-.0044	.0072	.0070	.0105	.0074
Widowed	.0155	.0078	.0194	.0088	.0084	.0017	.0122	.0072	-.0037	-.0089	.0017	.0003	-.0110	-.0149	-.0085	-.0208
Separated	.0134	.0091	.0164	.0047	.0120	.0054	.0164	.0039	.0030	.0000	.0056	-.0035	.0049	.0056	.0085	.0039
First child	.0075	.0038	.0027	.0011	-.0022	-.0034	-.0058	-.0082	-.0036	-.0050	-.0081	-.0079	-.0051	-.0044	-.0093	-.0084
Second child	.0117	.0103	.0022	-.0010	.0075	.0074	-.0036	-.0036	.0000	-.0020	-.0076	-.0055	.0014	-.0003	-.0069	-.0099
Third child	.0152	.0106	-.0006	-.0226	.0014	-.0013	-.0173	-.0234	.0052	-.0031	-.0081	-.0131	-.0063	-.0043	-.0154	-.0142

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 analyses additionally restricted to those who were in the same career ladder between two adjacent years.