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Trond Petersen, Andrew M. Penner, and Geir Høgnæsnes

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Author:

[Petersen, Trond](#), University of California, Berkeley
[Penner, Andrew M.](#), University of California, Irvine
[Høgnæsnes, Geir](#), University of Oslo

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Trond Petersen *University of California, Berkeley*

Andrew M. Penner *University of California, Irvine**

Geir Høgsnes *University of Oslo***

The Within-Job Motherhood Wage Penalty in Norway, 1979–1996

Department of Sociology, 410 Barrows Hall, University of California, Berkeley. Berkeley, CA 94720–1980, USA (trond@berkeley.edu).

*Andrew M. Penner, Department of Sociology, 3151 Social Science Plaza, University of California, Irvine. Irvine, CA 92697–5100, USA (penner@uci.edu).

**Geir Høgsnes, Department of Sociology and Human Geography, University of Oslo. P.O. Box 1096 — Blindern. 0317 Oslo, Norway.

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Note

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The motherhood wage penalty is a substantial obstacle to progress in gender equality at work. Using matched employer–employee data from Norway (1979–1996, N = 236,857 individuals, N = 1,027,462 individual–years), 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 the sorting of employees on occupations and establishments and (b) changes in the penalties during a period with major changes in family policies. We find that: (a) The penalty to motherhood was mostly due to sorting on occupations and occupation–establishment units (mothers and nonmothers working in the same occupation and establishment received similar pay), and (b) The wage penalties to motherhood declined substantially over the 18-year period.

Key Words: family policy, gender, inequality, marriage, motherhood, wages.

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 20th century, it was widely recognized that although major progress has been made in the first two of those three domains, significant obstacles to further progress arise today from a different source: the processes that occur in the family and their interrelationships with work. The sentiment is that “the emancipation of women has been one of the great historical events of the twentieth century,” but there is an increasingly serious problem: “the extraordinary difficulties for women of combining high professional posts with being mothers” (Hobsbawm, 2000, p. 136). There is a sense of a serious roadblock, even a stalled revolution (Hochschild, 1989).

For men, marriage and to some extent children have positive effects on wages and careers (see, e.g., Hundley, 2000). For women, however, there are no or small wage benefits for marriage and large penalties for having children (see, e.g., Waldfogel, 1997). Several key questions concerning this stalled revolution arise. Did it get stalled by employers, through differential pay favoring married men and fathers and discrimination against mothers? Or was it stalled by women and men, through the choices they make around family and work, arising both from differences in constraints and lifestyle preferences? Or was it the outcome of the interaction of discrimination from employers and gendered choices made as a result?

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 potentially easier to regulate than families, and gains may be made by further regulation of employers. But if employers are not a culprit, an increased emphasis on family policies and changes in family culture is warranted. The question then arises as to whether family 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. And whereas the United States led the world in the regulation of the workplace, being the first country to pass extensive equality legislation, Scandinavia is the vanguard in family policies, having rolled out major policies to reinvent the family over the last 20–30 years (Kitterød, 2002; Waldfogel, 1998). 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.

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) Do employers pay women differently according to motherhood (and marital) status, and (b) what is the role of sorting of employees on occupations and establishments (i.e., workplaces) for the size of penalties (and premia)? The second objective is to assess how these penalties (and premia) evolved during a period when significant family policies were unrolled. To address these two issues, we use matched employer–employee data from Norway in the period 1979–1996.

Our data enable us to provide entirely novel results by documenting where the penalties (and premia) arise: at the level of the employer, in how they pay mothers and nonmothers (and single and married women), or in how employees are sorted on occupations and establishments. The longitudinal aspect of the data allows us to investigate the change over time in the penalties (and premia).

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 (Leira, 2002). 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 declines in discrimination against women more generally (Bianchi, Robinson, & Mickie, 2006; Hook, 2006). 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 have likely been a major contributing cause.

Women and Family

Research on the United States has shown relatively small marital premia for women, mostly close to zero or up to 4–6% (Hersch, 1991; Hill, 1979; Hundley, 2000; Korenman & Neumark, 1992; Waldfogel, 1997). The wage penalties for having children have however been substantial, up to 15–20% for two or more children (Anderson, Binder, & Krause, 2002; Amuedo-Dorantes & Kimmel, 2005; Budig & England, 2001; Hundley, 2000; Waldfogel, 1997). Less is known about the effects of motherhood on wages in Norway, though evidence from elsewhere in Scandinavia has shown lower marital premia (around 1–2%) and lower children penalties (around 2–10% for two or more children) than in the United States (see, e.g., Albrecht, Edin, Sundström, & Vroman 1999; Datta, Gupta, & Smith, 2002; Davies & Pierre, 2005; Harkness & Waldfogel, 2003; Rosholm & Smith, 1996). Interestingly, Nielsen, Simonsen, and Verner (2004) showed that in the family-friendly public sector of Denmark there was a motherhood wage penalty of about 2.4%, but that in the private sector there were no such effects. The single study of Norway, however, found wage penalties to motherhood, with larger penalties in the private than the public sector (Hardoy & Schøne, 2008).

In addition to addressing many of the issues from previous research, as summarized above, the present paper provides entirely novel results by reporting the size of wage differentials at the workplace level and how these differentials have evolved over an important 18-year period. For these two purposes there is yet limited research.

Three central hypotheses have been put forth to explain the penalty to motherhood and the premium to marriage (Budig & England, 2001, p. 204; Chiodo & Owyang, 2003): the selection, treatment, and discrimination hypotheses. Below we proceed by (a) reviewing the three hypotheses, summarized in Table 1, (b) discussing our core errand, comparing the role of differential pay within versus sorting on establishments, occupations, and occupation–establishment units (i.e., a given occupation at a specific workplace, a so-called job) for how the penalties and bonuses arise, and (c) summarizing the implications

of the hypotheses. We focus primarily on the motherhood penalty, with less discussion of the marital premium.

(Table 1 about here)

Selection, Treatment, and Discrimination

The *selection hypothesis* suggests that the factors that lead women to have children are the same factors that cause them to earn less. Women who are less productive, due for example to lower career ambitions, may have children earlier and more children than more career-oriented women. Children as such do nothing to decrease women's wages; women who eventually have children earn lower wages even before they have children (Table 1, col. 1). Budig and England (2001) showed that selection likely did not account for the motherhood wage penalty observed in the United States.

According to the *treatment hypothesis*, having children (i.e., the treatment) changes the labor market behavior of women, and results in lower wages. Two mechanisms that could produce this result have been put forth, pointing to lower effort at work and loss of human capital respectively.

One mechanism posits that mothers exert less effort at work than nonmothers (Table 1, cols. 2–3). This could arise simply from household specialization (Table 1, col. 2), with women reducing their labor market effort (Becker, 1985). But even more important may be the time constraints introduced by children, even when mothers and fathers share equally in household work and in taking care of children (Table 1, col. 3). The lower effort in market work can occur without changing jobs, possibly leading to a wage penalty in the same job. It can also occur through changing to less demanding jobs, in which case the children penalty manifests itself not at the job level, but through occupational segregation. The policy implications for removing the motherhood wage penalty thus vary by the source of lower effort. If there is an 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 childcare at low cost. If, in addition, there is an

unequal distribution of work within the family, changes are also needed in how families operate, for example, through cultural campaigns or even changes in family taxation.

A second mechanism in the treatment hypothesis points to reduced human capital accumulation during marriage and motherhood (Table 1, col. 4). 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 the public provision of child care has increased, and in part because the division of household labor and caring for children has become more equal, providing women with more time for market work today than earlier. In Norway, among married or cohabiting parents with children 0–6 years old, women decreased the time spent on total household work—including caring for children—from 7.5 hours per day in 1971 to 5.5 hours in 2000. In contrast, men in the same period increased their hours from 2.3 to 3.4 (Kitterød, 2002, Table 1), an increase in the share done by men from 24% to 38%. In 1971 fathers did 22% of the caring for children aged 3–6 years, while in 2000 they did 42%, coming close to reaching parity.

The *discrimination* hypothesis, in contrast, does not rest on the claim that mothers are any different in their labor market behavior than nonmothers, at least not in its pure form. Rather, it argues that employers discriminate against mothers. In its pure form the hypothesis posits that the differential treatment arises from prejudice (or taste) discrimination (Table 1, col. 5), where an employer has a distaste for employing mothers, comparable to when an employer is willing to pay more for certain demographic groups, such as hiring more from and paying more for male than female employees, even in the absence of objective reasons for doing so (England, 1992, chap. 3). In a less pure form (Table 1, col. 6), mothers may actually be less productive on average 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 on the basis of these group averages, and will pay more for childless women than for mothers. This second form is an

instance of statistical discrimination (England, chap. 3), and is economically rational behavior if the costs of measuring productivity are high. It relates to an older historical phenomenon. Before and at the beginning of the 20th 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 (see, e.g., Goldin, 1991; Hagemann, 1994). Additionally, discrimination may be unintentional, as stressed in much recent psychological, legal, and sociological scholarship (e.g., Greenwald & Krieger, 2006), with the same effects as the intentional taste and statistical discrimination.

The Role of Sorting

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

And if the penalties arise due to sorting, it is important to understand whether the sorting comes from employee choices of which occupations and establishments to work in, or whether it comes 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 have children (or get married), sort into the better- or lower-paying occupations and occupation–establishments while they still are childless (or single), then some of the sorting is likely to occur due to choice or assessed productivity. This is so because the employers have no opportunity to discriminate on the basis of future motherhood or marital status, and if sorting still occurs, it likely is unrelated to employer preferences for childless women over mothers or single women over wives, and thus not caused by discrimination.

Summary of Main Implications

It is useful to summarize the main empirical implications of the hypotheses (see Table 1). All three hypotheses agree that mothers suffer a wage penalty relative to nonmothers. They differ in the mechanisms proposed for the penalty.

According to the selection hypothesis, the women who eventually become mothers will suffer a wage penalty also prior to entrance into motherhood, and will not increase their wages upon motherhood. Similarly, women who eventually become married will earn high wages also prior to marriage, will not increase their wages upon marriage, nor decrease wages upon separation, divorce, or widowhood (Table 1, col. 1).

According to the treatment hypothesis, women who are mothers suffer a wage penalty relative to nonmothers simply because of their decreased effort in market work upon motherhood, either in the same occupation in same workplace (i.e., establishment) or through change of occupation and workplace. Over time, there will also be a loss in human capital accumulation, inducing additional penalties (Table 1, cols. 2–3).

In the discrimination hypothesis, there will be low wages induced from being a mother, regardless of whether this is due to prejudice against working mothers or statistical discrimination (see Table 1, cols. 5–6). 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 mother (see Table 1, col. 6).

Two of the hypotheses also have specific implications for the historical trend in the penalties. Both the treatment hypothesis and the prejudice 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 prejudice against working mothers probably has declined (see Table 1, cols. 2–5).

Additionally, according to the selection hypothesis, to the extent that the penalties and premia arise from sorting rather than from differential pay for doing the same work for the same employer, and this sorting is due to employee choices or to observable productivity, we should observe that the sorting occurs even prior to motherhood and marriage. This would be evidence against discrimination from employers based on prejudice.

The processes implied by the three hypotheses also interact. For example, as the household division of labor has become more equal over time, 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.

The Present Study

The evidence is clear that Denmark and Sweden have lower marital premia and lower children penalties than the United States. 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 a motherhood penalty (and marital premium) for women working in the same occupation in the same establishment (i.e., the same job), that is, whether productivity differences or discrimination could have arisen at the occupation-establishment level (such as a financial analyst employed by the Norwegian Bank at a given location in Oslo). Nor has any study addressed the role of sorting on occupations and occupation–establishment units. Additionally, there is no comparable documentation of the evolution of the premia and penalties over time, of the extent to which they have changed as family policies have been rolled out. In addition to addressing these novel issues, we also address the traditional concerns of the existing research on selection and treatment processes. In doing so, we are careful to control for education and experience to ensure that our results do not simply reflect differences in human capital between mothers and nonmothers. Our first analysis presents information about wage differences between women

with different familial statuses, and in subsequent analyses we examine the degree to which these differences can be attributed to selection and treatment effects.

Before proceeding it is instructive to highlight the salient features of Norwegian family policies. These family policies have been considerably more elaborate than in most other countries (e.g., Leira, 2002), 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, chap. 4). Since 1977, fathers could share the parental leave, except for the first 6 weeks, and, since 1993, 4 weeks of the parental leave have been reserved for fathers. The policies also include tax and cash benefits for families with children. Most importantly, publicly supported high-quality child care is available at a relatively low cost. Additionally, part-time work and flexible hours are almost universally available, and there is no wage penalty for being employed part-time (though there is no research on how part-time status affects promotion rates). These are all institutional arrangements central to lessening the motherhood wage and career penalty (Waldfogel, 1998).

Although most Scandinavian family policies are gender neutral, their first-order impact is primarily on mothers, making it easier to combine family and career, 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 on the adjustments fathers make. In passing Norwegian family legislation, an explicit goal expressed during parliamentary debates was to redefine the family, by changing the norms for how families operate (Kitterød & Pettersen, 2006). There is some evidence that these policies have had their desired effect. Brandth and Kvande (2003) observed changes in how men view fatherhood, and Kitterød (2002) showed that women have decreased their time spent on household work and men have increased theirs. Internationally, Norway—along with Sweden, Canada and the United States—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). The division of household labor is likely influenced by a multitude of factors, not only family policies, but also the women's movement and family culture more broadly. This seems transparent from the fact that Norway and the United States have similar divisions of household work, but the latter has much weaker family policies.

Method

Data

We use matched employee–employer data on entire populations of white-collar employees in central sectors of the Norwegian economy in the period 1979–1996. These allow us (a) to compare employees working in the same occupation in the same establishment (i.e., same job), and to make those comparisons between single, married, previously married, and those with and without children, and (b) to assess the role of sorting. Establishments and their employees can be followed from year to year.

We restricted our analysis to women 20–50 years old, yielding about 1.03 million individual–years. On an annual basis, we use information on 45,293 to 69,051 female employees 20–50 years old, on 12,165 to 14,437 establishments, and on 23,272 to 33,170 occupation–establishment units (see the note to Table 2 below), although in 1979, 1980, and 1982 there were fewer employees and establishments due to incomplete data collection. For each employee, we have information on gender, occupation, age, part-versus full-time status, contractual hours worked, and monthly earnings from work on contracted hours, which excludes wages on overtime hours. Additionally, the data have been matched to registry data from the Central Bureau of Statistics containing detailed educational attainment (length and type, four-digit code), marital or civil status (five statuses), and the number and ages of children and adoptions. This gives annual educational, marital, and parental histories up to year 2005, 9 years beyond the last year for which we have employment data.

The data were collected from individual-level records kept by the establishments and compiled by the Norwegian Central Bureau of Statistics. Norwegian employers are bound by law to collect and report the data (e.g., Central Bureau of Statistics, 1991, pp. 120–123). This information is used in wage bargaining and economic planning, and should be more reliable than information from sample surveys with individual reports of pay rates, hours worked, and occupation. These data on white-collar employees cover all occupational groups with a few exceptions, the main one being CEOs. Wage setting for the white-collar employees is less centralized than among many other employees in the Norwegian economy and there are larger components of merit and individually-determined pay. Motherhood penalties may hence also be larger than among other employees. The employees may therefore present a good test case for how policies potentially may lessen motherhood wage penalties, and the employees are comparable to white-collar employees in the United States and elsewhere in Europe.

The analysis includes five broad sectors of the Norwegian economy: (a) manufacturing, oil extraction, mining, quarrying, transportation, storage, communication, and various other industries; (b) business services; (c) retail and wholesale trade; (d) banking; and (e) insurance. The data cover white-collar employees, such as technical, professional, administrative, and managerial employees. The sectors are representative and account for roughly 25% of all employees in the Norwegian economy.

Measures

From the contractual monthly earnings and contractual hours worked, we computed the hourly wage, which then refers to hourly wages paid on regular work hours, hence not mixing pay on regular and overtime hours. A key goal here is to assess whether there is differential pay by employers. This requires a focus on the rate of pay, and one must therefore keep the rates of pay on regular and overtime hours separate.

Five marital statuses are distinguished: single, married, divorced, separated, and widowed. Three dummy variables (coded 1 versus 0) coded the number of children aged 20 or younger: 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 reached.

The occupational code is quite detailed, with 373, 453, and 494 occupations in 1981, 1989, and 1996, respectively. We use data on employees in all of these occupations. We control for the occupations with dummy variables.

Labor force experience is imputed as age minus 16, minus years of education beyond age 16, minus 1 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, yielding only small differences in results.

Table 2 provides descriptive statistics for our key variables, with annual averages reported separately for each of three periods, 1979–1984, 1985–1989, and 1990–1996.

(Table 2 about Here)

On average employees were observed for 4.3 years (see the note to Table 2), about 45% had children 20 or younger (the percent with children increased over time), about 50% were married in any given year, and 40% were single. About 34% of the employees were single with no children and 6% were single with children (numbers not reported in the table). The percentages of women who were mothers and/or were married were the same in our data as in other sectors, although the percent female was 35% in our data versus 43% among all employees in the Norwegian economy.

If the composition of mothers and nonmothers in our data changed over the period this could by itself have lead to changes in the motherhood wage penalty. About 25% of the women in our data left every year (with about 20% of the men doing so). The retention rates did not vary by marital or motherhood status. Early in the period, mothers were more likely to leave than nonmothers, by 1–5 percentage points. Later in the period, mothers were less likely to leave than nonmothers, by 1–5 percentage points. We found

similar differences and changes in differences by marital status. The differences in years of education of mothers relative to nonmothers were very small over the period. It thus appears thus that the mothers did not become more select during the period; if anything they became less so.

Analytic Strategy

The data have a unique multilevel structure. One level arises from the cross-time dimension, the other level, at a given time point, arises from the nesting of employees within occupations and establishments. Most individuals were observed at several points in time (for panel data see Hsiao, 1986; Petersen, 2004), and some even every year in 1979–1996. Similarly, each establishment was observed at several points in time, in some cases every year from 1979–1996. In a given year, we can account for the clustering of employees into establishments, occupations, and occupation–establishment units. Across years, we can exploit the panel nature of the data, taking into account that some employees were observed at more than one point in time. Additionally, we can account for the fact that some employees remained in the same establishment, occupation, or occupation–establishment unit.

We report a sequence of four regression equations predicting wages. Each equation includes independent variables for education and imputed labor force experience, plus dummy variables for marital status and the number of children 20 years or younger. 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 (i.e., the job). In the second, third, and fourth specifications we introduce respectively a dummy variable (equal to 1 if in the unit, 0 otherwise) for the occupation, the establishment, and the occupation-establishment unit in which the individual is employed. Controlling for these dummy variables is referred to as introducing fixed effects. When we control for the occupation-establishment unit, the coefficient for having say one child, gives the average difference on the dependent variable between women with one child and childless women, given that the

women were employed in the same occupation-establishment unit (i.e., within same job), controlling for the other variables, with analogous interpretations when we control for the occupation or the establishment. We refer to the four specifications as the *Population*, *Establishment*, *Occupation*, and *Occupation–Establishment* estimators.

For our key children variables, 223 out of 228 coefficients were significantly different from zero at the .05 level, often with z- or t-statistics of 40–50 and significance levels of .000001 or better, and only 2 coefficients did not reach significance at the .10 level. No point is served in reporting these significance levels, as the gigantic z-statistics reflect the large number of observations each year, not superior model specification. The coefficients for some of the postmarital states (with few observations) did not reach significance at the .10 level. We indicate in notes to the tables the variables for which and the number of annual coefficients that fail to reach statistical significance at the .05 or .10 level.

The dependent variable is the natural logarithm of the hourly wage. When small (e.g., less than 0.10 in absolute value), a coefficient can be interpreted as giving the relative change in wages. A coefficient of $-.10$ for having one child implies that women with one child on average earn roughly 10% less than childless women, controlling for the other variables.

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 equations are estimated separately for each of the 18 years in the data, allowing us to assess changes over time.

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 the same work is done in the same workplace (i.e., job).

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.

We selected employees who in a given year were single and childless. The variables for current marital and children status are then excluded. But we introduce two new dummy variables: “Ever married,” coded one if the employee some time in the future got married and zero otherwise, and “Ever children,” coded one if the employee some time in the future had children and zero otherwise. The variables for future marital and motherhood status are measured up to 2005. Otherwise, the analysis is identical to the one described above.

This analysis addresses the question of selection effects most directly, assessing whether future marital and parenthood statuses can predict the women’s wages while they are single and childless, that is, whether the effects were present even before marriage or parenthood 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 next estimate the treatment effect more directly utilizing the longitudinal structure of the data. We add a dummy variable (i.e., fixed effect) for the individual employee in addition to dummy variables (i.e., fixed effects) for establishment, occupation, and occupation–establishment. We then assess whether individuals, as they transition between statuses—from single to married to previously married, and from childless to having one, two, and three or more children—experience within-individual changes in wages following such transitions. Here we compare women who changed from being childless to having 1, 2, or 3+ children, and the comparison is within the individual, that is, what were her wages before and after having children. We use the individual-level data from multiple years, observing employees before and after family transitions. A coefficient of $-.10$ for having 1 child

implies that wages at the individual level were roughly 10% lower after the transition from being childless to having 1 child relative to what the wages would have been had the women stayed childless. We refer to these results as within-individual analyses.

Accounting for only individual or for only occupation–establishment fixed effects is straightforward. Accounting for both simultaneously is difficult. With two sets of fixed effects there is no estimator where all the dummy variables “vanish” from the estimation procedure. And with about 60,000 employees each year, and some 29,000 occupation–establishment units (see the note to Table 2), estimating the effects of all the dummy variables may be impossible; no software known to us can do so. Our solution is to adapt a procedure from Goux and Maurin (1999). We create a fixed effect specific to the individual and respectively the establishment, the occupation, and the occupation–establishment unit in which she worked. When the individual changed 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, because 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, 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 within the individual, before and after the individual entered into marriage and parenthood.

A clarification of terminology should be noted. We can uniquely identify the selection effect. But what we refer to as the treatment effect really consists of two parts: The true treatment effect from possible employee adaptations to the family situation, plus the 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 the occupation–establishment

level, then a reasonable inference is to assume that there are neither true treatment nor discrimination effects at that level.

Results

Differential Pay Within Jobs Versus Sorting

Panel A of Table 3 gives the coefficients on wages of marital status and number of children 20 years or younger. Although each regression is estimated separately by year, to make the presentation more compact, we have averaged the coefficients across years within three periods: 1979–1984, 1985–1989, and 1990–1996.

(Table 3 about here)

The effects of children declined strongly from the beginning to the end of the period. At the population level, the penalties in 1979–1984 were 3.6%, 8.1%, and 13.7% for one, two, and three or more children. In 1985–1989, the penalties were similar, but by 1990–1996, they were reduced to 1.9%, 3.1%, and 4.8%. From the beginning to the end of the period, the penalties for having children thus dropped by 60%.

As one successively controls for establishment, occupation, and the occupation–establishment unit, the penalties decrease substantially. At the occupation–establishment (or within-job) level in 1979–1984, the effects of children on wages were 1.6%, 4.0%, and 6.2% for one, two, and three or more children, respectively, a 50–60% reduction relative to the population-level effects in same period. By 1990–1996, the effects of motherhoods at the occupation–establishment level had virtually disappeared. Women with one, two, and three or more children earned 0.6%, 0.9%, and 1.4% less than childless women once they worked side-by-side (in same occupation and establishment), and within-occupation effects were only marginally larger.

In contrast to the effects of children, the effects of marital status did not differ substantially across the three periods: Wages were 4–6% higher for married women than for their single counterparts. Having been previously married resulted in roughly same wages as being currently married, suggesting that it is not being married per se that leads to higher wages. These premia declined as controls were introduced for the various levels. When we compare only women who work in the same occupation or occupation–establishment unit the marriage premia were quite small at 1.0–1.5%.

What can we then conclude? For marriage, over the entire period, women experienced a wage premium for being married at the population level, but when comparing married and unmarried women who worked in the same occupation or occupation–establishment unit, only small differences remained. For children, there were sizeable penalties, especially earlier in the period for two and three or more children. These effects were largely due to the sorting of mothers and nonmothers into different jobs, but earlier in the period differences remained even at the occupation–establishment (i.e., job) level. By 1990–1996, when nonmothers and mothers worked side-by-side (in same occupation and establishment), they received about the same pay, regardless of whether the mothers had one, two, or three or more children. Employers, for all practical purposes, did not pay women with children less than they paid childless women. The strong decline in the children penalties from 1979–1984 to 1990–1996 at all levels constitutes a remarkable historical change over a short period.

Are Women Who Marry and Have Children Different?

Are the women who marry or have children different from those who remain single and 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 that come in conjunction with marriage and parenthood?

Panel B of Table 3 answers the question about selection effects. We selected the set of women who in a given year were single and childless and then examined the “effects” of eventually marrying or becoming a mother.

At the population level, there was a wage advantage of 1.8–2.5% for those who eventually marry, which was stable across time. It was entirely the result of sorting into different occupations and establishments. The premarital premia provide prima facie evidence that about a third to a half of the premium to actually being married was due either to employee choice or to higher productivity, not to differential treatment by employers, because employers cannot sort employees on the basis of their future marital status.

There was a selection penalty of about 1.6–2.4% for the women who eventually became mothers, which also was relatively stable over time. They earned lower wages even before motherhood. The penalty was entirely due to sorting. When the two groups of women—those who remained childless and those who eventually became mothers—worked in the same occupation and occupation–establishment unit, they received the same pay.

Over time, the selection effect has decreased slightly (from Panel B of Table 3), whereas the total effect of having children has decreased dramatically (from Panel A), implying that the role of selection relative to treatment went up. We can speculate that the family policies have been largely successful in reducing the penalties to having children, and that more of the wage penalty experienced by women who became mothers in later years was due to the fact that, even before they had children, they worked in jobs different from the jobs of women who did not become mothers.

In summary then, according to these analyses the motherhood penalty was mainly about treatment, but the treatment effect declined substantially over time. Women who married, however, received a minor premium of about 2% prior to getting married, a selection effect. These premia were primarily self-made, through seeking better opportunities or higher productivity.

Do Women's Wages Change Upon Marriage and Motherhood?

Finally, we report an analysis of within-individual dynamics, which shows how wage levels evolved as the women moved from one marital status to another and from being childless to having children. Because this analysis includes individual-level fixed effects, we cannot estimate coefficients separately by year. These models address treatment effects, as they compare wages within individuals, comparing the wages of an individual before and after having children. The results are given in Table 4.

(Table 4 about here)

There were sizeable negative treatment effects of about 2.1%, 5.2%, and 7.6% for one, two, and three or more children at the population level, with smaller effects at the other levels, so that upon becoming a mother wages were lower than they would have been had she stayed childless. For marriage, there were small positive treatment effects of 0.5–1%, with similar effects for postmarital states. This indicates that selection effects were most important in explaining the marital premia, but that the effects of children are not attributable to selection.

Summary of Results

From the within-individual analysis we see that selection effects explained some of the marital premium. There were also clear but small selection penalties for women who eventually became mothers. But once a mother, there were strong treatment penalties, which accounted for almost the entire children penalties. These penalties arose mostly through sorting on occupations and occupation-establishment units. When we compared mothers and nonmothers who worked in the same occupation in the same establishment (i.e., job), we still observed some differences, though by 1990–1996 these differences had practically disappeared, being of the order of 1%. Our novel comparison here reveals an entirely novel result. The decline over time in the motherhood penalties is also a novel finding.

Discussion

With women suffering significant wage penalties from motherhood, the processes that occur in the family are today probably the largest obstacle to continued progress in gender equality. To understand how to mitigate these penalties, one needs to identify both where they arise and the potential role of public policies.

Our study provides two unique angles on these questions. First, we offer the first investigation of the within-job motherhood wage penalty, that is, do mothers and nonmothers earn different wages when they work in the same occupation in the same establishment (i.e., job). This is important because we assess whether the penalties possibly arise from wage discrimination in the workplace. Second, we assess how the penalties changed over an 18-year period in Norway, a country where family policies have sought to facilitate the combination of family and career, including attempts to get fathers more involved in caring for children. Such policies should by themselves have led to lower penalties than in other countries, and to their decline over time.

We have three conclusions. First, there were major wage penalties to motherhood, but these declined strongly over an 18-year period: from 3.6%, 8.1%, and 13.7% for one, two, and three or more children aged 20 or younger in 1979–1984 to less than half that level in 1990–1996 (1.9%, 3.1%, and 4.8%). The marital wage premia were small, about 4.0–6.0%. The decline in the motherhood penalties was likely caused by changes in family policies, but also by changes in the culture of how families operate. No other study has reported changes over such a long and important time period.

Second, the penalty to motherhood (and premium to marriage) was mostly due to sorting on occupations and occupation–establishment units, and the relative role of sorting increased over the period. Women with children worked in different and lower-paying occupations and occupation–establishment units than childless women. By 1990–1996, once mothers and nonmothers worked in the same occupation in the same establishment (i.e., job), they for most practical purposes received the same pay, the motherhood

penalty was 0.6%, 0.9%, and 1.4% for one, two, and three or more children. The penalty at this level was significantly reduced over the 18-year period. This indicates a strong reduction of discrimination or of productivity differences. The results answer a question previously not addressed—What is the role of differential pay from employers?—showing that it is now relatively unimportant.

Third, mothers were, wage-wise, negatively selected: Among childless women, those who subsequently became mothers earned slightly lower wages than those who remained childless. But once a mother, major negative treatment effects of motherhood added to the very small negative selection effects. The motherhood penalty was thus mainly a treatment effect: Women, when they became mothers, tended to change employment to lower-paying occupations and occupation-establishment units. The small marital premium in contrast was almost exclusively due to selection.

What are the central implications? One is that by 1990–1996 the motherhood wage penalty no longer arose from employers paying mothers less than nonmothers. At that level there appeared to be minimal discrimination or productivity differences by motherhood status. The penalties to motherhood found in the market, must thus have arisen from differentials in hiring, wage increases, or promotions. We could not investigate hiring and promotions. We did find, in separate analyses not reported in the paper, that mothers received greater wage increases than nonmothers. This leaves differentials in hiring and promotions as the major culprits, with differences arising either from employee choice or employer discrimination. The single case study done in Norway reports no motherhood penalty in getting hired (Petersen & Togstad, 2006), though research in the United States suggests that this is likely important (Correll, Benard, & Paik, 2007). On balance, it is thus possible, perhaps even likely, that in Scandinavia further regulation of employers is unlikely to greatly reduce the motherhood penalty, at least not in wages and wage growth.

Another important implication follows from the strong decline in the motherhood wage penalties over time, which stands in contrast to Avellar and Smock's (2003) finding of no such decline in the United

States. Though difficult to ascertain, our finding that the motherhood penalty has diminished likely reflects (a) major changes in the interrelationships between family and work, including less demands on mothers' time in the household and better access to child care, which result from the gradual impact of family policies, and (b) changes either in employer behavior through lower prejudice 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. Concurrent with changes in family policies, there have been major changes in family culture, which may exert their own effects on the motherhood penalty. Comparable changes in family culture occurred in the United States, but without comparable changes in the motherhood wage penalty. This may lead one to conjecture that family policies were a major source of change.

As for the generalizability of our findings, it is important to note that pay setting for the white-collar groups in our data was less centralized than in other sectors of the Norwegian economy. Individual pay systems and merit pay were more widespread than among blue-collar and public-sector employees. Our findings on the within-job motherhood wage penalty may thus generalize to other settings, such as among white-collar employees in the United States and other Scandinavian countries. However, our findings on the decline of the motherhood wage penalty, and its virtual disappearance at the occupation and especially the job level, may at present be unique to the Scandinavian setting, though it may be indicative of what could happen in other countries given the right public policies.

What then are 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, in that there is more or less equal pay for equal work among mothers and nonmothers. Judging from the changes over time in the overall 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. From the available evidence, though difficult to ascertain, it appears that the policies have had one of their intended effects, the partial removal of the penalties from “the greatest barrier” (i.e., the impact of children) “to economic equality” between men and women (Fuchs, 1988, p. 147).

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Table 1

The Implications of the Three Hypotheses for the Wages of Women from Family Transitions and Changes in the Marital Premium and Children Penalty Over Time

	Treatment					
	Selection	Changed work effort from			Discrimination	
		Household specialization	Children time constraints	Human capital accumulation	Prejudice	Statistical
		1	2	3	4	5
Marital states						
Single	High wage	Low wage	NA	High wage	Low wage	Low wage
Married	High wage	High wage	NA	Low wage	High wage	High wage
Postmarried	High wage	Low wage	NA	Low wage	High wage	High wage
Change over time	Zero	Decline	NA	Zero	Decline	Decline
Motherhood states						
No children	Low wage	High wage	High wage	High wage	High wage	High wage
Children	Low wage	Low wage	Low wage	Low wage	Low wage	Low wage
Change over time	Zero	Decline	Decline	Decline	Decline	Decline

Note: NA = Not Applicable.

Table 2
Descriptive Statistics

	1979–1984	1985–1989	1990–1996
Marital status (%)			
Single	38.9	43.6	37.1
Married	50.9	44.3	49.3
Divorced	6.8	8.5	10.1
Widowed	1.0	0.7	0.7
Separated	2.4	2.9	2.8
Ever married	85.3	81.4	79.3
Parental status (%)			
No children	54.9	57.3	49.4
One child	22.2	23.2	26.2
Two children	17.3	16.3	20.5
Three+ children	5.6	3.1	4.0
Ever children	82.0	82.7	82.7
Education (%)			
Basic education	90.0	81.7	72.3
College/bachelor	3.3	6.8	12.6
Graduate	0.4	0.9	1.8
Professional	6.3	10.6	13.3
Wage			
Mean	43.8	72.8	103.7
Std. Dev.	14.0	20.7	28.7
Experience			
Mean	14.2	14.1	16.2
Std. Dev.	9.3	9.5	9.4
<i>N</i> individual–years	264,915	317,217	445,330
<i>N</i> individuals	119,763	120,812	130,755
<i>N</i> occupations	485	502	587
<i>N</i> establishments	24,382	24,020	24,166
<i>N</i> occ–est	66,409	75,572	87,075

Note: The statistics above have been computed separately for individual–years within each of three periods (1979–1984, 1985–1989, and 1990–1996). We computed the distributions (in percent) on marital status, parenthood status, educational attainment, and means and standard deviations for experience. We computed the average wage for each marital and parenthood status as proportion of average wage of single and childless employees. The last five lines of the table give for each of the three periods (a) the number of individual–years, (b) the number of distinct individuals, (c) the number of occupations, (d) the number of establishments, and (e) the number of occupation–establishment units. The total numbers across all years are as follows: individuals (236,857), individual–years (1,027,462), occupation–years (7,720), establishment–years (211,327), and occupation–establishment–years (483,965). Excluding the years 1979, 1980, 1982—the three years when our data are not complete—the average, the minimum and maximum number of observations per year are: individuals (Mean = 60,747, Min = 45,293, Max = 69,051), occupations (Mean = 464, Min = 373, Max = 523), establishments (Mean = 12,523, Min = 12,165, Max = 14,437), occupation–establishments (Mean = 29,099, Min = 23,272, Max = 33,170). Over the 18-year period 1979–1996 individuals are observed on average for 4.3 years.

Table 3

Effects of Marital Status and Children 20 Years or Younger on Logarithm of Hourly Wage

	1979–1984				1985–1989				1990–1996			
	Pop	Est	Occ	O–E	Pop	Est	Occ	O–E	Pop	Est	Occ	O–E
Panel A: Overall Population												
Married	.044	.029	.016	.016	.058	.039	.016	.013	.050	.033	.013	.010
Divorced	.059	.015	.025	.008	.071	.021	.028	.006	.050	.021	.016	.005
Widowed	.032	.013	.009	.006	.037	.019	.004	–.003	.033	.016	.008	.002
Separated	.044	.009	.022	.007	.060	.017	.025	.006	.041	.017	.016	.006
One child	–.036	–.024	–.022	–.016	–.034	–.020	–.018	–.009	–.019	–.015	–.010	–.006
Two children	–.081	–.058	–.049	–.040	–.077	–.052	–.044	–.029	–.031	–.024	–.017	–.009
Three+ children	–.137	–.095	–.077	–.062	–.125	–.082	–.067	–.046	–.048	–.032	–.022	–.014
Panel B: Childless singles												
Ever married	.018	.006	.007	.004	.021	.009	.007	.004	.025	.007	.008	.002
Ever children	–.024	–.011	–.006	–.003	–.018	–.006	–.004	–.001	–.016	–.003	–.001	.002

Note: These results control for experience—as experience and experience squared—and for five educational groups represented by dummy variables. The dummy variables for children are for having one child, two children, or three or more children 20 years or younger. In the column denoted Pop, no further controls are introduced. In the columns denoted Est, Occ, and O–E, we introduce fixed effects for the establishment the employee worked in, the occupation worked in, and the occupation-establishment unit worked in. The estimates are obtained separately for each of the 18 years in the period 1979–1996. The table reports the average of the yearly coefficients for three subperiods, 1979–1984, 1985–1989, and 1990–1996. The analysis is restricted to employees 20–50 years old. Panel A (labeled Overall population) pertains to the entire sample. Panel B (labeled Childless singles) reports regression coefficients estimated for employees who in the given year are singles and have no children. The dummy variable for Ever Married indicates whether the employee eventually got married (= 1) or not (= 0) by 2005. The dummy variable for Ever children indicates whether the employee had children (= 1) or not (= 0) by 2005. For marital status, for being married, 72 of 72 coefficients is statistically significant from zero at the .05 level, and for the postmarital states, 36 of 72 annual coefficients reached statistical significance at the .10 level. For children, 211 of 216 coefficients reached statistical significance at the .05 level, another 3 at the .10 level, and only 2 were not statistically significant at the .10 level. For Ever Married. 36 of 72 coefficients reached statistical significance at the .10 level, while for “Ever children” 71 of 72 coefficients did so at the .05 level.

Table 4

Effects of Marital Status and Children Aged 20 or Younger on Logarithm of Hourly Wage, Controlling for Individual-Level Fixed Effects

	Individual x			
	Individual	Est	Occ	Occ-Est
Married	.010	.008	.007	.006
Divorced	.013	.010	.008	.008
Widowed	.010	.009	.005	.005
Separated	.012	.010	.008	.008
One child	-.021	-.017	-.015	-.013
Two children	-.052	-.039	-.034	-.030
Three+ children	-.076	-.058	-.051	-.044

Note: In these analyses an individual-level fixed effect is included in each column. Where establishment-level fixed effects additionally are included, we created a dummy variable specific to the establishment and individual. As long as an individual remained in the same establishment, the fixed effect remained the same. When the individual changed establishment, the fixed effect also changed. The same procedure is used for the occupation and occupation-establishment level fixed effects. This procedure 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. Each coefficient in the table is statistically significant from zero at the .05 level.