# The Family Gap for Young Women in the United States and Britain: Can Maternity Leave Make a Difference?

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In the United States and Britain, there is a "family gap" between the wages of mothers and other women. Differential returns to marital and parental status explain 40%–50% of the gender gap. Another 30%–40% is explained by women's lower levels of work experience and lower returns to experience. Taking advantage of "quasi experiments" in job-protected maternity leave in the United States and Britain, this article finds that women who had leave coverage and returned to work after childbirth received a wage premium that offset the negative wage effects of children.

In both the United States and Great Britain, female-male differentials in hourly pay have narrowed, but a substantial gender gap persists, along

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with a "family gap," that is, a gap in pay between women with children and women without children. With women's earnings increasingly important to the support of their families, these pay differentials and policy interventions that might mitigate them have important implications for the well-being of families with children. Maternity leave policies are of particular interest since both the United States and Britain are currently expanding access to job-protected maternity leave for working women.

This article differs from previous research in focusing on young American and British "equal opportunities" cohorts.[1] The American women, who were on average 18 years old in 1978, entered the labor market at a time when equal opportunity and affirmative action laws (e.g., the 1963 Equal Pay Act; Title VII of the Civil Rights Act of 1964, outlawing sex discrimination in employment; the 1972 amendment to Title VII, barring discrimination in education; and the 1978 amendment to Title VII, barring pregnancy discrimination) were firmly in place. The British women turned 17 in 1975, the year the Equal Pay Act came into full effect, and 18 in 1976, the year maternity leave legislation passed.

The first part of this article reviews related research and the motivation for this research. Part II presents the analysis of the gender gap and family gap, using a young American cohort from the National Longitudinal Survey of Youth and a young British cohort from the National Child Development Survey. Part III uses these cohorts to look at the effects of maternity leave. Part IV presents conclusions.

## I. Related Research and the Motivation for This Research

### A. The Gender Gap and Family Gap

Despite recent progress, there is still an unexplained gender gap in pay in both the United States and Britain.[2] How much does this gender gap vary by family status? Blau and Kahn (1992) report that, in the late 1980s single, childless women in both the United States and Britain earned over

[1] See Dex and Shaw (1986) for a comparative study of the effects of equal opportunities policies for earlier cohorts.

[2] In Britain, most of the recent progress in closing the gender gap occurred in the 1970s, with the implementation of the Equal Pay Act in 1975 (Zabalza and Tzannatos 1985; Wright and Ermisch 1991; Joshi, Paci, and Waldfogel in press). There was also some slight convergence in the late 1980s, due in large part to changes in the male wage structure; these changes are detailed in Katz, Loveman, and Blanchflower (1992) and Schmitt (1993). In the United States, convergence occurred later but was more dramatic, with women's wages rising sharply relative to men's over the 1980s (Sorensen 1991; Blau and Kahn 1994). This progress was due in large part to increases in women's human capital (Goldin 1990; O'Neill and Polachek 1993) and to changes in the male earnings structure (Blackburn, Bloom, and Freeman 1990; Katz and Murphy 1992).

95% of single men's pay, while married mothers earned only 60% of married men's pay. Waldfogel (1994) found that mothers' hourly wages lagged behind nonmothers' in both the late 1970s and late 1980s. In the United States, the "family gap" between mothers' pay and nonmothers' pay grew over the 1980s while the gender gap narrowed; nonmothers' pay relative to the mean for all men rose from 68% to 80%, while mothers' grew from 63% to 70%. In Britain, the gender gap and family gap were both fairly stable, as nonmothers' wages rose from 72% to 74% of men's, while mothers' rose from 57% to 61%.

Research on marital status and wages has consistently found strongly positive effects of marriage for men but not for women, with estimates for women ranging from slightly negative to slightly positive depending on the specification (see Malkiel and Malkiel [1973] and Korenman and Neumark [1991, 1992] for the United States; Greenhalgh [1980] and Dolton and Makepeace [1987] for Britain; and Schoeni [1990] for a cross-country study). Research on children and wages (see Fuchs 1988; Korenman and Neumark 1992; Neumark and Korenman 1994; Waldfogel 1997*b* for the United States; Joshi 1991; Joshi and Newell 1989 for Britain) finds negative effects for women. An intriguing study (Wood, Corcoran, and Courant 1993) finds that 40% of the gender gap among American lawyers is explained by parenting responsibilities.

Several explanations have been advanced as to why women with children might have lower wages, even after controlling for observable characteristics. One possibility is that mothers might bring less effort to the labor market.[3] Another possible explanation is that employers might discriminate (e.g., in hiring and promotion) against women with family responsibilities. Alternatively, structural "family barriers" such as the lack of family leave might impede the progress of mothers in the labor market.

In estimating the effects of children on women's wages, two types of bias are of particular concern.[4] First, if women with children have lower wages because of the accumulated effect of the reduced investment they make in wage-enhancing human capital, in particular, the loss of work experience subsequent to marriage or child-bearing (Becker 1985, 1991; O'Neill and Polachek 1993), then estimates of the effects of family status

[3] Becker (1985) argues that, even when women have equal amounts of human capital, their "energy" for work is diminished by the greater energy demanded by their household tasks relative to the energy men devote to their leisure activities.

[4] Two other potential sources of bias are of lesser concern. Recent research on endogeneity bias (Korenman and Neumark 1992; Waldfogel 1994) did not reject the exogeneity of children in the wage equation. Waldfogel (1994) tested for selection bias in estimating the effects of children on women's wages and found none.

without actual experience would be biased. Prior research on the magnitude of this bias has been inconclusive. In the United States, Hill (1979) found that controlling for actual work experience eliminated virtually all the effects of children, but Korenman and Neumark (1992) and Waldfogel (1994, 1997*b*) found that a significant child penalty remained even after controlling for actual experience. This article provides further evidence on this point by examining the role of actual versus potential experience for these American and British young women.

Second, if women who differ in family status differ in some unmeasured productivity-related characteristic, such as "motivation to work," then unobserved heterogeneity could bias cross-sectional estimates. Prior research using short first differences found some evidence of heterogeneity bias (Korenman and Neumark 1992), while research using differences across sisters found none (Neumark and Korenman 1994). This article extends that research in two ways. First, a longer time interval is used in the first difference models since the wage effects of children may not be readily observable within the first year or two (particularly since many mothers will not yet have returned to work). Second, a standard (rather than family) fixed-effects specification is used, so that the entire sample of working women (rather than just a subsample with sisters in the data set) can be exploited.

### B. Maternity Leave

The lack of job-protected maternity leave can be viewed as a "family barrier" that might prevent women with children from competing on an equal footing in the labor market. This article uses young American and British cohorts, who had children in the 1980s when maternity-leave coverage and usage expanded dramatically, to investigate the effects of maternity leave on women's pay.

The United States and Britain in 1991 offer two contrasting case studies. Neither is a true "natural experiment"; that is, neither randomly assigns women to maternity leave coverage. In the absence of a true experiment, however, these two case studies do offer two quasi experiments regarding the potential effects of maternity leave coverage. The British case is particularly informative from the U.S. perspective since it is an experiment in statutory, rather than employer-provided, coverage. Thus it might prove helpful in thinking about the likely effects of leave mandated by legislation such as the Family and Medical Leave Act (FMLA).

As can be seen in appendix A, the United States had no national maternity-leave legislation prior to 1993, but an estimated 40% of working women had explicit maternity leave rights prior to the passage of the FMLA owing to state laws, union contracts, or voluntary employer provisions. This coverage depended very much on one's employer and was correlated with other employer characteristics (e.g., union status). In addition, the Pregnancy Discrimination Act of 1978 effectively provided maternity leave rights to some working women, as it mandated that employers with disability plans must cover pregnancy as they did other disabilities. Therefore, the 40% figure cited above understates the true proportion of women who had some form of leave coverage that they could use for maternity. Britain, in contrast, has had maternity-leave legislation since 1976, but only about half of women workers were covered because (until the reforms of 1993) only women with 2 years of full-time or 5 years of part-time job tenure were eligible. Thus in Britain, both legislation and one's own work experience and particularly job tenure determine coverage. This is the case under the new U.S. legislation as well, making the comparison with the British case particularly interesting.

What the leave entailed also differed a great deal across countries. In Britain, leave was quite long, even by European standards, with a maximum of 40 weeks permitted, and a substantial portion of the leave, 18 weeks, was paid. In the United States, in contrast, leave typically was quite short, 20 weeks on average, and nearly always unpaid (unless the woman had access to other paid leave time such as sick time).

How do these differences in coverage relate to differences in the labor force participation of new mothers? In both countries, the participation rate of women with children under the age of 1 has traditionally been low, but this changed dramatically in the 1980s. In Britain, nearly half (46%) of all women with children under age 1 were in the labor force in 1989 (vs. 24% in 1979), with a return rate of 65% for those who had been working before childbirth (vs. 38% in 1979) (Daniel 1980; McRae 1991). In the United States, the participation rate of women with children under age 1 rose from 31% in 1976 to 54% in 1992 (Bureau of the Census 1993). Return rates rose from 38% in 1975 to 51% in 1980 to 68% in 1984 (O'Connell 1990). Thus, participation rates for new mothers in Britain started at a lower base and rose somewhat more quickly than in the United States.

Factors other than the increased availability of leave influenced this rapid increase in the participation and return rates of new mothers. Chief among them were increased financial pressures on families in the 1980s (see Harkness, Machin, and Waldfogel [1996] for evidence on this point for Britain, but see also Juhn and Murphy [1996], who find that rising wages were the most important factor in the United States) and changed social norms about mothers of young children working. Employer attitudes changed as well, particularly in Britain. Many British employers reacted to the requirement to hold jobs open by implementing strategies such as contractual maternity pay (that makes up the difference between statutory maternity pay and full pay) and job-retention schemes (such as offering women the opportunity to return part-time) to bring more women back and to bring them back sooner (McRae 1991; Incomes Data Services 1994; Waldfogel 1994).

The effects of maternity-leave coverage on pay are unclear a priori (Blau and Kahn 1992; Waldfogel 1997*c*). To the extent that maternity leave allows women to take more time out of the labor market, expanding coverage might have a negative effect on pay because of the loss of human capital. There might also be negative pay effects for women in the aggregate if maternity leave is costly for employers and if those costs are passed on to women workers in the form of lower pay (see recent work by Waldfogel [1997*a*] and Ruhm [1998] for some evidence on this). However, to the extent that maternity leave allows women to benefit from pre-birth job tenure, maintain good job matches, and continue to progress up a firm's career ladder, extending rights would have a positive effect on pay.

In this regard, one can think of women who have children and lack the right to a job-protected maternity leave as analogous to displaced workers. In the case of displaced workers, the loss of seniority (Becker 1975), the interruption of a good job match (Jovanovic 1979), and the need to start over with a new employer (Lazear 1981) would be expected to result in earnings losses, although such losses might well diminish over time once a new match was established (Jacobson, LaLonde, and Sullivan 1993*b*). I would argue that the same analysis would apply to the case of women who lack maternity leave rights, as they too would lose the benefits of their accumulated job tenure with the old employer, have to find a new match, and perhaps enter further down the new employer's career ladder.

There is little direct evidence available on the wage effects of maternity leave coverage. Recent research in the United States (Waldfogel 1997*b*) and Britain (Waldfogel 1995; Joshi, Paci, and Waldfogel 1996) has found that women who return to their employers after childbirth have higher wages than do other mothers, but this work did not look at the effects of coverage per se. There are also two important sources of indirect evidence on this question. First, it is well known that job changes and gaps in employment have lasting negative wage effects for mothers above and beyond the loss of work experience (Martin and Roberts [1984] in Britain; Jacobsen and Levin [1995] in the United States), which suggests that there might be negative effects of not having coverage. Second, continuing the analogy with displaced workers, the literature on displacement (see, e.g., Jacobson, LaLonde, and Sullivan, 1993*a*, 1993*b*; see also Ruhm [1991] on the wage costs of displacement and Crossley, Jones, and Kuhn [1994] on displacement costs for women) also suggests that the wage effects of not having a job-protected maternity leave would be negative. This article provides direct evidence on this question by estimating the effects of maternity-leave coverage on women's wages.

## II. The Gender Gap and Family Gap

### A. Data and Empirical Strategy

I use two longitudinal data sets, the National Longitudinal Survey of Youth (NLSY) from the United States and the National Child Development Study (NCDS) from Britain, to track wages and wage changes over time and then use ordinary least squares (OLS), difference, and fixed-effects models to assess the effects of children on women's pay. Although the two data sets are not perfectly comparable (e.g., the NLSY oversamples minorities, while the NCDS underrepresents immigrants), they nevertheless offer a range of parallel variables for samples of young adults (for more on the comparability of NLSY and NCDS, see Blanchflower and Lynch [1994]).[5]

An important element of the empirical strategy employed here is a comparison of wages at different moments in time, when women may have different numbers of children. In the difference models, I use an "early" and a "late" wage, separated on average by about 8 years; in the fixed-effects models, I use an "intermediate" wage if available as well. A woman need not be working during a particular survey year in order to be included in the sample; rather, any woman who worked at some time in the early years of the survey and at some time in the later years of the survey is included. This is important both to maximize sample size but also, more important, to minimize the potential for sample selection bias.

There are two advantages to the strategy of comparing early and late wages. First, it provides a long, as opposed to a short, 1- or 2-year, first difference with which to assess the effects of children. Using long differences is important since many women with children may not be observed working until several years after the birth; thus, short differences may provide biased estimates of the effects of children on women's pay (Waldfogel 1997*b*). Second, and more pragmatically, it is the best way of utilizing the British data set, which collects information on wages at only two or three moments in time, as described below. Using the American data set in this way, then, allows for a more direct comparison across the two samples.

The British data set is the National Child Development Study. The NCDS includes every child born in Britain during the first week of March 1958, with surveys conducted at birth, age 7, age 11, age 16, age 23, and, most recently, in 1991, at age 33. Over 3,800 young women reported wage data from at least two jobs: a current or job in 1991, a prior job as of 1991, and/or a current or last job in 1981.[6] The American data set is

[5] I use the sampling weights in the NLSY in computing means and in decomposing the gender gap, in order to correct for the oversampling of minorities in that data set.

[6] In the NCDS, I use the wage from the current job at the age 33 interview in 1991, if available, for the late wage; if not, I work backward to find the most recent wage. For this reason, not all women are age 33 at the time of the late wage. For the early wage, I use the current job at the age 23 interview in 1983, if available; if not, I work backward to the most recent job, or if none is available

the National Longitudinal Survey of Youth, a representative sample of young men and women followed since 1979. I use the NLSY to construct a sample of young women as close as possible in age and year to the British sample. This results in an NLSY sample of nearly 4,400 young women who have wage data from an early job (defined as from the period 1979–83), when the women are ages 18–25 (I exclude those younger than 18), and a late job (defined as from the period 1987–91), when they are ages 26–34.[7] Thus, although the data sets are constructed in a very different way (the NCDS consists of observations at basically two moments in time [1981 and 1991] while the NLSY has observations annually over the period 1979–91) and the NLSY cohort is younger than the NCDS cohort, my two samples are quite comparable. (Means for both samples are shown in app. B.) I include part-time workers in both samples, as they represent an important share of working women, particularly in Great Britain.[8]

### B. Family Status and Young Women's Pay in the NLSY and NCDS

A first look at the NLSY wage data, shown in table 1A, confirms that there is still a sizeable gender gap in the United States, that the gap grows with age, and that family status matters. The female/male hourly pay ratio starts relatively high (85%) at mean age 21 and then falls (to 77%) by mean age 30. These ratios vary a great deal by family status. Nonmothers do very well in both years (86% of mean pay for all men at 21 and 90% of men's pay at 30), while mothers' relative pay is lower and falls over time, from 82% at 21 to 70% by 30. By the time the women are on average age 30, the gender gap is 20 percentage points higher for mothers than nonmothers. The bottom portion of the table presents average wages for women categorized by their previous and current family status. Reading across this table, a clear pattern emerges: wage growth from mean ages 21–30 is highest for those who had no children and lowest for those who had children by 30. There is very little difference in early wages between women with no children at 21 who are childless at 30 versus those who have children by 30. This suggests that the wage gap at mean age 30 may be due more to slower wage growth for mothers than to preexisting differences between mothers and others.

in 1983, I use an intermediate wage from the period between 1983 and 1991. Thus, not all women are age 23 at the time of the early wage.

[7] For the early wage, I use the wage in 1983, if available; if not, I work backward to 1979. If the only wage available for a woman is prior to her eighteenth birthday, she is not included in the analysis. For the late wage, I use the wage in 1991, if available; if not, I work backward to 1987.

[8] For more detailed work on the British part-timers, see Waldfogel (1995).

**Table 1A**
**Wages for Young Women and Men, U.S. Data**

**A. Mean Wages in the NLSY**

| | Women (N = 4,334) | Men (N = 4,771) | Female/Male Wage Ratio (%) |
|---|---|---|---|
| Mean wage at mean age 21 | 6.01 | 7.06 | 85 |
| Mean wage at mean age 30 | 8.20 | 10.60 | 77 |
| Mean wage change from mean age 21 to 30 (in %) | +36 | +50 | |

**B. Wages by Family Status for Young Women in the NLSY**

| | Nonmothers | Wage/Men's Wage (%) | Mothers | Wage/Men's Wage (%) |
|---|---|---|---|---|
| Mean wage at mean age 21 | 6.10 | 86 | 5.77 | 82 |
| Mean wage at mean age 30 | 9.53 | 90 | 7.45 | 70 |
| Mean wage change from mean age 21 to 30 (in %) | +56 | | +29 | |

**C. Wage Changes by Family Status Changes for Young Women in the NLSY**

| | No Children in Both Years (N = 1,573) | No Children to One Child (N = 784) | No Children to Two or More (N = 760) | One Child in Both Years (N = 242) | One Child to Two or More (N = 557) | Two or More Children in Both Years (N = 418) |
|---|---|---|---|---|---|---|
| Mean wage, mean age 21 | 6.06 | 6.14 | 6.14 | 5.58 | 5.87 | 5.76 |
| Mean wage, mean age 30 | 9.53 | 8.09 | 7.53 | 7.17 | 6.93 | 6.86 |
| Mean wage change (in %) | +57 | +32 | +23 | +28 | +18 | +19 |

NOTE.—Mean age 21 wages are from the 1979–83 NLSY. Mean age 30 wages are from the 1987–91 NLSY. All wages are in 1991 dollars. The NLSY sampling weights are used in computing all means for the NLSY.

An overview of the British wage data, shown in table 1B, tells the same basic story as in the United States, although the overall gender gap is higher at both ages for the British cohort. Mothers start out behind nonmothers, and by age 33, they are 20 percentage points behind, earning only 64% of men's pay versus 84% for nonmothers. The bottom portion of the table highlights the extent to which wage growth over the decade is related to family status. Again, there is very little difference in wages at age 23 between women who go on to have children by age 33 and those who do not, suggesting that the wage differences at age 33 are not driven by preexisting differences.

### C. Estimating the Wage Effects of Children

The OLS models in table 2A estimate the effects of children on women's wages in the United States after controlling for observable characteristics such as education and ethnicity. The model includes the standard human capital variables but does not include other variables (such as industry, part-time status, etc.) that might be outcomes of family status since my aim here is to estimate the full effects of family status after controlling for human capital. This model (and the subsequent ones) include year controls since observations may come from different years. The sample includes some women who are not currently working, as noted above, but models estimated for just current workers produce similar results.

In the first cross-sectional model, there are sizable family penalties: over 7.5% for having one child and 13% for having two or more children. In this model, and the others shown in tables 2A and 2B, there were no significant returns to the marital status variables, and they were therefore dropped from the models.

To correct for potential omitted variable bias, the next OLS model uses the longitudinal feature of the data set to control for actual work experience, in contrast to the first model, which used potential work experience (age minus years of schooling minus five).[9] The estimated effect of two children falls by about a third, to 8%, while the effect of one child falls only slightly, to about 7.5%. This is as expected since having children was acting as a proxy for having less actual work experience.

It is also important to control for heterogeneity bias since unobserved differences may play a role in explaining the wage differences associated with family status if, for example, women with lower unobserved earning power are more likely to have children. Two methods are used here.

[9] Actual experience is the sum of actual work experience starting in 1978 as recorded in the NLSY plus potential experience for the pre-1978 period for those who left school before 1978.

**Table 1B**
**Wages for Young Women and Men, British Data**

**A. Mean Wages in the NCDS**

| | Women ($N$ = 3,840) | Men ($N$ = 3,799) | Female/Male Wage Ratio (%) |
|---|---|---|---|
| Mean wage at mean age 23 | 3.82 | 4.68 | 82 |
| Mean wage at mean age 33 | 4.81 | 6.82 | 71 |
| Mean wage change from mean age 23 to 33 (in %) | +26 | +46 | |

**B. Wages by Family Status for Young Women in the NCDS**

| | Nonmothers | Wage/Men's Wage (%) | Mothers | Wage/Men's Wage (%) |
|---|---|---|---|---|
| Mean wage at mean age 23 | 3.93 | 84 | 3.23 | 70 |
| Mean wage at mean age 33 | 5.70 | 84 | 4.26 | 64 |
| Mean wage change from mean age 23 to 33 (in %) | +45 | | +32 | |

**C. Wage Changes by Family Status Changes for Young Women in the NCDS**

| | No Children in Both Years ($N$ = 1,551) | No Children to One Child ($N$ = 567) | No Children to Two or More ($N$ = 1,167) | One Child in Both Years ($N$ = 111) | One Child to Two or More ($N$ = 160) | Two or More Children in Both Years ($N$ = 284) |
|---|---|---|---|---|---|---|
| Mean wage, mean age 23 | 3.99 | 3.99 | 3.82 | 3.50 | 3.21 | 3.14 |
| Mean wage, mean age 33 | 5.70 | 5.05 | 4.12 | 4.37 | 3.83 | 3.68 |
| Mean wage change (in %) | +43 | +27 | +8 | +25 | +19 | +16 |

NOTE.—Mean age 23 wages from 1981 NCDS-IV. Mean age 33 wages from 1991 NCDS-V. All wages are in 1991 pounds.

**Table 2A**
**Wage Equations for Young Women, Controlling for Actual vs. Potential Work Experience and Controlling for Unobserved Heterogeneity, U.S. Data**

| Variable | OLS (1) | OLS (2) | First Differences (3) | Pooled OLS (4) | Fixed Effects (5) |
|---|---|---|---|---|---|
| Potential experience | .0089* | | | | |
| | (.0032) | | | | |
| Actual experience | | .0461* | .0452* | .0464* | .0527* |
| | | (.0030) | (.0042) | (.0024) | (.0022) |
| Educ1 | .6145* | .5802* | .4757* | .4842* | .6641* |
| | (.0305) | (.0237) | (.1093) | (.0185) | (.0289) |
| Educ2 | .3295* | .2451* | .1093* | .1743* | .2293* |
| | (.0248) | (.0222) | (.0279) | (.0143) | (.0240) |
| Educ3 | .1380* | .0584* | .0584* | .0516* | .1049* |
| | (.0217) | (.0208) | (.0200) | (.0124) | (.0217) |
| One child | −.0761* | −.0746* | −.0583* | −.0542* | −.0457* |
| | (.0194) | (.0189) | (.0221) | (.0117) | (.0115) |
| Two or more children | −.1318* | −.0813* | −.0998* | −.1032* | −.1260* |
| | (.0181) | (.0179) | (.0192) | (.0141) | (.0153) |
| Black | −.1038* | −.0617* | | −.0411* | |
| | (.0183) | (.0180) | | (.0116) | |
| Hispanic | .0161 | .0434* | | .0584* | |
| | (.0207) | (.0202) | | (.0128) | |
| Other: | | | | | |
| Year | yes | yes | yes | yes | yes |
| Age | no | yes | yes | yes | yes |
| Adjusted $R^2$ | .2180 | .2591 | .1166 | .2791 | .1935 |
| No. of observations | 4,334 | 4,334 | 4,334 | 12,767 | 12,767 |

NOTE.—Standard errors are in parentheses. Women in the NLSY data set are ages 21–34, depending on whether their current or last wages are from 1991 or an earlier year (1984–90). Wages below 33% of the minimum wage are excluded. Educ1 is a college degree or higher, Educ2 is some education beyond high school (e.g., community college), Educ3 is a high school diploma or equivalent, and Educ4 is less than a high school education. In the OLS models, the dependent variable is the log of hourly wage. In the first-difference model, the dependent variable is $\Delta$log wage = $(\mathrm{lw}_{it+1} - \mathrm{lw}_{it})$. The first-difference values used here for each variable are the change from 1983, if present, to 1991, if present. If no wage is available for 1983, the most recent year prior to 1983 is used. If no wage is available for 1991, the most recent year prior to 1991 (but later than 1983) is used. Models control for the difference in age and a full set of educational changes. In the pooled OLS model, the variables from 1983 (or earlier if 1983 not available), 1986 (or earlier if not available), and 1991 (or earlier if 1991 not available) are pooled. The panel is composed as follows: 4,099 women have observations over all 3 years, while 235 have observations over only 2 years. The dependent variable is the log of hourly wage. Standard errors in the pooled model are corrected to account for cluster sampling. In the fixed-effects model, each variable from the pooled data set (described above) is expressed as a deviation from the mean value for that variable for each individual in the pooled data set. The dependent variable is $\delta$logwage = $(\mathrm{lw}_{it} - \overline{\mathrm{lw}}_i)$. The coefficients on the child terms in the fixed-effects model are not significantly different from those in the pooled OLS model (in the Hausman test, $p > .10$ for the one child coefficient, $p > .05$ for the two or more children coefficient).

* $p < .01$.

The first method is a first-difference specification, using the early (mean age 21) and late (mean age 30) wage observations:

$$\Delta \mathrm{lw}_i = \Delta \mathrm{age}_i + \Delta \mathrm{ex}_i + \Delta \mathrm{ed1}_i + \Delta \mathrm{ed2}_i + \Delta \mathrm{ed3}_i + \Delta \mathrm{onekid}_i + \Delta \mathrm{kids}_i + \Delta\alpha_i + \Delta\mu_i,$$

where $\Delta lw_i = (lw_{it+1} - lw_{it})$, $\Delta age = (age_{it+1} - age_{it})$, and so on.[10] The individual fixed effect $\alpha_i$ is assumed to be time invariant and is potentially correlated with one or more of the independent variables. The disturbance term $\mu_i$ is assumed to be independent and identically distributed, with zero mean and variance $\sigma^2$. If the unobserved characteristic varies across individuals but not over time, this specification effectively removes it, as the fixed effect, $\alpha_i$, simply drops out. This is a reasonable assumption, if the unobserved variable is an individual characteristic such as motivation or unmeasured ability.

The first difference results for the NLSY are shown in the third column of table 2A. Comparing the difference model to the OLS model, it is apparent that controlling for unobserved heterogeneity has a negligible effect on the estimated effects of children, as neither of the child coefficients in the difference model is significantly different from those in the OLS estimates.

The second method used to control for unobserved heterogeneity is a fixed-effects specification:

$$\begin{aligned}(lw_{it} - \overline{lw}_i) = {} & (age_{it} - \overline{age}_i) + (ex_{it} - \overline{ex}_i) \\ & + (ed1_{it} - \overline{ed1}_i) + (ed2_{it} - \overline{ed2}_i) + (ed3_{it} - \overline{ed3}_i) \\ & + (onekid_{it} - \overline{onekid}_i) + (kids_{it} - \overline{kids}_i) \\ & + (\alpha_{it} - \overline{\alpha}_i) + (\mu_{it} - \overline{\mu}_i)\end{aligned}$$

where $lw_{it}$ = log wage for individual $i$ at time $t$; $\overline{lw}_i$ = mean log wage for individual $i$; and so on. As above, the individual effect $\alpha_i$ is assumed to be time invariant and potentially correlated with one or more independent variables, and the disturbance term $\mu_i$ is assumed to be independent and identically distributed, with mean zero and variance $\sigma^2$. Again, this specification relies on the assumption that the fixed effect is time invariant, that is, that $\alpha_{it} = \overline{\alpha}_i$ for all values of $t$. It allows for the possibility that the individual effect may be correlated with the other variables, and for this reason it is better suited to this problem than a random-effects specification (Hsiao 1986).

The fixed-effects specification uses up to three reported wages (the early wage from 1979–83, the late wage from 1987–91, and an intermediate wage from 1984–86, if available) from the NLSY to track the effects

[10] Education changes are included in the model because increases in education are not uncommon among young people. In the NLSY, education is recorded annually along with the wage. In the NCDS, education is only recorded at the time of the 1981 and 1991 surveys, so the 1981 value is used for the early wage observation, and the 1991 value is used for the late wage observation.

**Table 2B**
**Wage Equations for Young Women, Controlling for Actual vs. Potential Work Experience and Controlling for Unobserved Heterogeneity, British Data**

| Variable | OLS (1) | OLS (2) | First Differences (3) | Fixed Effects (4) |
|---|---|---|---|---|
| Potential experience | .0147* | | | |
| | (.0025) | | | |
| Actual experience | | .0288* | .0418* | .0791* |
| | | (.0020) | (.0027) | (.0043) |
| Educ1 | .8056* | .7772* | .2170* | .1849* |
| | (.0290) | (.0268) | (.0482) | (.0508) |
| Educ2 | .5912* | .5151* | .1827* | .1616* |
| | (.0259) | (.0252) | (.0404) | (.0430) |
| Educ3 | .3647* | .3023* | −.0498* | .0333 |
| | (.0274) | (.0265) | (.0237) | (.0413) |
| One child | −.1112* | −.0962* | −.0991* | −.0907* |
| | (.0191) | (.0192) | (.0234) | (.0142) |
| Two or more children | −.2707* | −.1949* | −.1674* | −.1609* |
| | (.0157) | (.0175) | (.0204) | (.0145) |
| Other: | | | | |
| Age | no | yes | yes | yes |
| Educ4,5 | yes | yes | yes | yes |
| Adjusted $R^2$ | .3287 | .3705 | .1179 | .2094 |
| No. of observations | 3,840 | 3,840 | 3,840 | 8,888 |

NOTE.—Standard errors are in parentheses. Women from the NCDS dataset are ages 23–33, depending on whether their current or last wage is from 1991 to an earlier year (1981–90). Wages below £.50 and above £50 are excluded. Educ1 is university degree or higher, Educ2 is other higher qualification (teaching, nursing), Educ3 is A-levels or equivalent, Educ4 (not shown) is O-levels or equivalent, Educ5 (not shown) is some other qualifications, and Educ6 (the reference category) is no qualifications. The NCDS contains wage data from up to 3 moments in time: 1991 (the current or last job as of 1991); an intermediate date (the previous job as of 1991); and 1981 (the current or last job as of 1981). The first-difference values used here for each variable are the change from 1981 to 1991, if present, or the change from the intermediate year to 1991, if present, or the change from 1981 to the intermediate year, if present. The fixed-effects variables are the values from 1981, the intermediate date, and 1991, if present, deviated from the mean value for the variable over all years. The panel is composed as follows: 1,540 women have observations over all 3 years, while 2,134 have observations over only 2 years.

* $p < .01$.

of children on wages over time. Ordinary least squares regression results with the pooled data set are shown in the fourth column of the table; the fixed-effects results are shown in the last column. The coefficient on one child is slightly lower, and the coefficient on two or more children slightly higher in the fixed-effects model, but neither is significantly different from those in the pooled OLS model.

Taken together, the first-difference and fixed-effects models provide no evidence of significant heterogeneity bias. Controlling for unobserved heterogeneity has had little effect on the estimated wage effects of children. Controlling for actual rather than potential experience did have an effect on the size of the child coefficients, but significant child penalties remained.

Table 2B repeats this analysis using the British cohort. In this case, the woman's age serves as a year control (since all the cohort members were born in the same year). As in the U.S. analysis, the sample includes some women who are not currently working; the results in models not shown here that restrict the sample to current workers are comparable.

The results of the British analysis are in many respects very similar to those from the American analysis. The estimated child penalties are overall higher in the British cohort, as can be seen in the first OLS model in tables 2A and 2B. Controlling for potential work experience and observable characteristics such as education, the penalty to one child is over 11%, and the penalty to two or more children is a staggering 27%.

As in the United States, controlling for actual (instead of potential) work experience has a small effect on the penalty to one child and reduces the penalty to two or more children more substantially, by about a third, to 19.5%.[11] Controlling for unobserved heterogeneity has little further effect on the child penalties; in both the first-difference and the fixed-effects models, the child coefficients tend to be slightly smaller but not significantly so. As in the United States, there is evidence here that lower work experience (but not unobserved heterogeneity) can explain some of the wage differentials between mothers and others, but a significant direct effect of children remains to be explained.[12]

### D. Accounting for the Gender Gap

The effects of family status go a long way toward accounting for the gender gap in pay in these cohorts, as can be seen in table 3. This table decomposes the gender gap (at mean age 30 in the United States and 32 in Britain), using fully interacted models that pool the young men and women and allow the coefficients to vary by gender. (These pooled models are shown in app. C: note that the U.S. model is weighted to correct for the oversampling of minorities in the NLSY.)

The bottom line of this decomposition for the United States is that nearly half (45%) of the gender gap at age 30 is due to family status, with 41% due to the differential returns that women and men receive for marital status and parental status and 4% due to differences between men and women in these characteristics. Work experience is very important as well, with differences in levels of experience (19%) and in returns to

[11] In the NCDS, actual experience for the period pre-1981 is taken from the 1981 survey; actual experience for the period 1981–91 is from the 1991 survey.

[12] One might also be concerned about selection bias, but as noted earlier, the sample was constructed to minimize the potential for such bias by including women even if they were not working in the current year. I nevertheless tested for selection bias using the standard method (Heckman 1979) and found none (details for the United States are available in Waldfogel [1994] and for Britain are in Waldfogel [1995]).

**Table 3**
**Accounting for the Gender Gap: Predicted Wage Levels and Decomposition**

| | U.S. Data (Mean Age = 30) | British Data (Mean Age = 32) |
|---|---|---|
| Predicted wage levels:* | | |
| Men's mean wage | 10.60 | 6.82 |
| Women's mean wage | 8.20 | 4.79 |
| Gender gap (in %) | 23 | 30 |
| Decomposition of gender gap:† | | |
| Education (if women had both men's characteristics & returns) (in %): | 17 | 18 |
| Characteristics only | 7 | 11 |
| Returns only | 10 | 7 |
| Experience (if women had both men's characteristics and returns) (in %): | 42 | 34 |
| Characteristics only | 19 | 17 |
| Returns only | 23 | 17 |
| Family status (if women had both men's characteristics and returns) (in %): | 45 | 48 |
| Characteristics only | 4 | 0 |
| Returns only | 41 | 48 |
| Ethnicity (if women had both men's characteristics and returns) (in %) | −4 | ... |
| Characteristics only | 0 | ... |
| Returns only | −4 | ... |

NOTE.—The NLSY sampling weights are used in producing the U.S. estimates.
* Wages are in dollars for U.S. data, pounds for British data.
† Gender gap is measured at mean age 30 for the U.S. data and at mean age 32 for the British data.

experience (23%) together accounting for another large portion (42%) of the total. Education accounts for a smaller share (17%) of the gender gap in the U.S. cohort, with differences in education levels accounting for 7% and educational returns 10%. The ethnicity effects are negative, that is, differential treatment of black and hispanic women and men tends to narrow the gender gap (by 4%).

The bottom line for Britain is strikingly similar: nearly half (48%) of the gender gap at age 32 is due to the direct effects of family status (i.e., differential returns to marital status and parental status), while another third (34%) is due to experience effects in the form of lower levels of work experience (17%) and lower returns to experience (17%). Differences in educational attainment (11%) and in returns to education (7%) together account for the remaining 18% of the gender gap in Britain.

## III. Can Maternity Leave Make a Difference?

### A. Data and Empirical Strategy

I use the same longitudinal data sets introduced earlier to estimate the effects of maternity leave on women's pay. The empirical strategy employed here is to use OLS models to assess the effect on current wages of past maternity leave coverage at the time of a woman's most recent birth, controlling for other factors that might be correlated both with past maternity leave coverage and with present higher pay. Because the effects of coverage should operate to a large extent through usage, I next use probit models to assess the effects of maternity leave coverage on maternity leave behavior. I then use OLS and first-difference models to investigate the joint effects of maternity leave coverage and behavior. If maternity leave coverage is beneficial to the extent that it allows women to use it to maintain employment continuity over childbirth, then the effects of coverage if used should be larger than the effects of coverage without regard to usage.

This strategy uses all women who have had at least one child, were working when pregnant with the most recent child, and then were observed working at some point subsequent to the birth. Thus, a woman need not have returned within a specific time frame in order to be included in the sample (so long as she returned prior to the 1991 interview). An alternative strategy would be to use all births and to observe wages at a fixed point in time (e.g., 2 years) pre- and postbirth. Although intuitively appealing, there are two pitfalls with such an approach. One, it would produce an unbalanced sample, with each woman contributing a different number of birth observations depending on how many children she had given birth to. Second, and more important, the requirement that a woman be in the labor market at a particular moment in time prior to and after the birth would result in the loss of a large number of birth observations, and adequately correcting for the resultant selection bias would not be straightforward. The strategy employed here, in contrast, provides a balanced sample, and one that includes a larger and more unbiased sample of women.

The strategy used here also has the advantage of providing variation across observations in the timing of the pre- and postbirth wages, relative to the most recent birth. There are two reasons that this is helpful. First, drawing on the analogy with displaced workers, one might be concerned that wage growth might be slowing immediately prebirth, so one would want a wage observation from some time prior to the birth.[13] In this study, the average time elapsed between the early wage and the most recent birth is 4 years, and in very few (10%) of the cases is the early wage from the

[13] Jacobson et al.'s (1993*a*, 1993*b*) research on displaced workers has demonstrated that the earnings of displaced workers actually begin to decline 2–3 years before the displacement, perhaps reflecting the declining status of the firm or industry. Among new mothers, this pattern of falling wages prior to the "displacement" seems to be less common. In the NLSY, for example, I could find no evidence of falling wages in the 2 or 3 years prior to the birth, although wage growth did slow a bit for some women in the year immediately prior to the birth.

year immediately prior to the birth. Second, and more important, one might think that the effects of maternity leave (whether positive or negative) might attenuate over time, so it would be important to be able to track these effects beyond the first year or two postbirth. This study design makes that possible, since the interval between the most recent birth and the late wage is not fixed. Very few of the late wage observations (less than 10%) are from the year immediately postbirth. The mean time elapsed postbirth is 4 years, with a range from 0 to 12 years. Since I employ specifications that control for years, this removes any bias of measuring the effect at different periods of time since the birth.

### B. Data on Maternity Leave Coverage and Usage in the United States and Britain

The NLSY specifically asked respondents about whether they had maternity leave coverage at work. The NCDS did not ask respondents about maternity leave rights, so qualification for maternity leave must be imputed using the work history data to determine whether a woman had been with her employer long enough prior to the birth to meet the statutory qualifications (2 years full-time or 5 years part-time).[14]

In the U.S. sample, many women had access to maternity leave despite the lack of national legislation. Table 4A indicates that nearly two-thirds (65%) of the women in the NLSY who were working at the time they had their most recent child reported being covered by a maternity leave policy. Over half (60%) of the working women (including some who did not report being covered by a formal maternity leave policy) took leave and returned to their jobs.[15] College-educated women were some-

[14] Because of the way the work histories are recorded, some of the women who are coded "not qualified" in fact might have been. For example, it is fairly common for a woman to work a few years full-time, take maternity leave, and then return part-time, but because each job can be recorded as only full-time or part-time, this employment segment would be listed as a part-time job, and this woman would erroneously be listed as "not qualified." Note also that, because of the statutory requirements, women who were working full-time prebirth would be much more likely to be covered, but of course these women might not necessarily be working full-time at the time of the current wage. Thus, maternity leave coverage in the past and current full-time or part-time status are by no means perfectly correlated.

[15] Maternity leave usage is imputed from the work history data. If the woman is in the same job both before and (12 months) after her most recent birth, then she is coded as a returner. If she is not working or working for a different employer 12 months after the birth, then she is coded as not a returner. Twelve months was selected as the cut-off in the United States because, while some employers offer up to 1 year of leave, it is extremely rare for a woman to be granted more than 12 months. Note that I cannot observe whether the women actually returned to the same or comparable job; thus, the estimates reported here would likely understate the effect of maternity leave usage on wages.

**Table 4A**
**Return Rates and Wage Changes for the United States**

**A. Return Rates of Mothers in the NLSY**

| | All ($N$ = 2,152) | College Degree ($N$ = 266) | Beyond High School ($N$ = 517) | High School Only ($N$ = 824) | High School Dropout ($N$ = 545) |
|---|---|---|---|---|---|
| Percentage in work while pregnant with most recent child | 65 | 84 | 68 | 66 | 48 |
| Percentage of those in work who were covered by formal employer maternity leave policy | 65 | 70 | 65 | 66 | 55 |
| Percentage of those covered who returned to same employer | 67 | 72 | 68 | 65 | 63 |
| Percentage of those not formally covered who returned to same employer | 47 | 61 | 44 | 46 | 42 |
| Total percentage (of those in work) who returned to same employer after most recent child | 60 | 69 | 60 | 59 | 54 |

**B. Wage Changes by Family Status: Changes for Returners**

| | No Children to One Child ($N$ = 258) | No Children to Two or More ($N$ = 254) | One Child to Two or More ($N$ = 201) |
|---|---|---|---|
| Mean wage at mean age 21 | 6.39 | 6.76 | 6.29 |
| Mean wage at mean age 30 | 9.33 | 9.33 | 7.79 |
| Mean wage change from mean age 21 to 30 (in %) | +46 | +38 | +24 |

**C. Wage Changes by Family Status: Changes for Other Women**

| | No Children to One Child ($N$ = 479) | No Children to Two or More ($N$ = 463) | One Child to Two or More ($N$ = 338) |
|---|---|---|---|
| Mean wage at mean age 21 | 5.95 | 5.82 | 5.59 |
| Mean wage at mean age 30 | 7.37 | 6.55 | 6.26 |
| Mean wage change from mean age 21 to 30 (in %) | +24 | +13 | +12 |

NOTE.—Tabulated from the NLSY. The NLSY sampling weights are used in computing all means. All wages are in 1991 dollars. Mothers include all women who had children by the time of the 1991 survey. Returners include all women who returned to same employer within a year after the birth of the most recent child. Nonreturners include all other mothers (i.e., both those who were not working prior to the most recent birth and those who were working prior to the birth but did not return to work within a year after the birth).

what more likely to be offered and to use maternity leave than less-educated women.[16]

In the raw wage data from the NLSY, also shown in table 4A, it is apparent that the women who had children and used maternity leave have higher wages at mean age 30 than other new mothers. In part, this is due to the fact that they had on average slightly higher wages at 21, but for the most part this is due to higher wage growth over the decade relative to women who did not use maternity leave. This suggests that women who did not have access to and did not use maternity leave may have suffered lower wages as a result.

In the NCDS sample, as shown in table 4B, over half (54%) of the women who were in work while pregnant with their most recent child were qualified for maternity leave on a statutory basis, and over half (55%) of new mothers who had worked prior to the birth took maternity leave and returned to work after the birth.[17] Return rates are more strongly related to educational level than in the United States, with more highly educated women much more likely to return. The raw wage figures show substantial wage growth over the decade for women who took job-protected maternity leave and returned to their jobs after childbirth versus much flatter wage profiles for other new mothers.

### C. Estimating the Wage Effects of Maternity Leave Policies

The regression models in table 5A test for the effects of maternity leave policies among the subsample of NLSY women who have had one or more children and were working at the time of their most recent pregnancy. This is the group for whom maternity leave coverage is relevant.

In the first OLS model, being covered by a maternity leave policy at the time of the most recent birth has a fairly large (nearly 12%) positive effect on current wages. As noted earlier, one might expect the positive effect of coverage to dissipate over time (just as the cost of a job loss for a displaced worker falls over time) if the wages of women who did not have coverage eventually catch up again. The second OLS model adds a

[16] As noted earlier, there were state maternity leave laws during this period, but the numbers here are too small to identify with precision how these laws affected coverage. In this sample of mothers, just 56 (under 3%) were in a state with job-protected maternity leave legislation, about 400 (21%) were in a state with minimal legislation, and the vast majority (76%) were in a state with no legislation at the time they had their most recent child. Despite the small sample size, the pattern of coverage is as expected: women living in states with comprehensive legislation were more likely to be covered than those in states with no legislation at all, while women in states with minimal legislation were not.

[17] Maternity leave usage is imputed in the NCDS in the same way as it was in the NLSY, except that in this sample the cut-off was set at 10 months, since in Britain a leave of longer than 10 months would rarely, if ever, be granted.

**Table 4B**
**Return Rates and Wage Changes for Britain**

**A. Return Rates for Mothers in the NCDS**

| | All ($N$ = 2,158) | University Degree ($N$ = 194) | A-Levels or Higher ($N$ = 583) | Some Qualifications ($N$ = 1,149) | No Qualifications ($N$ = 232) |
|---|---|---|---|---|---|
| Percentage in work while pregnant with most recent child | 55 | 76 | 63 | 49 | 41 |
| Percentage of those in work who qualified for maternity leave under the statute | 54 | 56 | 59 | 52 | 45 |
| Percentage of those qualified for maternity leave who returned to work for same employer | 63 | 75 | 72 | 54 | 49 |
| Percentage of those "not qualified" who returned to work for same employer | 45 | 69 | 48 | 37 | 43 |
| Total percentage (of those in work) who returned to work for same employer (in %) | 54 | 72 | 62 | 46 | 46 |

**B. Wage Changes by Family Status: Changes for Returners**

| | No Children to One Child ($N$ = 280) | No Children to Two or More ($N$ = 291) | One Child to Two or More ($N$ = 52) |
|---|---|---|---|
| Mean wage at mean age 23 | 4.27 | 4.07 | 3.37 |
| Mean wage at mean age 33 | 6.20 | 5.60 | 4.42 |
| Mean wage change from mean age 23 to 33 (in %) | +45 | +38 | +31 |

**C. Wage Changes by Family Status: Changes for Other Women**

| | No Children to One Child ($N$ = 287) | No Children to Two or More ($N$ = 876) | One Child to Two or More ($N$ = 108) |
|---|---|---|---|
| Mean wage at mean age 23 | 3.75 | 3.74 | 3.13 |
| Mean wage at mean age 33 | 4.14 | 3.72 | 3.58 |
| Mean wage change from mean age 23 to 33 (in %) | +10 | −1 | +14 |

NOTE.—Tabulated from NCDS. All wages are in 1991 pounds.

**Table 5A**
**The Effects of Maternity Leave Coverage on Women's Pay, U.S. Data**

| | OLS Log Wage (1) | OLS Log Wage (2) | OLS Log Wage (3) | OLS Log Wage (4) | OLS Log Wage (5) |
|---|---|---|---|---|---|
| Experience | .0408* | .0400* | .0414* | .0376* | .0381* |
| | (.0055) | (.0056) | (.0055) | (.0054) | (.0054) |
| Educ1 | .5423* | .5122* | .5205* | .5372* | .5200* |
| | (.0412) | (.0443) | (.0424) | (.0403) | (.0415) |
| Educ2 | .2466* | .2324* | .2378* | .2480* | .2409* |
| | (.0365) | (.0373) | (.0368) | (.0358) | (.0360) |
| Educ3 | .0795* | .0706* | .0765* | .0840* | .0816* |
| | (.0339) | (.0342) | (.0340) | (.0332) | (.0333) |
| Covered by maternity leave at last birth | .1176* | .1077* | | .0605* | |
| | (.0256) | (.0263) | | (.0275) | |
| Years since last birth | | −.0079 | | | |
| | | (.0044) | | | |
| Covered at last birth, last birth 1 year ago | | | .0331 | | −.0264 |
| | | | (.0668) | | (.0673) |
| Covered at last birth, last birth 2 years ago | | | .1764* | | .1074* |
| | | | (.0444) | | (.0451) |
| Covered at last birth, last birth 3 years ago | | | .1489* | | .0852* |
| | | | (.0393) | | (.0402) |
| Covered at last birth, last birth 4 years ago | | | .1101* | | .0550 |
| | | | (.0446) | | (.0451) |
| Covered at last birth, last birth 5 years ago | | | .0967 | | .0374 |
| | | | (.0508) | | (.0510) |
| Covered at last birth, last birth 6 years ago | | | .1402* | | .0841 |
| | | | (.0535) | | (.0533) |
| Covered at last birth, last birth 7 years ago | | | .1582* | | .1051* |
| | | | (.0539) | | (.0539) |
| Covered at last birth, last birth 8+ years ago | | | .0626 | | .0187 |
| | | | (.0403) | | (.0404) |
| Current employer has maternity leave policy | | | | .0949* | .0959* |
| | | | | (.0290) | (.0291) |
| Current employer is small firm (<50) | | | | −.1108* | −.1089* |
| | | | | (.0245) | (.0246) |
| Current employer has union | | | | .1269* | .1233* |
| | | | | (.0336) | (.0337) |
| Black | −.0503 | −.0427 | −.0513 | −.0906* | −.0914* |
| | (.0299) | (.0301) | (.0299) | (.0296) | (.0297) |
| Hispanic | .0800* | .0805* | .0798* | .0676* | .0678* |
| | (.0335) | (.0335) | (.0335) | (.0328) | (.0328) |
| Adjusted $R^2$ | .2178 | .2188 | .2188 | .2536 | .2556 |

NOTE.—$N$ = 1,347. Standard errors are in parentheses. The sample includes all women who were in work at the time of their most recent pregnancy and for whom current employer variables are available. All models include an intercept, age, and year.
* $p < .01$.

control for years since the last birth, but this specification does not capture the potential interaction between coverage and years elapsed. For this reason, the third OLS model allows the effects of coverage to vary with the time elapsed since the birth of the most recent child. With the exception of the first year (when the return to maternity leave is small and insignificant), the expected pattern emerges.[18] Compared to the reference category of not having coverage at all, the largest positive effect of maternity leave occurs 2 years after the most recent birth, and the positive effect then falls to zero by 8 years after the birth. This suggests that women who did not have maternity leave coverage on average take 8 years or more to make up the ground they lost.

An important feature of the U.S. labor market (prior to 1993) is that employers to a large extent selected whether or not to offer maternity leave. Offering maternity leave is correlated with other employer characteristics (e.g., firm size and union status) that are associated with higher wages. If women who used maternity leave are still working for the same employer, the positive coefficient on maternity leave coverage may simply reflect the positive effects of current employer characteristics. Up until now, the models have not included controls for employer characteristics since I have been concerned with estimating the total effects of family status, including effects that might operate through employer characteristics. Here, however, the concern is that maternity leave benefits are correlated with, and reflecting the effects of, other employer attributes. Therefore, the fourth and fifth OLS models control not only for whether a women was covered at her most recent birth but also for current employer characteristics (i.e., whether the woman's current employer offers maternity leave as well as the current employer's firm size and union status). As expected, the results indicate that employers who offer maternity leave do pay higher wages to women, as do large firms and union firms. Controlling for these current employer characteristics reduces the estimated return to past maternity leave coverage, but having been covered at the last birth still results in an additional wage boost. The average effect, from model 4, is 6%, and again, as can be seen in model 5, the effects peak at 2 years after the most recent birth and then diminish with time elapsed since the birth.

Table 5B presents the analysis of the effects of maternity leave coverage in Britain. In contrast to the United States, the coverage variable here is not the self-reported coverage offered by the employer but rather the imputed qualification for coverage under the statute.

[18] The absence of a coverage effect for those who gave birth within the past year is surprising, although it may well be due to measurement error around the timing of the birth and return to work relative to the timing of the late wage.

**Table 5B**
**The Effects of Maternity Leave Coverage on Women's Pay, British Data**

| | OLS Log Wage (1) | OLS Log Wage (2) | OLS Log Wage (3) |
|---|---|---|---|
| Actual experience | .0306* | .0296* | .0302* |
| | (.0041) | (.0042) | (.0040) |
| Educ1 | .8591* | .8216* | .8198* |
| | (.0480) | (.0499) | (.0481) |
| Educ2 | .5030* | .4895* | .4950* |
| | (.0434) | (.0444) | (.0430) |
| Educ3 | .2704* | .2616* | .2707* |
| | (.0482) | (.0489) | (.0476) |
| Qualified for maternity leave, last birth | .0989* | .0893* | |
| | (.0241) | (.0245) | |
| Years since last birth | | −.0106* | |
| | | (.0036) | |
| Qualified at last birth, last birth 1 year ago | | | .1859* |
| | | | (.0469) |
| Qualified at last birth, last birth 2 years ago | | | .2364* |
| | | | (.0428) |
| Qualified at last birth, last birth 3 years ago | | | .1467* |
| | | | (.0413) |
| Qualified at last birth, last birth 4 years ago | | | .0772 |
| | | | (.0470) |
| Qualified at last birth, last birth 5+ years ago | | | .0181 |
| | | | (.0535) |
| Adjusted $R^2$ | .3383 | .3438 | .3514 |

NOTE.—$N$ = 1,333. Standard errors are in parentheses. The sample is all women who were in work at the time of their most recent pregnancy. All models include an intercept, age/year, and a full set of educational controls.
* $p < .01$.

The first model indicates that there is a positive wage effect of about 10% of qualifying for maternity leave. The second model adds a control for time elapsed since birth, which is negative and significant, and the third model allows the effect of qualification to vary by time elapsed since birth. The results are striking. As in the United States, the return to coverage peaks at 2 years after birth (although it is quite strong at 1 year as well), but the return then seems to fall off more rapidly than in the United States. By 5 years after birth, the return to having been qualified is essentially zero.

## D. The Joint Effects of Coverage and Usage

The models presented thus far have looked at coverage only, but it is also of interest to look at women's return to work behavior since coverage

**Table 6A**
**The Effects of Maternity Leave Coverage and Usage on Women's Pay, U.S. Data**

| | Probit Returned to Same Employer (1) | OLS Log Wage (2) | OLS Log Wage (3) |
|---|---|---|---|
| Experience | | .0356* | .0346* |
| | | (.0055) | (.0032) |
| Educ1 | .2226 | .5327* | .5431* |
| | (.1241) | (.0403) | (.0238) |
| Educ2 | −.0108 | .2507* | .2481* |
| | (.1081) | (.0358) | (.0220) |
| Educ3 | .0282 | .0872* | .0691* |
| | (.0996) | (.0332) | (.0207) |
| Has any children | | | −.0844* |
| | | | (.0193) |
| Covered by maternity leave policy last birth | .5313* | | |
| | (.0745) | | |
| Covered and returned to employer after last birth | | .0974* | .0626* |
| | | (.0356) | (.0241) |
| Covered but did not return | | .0285 | .0143 |
| | | (.0370) | (.0245) |
| Not covered but did return | | .0164 | .0100 |
| | | (.0402) | (.0327) |
| Current employer has maternity leave policy | | .0885* | .1453* |
| | | (.0293) | (.0166) |
| Current employer is small firm (<50) | | −.1072* | −.1057* |
| | | (.0246) | (.0147) |
| Current employer has union | | .1227* | .1083* |
| | | (.0336) | (.0200) |
| Black | .1880* | −.0927* | −.1090* |
| | (.0875) | (.0296) | (.0180) |
| Hispanic | .1458 | .0659* | .0261 |
| | (.0994) | (.0328) | (.0200) |
| Adjusted $R^2$ | .0645 | .2552 | .3235 |
| No. of observations | 1,347 | 1,347 | 3,723 |

NOTE.—Standard errors are in parentheses. The sample for the first two models is all women who were in work at the time of their most recent pregnancy and for whom current employer variables are available. The third model includes all women in the full sample used earlier for whom employer variables are available. The returned-to-same-employer variable is set to one if the woman returned within 12 months after the birth. All models include an intercept and age; wage equations also include year.
* $p < .01$.

is likely to have a strong effect on this. As noted earlier, the joint effect of coverage and usage should be larger than that of coverage without regard to usage to the extent that the positive wage effects of coverage reflect the fact that women are able to use it to maintain employment continuity over childbirth.

In the raw wage data for the United States, women who were covered by maternity leave policies were more likely to return to work for their employer after birth. The probit model shown in the first column of Table 6A confirms that being covered has a sizable positive effect on the

**Table 6B**
**The Effects of Maternity Leave Coverage and Usage on Women's Pay, British Data**

| | Probit Returned to Same Employer (1) | OLS Log Wage (2) | OLS Log Wage (3) |
|---|---|---|---|
| Actual experience | | .0201* | .0248* |
| | | (.0044) | (.0022) |
| Educ1 | .6514* | .8144* | .7607* |
| | (.1475) | (.0480) | (.0272) |
| Educ2 | .4131* | .4923* | .5038* |
| | (.1311) | (.0428) | (.0253) |
| Educ3 | .2231 | .2628* | .2955* |
| | (.1461) | (.0474) | (.0265) |
| Has any children | | | −.2018* |
| | | | (.0176) |
| Qualified for maternity leave, last birth | .4053* | | |
| | (.0707) | | |
| Qualified and returned to employer after last birth | | .2369* | .2009* |
| | | (.0339) | (.0246) |
| Qualified but did not return | | .0144 | −.0178 |
| | | (.0332) | (.0262) |
| Not qualified but returned | | .0699* | .0436 |
| | | (.0344) | (.0271) |
| Adjusted $R^2$ | .0532 | .3616 | .3737 |
| No. of observations | 1,329 | 1,329 | 3,840 |

NOTE.—Standard errors are in parentheses. The return-to-work variable is set to one if a woman returned to her job within 10 months after the most recent child was born. All models include an intercept and a full set of educational controls; wage equations also include age/year.
* $p < .01$.

probability that a woman in the United States will in fact return to the same employer after birth. Educational level, however, has no significant effect on the probability of returning to work.

The second model in the table examines the joint effects of maternity leave coverage and returning to work among the subsample of women who were in work at the time of their most recent pregnancy; the third model examines these effects in the full sample from the NLSY. These models confirm that there is a positive effect of maternity leave and suggest that, as expected, the largest positive effect of maternity leave is for women who were covered and used the leave. In the full sample, the premium to having and using maternity leave (6%) is nearly as large as the penalty to being a mother (8%). Taken together, this evidence on maternity leave suggests that having access to a job-protected maternity leave has a positive effect on wages (even after controlling for other employer characteristics) and that this effect is due to the increased propensity to return to work with the same employer after childbirth. Models allowing these effects to vary by years since birth, not shown here, indi-

**Table 7A**
**The Effects of Maternity Leave Coverage and Usage on Women's Pay: A Comparison of OLS and First-Difference Models, U.S. Data**

| | OLS Log Wage (1) | First Difference Log Wage (2) | OLS Log Wage (3) | First Difference Log Wage (4) |
|---|---|---|---|---|
| Experience | .0358* | .0140 | .0402* | .0412* |
| | (.0060) | (.0096) | (.0033) | (.0045) |
| Educ1 | .5609* | .3760* | .5656* | .4708* |
| | (.0437) | (.0528) | (.0243) | (.0250) |
| Educ2 | .2739* | .0723 | .2400* | .1066* |
| | (.0398) | (.0526) | (.0223) | (.0280) |
| Educ3 | .1204* | .0127 | .0526* | .0539 |
| | (.0370) | (.0709) | (.0209) | (.0400) |
| Number of children | | | −.0402* | −.0563* |
| | | | (.0075) | (.0101) |
| Covered and returned after last birth | .1956* | .1246* | .0977* | .0444 |
| | (.0374) | (.0413) | (.0229) | (.0261) |
| Covered, did not return | .0464 | .0048 | .0386 | .0342 |
| | (.0397) | (.0439) | (.0225) | (.0261) |
| Not covered, did return | .0382 | .0282 | −.0440 | −.0370 |
| | (.0456) | (.0507) | (.0302) | (.0346) |
| Black | −.0655* | | −.0666* | |
| | (.0327) | | (.0180) | |
| Hispanic | .0580 | | .0416* | |
| | (.0353) | | (.0202) | |
| Adjusted $R^2$ | .2465 | .0752 | .2645 | .1189 |
| No. of observations | 1,157 | 1,157 | 4,322 | 4,322 |

NOTE.—Standard errors are in parentheses. The sample for the first two models is all women who were in work at the time of the most recent pregnancy and for whom that pregnancy occurred between the time of the early and late wage observations. (Models do not include employer variables as these are not consistently available at the time of the early wage.) The sample for third and fourth models is the full sample used earlier. All models include an intercept, age, and year.
* $p < .01$.

cate a similar pattern to that shown above, with the effects falling to zero by 8 years after the most recent birth.

As noted earlier, part of this positive return to maternity leave coverage and usage might reflect the fact that these women had higher prebirth wages. For this reason, table 7A provides a comparison of OLS and first-difference models, first for the sample of women who have had children and were working at the time of the most recent birth and second for the women in the full NLSY sample.[19] Looking first at models 1 and 2

[19] Unfortunately, it is not possible to include employer characteristics such as firm size or policy coverage (as in the models shown in tables 5A–6B) in these models because these variables are not consistently available for the early wage observations in the NLSY. Union status is available for a subsample of the early observations, and in other models (not shown here), including union status in the difference models did not change the results.

(for women who have had children), the return to maternity leave coverage and usage falls from about 19.5% in the levels model to 12.5% in the first-difference model. Similarly, in the full sample, the return to maternity leave coverage and usage falls from over 9% in the OLS model to a not quite significant 4.5% in the difference model. This suggests that higher starting wages account for some, but not all, of the estimated return to maternity leave coverage and usage. It is interesting to note that, in this final difference model, the return to maternity leave and returning to work is nearly as large as the penalty to having one child.

Table 6B presents the analysis of the effects of maternity leave coverage and returning to work behavior in Britain. The probit model confirms the pattern seen in the raw data, that educational level and qualification for maternity leave both have a strong effect on maternity leave usage. The second and third models examine the joint effects of coverage and usage and find, as in the United States, that those who are qualified and who use the leave receive the largest premium. The magnitude of the effects are much larger than in the United States, consistent with the larger family penalties observed in this British cohort. Among the full sample, the penalty to being a mother is approximately 20%, as is the premium to having and using maternity leave. Models that allow these effects to vary by years since birth, not shown here, suggest that these effects may fall off more slowly than they did in the earlier British model, as some weak positive effects are still discernible 8 or more years after the most recent birth.

Table 7B presents a comparison of OLS and difference models for the British data. In the first set of models, for women who have had children, the effects of qualifying for maternity leave and returning to work fall by nearly half, from 27% in the OLS model to 14% in the difference model. In the sample as a whole, as shown in the second set of models, the return to children falls only slightly, while the return to qualifying for maternity leave and returning to work falls by slightly more than half, from 16% to 7%. In the final difference model, then, the return to maternity leave is nearly as large as the penalty to having one child, just as it was in the United States.

## IV. Conclusions

First, with regard to the size of the gender gap and family gap, mothers are far behind nonmothers in these young cohorts. In the United States, the family gap is 20 percentage points: mothers at age 30 earn on average 70% of men's pay, while nonmothers earn 90%. In Britain, the family gap is also 20 percentage points: mothers at age 33 are earning only 64% of men's pay, while nonmothers are earning 84%.

Regression results indicated a negative effect of children but not marital status on women's wages, even after controlling for observable character-

**Table 7B**
**The Effects of Maternity Leave Coverage and Usage on Women's Pay: A Comparison of OLS and First-Difference Models, British Data**

| | OLS Log Wage (1) | First Difference Log Wage (2) | OLS Log Wage (3) | First Difference Log Wage (4) |
|---|---|---|---|---|
| Actual experience | .0200* | .0453* | .0223* | .0363* |
| | (.0044) | (.0063) | (.0023) | (.0031) |
| Educ1 | .8064* | .3521* | .7458* | .2166* |
| | (.0508) | (.0923) | (.0273) | (.0483) |
| Educ2 | .4993* | .2139* | .4989* | .1832* |
| | (.0457) | (.0718) | (.0252) | (.0402) |
| Educ3 | .2638* | −.0709 | .2908* | −.0495* |
| | (.0507) | (.0467) | (.0240) | (.0236) |
| Number of children | | | −.0900* | −.0835* |
| | | | (.0074) | (.0099) |
| Qualified and returned after last birth | .2698* | .1407* | .1623* | .0699* |
| | (.0347) | (.0420) | (.0229) | (.0274) |
| Qualified, did not return | .0138 | −.0596 | −.0807* | −.1469* |
| | (.0352) | (.0422) | (.0249) | (.0298) |
| Not qualified, did return | .1062* | .1419* | .0388 | .0714* |
| | (.0358) | (.0429) | (.0267) | (.0315) |
| Adjusted $R^2$ | .3738 | .1325 | .3762 | .1255 |
| No. of observations | 1,188 | 1,188 | 3,840 | 3,840 |

NOTE.—Standard errors are in parentheses. All models include an intercept, age/year, and a full set of educational controls.
* $p < .01$.

istics such as education. Two types of bias in estimating the effects of children on women's wages were explored here. Controlling for unobserved heterogeneity did not significantly reduce the estimated child penalties, indicating that differences in motivation or other unobserved attributes cannot explain the family gap. Controlling for actual work experience was important in reducing the estimated child penalties, so clearly omitted human capital can explain part of the family gap. The majority of the child penalties, however, remained to be explained after controlling for actual work experience and unobserved heterogeneity.

Decomposing the gender gap indicates that 40%–50% of the gap is explained by the effects of family status in the United States and Britain, primarily because men receive a premium if they are married and because women are penalized if they have children; in the United States, a small portion (under 5%) is due to the fact that women are more likely to be married and/or to have children than are men in their age group. Another 30%–40% of the gap is explained by the indirect effects of family status, due to mothers taking more time out of the labor market and due to women receiving lower returns to work experience than do men. Differences in education levels and in returns to education account for the reminder of the gender gap (just under 20%) in both countries.

Second, with regard to maternity leave, having access to a job-protected maternity leave was found to have a substantial positive wage effect for mothers in both the United States and Britain. Women who have access to leave are, all else being equal, more likely to return to their previous employers after childbirth, and women who are covered by and use maternity leave receive a significant wage premium. Employer characteristics (in the United States) and prior wages (particularly in Britain) were important in explaining some of the maternity leave premium, but in most instances, a significant premium remained even after controlling for these other factors. In both countries, this maternity leave premium was large enough to offset some of the negative wage effects of children.

The policy implication is that extending rights to job-protected maternity leave, as the recent legislation did in the United States, should reduce the family gap for future working mothers by increasing the likelihood that they return to their employers after childbirth. The caveat, of course, is that, if extending maternity leave rights imposes costs on employers, these costs may be passed on to women in the form of lower wages or employment (Summers 1989). There is little direct evidence yet on this question.[20]

It is possible to speculate on the potential effect of the FMLA. Under the FMLA, employers face two sets of costs: continuing a woman's health insurance coverage during her leave, and replacing the woman during her leave. According to one estimate by the American Management Association, the average cost of the FMLA is likely to be only $220 per each woman using leave per year, mostly in health insurance costs (Matthes 1993).[21] There may also be benefits to employers in terms of decreased

[20] Related research (Gruber 1994) on legislation mandating maternity health insurance coverage, at a cost to employers of up to $1,000 per year per affected employee, did find wage costs passed on to women employees with little effect on employment. Preliminary research on the FMLA (Waldfogel 1997*a*) found no wage effects as of 1995 and, if anything, positive employment effects. Research on European parental leave mandates (Ruhm, 1998) also finds positive employment effects, with wage effects that vary with the duration of the leave; short leaves seem to be associated with higher wages, while longer leaves are associated with lower wages.

[21] The FMLA mandates that only existing health insurance coverage be continued. For those women who would be eligible under the FMLA but do not receive employer health insurance (nearly two-thirds in the NLSY in 1991), the health insurance cost would be zero. For the third who would be covered, the cost of the employer portion of the health insurance premium for the 12-week leave period would be approximately $750 (personal communication, Mary Reed, National Federation of Independent Businesses legislative office Washington, DC, November 1993). This would mean an average cost for health insurance of about $250 per woman on leave per year, roughly the same as the American Management Association estimate above. There are no firm estimates on replacement costs. A 1987 General Accounting Office study found that only a third of firms hired any replacement at all and that those that did typically paid less for the replacement worker than for the employee who had gone out on leave; despite this, most

turnover costs and increased employee commitment and productivity, if valued employees return to their jobs after leave instead of leaving altogether (Friedman 1990; Friedman, Galinsky, and Plowden 1992).

For their part, employers typically report that handling an employee's maternity leave is compounded by uncertainty as to whether the woman will in fact return from her leave (Meyer 1978; Catalyst 1986). This asymmetric information problem would make it difficult for employers to choose the optimal replacement strategy. It would also make it difficult for the employer to decide how much to invest in the worker's firm-specific human capital (e.g., through on-the-job training).

The evidence from the British case is informative here. Spurred by the legislation, British employers have successfully implemented retention strategies (such as maintaining contact with the employee during the leave and offering the option to return on a part-time basis) that have boosted the likelihood that women will actually return from leave. Some employers have also implemented voluntary contractual maternity pay that is paid only to women who agree to return for a designated length of time (and that would be forfeited if the woman reneged on her commitment); this has proven very effective in eliciting credible commitments as to return plans. These strategies are not unique to Britain, and it is possible that more American firms may use them in response to the FMLA.

The British results in this article provide some preliminary evidence on the effects of maternity leave legislation on women's qualification for maternity leave, their propensity to return to work after childbirth, and their pay after childbirth. The passage of the FMLA in the United States provides an opportunity to further investigate these effects. This is a promising direction for future research.

## Appendix A

## Maternity Leave Legislation in the United States and Britain

### A. Maternity Leave Coverage in the United States

Rights under federal law prior to Family and Medical Leave Act (FMLA):

1. Right not to be dismissed because of pregnancy (1978 Pregnancy Discrimination Act).

Rights under state law prior to FMLA:

1. In 12 states and the District of Columbia,[22] right to job-protected

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firms did not report a loss in overall productivity. A study of four states that implemented comprehensive laws in the late 1980s (Bond et al. 1991) also found that only a minority of firms (approximately 40%) hired replacements, while most firms simply moved work temporarily to other employees.

[22] Massachusetts, October 1972; Washington, October 1973; Connecticut, December 1973; California, January 1980; Rhode Island, July 1987; Minnesota, July 1987; Oregon, January 1988; Tennessee, January 1988; Wisconsin, April 1988; Maine, April 1988; New Jersey, April 1990; Washington D.C., April 1991; Vermont, July 1992.

unpaid leave for childbirth (as part of family- and medical-leave, or family-leave, or parental-leave legislation). These laws covered an estimated 10% of U.S. workers and appear to have been effective in raising coverage rates: among full-time workers in the NLSY, coverage in these states rose from 73% in 1986 to 86% in 1991, while coverage in states with no laws was constant at 77%. Eight states had laws providing limited rights to leave for childbirth but no reinstatement rights.[23] Twelve had laws covering state employees only;[24] 18 states had no legislation at all.[25] Only Puerto Rico mandates paid time off.

Distinctive features of rights prior to FMLA:

1. Rights varied by employer. In 1991, an estimated 40% of working women had employers who offered maternity leave. Union employers were much more likely to offer leave (83% vs. 57%), as were employers with more than 50 workers (77% vs. 45%). Coverage sometimes depended on seniority.
2. The amount of maternity leave payment, if any, also depended on employer policy. According to one estimate (Hyland 1990), less than 3% of employees had access to paid leave, while 37% had only unpaid leave.
3. The leave was very short by European standards. Among those with leave, one-third had less than 3 months, while one-half had 3–6 months. The average was 20 weeks.

Rights under FMLA (effective August 1993):

1. The law covers women who are employed by firms with more than 50 employees and who have worked more than half-time for their employer for at least 1 year. This is an estimated 45% of women workers.
2. For those covered, the law provides the right to unpaid leave of up to 12 weeks with continued health benefits and with the right to reinstatement at the end of the leave.
3. Existing provisions (under state laws, union contracts, and company policies) will still be important for women not covered by FMLA and for women whose existing coverage is more generous.

SOURCES.—Hyland (1990); Spalter-Roth and Hartmann (1990); Women's Legal Defense Fund (1993); Waldfogel (1994); Westat (1995).

### B. Maternity Leave Legislation in Britain

Statutory rights (prior to 1993 reforms):

1. Right not to be dismissed because of pregnancy (1976).

[23] Hawaii, Iowa, Kansas, Louisiana, Montana, New Hampshire, New York, Ohio.

[24] Alaska, Arizona, Colorado, Florida, Georgia, Illinois, Maryland, North Dakota, Oklahoma, Pennsylvania, Virginia, West Virginia.

[25] Alabama, Arkansas, Idaho, Indiana, Michigan, Mississippi, Missouri, Nebraska, Nevada, New Mexico, North Carolina, South Carolina, South Dakota, Texas, Utah, Wyoming, Delaware and Kentucky also had no laws guaranteeing leave for childbirth although they did have laws regarding adoptive leave.

2. Right to time off for childbirth and reinstatement after childbirth (1976).
3. Right to maternity payments (1977, amended 1987).
4. Right to time off for prenatal care (1980).

Distinctive features:

1. Coverage depends on length of service and is not universal. Qualifying for maternity leave requires 2 years of continuous employment at more than 16 hours per week (or 5 years at more than 8 hours per week) with the same employer. Nearly 50% of working women do not qualify.
2. The amount of maternity leave pay depends on length of service and previous earnings. Eighty percent of working women receive some form of maternity pay. Two groups receive maternity pay from the employer (which is reimbursed by the government): 40%, who meet the continuous employment requirement (above), receive 90% of regular pay for 6 weeks followed by 12 weeks of statutory maternity pay (£46.30 in 1993); 25%, who do not meet the continuous service requirement but have worked and paid National Insurance for 26 weeks, receive 18 weeks of statutory maternity pay. Fifteen percent, who work more than 8 hours a week but do not meet either of the above requirements, receive maternity allowance (18 weeks at £42.25) paid directly by the Department of Social Security.
3. The period of leave is relatively long by European standards. Although maternity pay by statute is only paid for 18 weeks, the job must be kept open for 40 weeks (11 weeks before and 29 weeks after the birth).
4. The maternity leave legislation leaves room for variation by employer. Employers can and do offer maternity leave benefits that go beyond the statute. Examples include contractual maternity pay in addition to statutory pay (offered by an estimated 15% of employers), the right to return for women who do not meet statutory requirement, and family-friendly policies to boost return rates. At the other extreme, there are cases of employers not complying with the law (e.g., the Royal Navy was recently sued for discharging women for being pregnant).

Reforms (Trade Union Reform and Employment Rights Act, July 1993, and European Directive of Council of Ministers, implemented March 1994):

1. The requirement to qualify for the most generous level of maternity pay (90% pay for 6 weeks followed by 12 weeks of statutory maternity pay at £52.50) has been reduced to 26 weeks of continuous service, covering approximately two-thirds of working women. Women who do not meet the continuous service requirement but who have made National Insurance contributions in 26 of the past 66 weeks can receive maternity allowance (18 weeks at £52.50 or £44.55). Women who do not meet either of the above requirements are entitled to 14 weeks job-protected leave but without pay.

SOURCES.—McRae (1991); Incomes Data Services (1994), Waldfogel (1994).

## Appendix B

**Table B1**
**Means of Variables, U.S. Data**

| | Women ($N$ = 4,334) | Men ($N$ = 4,771) |
|---|---|---|
| NLSY variables at age 30: | | |
| Logwage | 2.1034 | 2.3610 |
| Age | 29.5567 | 29.6188 |
| Potential experience | 11.3065 | 11.2815 |
| Actual work experience | 7.8077 | 8.5126 |
| Hometime | 3.4988 | 2.7689 |
| Educ1 (university or higher) | .2383 | .2455 |
| Educ2 (some college) | .2477 | .3193 |
| Educ3 (high school only) | .3443 | .2682 |
| Educ4 (less than high school) | .1696 | .1670 |
| One child | .2373 | .2067 |
| Two or more children | .3766 | .2669 |
| Married | .6015 | .5518 |
| Divorced | .1110 | .0889 |
| Separated | .0465 | .0309 |
| Widowed | .0062 | .0010 |
| Never married | .2287 | .3254 |
| Part-time | .2526 | .0721 |
| Age | 29.5567 | 29.6188 |
| Black | .1521 | .0763 |
| Hispanic | .0960 | .0430 |
| NLSY variables at age 21: | | |
| Logwage | 1.7941 | 1.9539 |
| Age | 21.2713 | 21.0809 |
| Actual work experience | 2.4239 | 2.2470 |
| Educ1 | .0875 | .0781 |
| Educ2 | .2603 | .3652 |
| Educ3 | .4030 | .2887 |
| Educ4 | .2491 | .2680 |
| Married | .2986 | .2064 |
| One child | .1674 | .0923 |
| Two or more children | .0835 | .0405 |
| Part-time | .4055 | .3048 |
| NLSY Difference Variables: | | |
| ΔLogwage | .3092 | |
| ΔAge | 8.2854 | |
| ΔActual work experience | 5.3838 | |
| ΔHometime | 2.0097 | |
| ΔEduc1 | .1508 | |
| Educ1 both years | .0875 | |
| ΔEduc2 | .0954 | |
| Educ2 both years | .1523 | |
| ΔEduc3 | .0424 | |
| Educ3 both years | .3019 | |
| Δ1 child | .1884 | |
| Δ2+ children | .2931 | |
| ΔMaternity leave | .1661 | |

**Table B2**
**Means of Variables, British Data**

| | Women ($N$ = 3,840) | Men ($N$ = 3,799) |
|---|---|---|
| NCDS variable at age 33: | | |
| Logwage | 1.5672 | 1.9199 |
| Age | 31.9135 | 32.4017 |
| Potential experience | 14.7127 | 15.1142 |
| Actual work experience | 11.1278 | 13.4327 |
| Hometime | 3.5849 | 1.6815 |
| Educ1 (university or higher) | .1250 | .1516 |
| Educ2 (teaching, nursing) | .1526 | .1593 |
| Educ3 (A-levels or equivalent) | .1221 | .2466 |
| Educ4 (O-levels or equivalent) | .3560 | .2314 |
| Educ5 (some qualifications) | .1279 | .1011 |
| Educ6 (no qualifications) | .1164 | .1100 |
| One child | .1766 | .1782 |
| Two or more children | .4195 | .4075 |
| Married | .7096 | .6790 |
| Divorced | .0682 | .0480 |
| Separated | .0429 | .0412 |
| Widowed | .0029 | .0008 |
| Never married | .1764 | .2310 |
| Part-time | .4163 | .0274 |
| NCDS variables at age 23: | | |
| Logwage | 1.3398 | 1.5432 |
| Actual work experience | 5.0930 | 5.8702 |
| Educ1 | .0977 | .1063 |
| Educ2 | .1200 | .1003 |
| Educ3 | .1299 | .2895 |
| Educ4 | .3891 | .2645 |
| Educ5 | .1359 | .1113 |
| Educ6 | .1274 | .1281 |
| Married | .5388 | .4083 |
| One child | .0721 | .1116 |
| Two or more children | .0747 | .0771 |
| Age | 23.1734 | 23.6446 |
| NCDS difference variables: | | |
| ΔLogwage | .2276 | |
| ΔAge | 8.7343 | |
| ΔActual work experience | 6.0496 | |
| ΔHometime | 2.7403 | |
| ΔEduc1 | .0263 | |
| Educ1 both years | .0976 | |
| ΔEduc2 | .0376 | |
| Educ2 both years | .1139 | |
| ΔEduc3 | .1234 | |
| Educ3 both years | .0950 | |
| Δ1 child | .1871 | |
| Δ2+ children | .3630 | |
| ΔMaternity leaver | .1622 | |

NOTE.—The NLSY sampling weights are used in computing all means for the NLSY data.

## Appendix C

**Table C1**
**OLS Wage Equations for Young Women and Men Together, U.S. Data**

| | Mean Age 21 | Mean Age 30 |
|---|---|---|
| Actual experience | .0606* | .0391* |
| | (.0048) | (.0026) |
| Exp*Woman | −.0007 | .0098* |
| | (.0077) | (.0049) |
| Educ1 | .2473* | .5831* |
| | (.0235) | (.0184) |
| Educ1*Woman | .0712 | −.0123 |
| | (.0459) | (.0414) |
| Educ2 | .0751* | .2412* |
| | (.0141) | (.0171) |
| Educ2*Woman | −.0020 | −.0069 |
| | (.0318) | (.0388) |
| Educ3 | .0581* | .1140* |
| | (.0131) | (.0171) |
| Educ3*Woman | −.0467 | −.0762* |
| | (.0285) | (.0364) |
| Married | .0981* | .1136* |
| | (.0145) | (.0140) |
| Married*Woman | −.1047* | −.0758* |
| | (.0286) | (.0324) |
| One child | .0327 | −.0005 |
| | (.0186) | (.0154) |
| One child*Woman | −.0845* | −.0971* |
| | (.0343) | (.0336) |
| Two or more children | .0638* | .0391* |
| | (.0264) | (.0152) |
| Two or more children*Woman | −.1239* | −.1428* |
| | (.0469) | (.0327) |
| Black | −.1009* | −.1448* |
| | (.0183) | (.0210) |
| Black*Woman | .0968* | .0958* |
| | (.0330) | (.0380) |
| Hispanic | −.0093 | −.0395 |
| | (.0234) | (.0268) |
| Hispanic*Woman | .0578 | .0859 |
| | (.0399) | (.0459) |
| Age | .0325* | −.0096* |
| | (.0033) | (.0027) |
| Age*Woman | −.0086* | −.0101* |
| | (.0016) | (.0019) |
| Adjusted $R^2$ | .2115 | .2433 |

NOTE.—$N$ = 9,105. Standard errors are in parentheses.
* $p < .01$.

**Table C2**
**OLS Wage Equations for Young Women and Men Together, British Data**

| | Mean Age 23 | Mean Age 33 |
|---|---|---|
| Actual experience | .0251* | .0161* |
| | (.0032) | (.0023) |
| Exp*Woman | −.0096* | .0127* |
| | (.0046) | (.0030) |
| Educ1 | .3218* | .7286* |
| | (.0276) | (.0274) |
| Educ1*Woman | .1433* | .0504 |
| | (.0399) | (.0387) |
| Educ2 | .2260* | .4859* |
| | (.0246) | (.0259) |
| Educ2*Woman | .0703* | .0304 |
| | (.0342) | (.0365) |
| Educ3 | .1745* | .3278* |
| | (.0195) | (.0238) |
| Educ3*Woman | .0678* | −.0247 |
| | (.0304) | (.0358) |
| Educ4 | .0979* | .2252* |
| | (.0196) | (.0241) |
| Educ4*Woman | .0440 | −.0912* |
| | (.0268) | (.0327) |
| Educ5 | .0268 | .1200* |
| | (.0235) | (.0286) |
| Educ5*Woman | .0006 | −.0592 |
| | (.0322) | (.0389) |
| Married | .0698* | .0851* |
| | (.0129) | (.0158) |
| Married*Woman | −.0391* | −.0972* |
| | (.0174) | (.0218) |
| One child | .0253 | .0051 |
| | (.0198) | (.0197) |
| One child*Woman | −.1540* | −.1059* |
| | (.0304) | (.0274) |
| Two or more children | .0137 | .0022 |
| | (.0242) | (.0164) |
| Two or more children*Woman | −.2150* | −.2024* |
| | (.0342) | (.0236) |
| Age | .0135* | .0118* |
| | (.0021) | (.0029) |
| Age*Woman | −.0054* | −.0068* |
| | (.0016) | (.0015) |
| Adjusted $R^2$ | .1770 | .3827 |

NOTE.—$N$ = 7,639. Coefficients (and standard errors) are from OLS models. Dependent variable is the log of hourly wages. All regressions also include previously married and year. NLSY regressions shown in this table are weighted using the NLSY sampling weights. Standard errors are in parentheses.
* $p < .01$.

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