# The Effect of Changes in Maternity Leave Legislation in the 1990s on Women's Labor Market Outcomes in the U.K.

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Abstract

This paper uses cross-sectional data from the UK's General Household Survey from 1991 - 1996 to explore the changes in wages and employment status of new mothers ("takers") and of women of childbearing age ("potential takers") due to changes in maternal leave legislation in 1994. These changes included an increase in the amount and expansion in the accessibility of paid leave to pregnant women and new mothers. Through a difference-in-difference analysis we assess changes in wages and employment of exposed women, before and after the policy change. We find that the wages decrease with the policy change, although this finding is not statistically significant. We also find considerable positive effects of the policy change on employment status of both new mothers and women of childbearing age. This suggests a possible causal relationship between improvements in maternity leave policy (increased access and benefits) and increased rates of employment of women. These results are generally stronger for the "takers" of the policy (i.e. women with young children), but are also statistically significant for "potential takers" of the policy.**

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** This paper is the first part of a larger project aimed at providing evidence based recommendations for redesigning US family leave policies. An alternate version of the paper titled "Employment and Wage Effects of Removing Barriers to Accessing Job-Protected Leave –Evidence from a change in Maternity Leave Legislation in the UK" is available upon request.

## INTRODUCTION

Maternity Leave Legislations (MLL) allows women to take time off from employment immediately before and after childbirth and provides some income replacement during the period of leave. This paper examines the effect of changes in MLL enacted in the United Kingdom in 1994 on employment status and wages of new mothers who are directly impacted by MLL changes (the "takers"), and of women of childbearing age who are indirectly affected by changes in MLL ("the potential takers").

Between 1975 and the present, the UK has seen several changes to its maternity leave legislation, some but not all of which have been as a result of larger EU directives. Duration of job-protected leave, statutory maternity pay (paid by the employer) as well as maternity allowance (paid by the department of social security) have each increased steadily over time[1]. What is unique about the 1994 legislative changes was that the UK removed all eligibility restrictions for the "Right of Reinstatement", allowing job-protected leave to all pregnant women regardless of their hours of work, which effectively removed the barriers to access of maternity leave for a large number of women. Overall, MLL changes in 1994 expanded and opened up paid benefits for a larger population of pregnant workers (Gregg et al., 2007; Callender et al., 1996). We expect, and theory suggests, that this change would substantially expand rates of women's employment.

We use micro-data from the General Household Survey (GHS) in the United Kingdom for three years prior to the 1994 MLL changes, 1991-1993, as well as two years post-change, 1995-1996, and implement a difference in differences research design to identify the effect of the policy change. Using the same dataset, Gregg, Gutierrez-Domenech and Waldfogel (2007)

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[1] See Gregg, Gutierrez-Domenech and Waldfogel (2007) Table 2 for a history of changes in MLL; for details of the most recent legislation, see Moss (2014)

examined how long term trends in employment and wage gaps of married women with children compared to married women without children in the UK, corresponding to changes in government policies pertaining to maternity leave, taxation, childcare and welfare. They found evidence of significant increases in employment of 20 percentage points for the 1995-97 cohort of married women, compared to a 1974-79 cohort. They also found a relative wage increase of 6% for the cohort of married mothers that gave birth in 1992-1994 when the children were 5-6 year old.

Our study builds on prior work and extends it in a few important ways: 1) we include *all* mothers, both married and unmarried, 2) we extend our analysis to include women of childbearing age ("potential takers") whose employment and wages would, as theory posits, also be affected by maternal leave legislation, and 3) by focusing on the 1994 policy change, we are able to specifically isolate the effects of removing significant barriers to accessing job-protected maternity leave on women's employment and wages. To the extent of our knowledge, there are no other studies of the impact of the 1994 MLL changes in United Kingdom on mother's wages or employment status.

**THEORY**

The effects of parental leave policy on women's employment and wages are unclear a priori. Scholars have posited that enacting maternal leave legislation could increase participation of women who are planning on having a child in the near future and could increase returns for women who would have otherwise exited after childbirth. It could also encourage those women who would have taken sub-optimal leaves and returned to work shortly after childbirth, to be on leave for a longer duration. Additionally, it could induce some women to take shorter leaves and return to the same employer instead of taking longer leaves and returning to poorer quality job matches at a later date. Finally, for some women, longer periods of leave might produce a shift in taste away from market work and lead them to reduce work hours or quit altogether.[2]

Leaving the labor market might result in the depreciation of a woman's human capital and a weakened labor force attachment; make re-entry or suitable job match difficult; and reduce cumulative work experience, thereby lowering productivity and wages. Therefore, by helping to maintain job continuity and firm-specific human capital over the child-birth period, maternal leave policies could positively affect wages. If such policies induce longer periods of leave taking, then through the same mechanisms, they could negatively affect wages.

Additional potential benefits of maternal leave policies are the potential protection of employers from forfeiting the resources spent on firm specific training or from incurring search and training costs to replace the employee. Such a policy might then increase labor demand and produce positive wage effects on all women. On the other hand, firms might shift entire costs incurred in providing the benefit on to women's wages, without distorting employment –in that case, parental leave policy would have a negative effect on women's wages.[3]

There are several methodological challenges in empirically testing these effects. The central identification problem arises from potential endogeneity of parental leave policies and unobserved heterogeneity –there may be systematic differences between countries that affect both the pattern of implementation and extension of parental leave policies women's employment patterns in the same countries. Again, countries with higher levels of women's participation might be the ones implementing such policy changes. At an individual level, mothers' decisions regarding leave-taking, returns, and work-hours, are likely to depend on

[2] Klerman and Leibowitz (1997) model women's leave-taking and employment decisions for a labor market with enduring employment relations. This is the core theoretical framework drawn upon by researchers in this literature.

[3] Theoretical discussion based on Gruber, 1994; Waldfogel, 1998; Ruhm, 1998.

observed and unobserved factors that are correlated with other employment outcomes (such as spousal income, child's health, and cultural norms regarding working mothers). Finally, when the policy is not universal (as it was in the UK prior to the1994 change), there is additional concern that women who have access to jobs with coverage may have unobservable characteristics that would affect their post-birth decisions.

## LITERATURE REVIEW

Taking advantage of policy variations across countries, across states within the U.S., and over time; and using quasi-experimental designs on large-scale observational data, empirical research has provided credible evidence about the effects of parental leave policy on women's employment and wages.

### *Employment Effects*

Studies examining the employment effects of maternity and parental leave policies using aggregate data from OECD countries consistently report an inverted U shaped trajectory for women's employment in response to changes in parental leave policy (with employment increasing with short leave extensions but then turning down at higher leave lengths), but do not agree about the turning point (Ruhm, 1998; Akgunduz and Plantenga 2012; Thevenon and Solaz 2013).

These studies using aggregate data suffer from a potential bias, as they do not account for supra-national policy changes (such as EU directives) that are likely to affect all the EU countries simultaneously, but not all the countries in the studies. There is also some concern in the literature regarding treatment of certain countries, such as Austria and Germany, or, Denmark and Norway, as discrete observations in cross-national panels. Doing so potentially ignores cross-national path dependence, which might lead to biased estimates.

Several studies have examined the impact of MLL for Canada and various European countries. Reviews of single-country studies also tend to agree with the main finding from the aggregate studies –modest positive employment effects of short, paid leaves and negative effects of longer leaves. [4] Burgess et al. (2008) studied the impact of 2003 UK legislative changes on post-birth return to employment decisions of mothers. The 2003 legislation extends the length of paid and unpaid maternity leave in the UK, thereby expanding rights to maternity leave. Using the Avon Longitudinal Study of Parents and Children, the authors found that, relative to having no rights, the effect of legislation changes is that women return sooner to the workplace than they would do without the extension of rights, with differences in return timing for high and low wage workers.

*Wage Effects*

Ruhm (1998) found that wage effects varied by the duration of paid leave; short leaves had no significant effect on wages, while longer leaves (more than six months) had negative effects. Using similar data sources and estimation strategy but studying a longer time period (1970-2010), Akgunduz and Plantenga (2012) found heterogeneous wage effects across the skill distribution, with insignificant effects for lower skilled workers and significant negative effects for higher skilled workers. Schonberg and Ludstek (2014) examined the effect of policy changes on women's wages in Germany. Using six different extensions of maximum duration of parental leave from 2 months to 3 years, they found a large negative effect on women's wages, even for short extensions. Lalive and Zweimüller (2009) analyzed parental leave reforms in Austria that increased paid leave from 12 to 24 months in 1990, and in 1996 reduced it to 18 months . They found evidence of negative effects in the short run but no such effects over the long run (10 years after birth). Wurtz-Rasmussen (2010) examined the effects of a 1984 Danish reform that

[4] Selected reviews in Gupta, Smith, and Verner, 2008; Hegewisch and Gornick, 2011; Ruhm, 2011.

extended paid parental leave from 14 weeks to 20 weeks, and implemented a guaranteed, paid two-week paternity leave period immediately after birth and found small positive effects on women's earnings, several years after birth. Taken together, the studies seem to indicate a lack of consistency in effects across different countries.

Literature establishes that working mothers in United Kingdom face a "family gap" in wages, meaning that mothers earn lower wages when compared to non-mothers (Waldfogel 1995, 1997a, 1998a, 1998b; Harkness and Waldfogel, 1999). A large part of the lower earnings of mothers has been attributed to differences in human capital, including education and work experience, differences in full time or part time work, as well as unobserved differences in motivation and commitment to work. After controlling for all the above factors, and using a sample of young women in the U.S. between 1968-1988, Waldfogel (1997a) found that an unexplained family gap remained with a 4% wage penalty for one child and a 12% penalty for two or more children. She posits that this gap may be due to a work-family conflict and focuses attention on the institutional aspects of the labor market, including the existence and level of supportiveness of MLL, that address the barriers to employment and equal wages that new mothers face.

In a study that examined the wage effect of actual leave taking, women who had leave coverage from their employers and took them, received a wage premium almost large enough to offset the penalty from having one child (Waldfogel 1998). The study detected such effects in comparable cohorts of American and British mothers, using longitudinal data (National Longitudinal Survey of Youth and National Child Development Study respectively) that allowed comparison of wages earned by the same woman at different points in time, mainly over the 1980s.

As far as we know, there are currently *no* studies of the impact of the 1994 MLL changes in United Kingdom on mother's wages or employment status.

### UK MLL and 1994 Changes

The UK first introduced MLL in 1975, including paid leave and the right to return to one's previous job, but the policies had strict limitations and were not universally accessible to pregnant and parenting women. Between 1975 and 2000, MLL in the UK became increasingly liberal and conditions to access them were relaxed. In 1994, the UK was obliged to enact the EU Pregnant Workers Directive, which extended the right to return to work after 14 weeks for all pregnant women, regardless of time worked. In the same year, pregnant women in UK who worked for a minimum of 26 weeks with the same employer were offered a higher rate of maternity pay at 90% of average weekly earnings for six weeks and 12 weeks' pay at a set flat-rate. Overall, MLL changes in 1994 expanded and opened up paid benefits for a larger population of pregnant workers (Gregg et al., 2007; Callender et al., 1996).

## DATA AND METHODS

### Data

This study uses the General Household Survey (GHS) from UK's Economic and Social Data Services (ESDS). The years extracted from the GHS are 1991-92, 1992-93, and 1993-94 for the pre-policy years and 1995-96 and 1996-97 in the two years after the policy was reformed. The dataset is a repeated cross section household survey. The total sample size includes 26,474 women for the pre-policy years and 12,410 women for the post-policy years. Both samples are restricted to women between 20 and 65 years old in order to capture working-age women.

### Dependent variables

We have two dependent variables. Our first dependent variable is *employment status*, which has three mutually exclusive and exhaustive categories. A woman is deemed (a) Employed full-time if she is categorized as working 35 hours per week or more; (b) Employed part-time if she works less than 35 hours per week; and (c) Unemployed if she is categorized as "unemployed" and "not working", or if she is categorized as "economically inactive" and "not working", as well as those who are categorized as "not working". These variables were created from two separate but somewhat overlapping variables: "status" (full-time, part-time, or not working) and "economic status" (working, not-working, unemployed or economically inactive).

W*eekly earnings* are directly available in the GHS. The series were deflated using the Consumer Price Index published by the Official Statistical Office of the UK. We report earnings in 2005 constant pounds sterling and use the logged weekly earnings in our models.

**Key independent variable: *Treatment***

Our *first treatment group* is defined as those who may have received benefits from the policy, thus mothers of working age (20 to 65) with children between ages 0 to 2 years, in the years after the policy implementation (i.e the "takers"). Our *second treatment group* is defined as those working women that are "potential takers" of the maternity leave policy: mothers of working age who are also of fertile age (20-44). The *control group* we will use for both treatment groups is women of working age who are not of fertile age (45-65). The disaggregated sample sizes per treatment (control) status are: $N_{T1}$=1,189, $N_{T2}$=12,582, $N_C$ =6,340, for the pre-policy years (1991, 1992 & 1993) and $N_{T1}$=676, $N_{T2}$=7,646, $N_C$ =4,088 for the post-policy years (1995 & 1996).

**Control variables**

*Age* is a demographic variable given in years. Additionally, we create a quadratic term to account for the non-linear relationship between the dependent variables (earnings and employment status) and age. E*ducational qualification* is coded intro six categories. 1) l*ess-than-secondary education* includes: “No O-levels” (i.e. 1-10 grade without secondary education certificate), and other categories such as “never went to school”; 2) *secondary education* includes “GCSE O-levels” (at least one subject); 3) *high school education* includes “A-levels” with at least one subject (equivalent to a high school degree); 4) *college degree and higher qualifications* includes “First degree” (Bachelor’s degree or equivalent), “Higher degree”, “Graduate and Professional Qualifications” (such as nursing, teaching and other higher qualifications); and 5) *vocational and other qualifications* includes “apprenticeship”, “foreign qualification” and “other qualifications” (following the National Vocational Qualification (NVQ) framework). 6) The sixth category is *No qualification*. Following the hierarchical order provided by the UK’s Office for National Statistics,[5] this is the lowest educational category and we use this as our base category. *Marital status* is coded into *married*, *previously married* and *never married*. Previously married includes divorced, separated and widowed. The GHS has two kinds of data on marital status: legal marital status and de-facto marital status, taking into account common law. We decided to use the legal marital status. In order to account for the distinctive effect of additional children on women’s wages and employment status, we created three dummies for the different number of children: *one child*, *two children* and *three or more children.* The *region* variables have four categories -*England*, *Scotland*, *Wales* and *Northern Ireland*. The GHS sample is overwhelmingly white (over 90%), so we coded *race* into just two simple categories –*White* and *non-White.*

**Methods**

[5] http://www.ons.gov.uk/ons/guide-method/census/census-2001/about-census-2001/census-2001-forms/index.html

Our analysis addresses two distinct questions about groups of women that were exposed to the MLL change in United Kingdom in 1994. The first question asks whether the policy change impacted the employment status of "treated" women (those with children aged 0-2) and "potential takers", that is women of childbearing age exposed to the policy (20-44 years old). The second question asks *the effect of the MLL on the wages* of

In our analysis of both outcome variables with the two groups of "treated" women, we use women of non-childbearing age (45-65) as our control group because it is unlikely that they take advantage of the change in the law, and thus their employment decisions or wages would not be affected due to changes in MLL.

In order to answer both questions, we conduct two difference-in-difference analyses to compare the changes in the mean values of wages and the marginal effect on employment across pre- and post-policy years for both treatments and the control group. In order to do this, we use three pre-policy (1991, 1992 and 1993) and two post-policy years (1995-1996) for all relevant variables.

*Question 1: Assessing the impact on employment status*

The employment status is an ordinal but not continuous variable that has three distinct categories: unemployed, employed part-time and employed full-time. Thus, to correctly address our second outcome of interest, we use a *multinomial logit* model of the following way:

$$\Pr (E_{it}=j) = C + \beta_i X_i + \beta_1 P_t + \beta_2 T_i + \beta_3 P_t T_i + \varepsilon_{it} \qquad (1)$$

Where:

$E_{it}$ : Employment status of woman *i* in time *t,* and j are the three alternatives of employment. Where j can take on three distinct values j=0 signifies that a woman is unemployed, j=1 that a woman is employed part-time and, j=2 that a woman is employed full-time.

$X_i$: *Age* and *age squared, education* dummies (less-than-secondary education, secondary education, high school, college and above and vocational and other qualifications, where the omitted category is "no qualification"), *marital status* dummies (married and previously married, where "never married" is the omitted category), *child* dummies (one child, two children, three or more children, where the omitted category is "no children").

$P_t$: Policy: 1 if year>1994 and 0 if year<1994 (since 1994 is the year of the policy change)

$T_i$: Treatment dummy (Treat)

$T_1$ Group = Mothers with children 0-2 years old

$C_1$ Group = Women aged 46-65

$T_2$ Group = All women aged 20-45

$C_2$ Group = Women aged 46-65

$P_tT_i$: Policy*Treat = Difference-in-difference interaction

In a difference-in-difference analysis, the coefficient $\beta_1$ measures the difference in pre- and post-policy outcomes for the control group, while $(\beta_1 + \beta_3)$ measure the difference in pre- and post-policy outcomes for the treatment group. Consequently, the difference-in-difference estimate is given by $\beta_3$ and this is our coefficient of interest. It captures the true effect of the policy change.

Once we have probabilities, we calculate the marginal effects from the employment logits, comparing them across treatment and control groups in pre- and post-policy years.

*Question 2: Assessing the impact on earnings*

For the second analysis, we have the following earnings' model:

$$Y_i = C + \beta_i X_i + \alpha_1 P_t + \alpha_2 T_i + \alpha_3 P_tT_i + \mu_{it} \qquad (2)$$

Where:

Y: logged weekly *earnings*

X: *age* and *age squared*, *education* dummies (less-than-secondary education, secondary education, high school, college and above and vocational and other qualifications, where the omitted category is "no qualification"), *marital status* dummies (married and previously married, where "never married" is the omitted category), *child* dummies (one child, two children, three or more children, where the omitted category is "no children").

$P_t$: Policy: 1 if year>1994 and 0 if year<1994 (since 1994 is the year of the policy change)

$T_i$: Treatment dummy (Treat)

$T_1$ Group = Mothers with children 0-2 years old

$C_1$ Group = Women aged 46-65

$T_2$ Group = All women aged 20-45

$C_2$ Group = Women aged 46-65

$P_tT_i$: Policy*Treat = Difference-in-difference interaction

In a difference-in-difference analysis, the coefficient $\alpha_1$ measures the difference in pre- and post-policy outcomes for the control group, while ($\alpha_1 + \alpha_3$) measures the difference in pre- and post-policy outcomes for the treatment group. Consequently, the difference-in-difference estimate is given by $\alpha_3$ and this is our coefficient of interest. It captures the true effect of the policy change.

In both cases we have three specifications of the models: specification (1) includes all employed workers and the set of controls described above. Specification (2): In addition to model (1), this model also includes "region" dummies (omitted category is "Region 1: England"). Specification (3): In addition to specification (2), it also includes interactions between treatment and marital status dummies and treatment and education dummies.

## IV. RESULTS

### Descriptive Results

Our first dependent variable is employment status. In the pre-policy years (1991-1993), 16.5% of women in Treatment 1 Group (women with children less than 2 years) were employed full-time, 23% were employed part-time and 60% were unemployed. Again, in the Control Group, 22% were employed fulltime, 24% part time and 54% were unemployed. The differences between Treatment 1 and Control in case of full-time employment and unemployment were statistically significant (at $p<0.05$). For Treatment Group 2 (women of child bearing age, 20-45 years), in the pre-policy years, 34% were employed full time, 24% were employed part time and 42% were unemployed. When compared to the control group, differences were statistically significant for full-time employed and unemployed (at $p<0.01$). In the post-policy years (1995, 1996), for Treatment Group 1, proportion of full-time employed increased to 25%, proportion of part-time employed increased to 29.5% and therefore, the proportion of unemployed decreased to 45%. In the Treatment 2 group too, proportion of full time employed increased to 40%, part time to 29% and that of unemployed thus decreased to 32%. Finally, in the Control Group, proportion of full time employed increased slightly to 25%, part time to 29% and that of unemployed thus decreased to 46%. The differences in proportions between Treatment 2 and Control were statistically significant (at $p<0.01$) for full time employment and unemployment.

Our second dependent variable is weekly earnings (constant 2005 pounds). Overall, there appears to be a positive secular trend in earnings, which is more pronounced for the Treatment group 1, i.e. mothers with children less than 2 years. In the pre-policy years (1991 to 1993), women in Treatment 1, on the average earned 88£ a week, women in Treatment 2 (women of child bearing age) earned on the average 140£ a week and women in the Control group earned

96.5£ per week on the average. In the post policy years, mothers in Treatment 1 on the average, earned 122 £ per week while women in Treatment 2 earned 149£ per week and those in the Control earned on the average, 102£ per week. The mean earnings differed significantly between Treatment 2 and Control both in the pre-policy as well as in the post-policy years and differed significantly between Treatment 1 and Control in the post-policy years.

Mothers were naturally younger in Treatment 1 than in Treatment 2. The sample for all groups was predominantly white (between 93 to 95%). Overall, the proportion of married mothers decreased slightly over the years with a corresponding increase in mothers who had never been married. Around 82% of women in Treatment 1, around 23% women in Treatment 2 and around 10% of women in the Control group had only one child both in the pre-policy and in the post-policy years. There weren't much changes in the proportions of women in each sample with just two children, but there were drastic shifts in the proportions of women with three or more children in the Treatment 1 sample –while 41% of mothers had three or more children in the pre-policy years, 73% of mothers had three or more children in the post-policy years. Finally, with respect to educational qualifications, about 12% of Treatment 1 mothers had no qualifications in the pre-policy years' sample and similar proportion for post-policy years' sample. There were decreases in all the groups in the proportion of women having less than a secondary school qualification, with a corresponding increase in the proportions of women completing O-levels (secondary certificate) and A-levels (equivalent to a high school degree). In the pre-policy sample of Treatment 1, 15% had a college degree or other equivalent qualifications, while in the post-policy sample of Treatment 1, 24% had the same. Again, 16% of the pre-policy Treatment 2 group, 22% of the post-policy Treatment 2 group, 12% of the pre-policy Control group and 14.5% of the post-policy Control group had a college degree or equivalent. There were statistically significant differences between each treatment group and the control group with respect to key covariates, thus necessitating multivariate analyses.

**Table 1. Descriptive statistics of working-age women in the UK for the years 1991 to 1996**

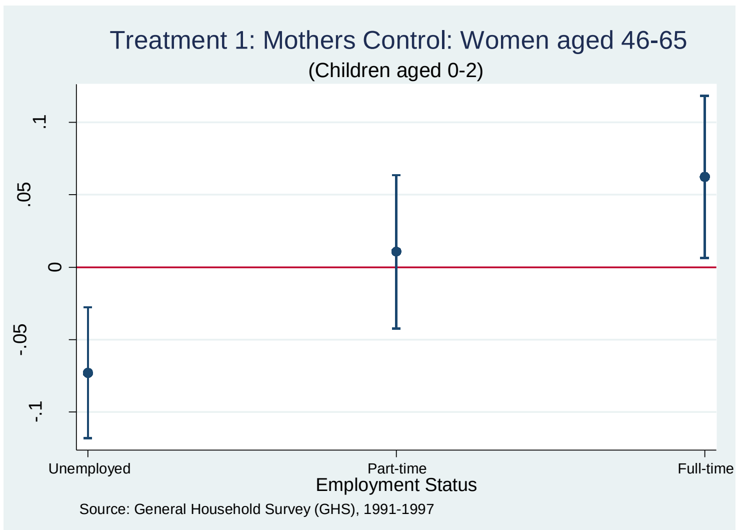
| **Years** | **Pre-Policy Years (1991-1993)** | | | **Post-Policy Years (1995-1996)** | | |
|---|---|---|---|---|---|---|
| | **Treatments** | | **Control** | **Treatments** | | **Control** |
| | **T1** All women (w/children < 2 years old) | **T2** Women of childbearing age (20-45) | **C** Women of non-child bearing age (+45) | **T1** All women (w/children < 2 years old) | **T2** Women of childbearing age (20-45) | **C** Women of non-childbearing age (+45) |
| ***Dependent Variables*** | | | | | | |
| Full time (%) | 16.57** | 34.12*** | 21.65 | 25.39 | 39.80*** | 24.90 |
| Part time (%) | 23.04 | 23.95 | 24.35 | 29.53 | 28.57 | 28.77 |
| Unemployed (%) | 60.38** | 41.93*** | 54.00 | 45.08 | 31.62*** | 46.33 |
| Weekly earnings | 87.66 | 139.76*** | 96.57 | 121.56** | 148.90*** | 102.28 |
| | (4.167) | (1.479) | (2.197) | (6.465) | (2.139) | (2.422) |
| ***Independent Variables*** | | | | | | |
| Age | 27.46*** | 31.93*** | 54.30 | 28.72*** | 32.37*** | 54.17 |
| | (0.124) | (0.058) | (0.062) | (0.196) | (0.075) | (0.081) |
| White (%) | 93.28*** | 92.78*** | 95.91 | 93.58** | 93.30*** | 96.36 |
| Non White (%) | 6.72*** | 7.22*** | 4.09 | 06.42** | 06.70*** | 03.64 |
| Married (%) | 82.29*** | 68.19*** | 76.58 | 84.31** | 68.07*** | 77.87 |
| Previously married (%) | 3.41*** | 9.39*** | 18.95 | 03.85*** | 09.91*** | 18.33 |
| Never married (%) | 14.3*** | 22.42*** | 4.47 | 11.84*** | 22.02*** | 03.80 |
| One child (%) | 82.45*** | 22.44*** | 8.96 | 81.61*** | 22.78*** | 10.13 |
| Two children (%) | 16.14*** | 25.77*** | 3.04 | 15.91*** | 26.29*** | 02.81 |
| Three or more children (%) | 41.00 | 10.86*** | 0.66 | 73.00 | 11.42*** | 0.56 |
| Less than secondary(%) | 26.8** | 25.4*** | 23.7 | 11.5 | 11.4* | 10.6 |
| Secondary (o levels)(%) | 33.1*** | 26.4*** | 11.3 | 34.3*** | 30.3*** | 16.9 |
| High School (a level)(%) | 10.7*** | 10.9*** | 2.83 | 15.2*** | 14.6*** | 4.62 |
| College & other qual (%) | 15.3*** | 15.6*** | 11.8 | 24.2*** | 21.9*** | 14.5 |
| Vocational qual (%) | 2.2** | 2.43*** | 3.75 | 1.9** | 2.71*** | 3.65 |
| No Qualifications (%) | 11.8*** | 19.3*** | 46.6 | 12.8*** | 19.2*** | 49.7 |
| Region 1 (England) | 68.1 | 67.6 | 66.6 | 77.9 | 76.3 | 76.4 |
| Region 2 (Scotland) | 18.1* | 19* | 20.3 | 8.84 | 9.21 | 9.99 |
| Region 3 (Wales) | 4.91 | 3.78** | 4.4 | 5.28 | 5.03 | 4.94 |
| Region 4 (North Ireland) | 8.82 | 9.63*** | 8.67 | 7.99 | 9.45 | 8.65 |
| Observations | 1,189 | 12,582 | 6,340 | 676 | 7,646 | 4,088 |

Notes: Significance levels *** p<0.01, ** p<0.05, * p<0.1 in mean difference (ttest) comparisons of T1 with C, and T2 with C. Data come from the General Household Survey for the years 1991, 1992, 1993, 1995 and 1996.

## Effect on employment status

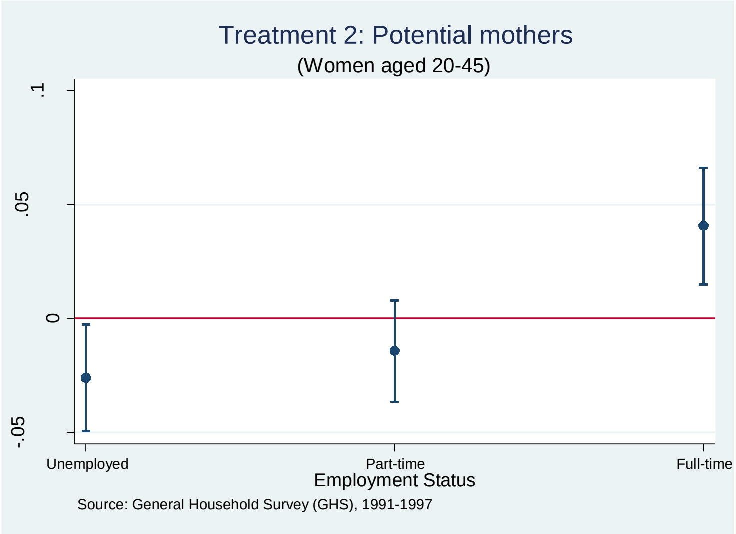
In order to analyze the first outcome of interest, we used model (1) previously described through a *multinomial logit* model specification. Then we calculated the marginal effects for the policy change in the probabilities of employment between treatment and control groups.

**Figure 1: Coefficients of marginal probabilities of employment for women with young children**




Results from table 2 (see appendix) indicate that the probability of being employed increased 6.2% for full-time employment and 1% for part-time employment for the $T_1$ group post-policy, when compared to the control group. Evidence from our model also shows that there is a 7% decrease in the probability of being unemployed between $T_1$ group and the control group after the policy change. This coefficient is statistically significant at the 99% level of confidence. Together these results imply that the women that directly benefit from the policy (women with children less than 2 years old) were more likely to look for a full-time job after the policy, compared to "untreated" women. And at a lesser extent, they also increased their part time employment.

**Figure 2: Coefficients of marginal probabilities of employment for women of fertile age**




In the case of women who were exposed indirectly to the treatment ($T_2$), we also observe statistically significant changes in their probabilities of employment between the treatment and the control groups post-policy. Results from table 2 (see appendix) indicate that the probability of being employed full-time increased 4%. In contrast to the results for the $T_1$ group, the probability of part-time employment decreases, in about 1%, for $T_2$ post policy, compared to the control group. The probability of being unemployed decreased 2.6% for the $T_2$ group post-policy compared to the control group. Both results indicate that some of the potential takers of the maternity leave policy do change their labor decisions. It might be the case that these women look for better employment opportunities in order to qualify for higher benefits in case they do take advantage of the maternity leave policy in the near future. Although, we do not have sufficient evidence to prove that these potential takers change also change their fertility decisions as a consequence of the policy.

**Effect on earnings**

*Treatment 1: Women with children age 0-2*

For all models in the first treatment group, the coefficients of covariates are directionally consistent with theory. Increases in *age* and in *level of education* for employed women are associated with statistically significant increases in earnings. Those employed women who are *not white* have higher earnings than those who are white. This may be the case as the vast majority of population in the UK around 1994 was white (over 93%), and the group of minority women who are employed may be a selective group, possibly with higher skills and levels of motivation. Being *married* or *previously married* decreases earnings (as compared to being single). Having *children* results in a statistically significant decrease in earnings, and having *additional children (two, three or more)* has a cumulative effect in increasing the negative impact on earnings. These results are consistent with the literature on family wage gap and that on the penalty associated with having more children (Waldfogel, 1997a, 1998b).

Including *region dummies* (specification 2) and additionally, *interactions of treatment and marital status dummies* as well as *interactions of treatment and education dummies* (specification 3), we found no changes in direction, slight changes in magnitude and almost no changes in significance of any of the coefficients.

Our *difference-in-difference* coefficient is negative across all models. These coefficients are not statistically significant. All employed women affected by the 1994 policy change, i.e. those with children age 0-2 in the years following the change, have less than 1% lower earnings than the control group of women unaffected by the policy, although this finding is not significant. These results are consistent with the literature (Ruhm, 1996, Baum, 2003).

**Table 3: Effect of MLL on log weekly earnings (2005** £)

| | Treatment (1): Mothers of children 0-2 years old | | | Treatment (2): Potential mothers | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (1) | (2) | (3) |
| **Policy*Treatment** | -0.00452<br>(0.0747) | -0.00740<br>(0.0746) | -0.00585<br>(0.0746) | -0.0361<br>(0.0301) | -0.0349<br>(0.0301) | -0.0377<br>(0.0302) |

| | | | | | | |
|---|---|---|---|---|---|---|
| **Policy** | 0.0559** | 0.0572** | 0.0579** | 0.0549** | 0.0549** | 0.0577** |
| | (0.0266) | (0.0266) | (0.0266) | (0.0254) | (0.0254) | (0.0253) |
| **Treatment** | 0.296** | 0.296** | -0.758*** | 0.132*** | 0.128*** | -0.424*** |
| | (0.123) | (0.122) | (0.227) | (0.0328) | (0.0328) | (0.0703) |
| Age | 0.115*** | 0.114*** | 0.0925*** | 0.0853*** | 0.0850*** | 0.0849*** |
| | (0.0193) | (0.0193) | (0.0193) | (0.00572) | (0.00572) | (0.00576) |
| Age squared | -0.00133*** | -0.00132*** | -0.00111*** | -0.00112*** | -0.00112*** | -0.00113*** |
| | (0.000196) | (0.000195) | (0.000195) | (7.97e-05) | (7.97e-05) | (7.99e-05) |
| Less than secondary | 0.224*** | 0.223*** | 0.233*** | 0.245*** | 0.245*** | 0.242*** |
| | (0.0407) | (0.0407) | (0.0421) | (0.0241) | (0.0241) | (0.0401) |
| Secondary (o level) | 0.304*** | 0.305*** | 0.300*** | 0.366*** | 0.368*** | 0.311*** |
| | (0.0332) | (0.0334) | (0.0348) | (0.0197) | (0.0197) | (0.0332) |
| High School (a level) | 0.424*** | 0.422*** | 0.373*** | 0.438*** | 0.436*** | 0.392*** |
| | (0.0587) | (0.0588) | (0.0709) | (0.0266) | (0.0266) | (0.0654) |
| College & other qualifications | 0.846*** | 0.841*** | 0.818*** | 0.902*** | 0.898*** | 0.867*** |
| | (0.0362) | (0.0363) | (0.0387) | (0.0211) | (0.0211) | (0.0363) |
| Vocational qualifications | 0.00657 | -0.00869 | -0.0391 | 0.213*** | 0.169*** | -0.0424 |
| | (0.0780) | (0.0781) | (0.0806) | (0.0491) | (0.0488) | (0.0753) |
| Non white | 0.336*** | 0.254*** | 0.269*** | 0.222*** | 0.123*** | 0.130*** |
| | (0.0711) | (0.0896) | (0.0879) | (0.0341) | (0.0387) | (0.0386) |
| Married | -0.386*** | -0.388*** | -0.592*** | -0.0458** | -0.0444** | -0.523*** |
| | (0.0551) | (0.0549) | (0.0532) | (0.0199) | (0.0198) | (0.0560) |
| Previously married | -0.231*** | -0.235*** | -0.417*** | 0.00566 | 0.00461 | -0.344*** |
| | (0.0634) | (0.0632) | (0.0612) | (0.0297) | (0.0297) | (0.0629) |
| One child | -0.223*** | -0.222*** | -0.211*** | -0.429*** | -0.427*** | -0.429*** |
| | (0.0440) | (0.0441) | (0.0441) | (0.0199) | (0.0198) | (0.0199) |
| Two children | -0.419*** | -0.420*** | -0.420*** | -0.713*** | -0.712*** | -0.722*** |
| | (0.0762) | (0.0765) | (0.0762) | (0.0222) | (0.0222) | (0.0223) |
| Three children | -0.526*** | -0.539*** | -0.516** | -0.862*** | -0.865*** | -0.876*** |
| | (0.203) | (0.200) | (0.201) | (0.0387) | (0.0386) | (0.0387) |
| Region Dummies | | YES | YES | | YES | YES |
| Marital Status X Treat | | | YES | | | YES |
| Education X Treat | | | YES | | | YES |
| Observations | 6,561 | 6,561 | 6,561 | 19,236 | 19,236 | 19,236 |
| R-squared | 0.115 | 0.116 | 0.123 | 0.175 | 0.177 | 0.181 |

Notes: 1.Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1 2. Base category for education dummies is “No Qualifications”

*Treatment 2: Women aged 20-45*

Similar to what we observe in $T_1$, the coefficients of the covariates for all models in $T_2$, women who are 20-45 and who are indirectly affected by the 1994 changes in maternal leave legislation, are directionally consistent with theory. *Age, education level, being non-white,* and *being married* all have similar effects on earnings as in $T_1$, detailed above. But we see a slight difference with $T_2$ in that being *previously married* seems to show a very small positive but insignificant effect on earnings for models 1 and 2. But being *previously married* in $T_2$ model 3, including all interactions, is statistically significant and shows a negative wage impact, consistent with findings in $T_1$. Having *children* again results in a statistically significant decrease in earnings, and having *additional children (>1)* has a cumulative effect in decreasing earnings. But the magnitude of the decreases in earnings with each additional child is greater for $T_2$ than in $T_1$. For the most part and similar to what we observe with $T_1$, including *region dummies* (specification 2), and additionally *interactions of treatment and marital status dummies* as well as *interactions of treatment and education dummies* (specification 3), we found almost no changes in direction, slight changes in magnitude and almost no changes in significance of any of the coefficients.

Our *difference-in-difference* coefficients are negative for all models, but the coefficients are not statistically significant. This is a similar pattern with women employed part-time to what we saw in $T_1$. All employed women indirectly affected by the 1994 policy change, i.e. those aged 20-45 (in all models) have approximately 4% less earnings than the control group of women unaffected by the policy, although this finding is insignificant. These findings, which show negative impacts on earnings for women of childbearing age in the years post policy, may reflect theories that posit that women bear the cost burden of mandated MLL. But the findings remain ambiguous, as they are statistically insignificant. This is consistent with the existing literature in the area.

## V. CONCLUSIONS

Evidence from this analysis shows that the changes in MLL implemented by the UK government in 1994 had a *negative* effect on the wages of treated women, when compared to untreated women. Conversely, the policy increased the probability of employment for treated women in comparison to untreated women and thus had a *positive* effect on employment.

Those women with children age 0-2 in the years following the change have a decrease in earnings below 1%, when compared to women unaffected by the policy. The women that were indirectly or potentially affected by the 1994 policy change (i.e. those aged 20-45) have approximately 4% less earnings than those women unaffected by the policy. However, neither of our earnings models presents statistically significant results under the usual levels of confidence.

In terms of the effects on employment status, our findings indicate that the policy had a positive and statistically significant effect on the probability of employment (both full- and part-time). The effect is stronger on the "takers" of the policy.

We find that there is a six-percentage point difference in the probabilities of being employed full-time between $T_1$ women and the control group post-policy. The analysis of potential takers of the policy also shows an increase in the probability of being employed by 4% between the treatment and the control groups post policy. There is a lower probability of being unemployed for both treatment groups, when compared to their respective control groups, post policy. The decrease ranges from 6-7% for $T_1$ and has an approximate range of 2-3% for the $T_{2.}$

Consistent with theory, we observe that expansion of paid MLL has a slight negative effect on wages in the short term (two years post-policy), but an increase in the probability for both women of childbearing age and new mothers of being employed. Future research could focus on analyzing the trajectory of wages on the long term to determine whether the treated women "catch up" with their female counterparts. In addition, it would be interesting to analyze changes in the family gap due to maternity leave changes.

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## Appendix

**Table 2. Effects of MLL on the marginal probability of *employment***

| | $T_{(1)}$: Mothers of children 0-2 years old | $T_{(2)}$: Potential mothers 20-45 years of age |
|---|---|---|
| Probability of being *unemployed* | -0.0729*** | -0.0261** |
| | (0.0231) | (0.0119) |
| Probability of being *employed part-time* | 0.0106 | -0.0144 |
| | (0.0271) | (0.0113) |
| Probability of being *employed full-time* | 0.0623** | 0.0405*** |
| | (0.0286) | (0.0131) |
| Region dummies | YES | YES |
| Marital status*Treatment | YES | YES |
| Education*Treatment | YES | YES |

Notes: Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1