Maternity Leave Duration and Female-Male Relative Labour Market Outcomes*

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Abstract

This paper finds that longer maternity leave durations lead to a deterioration in relative female labour market outcomes. A 50% increase in the duration of paid maternity leave in the UK from April 2007 is used to provide exogenous variation. A quasi-experimental difference in differences estimation approach is used to estimate the impact on the relative wage gap. Furthermore, for the impact of the extension on discrete outcomes (employment, hiring and redundancy), an alternative estimation approach is proposed that has the advantage of providing interpretable treatment effects in the presence of substitution effects.

1 Introduction

Large gender pay gaps exist in most countries, with the unadjusted average EU gender pay gap standing at 16.5% in 2012. The unadjusted UK gender gap stands slightly higher than the EU average, at 19.1%.[1] These gaps are persistent despite the high priority placed on closing the gender pay gap, both at an EU and a UK level, and despite female educational attainment surpassing male attain-

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[1]Eurostat

ment over the last decade.[2] Motivating factors for decreasing the gender pay gap include promoting gender fairness and equality, increasing female labour market participation and decreasing litigation costs. At the same time, many countries have recently increased the generosity of maternity leave benefits, and have introduced increasingly flexible working arrangements. Numerous arguments can be made for paid parental leave/flexible working arrangements, such as increasing female labour market opportunity and participation. A key question arises as to whether the increasing flexibility of maternity leave and working arrangements (which potentially impose direct or indirect employer costs), hinders progress in closing the gender gap or impacts on other relative female-male labour market outcomes.

This paper analyses the impact of an increase in the duration of paid maternity leave on relative female labour market outcomes. The analysis is based in the UK, which experienced a 50% increase in the duration of paid maternity leave for female employees, from a maximum of 26 weeks to a maximum of 39 weeks from 1st April 2007. While similar reforms in other countries/time periods tended to increase parental leave for male and female employees simultaneously (e.g. the FMLA in the United States), this reform was unusual in changing parental rights for female employees only. Furthermore, the quasi-experimental estimation approach implemented in this paper is unique in that it estimates the role of statistical discrimination by employers on relative female-male labour market outcomes, separately from the impact of increased human capital depreciation or higher numbers of retained job matches. The existing literature tends to estimate an impact that is an aggregate of employer discrimination, the increased number of retained job matches and the impact of longer leave periods on human capital depreciation.

A difference in differences estimation strategy is implemented, with an alternative approach being suggested for binary outcome models that respects the discrete nature of the outcome. The proposed approach builds on that of Athey and Imbens (2006) and Blundell et al. (2004). One key benefit of the proposed approach in contrast with the alternatives is that it facilitates estimation of the

[2] In 2012 the UK average share of males aged 30-34 who had completed tertiary education was 44.0% compared to 50.2% for females. The corresponding figures for 2002 were 32.4% for males and 30.7% for females. (Eurostat)

impact of a policy change when there are possible substitution effects impacting the control group.

A simple theoretical model of the role of differential parental leave uptake by male and female employees on relative labour market outcomes is developed. While on parental leave employees do not receive pay from their firms, but employees taking longer leave periods are less profitable for the firm because the firm has equal sunk hiring and training costs for all employees. This implies a male-female wage gap for otherwise identical employees. Furthermore, in the face of expanding differentials in male-female parental leave uptake, the model predicts an increase in the male-female wage gap.

This paper shows that the policy change increased the relative uptake of parental leave by females compared to males, particularly for those aged 25-34. The aggregate effect was decomposed into specific effects due to fertility responses and relative female-male uptake of parental leave, both of which play a significant role in the divergence. Furthermore, empirical evidence was found in support of the theoretical model, with evidence of a divergence in male-female relative wages after the expansion of paid maternity leave in the UK. Evidence was also found of an increase in relative female redundancies after the policy change. Although a negative impact was estimated on relative female hiring rates it was not statistically significant. The low magnitude of the hiring effect may be due to higher rates of female replacements necessitated by fertility responses and/or higher maternity leave durations, combined with the likelihood of a female employee being replaced with another female due to occupational sorting. Similarly, a negative but insignificant impact was found on relative female conditional employment rates. It is possible that a longer observation period would lead to larger estimates on relative employment rates, as differential hiring/firing rates accumulate.

This research is related to a number of papers analysing changes in maternity leave policies on female labour market outcomes. Waldfogel (1999) and Baum (2003) analyse the introduction of the Family and Medical Leave Act of 1993 in the US. Baum and Ruhm (2013), Curtis et al. (2014) and Das and Polachek (2014) analyse the introduction of the 2004 Californian Paid Family Leave policy. There is also a strand of research which uses cross-country analysis to estimate the impact of maternity leave policies on female labour market outcomes

(Ruhm, 1998; Akgunduz and Plantenga, 2012). In comparison with the previous literature, this paper finds evidence of larger negative effects on relative female labour market outcomes. A number of possible explanations are considered in the discussion at the end of the paper.[3]

The paper is laid out as follows; Section 2 outlines the theoretical model, Section 3 discusses the estimation methodology both for continuous outcomes and the proposed methodology for binary outcomes, Section 4 provides an overview of the legislative context in the UK, the data and empirical strategy are outlined in Section 5, the results are presented in Section 6, and finally, Section 7 concludes.

2 Simple Theoretical Model

In this section, a simple theoretical model is presented. Higher levels of parental leave taken by female employees leads to a predicted male-female wage gap in the model. Furthermore, divergence in relative female-male leave taking is predicted to increase the male-female wage gap. This prediction is tested empirically later in the paper, facilitated by a common trends assumption. The model also predicts that a greater divergence in relative female-male leave taking is associated with higher male wages and higher male employment.[4] There are ambiguous predictions for female wages and employment.

A single period model where firms can discriminate without sanction is considered. A risk neutral, profit maximizing, price taking representative firm chooses the optimal numbers of male and female hires. The firm pays hiring costs c and training costs t for each worker. It also pays male workers the market wage w_m and female workers the market wage w_f. Female employees that take maternity leave - a proportion $\gamma(\theta)$, do not work for the duration of their maternity leave, θ. During this time they are not being paid a wage by the employer. A higher θ is interpreted as a more generous maternity leave period. $1-\delta(\theta)$ is the propor-

[3] A large number of related papers look at the impact of maternity/paternity leave duration on the wages of mothers/fathers, e.g.; Ondrich and Spiess (2002) - Germany, Buligescu et al. (2008) - Germany, Schönberg and Ludsteck (2007) - Germany, Ekberg et al. (2013) - Sweden, Waldfogel (1998) - US and Britain, Joseph et al. (2013) - France, Lalive et al. (2013) - Austria, Rege and Solli (2013) - Norway, Baum and Ruhm (2013) - US, Dahl et al. (2013) - Norway.

[4] As long as there is not a large positive female participation response. See Appendix A for more detail.

tion of productive females, where $\delta(\theta) = \gamma(\theta) * \theta$. Assume $0 < \delta(\theta) < 1$. A firm with concave production function F, (with $F \geq 0$, $F' \geq 0$, $F'' \leq 0$), where male and female workers are assumed to be perfect substitutes solves the following problem:

$$\pi = \max_{L_m, L_f} [-(c+t)(L_m + L_f) - w_m L_m - w_f L_f + F(L(L_m, L_f, \delta(\theta)))]$$

Since male and female workers are perfect substitutes, $L(L_m, L_f, \delta(\theta)) = L_m + (1 - \delta(\theta))L_f$.[5,6]

This model could be considered a formalisation of the argument made by Thurow (1975) where he discusses employer's use of statistical discrimination in making employment decisions. Thurow points out that employers who invest in on-the-job training (t in the model above) are less likely to be able to recoup the investment from women.

Solving the firm's optimisation problem;

$$FOC[L_m^D] : w_m = \frac{\partial F(L(L_m, L_f, \delta(\theta)))}{\partial L(L_m, L_f, \delta(\theta))} - (c+t)$$

$$FOC[L_f^D] : w_f = \frac{\partial F(L(L_m, L_f, \delta(\theta)))}{\partial L(L_m, L_f, \delta(\theta))} - \frac{(c+t)}{1 - \delta(\theta)}$$

Firms solve for the number of males and females to hire such that the above first order conditions are satisfied, which will be jointly determined by male and female labour market supply.

This model implies that in equilibrium there will be a male-female wage gap, with

$$w_m^* - w_f^* = \frac{\delta(\theta)(c+t)}{1 - \delta(\theta)}$$

where w_m^* and w_f^* denote equilibrium male and female wages respectively.

[5] In the empirical work, it is assumed that males and female workers are perfect substitutes conditionally

[6] Acemoglu et al. (2004) estimated the degree of substitution between female and male workers and found that male and female workers were imperfect substitutes. However, this research used data mainly from 1940-1960. At that time, male workers differed from female workers due to their being more highly productive in jobs where brawn was required. It is likely that male and female workers are closer substitutes now than they were fifty year ago.

In a model with training costs and hiring costs, this model predicts that when female workers take more leave than male workers, profit maximising firms will pay female workers lower wages.

2.1 The impact of increasing maternity leave on the male-female wage gap

An increase in maternity leave increases the male-female wage gap, since

$$\frac{d(w_m^* - w_f^*)}{d\theta} = \frac{\frac{\partial \delta(\theta)}{\partial \theta}(c+t)}{(1-\delta(\theta))^2}$$

Proposition: Increases in maternity leave increase the male-female wage gap

Longer maternity leave periods lead to female workers being less productive for a given initial investment, which results in downwards pressure on female employment and wages. Because male and females are perfect substitutes, this facilitates firms switching from female to male workers. This puts additional downwards pressure on female employment and wages, but upwards pressure on male employment and wages. Assuming the longer maternity leave results in fewer productive workers, then due to higher marginal productivity there is an upwards pressure on both male and female wages, which is of the same magnitude.[7]

In the empirical section a difference in differences estimation approach is implemented, where identification is through a common trends assumption. As discussed in the methodology section, this allows for estimation of the treatment effect on relative female-male labour outcomes (rather than gender specific treatment effects). Therefore, the empirical section focusses on the key testable model implication given the common trends assumption: increases in maternity leave duration lead to increases in the male-female wage gap. The empirical section will also estimate the treatment effect on relative female-male employment, redundancies and hiring rates.

[7]See Appendix A for more detail.

3 Methodology

The impact of the expansion in maternity leave duration in the UK on relative female labour market outcomes is estimated using a difference in differences approach. A time-gender dimension is used. A standard difference in differences estimation approach is used for the continuous outcome variable (hourly wages). The standard difference in differences treatment effect estimator provides an estimate of the relative impact of the treatment when there are substitution effects on the control group.

Let $Y_i^P(T)$ denote the potential outcome of an individual i at time T with policy P, and let $Y_i(T)$ denote the observed outcome. There are two time periods - before and after the policy change (T=0, T=1 respectively), two policy environments - pre and post policy change (P=0, P=1 respectively), and two groups, females and males (F=1, F=0 respectively). The policy change occurs between time period T=0 and time period T=1. Let $W_{iT} = 1$ if an individual i is observed in the data at time T, and 0 otherwise. X is a set of observable characteristics. Given a number of assumptions,[8] the difference in differences estimator provides an estimate of the relative impact of the treatment.

The difference in differences model can be estimated with the following regression;

$$Y_i(T) = a_1 + a_2 F_i + a_3 X_i + a_4 T_i + \beta F_i T_i + \varepsilon_{it}$$

Where

$$\beta = E[Y^1(1) - Y^0(1)|F = 1, W_1 = 1, X] - E[Y^1(1) - Y^0(1)|F = 0, W_1 = 1, X]$$

is the estimate of the differential impact of the policy change on females compared to males.

[8]Common time trend, no composition effects, no heterogeneity in time trend or treatment effect and linear index restrictions on conditional means

3.1 Binary Outcome Variables

There have been a number of approaches proposed in the literature for estimating binary outcomes models within the difference in differences framework. The problem with using the standard approach discussed in the above, is that the assumptions can lead to predicted counterfactual probabilities outside the zero-one interval which can bias the results (discussed for instance in Athey and Imbens (2006)).

In this section the two main alternatives to point identification of binary outcome difference in differences models are discussed (Athey and Imbens (2006) and Blundell et al. (2004)), and an alternative approach which builds upon this previous literature is suggested. This alternative approach is based on the assumption that the odds ratio of the treatment group and the odds ratio of the control group have the same growth rate in the absence of a policy change. In contrast with the other methods, this approach facilitates interpretation when one allows for the control group to be impacted by substitution effects.[9]

Athey and Imbens (2006) assume the following:[10]

(1) The control group is not impacted by the policy change
(2) If the probability of success (Y=1) decreases for males (the non-treated group) then the rate of decrease in the probability of success for females (the treated group) had they not been treated would have been the same as the rate of decrease in the probability of success for males. On the other hand, if the probability of success increases for males, then the method assumes the rate of decrease in the probability of failure for females had they not been treated would have been the same as the rate of decrease in the probability of failure for males.

In general, the approach in Athey and Imbens (2006) does not preserve the condition that when macro conditions are such that non-treated outcomes for the control group are constant across two time periods, then the predicted non-treated outcomes for the treatment group are also constant across the same two time periods.

[9] The no composition effect assumption is assumed throughout this discussion.

[10] Athey and Imbens (2006) also discuss bound estimation of the policy impact under alternative assumptions.

In fact, their approach assumes convergence of the non-treated outcomes over time (one exception of this rule is when in the initial period (T=0) mean non-treated outcomes for males and females are the same, in which case the approach assumes that the non-treated mean outcomes of males and females will always be equal. Therefore, if the non-treated outcomes for males are constant across two time periods it must also be the case for females).

To understand the intuition behind this convergence point, note that imposing the same rate of decrease of non-treated outcomes implies a higher absolute decrease for whichever group had the highest starting value.

Blundell et al. (2004) assume:
(1) The control group are not impacted by the policy change
(2) Common trends on inverse probability function for non-treated outcomes

$$E[Y^0(T)|F,X] = f(g(F,T,X))$$

where

$$g(F=1,T=1,X)-g(F=1,T=0,X)=g(F=0,T=1,X)-g(F=0,T=0,X)$$

If some additional assumptions are imposed (that are somewhat analogous to the heterogeneity assumptions imposed in the linear case), then

$$g(F,T,X) = \alpha_0 + \alpha_1 F + \alpha_2 T + \beta X$$

In this approach a probability function $f(.)$ has to be specified. Typical specifications include the logistic function or the cumulative normal distribution function.

Alternative Assumptions for Binary Outcome DD Model
Alternative identifying assumptions for the policy impact are considered in this section. The main assumption is that the odds ratio of the treatment group and the odds ratio of the control group have the same growth rate in the absence of a policy change, or equivalently, that non-treated relative female-male odds ratios are constant over time. This approach allows for interpretation of the policy effect when substitution effects for males are not assumed away. Similarly to Athey and Imbens (2006), assumptions are non-parametrically speci-

fied. Similarly to Blundell et al. (2004) when macro conditions are such that the non-treated outcomes for one group are constant across two time periods, then this assumption implies non-treated outcomes for the other group are also constant. There are close similarities between this approach and that suggested by Blundell et al. (2004), which are discussed in more detail in the following.

Assume to begin with
(1) The control group are not impacted by the policy change

$$E[Y^1(1) - Y^0(1)|F = 0, X] = 0$$

(2) The conditional relative odds ratio is constant over time in the absence of the policy. This can be expressed as

$$\frac{E[Y^0(T)|F,X]}{1 - E[Y^0(T)|F,X]} \equiv l(F,T,X) = l_1(F,X)l_2(T,X)$$

or

$$\frac{\frac{E[Y^0(1)|F=1,X]}{1-E[Y^0(1)|F=1,X]}}{\frac{E[Y^0(1)|F=0,X]}{1-E[Y^0(1)|F=0,X]}} = \frac{\frac{E[Y^0(0)|F=1,X]}{1-E[Y^0(0)|F=1,X]}}{\frac{E[Y^0(0)|F=0,X]}{1-E[Y^0(0)|F=0,X]}}$$

Therefore the impact of the policy can be estimated from

$$E[Y^1(1)|F = 1, X] - E[Y^0(1)|F = 1, X] =$$

$$E[Y|F = 1, T = 1, X] - \frac{\frac{\frac{E[Y|F=1,T=0,X]}{1-E[Y|F=1,T=0,X]}}{\frac{E[Y|F=0,T=0,X]}{1-E[Y|F=0,T=0,X]}} * \frac{E[Y|F=0,T=1,X]}{1-E[Y|F=0,T=1,X]}}{1 + \frac{\frac{E[Y|F=1,T=0,X]}{1-E[Y|F=1,T=0,X]}}{\frac{E[Y|F=0,T=0,X]}{1-E[Y|F=0,T=0,X]}} * \frac{E[Y|F=0,T=1,X]}{1-E[Y|F=0,T=1,X]}}$$

With iid assumptions (either on joint/separate observations over individuals) and application of CLT and delta theorems, confidence intervals for the above policy impact can be estimated. However, it is possible to show that under the imposed assumptions, the conditional expectations (conditioning on group and time) can be written as a logistic function with group and time additive effects which suggests an alternative estimation approach.

Ignoring covariates for now, in the current setting with two groups and two time periods the non-treated odds ratio can be written as

$$\frac{E[Y^0(T)|F]}{1-E[Y^0(T)|F]} = \frac{1}{\beta_{11}\beta_{21}}(1+\frac{\beta_{12}}{\beta_{11}})^F(1+\frac{\beta_{22}}{\beta_{21}})^T$$
$$\Rightarrow \frac{E[Y^0(T)|F]}{1-E[Y^0(T)|F]} = e^{\alpha_0+\alpha_1 F+\alpha_2 T}$$
$$\Rightarrow E[Y^0(T)|F] = \frac{e^{\alpha_0+\alpha_1 F+\alpha_2 T}}{1+e^{\alpha_0+\alpha_1 F+\alpha_2 T}}$$

where

$$\alpha_0 = ln(\frac{1}{\beta_{11}\beta_{21}}) \qquad \alpha_1 = ln(1+\frac{\beta_{12}}{\beta_{11}}) \qquad \alpha_2 = ln(1+\frac{\beta_{22}}{\beta_{21}})$$

In reverse, if the conditional expectation takes a logistic form with group and time additive effects then the odds ratio is multiplicatively separable in group and time effects. This explains the close relationship between this approach and that in Blundell et al. (2004) - if the probability function in the Blundell et al. (2004) approach is assumed to be the logistic function, then in a model with no covariates the rate of change of non-treated odds ratios must be the same for both groups. Therefore, this approach provides interpretable restrictions that lead to the estimation approach proposed in Blundell et al. (2004).

The impact of the policy can be estimated from

$$E[Y^1(1)|F=1]-E[Y^0(1)|F=1] = E[Y|F=1,T=1]-\frac{e^{\alpha_0+\alpha_1+\alpha_2}}{1+e^{\alpha_0+\alpha_1+\alpha_2}}$$

If it is also assumed that the conditional mean outcome for females in time period T=1 (treated outcomes) can be modelled as a logistic function then the four conditional mean outcomes can be modelled as

$$E[Y|F,T] = \frac{e^{\alpha_0+\alpha_1 F+\alpha_2 T+\alpha_3 FT}}{1+e^{\alpha_0+\alpha_1 F+\alpha_2 T+\alpha_3 FT}}$$

where now the coefficients are estimated using observed data on non-treated outcomes for males in time periods T=0 and T=1, on non-treated outcomes for females in time periods T=0 and on treated outcomes for females in time period T=1.

And the impact of the policy can be estimated from

$$E[Y^1(1)|F=1]-E[Y^0(1)|F=1]=\frac{e^{\alpha_0+\alpha_1+\alpha_2+\alpha_3}}{1+e^{\alpha_0+\alpha_1+\alpha_2+\alpha_3}}-\frac{e^{\alpha_0+\alpha_1+\alpha_2}}{1+e^{\alpha_0+\alpha_1+\alpha_2}}$$

If the observations are assumed iid, then ML estimation is efficient, and application of the delta method allows estimation of the standard error of the estimate of the policy impact. If observations are not assumed to be iid, then pseudo maximum likelihood estimation can be implemented, and subsequent application of the delta method allows estimation of the standard error of the estimate of the policy impact.

With discrete covariates all the above follows through with:

$$l_1(F,X)l_2(T,X)=\sum_{x\in X}1[X=x][(\beta_{11x}+\beta_{12x}F)(\beta_{21x}+\beta_{22x}T)]$$

However, for empirical tractability additional assumptions are imposed in the empirical section; it is assumed that the covariates enter multiplicatively into $l_1(F,X)$ and $l_2(T,X)$. In other words, it is assumed that

$$\frac{E[Y^0(T)|F,X]}{1-E[Y^0(T)|F,X]}\equiv l(F,T,X)=l_1(F)l_2(T)l_3(X)$$

and furthermore, the function $l_3(X)$ is assumed log-linear. Therefore, note that the conditional means of non-treated outcomes can be modelled:

$$E[Y^0(T)|F,X]=\frac{e^{\alpha_0+\alpha_1F+\alpha_2T+\beta X}}{1+e^{\alpha_0+\alpha_1F+\alpha_2T+\beta X}}$$

and the policy impact can be estimated from

$$E[Y^1(1)|F=1,X]-E[Y^0(1)|F=1,X]=E[Y|F=1,T=1,X]-\frac{e^{\alpha_0+\alpha_1+\alpha_2+\beta X}}{1+e^{\alpha_0+\alpha_1+\alpha_2+\beta X}}$$

If it is also assumed that covariates enter multiplicatively into the odds ratio for treated outcomes for females in period T=1 in the same way as they enter into the non-treated odds ratios, then

$$\frac{E[Y^1(1)|F=1,X]}{1-E[Y^1(1)|F=1,X]} = l_4 l_3(X)$$

Therefore:

$$\frac{E[Y^1(1)|F=1,X]}{1-E[Y^1(1)|F=1,X]} = e^{\alpha_3\prime+\beta X}$$

Where $\alpha_3\prime = ln(l_4)$, and the four observed conditional mean outcomes can be modelled as:

$$E[Y|F,T,X] = \frac{e^{\alpha_0+\alpha_1 F+\alpha_2 T+\alpha_3 FT+\beta X}}{1+e^{\alpha_0+\alpha_1 F+\alpha_2 T+\alpha_3 FT+\beta X}}$$

where $\alpha_3 = \alpha_3\prime - \alpha_0 - \alpha_1 - \alpha_2$.

And the impact of the policy can be estimated from

$$E[Y^1(1)|F=1,X]-E[Y^0(1)|F=1,X] = \frac{e^{\alpha_0+\alpha_1+\alpha_2+\alpha_3+\beta X}}{1+e^{\alpha_0+\alpha_1+\alpha_2+\alpha_3+\beta X}} - \frac{e^{\alpha_0+\alpha_1+\alpha_2+\beta X}}{1+e^{\alpha_0+\alpha_1+\alpha_2+\beta X}}$$

What if there are substitution effects on the control group?

In this case, and maintaining the other assumptions, the impact of the policy on the relative odds ratio can be estimated. There is by assumption,

$$\frac{\frac{E[Y^0(1)|F=1,X]}{1-E[Y^0(1)|F=1,X]}}{\frac{E[Y^0(1)|F=0,X]}{1-E[Y^0(1)|F=0,X]}} = \frac{\frac{E[Y^0(0)|F=1,X]}{1-E[Y^0(0)|F=1,X]}}{\frac{E[Y^0(0)|F=0,X]}{1-E[Y^0(0)|F=0,X]}}$$

Also, as shown in the above (with the multiplicative and log linearity assumptions on how X enters the non-treated odds ratios)

$$\frac{E[Y^0(T)|F,X]}{1-E[Y^0(T)|F,X]} = e^{\alpha_0+\alpha_1 F+\alpha_2 T+\beta X}$$

Therefore,

$$\frac{\frac{E[Y^0(1)|F=1,X]}{1-E[Y^0(1)|F=1,X]}}{\frac{E[Y^0(1)|F=0,X]}{1-E[Y^0(1)|F=0,X]}} = \frac{e^{\alpha_0+\alpha_1+\alpha_2+\beta X}}{e^{\alpha_0+\alpha_2+\beta X}} = e^{\alpha_1}$$

And the impact of the policy on the relative odds ratio in time period T=1 can be estimated from

$$\frac{\frac{E[Y^1(1)|F=1,X]}{1-E[Y^1(1)|F=1,X]}}{\frac{E[Y^1(1)|F=0,X]}{1-E[Y^1(1)|F=0,X]}} - e^{\alpha_1}$$

If it is also assumed that covariates enter multiplicatively into the odds ratio for treated outcomes for males and females in period T=1 in the same way as they enter into the non-treated odds ratios, so

$$\frac{E[Y^1(1)|F,X]}{1-E[Y^1(1)|F,X]} = l_4(F)l_3(X)$$

then:

$$\frac{E[Y^1(1)|F,X]}{1-E[Y^1(1)|F,X]} = e^{\alpha_2\prime\prime+\alpha_3\prime\prime F+\beta X}$$

Where $\alpha_2\prime\prime = ln(l_4(F=0))$, $\alpha_3\prime\prime = ln(l_4(F=1)) - ln(l_4(F=0))$, and the four observed conditional mean outcomes can be modelled as:

$$E[Y|F,T,X] = \frac{e^{\alpha_0+\alpha_1 F+\alpha_2 T+\alpha_3 FT+\beta X}}{1+e^{\alpha_0+\alpha_1 F+\alpha_2 T+\alpha_3 FT+\beta X}}$$

where $\alpha_2 = \alpha_2\prime\prime - \alpha_0$, $\alpha_3 = \alpha_3\prime\prime - \alpha_1$ and the impact of the policy on the relative odds ratio in time period T=1 can be estimated from

$$\frac{\frac{E[Y^1(1)|F=1,X]}{1-E[Y^1(1)|F=1,X]}}{\frac{E[Y^1(1)|F=0,X]}{1-E[Y^1(1)|F=0,X]}} - \frac{\frac{E[Y^0(1)|F=1,X]}{1-E[Y^0(1)|F=1,X]}}{\frac{E[Y^0(1)|F=0,X]}{1-E[Y^0(1)|F=0,X]}} =$$
$$\frac{e^{\alpha_0+\alpha_1+\alpha_2+\alpha_3+\beta X}}{e^{\alpha_0+\alpha_2+\beta X}} - \frac{e^{\alpha_0+\alpha_1+\beta X}}{e^{\alpha_0+\beta X}} =$$
$$e^{\alpha_1}(e^{\alpha_3}-1)$$

Beyond Relative Effects

In the standard difference in differences model (for continuous outcomes), a positive relative treatment effect in the presence of substitution effects can be found when the treatment and control groups are both positively impacted by the policy change, both negatively impacted by the policy change or when the treatment group is positively effected and the control group negatively impacted. Without further assumptions nothing more can be inferred about the direction or magnitude of the treatment effect for either group. However, if the theory suggests that the treatment group are positively impacted by the policy change, then the impact of the policy effect can be bounded from below for both the treatment and the control group. The treatment group has a lower bound simply of zero, and the control group has a lower bound of $-\beta$, where β is the estimate from the interaction term in the standard difference in differences model; $\beta = E[Y_1 - Y_0|G = T] - E[Y_1 - Y_0|G = C]$ where G=T for the treatment group and G=C for the control group.

If the theory suggests that the control group are negatively impacted by the policy change, then the impact of the policy effect can be bounded from above for both the treatment and the control groups. The control group has an upper bound of zero, and the treatment group has an upper bound of β.

If both assumptions hold (the treatment group are positively impacted and the control group negatively impacted), then the treatment effect for the treated group lies in the interval [0, β], and the treatment effect for the control group lies in the interval [$-\beta$,0].[11]

The same intuition holds in the binary difference in differences model. An increase in the relative odds ratio due to a policy change in the presence of substitution effects could be found when there are positive treatment effects for both the treatment and control group, negative treatment effects for both the treatment and control group, or when there are positive treatment effects for the treatment group and negative treatment effects for the control group. Note that an increase in the relative odds ratio corresponds to the case where $\alpha_3 > 0$. Under the same assumptions (the treatment group (females) are positively impacted and the control group (males) are negatively impacted), then the treatment effect for females lies in the interval [0, $\frac{e^{\alpha_0+\alpha_1+\alpha_2+\alpha_3+\beta X}}{1+e^{\alpha_0+\alpha_1+\alpha_2+\alpha_3+\beta X}} - \frac{e^{\alpha_0+\alpha_1+\alpha_2+\beta X}}{1+e^{\alpha_0+\alpha_1+\alpha_2+\beta X}}$] and the treatment effect for males lies in the interval [$\frac{e^{\alpha_0+\alpha_2+\beta X}}{1+e^{\alpha_0+\alpha_2+\beta X}} - \frac{e^{\alpha_0+\alpha_2+\alpha_3+\beta X}}{1+e^{\alpha_0+\alpha_2+\alpha_3+\beta X}}$, 0].[12]

[11]If a negative relative treatment effect was estimated ($\beta < 0$), and it is assumed that the treatment group are negatively impacted and the control group positively impacted, then the treatment effect for the treatment group lies in the interval [β,0], and the treatment effect for the control group lies in the interval [0,$-\beta$].

[12]If there is an estimated decrease in the relative odds ratio, and the impact on the treated group (females) is assumed negative and the impact on the control group (males) assumed positive, then the treatment effect for females lies in the interval [$\frac{e^{\alpha_0+\alpha_1+\alpha_2+\alpha_3+\beta X}}{1+e^{\alpha_0+\alpha_1+\alpha_2+\alpha_3+\beta X}} - \frac{e^{\alpha_0+\alpha_1+\alpha_2+\beta X}}{1+e^{\alpha_0+\alpha_1+\alpha_2+\beta X}}$,0] and the treatment effect for males lies in the interval [0, $\frac{e^{\alpha_0+\alpha_2+\beta X}}{1+e^{\alpha_0+\alpha_2+\beta X}} -$

This analysis suggests a metric that can be used to compare the model based on the assumption of constant relative odds ratios to the widely used linear probability model. Under the assumption that males and females are impacted in opposite directions by the policy change, then treatment effect bounds for both models can be estimated and compared.

4 Legislative Environment

Some form of legislated maternity benefits have existed in the United Kingdom since the introduction of the *National Insurance Act* (1911). There have been many changes to the legislation and provisions since then. The period from 1996 - 2006 is used as a placebo period in the empirical analysis, with the policy change under analysis being implemented in 2007. This section discusses legislative changes over this period.

There were two major policy changes impacting maternity benefits in the time period over which the placebo analysis is carried out (1996-2006). Prior to this period, all female employees had the right to 14 weeks of maternity leave (which was not necessarily paid).[13] To receive statutory maternity pay (SMP) (paid for up to 18 weeks) from your employer you typically had to be continuously employed for 26 weeks before the expected week of childbirth. To receive state maternity allowance (also paid for up to 18 weeks) you typically had to have made at least 26 national insurance contributions in the previous year. The first 6 weeks of statutory maternity pay were paid at 90% of average weekly earnings, with the remaining 12 weeks being paid at a flat rate set by the government each year (which was £54.55 in 1996, increasing incrementally to £108.85 in 2006). The state maternity allowance was paid at the flat SMP rate. 92% of SMP was reclaimable by the employer through a rebate from the government. However small employers received a rebate of 104% of the SMP paid.[14] If an employee had been continuously employed for 2 years they typically qualified

$\frac{e^{\alpha_0+\alpha_2+\alpha_3+\beta X}}{1+e^{\alpha_0+\alpha_2+\alpha_3+\beta X}}]$.

[13] Two weeks of which were compulsory immediately after birth

[14] This percentage was changed slightly over time. Small employers were defined as those whose annual national insurance contributions were less than £20,000 - this figure was increased to £45,000 in 2004

for an additional leave period, which ended 29 weeks after birth.

The first major policy change in the placebo period occurred in 1999, and applied to parents of children expected in the week beginning the 30th April 2000. This legislative change affected the leave rights of both the mother and the father, therefore the predicted impact on relative labour market effects would depend on the anticipated impact of the policy changes on relative male-female leave taking. The amount of maternity leave all female employees were entitled to was increased from 14 to 18 weeks. The tenure qualifying condition for the additional leave period was decreased from 2 years of continuous employment to 1 year of continuous employment. Unpaid parental leave of up to 13 weeks was also introduced, with a maximum of 4 weeks in any one year. Additionally, in 1999 an exemption was introduced for small employees which stated an employee being dismissed for any reason connected with her pregnancy or maternity leave was not considered to have been unfairly dismissed if her employer had fewer than five employees.

The second major policy change in the policy period occurred in 2002, and applied to parents of children expected in the week beginning the 6th April 2003. As with the previous change, this affected the leave rights of both the mother and the father. The amount of maternity leave all female employees were entitled to was increased from 18 to 26 weeks. The additional leave period was changed from being up until the 29th week after birth to being the 26 week period continuing on from the first 26 week leave period. The tenure qualifying condition for this leave period was changed from 1 year of continuous employment to 26 weeks of continuous employment. In 2002, paid paternity leave of 2 weeks was also introduced, which had a tenure qualifying condition of 26 weeks of continuous employment.[15]

The key policy change of interest occurred in 2006. This legislative change was passed into law on the 1st October 2006, with women who qualified for SMP/the state maternity allowance, and whose expected week of childbirth fell on or after the 1st April 2007, being eligible for an additional 13 weeks of paid maternity leave/maternity allowance. This gave a maximum statutory paid maternity leave duration of 39 weeks compared to 26 weeks previously. In addition, from the

[15]The rate of statutory paternity pay was equivalent to that of female employee on the state maternity allowance, and had the same earnings qualifying rule

1st April 2007 the tenure required to qualify for the additional maternity leave period was abolished, implying that all employed women were entitled to a job-protected leave period of up to 52 weeks. At this juncture, the small employer exemption introduced in 1999 was also removed, which meant that all employees had the right to return to the same or similar job regardless of the size of her employer's firm. In contrast to the other two policy changes in the placebo period, this was the only policy change that affected only mother's leave rights.

The intention to increase paid maternity leave duration was published in the Labour Party's 2005 election manifesto on 13th April 2005 (The Labour Party, 2005), where they stated their intention to increase paid maternity leave from 26 to 39 weeks. The election took place on 5th May 2005, resulting in a Labour majority. In the analysis it is assumed that employers react to the actual legislative change that took place on 1st October 2006 rather than proposed legislative change. If this assumption is invalid, and in fact employers pre-empt the legislative change then it is possible that the estimation approach results in a downwards biased estimate of the impact of the policy change.

5 Data and Empirical Specification

The analysis is this paper is based on data from the UK Labour Force Survey (LFS).[16] The analysis uses a quasi-experimental difference in differences approach, comparing changes in female and male labour market outcomes during a period in which an expansion in maternity leave duration occurred.

Most of the analysis uses LFS data from quarters 2 and 3 (April - September) in 2006 and 2007. The legislative change occurred in October 2006, but only started affecting women whose expected week of childbirth began on or after the 1st April 2007. Therefore, employers looking to avoid the additional costs associated with women taking longer periods of maternity leave did not have to react immediately. Therefore, the six month period running from the 1st April 2007 until the 30th September 2007 is taken as the after period, and the

[16] Office for National Statistics. Social Survey Division and Northern Ireland Statistics and Research Agency. Central Survey Unit, Quarterly Labour Force Survey. Colchester, Essex: UK Data Archive [distributor]

corresponding period in 2006 as the before period. Using only this 6 month window is beneficial as it avoids the potentially confounding effect of higher numbers of retained job matches (amongst women who wanted to take more than 6 months of maternity leave), and also the effect of greater human capital depreciation among women coming back into the labour market after taking longer periods of maternity leave. Restricting the analysis to this time period also avoids picking up the potential impact of the recession on relative female labour market outcomes.

The analysis focuses on relative male-female outcomes aged between 25-34. This age category is chosen as the majority of births (over 50%) were to mothers in this age range in 2007.[17] Estimates for those aged 16-24, 35-44 and over 45 are also presented as a comparison.

The key outcomes considered are hourly wages, employment conditional on participation, redundancy and hiring (new starts and job changers combined). Hourly wages of employed individuals are considered (excluding self-employed). The hourly wage outcome is measured using the hourpay variable in the LFS dataset for the most part, which is constructed using gross reported last earnings, the period of time that payment covered and paid hours of work (including paid overtime). From 1999 individuals in the LFS were asked whether their gross reported last earnings was the same as that received each similar period. For those that reported no, this analysis replaces the hourpay variable with the hourly wage corresponding to the gross reported typical earnings. Since there may have been a fertility response to the legislative change, and while on maternity leave employees often receive some proportion of their typical earnings, this approach avoids picking up this confounding effect in the comparison of male and female earnings. All earnings are adjusted for inflation using the ONS annual RPI figures.

Individuals are counted as in employment in accordance with the LFS definitions of employment. Therefore, employees, self-employed, those in government employment or training programmes and unpaid family workers are treated as being employed. Those in the labour force (seeking and available for work) are treated as unemployed.

[17] Office for National Statistics Birth Summary Tables; England and Wales 2013

Experience of redundancy in the previous three months is analysed. This time period is analysed because employees are only asked if and why they left a paid job if they started a new job in the previous 3 months (unemployed individuals are asked if they have become unemployed in the previous 8 years). Individuals are treated as having experienced redundancy if they stated that the reason they left their last job was due to being dismissed, or they were made redundant or took voluntary redundancy (voluntary redundancy was unfortunately not asked as a separate category).

Experience of starting a new job (either from unemployment or job change) in the previous three months is also analysed. Unemployed individuals are assumed not to have started a job within the previous three months.

The impact of the policy change on continuous variables is analysed using the following difference in differences equation

$$Y_i(T) = a_1 + a_2 F_i + a_3 X_i + a_4 T_i + \beta F_i T_i + \varepsilon_{it}$$

where β is the estimate of the differential impact of the policy change on females compared to males (as discussed in the methodology section). There is an overlapping panel structure in the LFS, whereby participants recruited into the LFS are surveyed over 5 quarters. Therefore, some individuals may appear in the data set twice. It is not possible to identify these individuals in the standard LFS data files. This introduces potential serial correlation. To account for this, cluster-robust standard errors are reported, with clustering on region-industry level. Under the assumption that individuals observed twice remain in the same region-industry group, this method will account for serial correlation. This approach also allows for group errors at the region-industry level.

The impact of the policy change for binary outcome variables is analysed using a logit model

$$E[Y|F,T,X] = \frac{e^{\alpha_0+\alpha_1 F+\alpha_2 T+\alpha_3 FT+\beta X}}{1+e^{\alpha_0+\alpha_1 F+\alpha_2 T+\alpha_3 FT+\beta X}}$$

where the impact of the policy on the relative odds ratio is estimated from $e^{\alpha_1}(e^{\alpha_3}-1)$ (as discussed in the methodology section). To allow for repeated observations and possible error correlation, pseudo maximum likelihood was used to estimate the above model.

The set of control variables common to all models are; age finished continuous full time education, government office region and an indicator for whether the individual was born in the UK. For the wage analysis, industry-region fixed effects are included, as well as an indicator for part time/full time status and an indicator for public or private sector. The redundancy analysis controls for the industry in which the individual was made redundant from (or if they were not made redundant, their current or last industry). The hiring analysis controls for the industry the individual works in or last worked in. Furthermore, 1% of wages are trimmed above and below to avoid undue influence of outliers. The analysis does not include individuals who report they left full time education at an age younger than 12 or older than 30.[18] See Table I for summary statistics of the outcome and control variables.

6 Results

6.1 First Stage

The first set of results look at whether the policy change impacted the amount of leave taken by females relative to males of the same age. A difference in differences model is used to analyse this, with the after period being April - September 2008 and the before period being April - September 2006. It is assumed that the rate of change in the female and male odds ratio of being on leave between 2006 and 2008 would have been the same in the absence of a policy change. Note that the after period is different from that used in the main outcome analysis. The LFS has information on whether the youngest child in your family unit is under 1, but the age is not more specifically determinable. Therefore, whereas all women from April 2008 with a child under the age of 1 qualified for the more generous leave allowance, this was not the case for all women with a child under the age of 1 in the previous year. Some of these women would have had their child after April 2007 and qualified, and some before.

[18]This only results in 25 people aged 25-34 being dropped from the wage analysis, and does not affect the results.

In the first instance, the impact of the policy change on being on leave from paid work is estimated. Individuals who had a paid job in the reference week are considered. Individuals who had a paid job in the reference week, which they were away from temporarily, were said to be on leave.[19] Column 2 of Table II shows a significant increase in the relative odds ratio of a female employee being on leave compared to a male employee over the period 2006-2008 for the age group 25-34.

The impact of the policy change on parental leave of females relative to males is also decomposed into an increase due to fertility response and an increase due to changing lengths of maternity/paternity leave taken. This decomposition uses a linear probability model approach. The average length of time out of the labour market for a female employee due to maternity leave is given by:

$$E[ML] = \lambda_F \gamma_{FL}$$

where γ_{FL} denotes the fertility rate of a female employee and λ_F represents the average leave taken by a female worker who gives birth that year. The average length of time out of the labour market for a male employee due to paternity leave is given by:

$$E[PL] = \lambda_M \gamma_{ML}$$

where γ_{ML} denotes the probability a male employee has a child, and λ_M represents the average leave taken by a male worker who has a child that year. Therefore note that the gap in the average length of time out of the labour market for a female employee compared to a male employee due to parental leave is given by:

$$E[ML - PL] = \lambda_F \gamma_{FL} - \lambda_M \gamma_{ML}$$

Therefore,

$$\frac{dE[ML - PL]}{d\theta} = \gamma_{FL}\frac{d\lambda_F}{d\theta} + \lambda_F \frac{d\gamma_{FL}}{d\theta} - \gamma_{ML}\frac{d\lambda_M}{d\theta} - \lambda_M \frac{d\gamma_{ML}}{d\theta}$$

[19]They are many leave reasons besides parental leave, however, if patterns of uptake of non-parental leave did not change for females relative to males across the comparison period then comparing aggregate leave before and after should give an estimate of the relative impact of the policy change on parental leave uptake of females relative to males.

The divergence in female-male leave can therefore be decomposed into a component due to the policy impact on; maternity leave duration, fertility among working women, paternity leave duration and paternity among working men.

The results from this analysis are shown in Table III. γ_{FL} (γ_{ML}) is estimated from the proportion of females (males) who have a paid job (which they may have been away from), who have a child under the age of 1 in 2006. λ_F (λ_M) is estimated from the proportion of females (males) who both had a paid job and a child under the age of 1 in 2006, who were on leave. $\frac{d\gamma_{FL}}{d\theta}$ ($\frac{d\gamma_{ML}}{d\theta}$) is estimated from a single difference linear probability model of females (males), estimating the increase in the fertility rate of females who have a paid job (paternity rate of males) from 2006 to 2008, controlling for age finished full time continuous education, region of residence and indicator for whether UK born. Similarly, $\frac{d\lambda_F}{d\theta}$ ($\frac{d\lambda_M}{d\theta}$) is estimated from a single difference linear probability model, estimating the increase in the leave rate of female(male) workers with a child under the age of 1. Since the sample of individuals used to estimate fertility rates and leave rates in 2006 are also used in the regression analysis to estimate the changes in fertility rates and leave rates, the female/male set of parameters are estimated using a GMM model (a separate model is used for males/females, and the male and female samples are assumed to be independent). Results are reported only for the age categories 25-34 and 35-44. This is because the GMM estimation did not converge for the age category 16-24 due to the low probability of men in this age category having children. Similarly, the procedure did not converge for the age category 45-64 due to the low probability of women in this age category having children.

As shown in Table III, the maternity-paternity leave gap was estimated to increase by 1.00% for the 25-34 age group and by 0.43% for the 35-44 age group. Approximately half of the divergence in leave taking is explained by the increase in the number of females having children and half explained by the increase in the duration of maternity leave taken.

6.2 Impact on Wages

The key testable model prediction is that an increase in maternity leave uptake will lead to an increase in the male-female wage gap. Table IV shows the results from the difference in differences analysis, which compares female and male wage growth over the period 2006-2007. The empirical results suggest there was a large and significant negative impact on the wages of females relative to males in the age group 25-34. This is in line with the model implications. The model estimated that the male-female wage gap for those aged 25-34 increased by £0.29.[20] There was a smaller negative impact estimated for the relative wages of females compared to males for the 16-24 age group (this age group also experienced a large divergence in male-female leave uptake after the policy change), however the impact was not estimated to be significant.

Results from the placebo analysis are presented in Appendix B Figure 1 for the age group 25-34. This placebo analysis estimates the same difference in differences model using previous comparison years (so the 2006 figure represents the 2005-2006 comparison, 2005 represents the 2004-2005 comparison, etc.). There are a couple of observations to make. Firstly, the estimated impact for the relevant year (2007 versus 2006) was found to be the largest negative impact across the 11 estimates. Secondly, across the 11 year period there is only one other year for which a statistically significant estimate was found; 2005. However, this significant estimate went in the opposite direction to the estimated policy effect. Therefore, to the extent that the significant value estimate for 2005 suggests the common trends assumption may be invalid, it in fact suggests that the male-female wage gap is converging. In light of this, the diverging estimate found for 2007 is even stronger evidence in favour of a negative impact of the policy on relative female wages.

The large convergence in the male-female wage gap estimated between 2004-2005 might be due to a number of factors. The then Prime Minister Tony Blair established a Women and Work Commission which focused on narrowing the gender pay gap in July 2004.[21] One proposal that received some media coverage at the time was the possible implementation of equal pay reviews, which may have impacted relative male-female wages.[22] There were also a number of high profile equal pay cases, for instance the North Cumbria Acute NHS Trust v Uni-

[20] This corresponds to 2.4% of average male wages, or 2.7% of average female wages

[21] BBC News (2004)

[22] The Sunday Times (2005)

son Trade Union case[23] and the Home Office v Bailey case[24]. Additionally, the Equal Opportunities Commission (subsumed by the Equality and Human Rights Commission in 2007) launched a pregnancy discrimination campaign in January 2005, which they estimated was heard or seen by 50% of the population at least twice (Equal Opportunities Commission, 2005). Finally, the *Sex Discrimination Act* (1975) was amended in 2005 by the *Employment Equality (Sex Discrimination) Regulations* (2005), which explicitly stated that differential treatment due to pregnancy or maternity leave amounted to sex discrimination (although this had previously been established by case law).

More evidence supporting the common trends assumption is shown in Appendix B Figure 2. This trend analysis shows the time trend of male and female conditional wages in the age group 25-34 over the period 1996-2006. It also shows the predicted mean male-female wage gap over this time period. The results suggest that the gap may have been converging during this period. Again, as with the placebo analysis, evidence of the diverging impact of the policy change on the male-female wage gap is made stronger in view of this converging trend.[25]

There are a number of channels through which selection on unobservables could affect the results. Heterogeneous fertility responses, participation choices and experience of redundancy and hiring could imply that the group of workers in 2007 are not comparable to those observed in 2006. Intuitively, it might be assumed that positive fertility responses are most likely amongst those with weak labour market attachment and lower wages (this mechanism would work to increase observed average female wages). Similarly, redundancies might be more common among the group of females with the lowest match surplus, which might also be expected to be the lowest wage workers (this mechanism would also work to increase observed average female wages). By the same argument, newly hired female workers may have to generate a higher surplus in order to be hired which might be more likely among higher productivity workers (this mechanism would also work to increase observed average female wages).

[23] The Guardian (2005)

[24] The Times (2005)

[25] The treatment effect for the sample without any dependent children was found to be of a similar magnitude (the male-female wage gap for those without children aged 25-34 was estimated to increase by £0.23), although is no longer statistically significant (due to a halving of the sample size).

Working in the opposite direction, the largest labour market participation effects might be expected among the group of low potential wage earners. Therefore, it is not clear a priori in which direction the selection effect would work. However, there is little evidence of any positive participation effects in response to the policy change. A small negative impact of the policy change on relative participation effects was estimated, implying that the odds of female participation actually decreased relative to males. This suggests that negative selection effects may be ruled out. To the extent that there are remaining selection effects through the fertility/redundancy/hiring mechanisms, they are expected to increase observed female wages, which would in fact narrow the male-female wage gap. Therefore the evidence of the increasing male-female wage gap still stands as evidence of deteriorating female labour market outcomes as a result of the policy.

Further evidence in favour of this argument comes from comparing predicted wages (from the labour market in 2006) of the sample of employed individuals observed before and after the policy change. To the extent that there is any selection on unobservables you might reasonably expect the selection on observables to work in the same direction. The predicted mean hourly wage for the sample of females observed in the pre-period was £10.48 compared to £10.53 for the sample of females observed in the post-period. This suggests that the sample of women working after the policy change were if anything, positively selected relative to the sample working before the policy change.

Heterogeneity by age group has already been analysed, with the age category with the highest fertility rates experiencing the greatest decline in relative female wages. Heterogeneity in other dimensions was also considered, in particular, heterogeneity by education and by size of employer. No evidence of any significant heterogeneity in policy treatment effect was found. See Appendix C for these results and a discussion.

6.3 Impact on Employment

Although there are no model predictions relating to relative male/female employment rates, a common trends assumption can still allow for estimation of the impact of the policy change on relative male/female employment outcomes.

Table V shows that although negative, the impact on the relative odds ratio of female-male employment was not found to be statistically significant. The placebo analysis (shown in Appendix B Figure 3 also suggests that 2007 was nothing out of the ordinary in the evolution of male-female relative odds ratio of employment (in the age category 25-34). The trend analysis (shown in Appendix B Figure 4) also corroborates this; the analysis of the male-female relative odds ratio of conditional employment suggests that the relative odds ratio was decreasing over time, and so the small (insignificant) decrease in relative conditional employment rates in 2007 seems to be in line with other years.

As noted in Curtis et al. (2014), a change in policy that impacts the labour market should be observed more quickly on labour market flows (redundancies and hires) than on aggregate levels. While it may take time to adjust to a new aggregate equilibrium, short term effects may be more quickly observable in flow data. Therefore, how relative female-male redundancy and hiring rates changed between 2006 and 2007 are analysed.

Table VI suggests that the odds ratio of female redundancy increased significantly relative to males over the period in question for the age group 25-34. The placebo analysis shown in Appendix B Figure 5 shows that the positive impact on the female-male relative odds ratio was the largest absolute impact estimated over the 11 year period. Furthermore, the trend analysis shown in the Appendix suggests that the discrete model version of the common trends assumption is not rejected (constant relative odds ratio). These results corroborate the finding that female redundancies increased relative to males as a result of the longer paid maternity leave duration.

If it is assumed that female redundancies increased as a result of the policy, and male redundancies decreased, then as discussed in section 3.1, the policy treatment effect on the outcome can be bounded. As shown in Table VI, the bound for the impact of the policy change on female redundancies is [0, 0.002**], suggesting that the impact of the policy change on the female redundancy rate was between 0 and 0.2% from a base rate of 0.44%. The linear probability model estimates a range of [0, 0.004**]. The discrete model results in a bound that is half the width of the linear probability model in this case. When the probabilities are close to one of the probability limits, the predictions of the linear probability model and the model discussed in section 3.1 can diverge significantly.

This is due to the convexity of the assumptions in the discrete outcome model. Consider a numerical example where the possibility of substitution effects are ignored for ease of discussion. Suppose observed male redundancy rates are 1% to begin with and female rates are 0.5%. Furthermore, suppose observed male redundancy rates fall to 0.5%. Then the difference in differences linear probability model predicts the counterfactual female redundancy rate would be 0%. While the male redundancy rate decreased by 50%, the female redundancy rate is predicted to decrease by 100%! This can have a large impact on estimated treatment effects. If the observed female redundancy rate in the after period is 0.5%, then the linear probability model predicts a treatment effect of 0.5%. In comparison in this example the discrete model predicts a treatment effect of 0.25% (approximately half the magnitude of the linear probability model).

Finally, Table VII suggests that the policy change had little impact on the odds ratio of female hiring relative to males. The estimate is slightly negative, but is not statistically significant. This outcome variable measures both hiring from unemployment and job switches. The placebo analysis presented in the Appendix also suggests that 2007 was nothing out of the ordinary in the evolution of male-female relative odds ratio of hiring (in the age category 25-34). The trend analysis also corroborates this. One explanation for this finding in the context of the other results, which suggest female labour market outcomes deteriorated, is that the employees hired to cover the higher number of female employees taking longer or more frequent maternity leave tend to be female workers (due to occupational sorting), thus hiding any potential negative impact of the policy change on relative female-male hiring rates.

7 Conclusion

The amount of maternity leave available to women in the UK has increased almost four-fold from 14 weeks to 52 weeks over the past two decades. Longer maternity leave durations may impose higher employer costs. In the face of a persistent gender pay gap, this paper asks whether more generous leave packages contribute to worse female labour market outcomes through employer discrimination.

In comparison with the previous literature, this paper finds evidence of larger negative effects on relative female labour market wages. There are a number of possible explanations for this. Firstly, the quasi-experimental literature reports an aggregated effect of employer discrimination (negative effect on female wages), the increased number of retained job matches (positive effect on female wages) and the impact of longer leave periods on human capital depreciation (negative effect on female wages). This research focuses on the role of discriminatory behaviour by firms. It may be that the increased number of retained job matches has a large positive effect on wages. Secondly, the policy changes in the US brought about a much smaller first stage effect than the policy change in the UK. In fact, there is evidence that the Family and Medical Leave Act did not bring about any differential leave uptake by females relative to males (The Commission on Leave, 1995; Waldfogel, 1999). Thirdly, the cross country analysis may suffer from endogeneity - with countries introducing more generous maternity policies at the same time as related policies, or at times when the male-female wage gap is naturally narrowing. Furthermore, cross-country analysis that analyses the impact on skilled wages (rather than unskilled wages) finds much larger negative impacts. This is because unskilled wages tend to be close to legal minimum wages. These wages can not adjust in response to these types of policy changes (Akgunduz and Plantenga, 2012).

This paper found evidence that longer maternity leave is associated with worse female labour market outcomes, with an increase in the duration of paid maternity leave resulting in firms paying females less and firing them more frequently than comparable males. When firms treat females differently to males due to expected leave taking behaviour is this discrimination? The point has been made in the economics literature that there is no group discrimination in wages in the theoretical model, since women who participate the most will be underpaid relative to their productivity, and women who participate the least will be overpaid relative to their productivity. On average, females are paid a rate equal to their average productivity, since under- and overpayments cancel out (Cain, 1986). However, using common definitions of labour market discrimination every female experiences labour market discrimination if employers base hiring or wage decisions on this type of statistical information. Heckman and Siegelman (1993) define labour market discrimination as: "it occurs if persons in one groups with the same relevant productivity characteristics as persons in another group are treated unfavourably by the labor market solely as a consequence of their demographic status". Similarly, Altonji and Blank (1999) define labour market discrimination as "a situation in which persons who provide labor market services and who are equally productive in a physical or material sense are treated unequally in a way that is related to an observable characteristic such as race, ethnicity, or gender. By "unequal" we mean these persons receive different wages or face different demands for their services at a given wage." Either definition would lead us to conclude that women experience labour market discrimination. To clarify this point, consider a distribution of propensities to be out of the labour market on parental leave for male and female employees who have the same levels of all other productivity characteristics. Male and female employees have different distributions of the propensity to be out of the labour market, with the female distribution much more skewed to the right relative to the male distribution. However, for any given propensity to be out of the labour market, females will be paid less than a comparable male. This is because propensity to be out of the labour market is not observed by the employer, and hiring/wage decisions are based on gender means.

This analysis finds that relative female labour market outcomes deteriorate in response to longer maternity leave durations. Possible government actions to mitigate this include encouraging higher levels of male parental leave taking and stronger enforcement of current anti-discrimination legislation, for instance through mandatory equal pay reviews or equal pay audits.

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Table I: Descriptives - Aged 25-34

	Before		After*	
	Male	Female	Male	Female
Child under 1	8.68%	10.43%	9.93%	11.51%
On leave	6.14%	14.50%	5.69%	15.25%
On leave - child under 1	5.68%	62.29%	6.88%	68.02%
Participation	93.57%	76.51%	93.91%	76.08%
Employed	95.04%	95.28%	95.74%	95.42%
Hourlywage	11.27	10.17	11.92	10.64
Redundancy	0.84%	0.44%	0.60%	0.57%
Hired	6.47%	6.67%	7.14%	7.05%
Age	29.75	29.78	29.79	29.71
Age left full time education	18.61	18.61	18.64	18.72
UK born	84.28%	84.63%	82.25%	82.64%
Public sector	15.21%	32.66%	14.53%	32.33%
Permanent	95.39%	94.57%	95.70%	94.13%
Region				
North East	4.17%	4.49%	4.47%	4.17%
North West	8.44%	8.79%	9.19%	8.70%
Merseyside	1.89%	1.87%	1.64%	2.04%
Yorkshire and Humberside	8.97%	9.11%	8.93%	9.01%
East Midlands	7.09%	7.32%	6.83%	7.13%
West Midlands	8.64%	8.78%	7.46%	8.04%
Eastern	9.31%	8.31%	9.90%	9.48%
London	12.86%	12.88%	12.91%	13.02%
South East	13.00%	13.03%	12.87%	13.67%
South West	7.71%	8.17%	8.02%	7.77%
Wales	4.68%	4.79%	4.36%	5.03%
Scotland	8.53%	8.02%	8.19%	7.75%
Northern Ireland	4.72%	4.44%	5.23%	4.20%
N	11,019	12,982	10,806	12,715

*Before period: April-Sept 2006, after period: April-Sept 2007 with exception of fertility and leave variables which have April-Sept 2008 as the after period

Table II: Leave 2006-2008 - Logit Model

	(1)	(2)	(3)	(4)
VARIABLES	16-24	25-34	35-44	45-64
female	0.398***	0.942***	0.666***	0.340***
	(0.086)	(0.051)	(0.040)	(0.032)
yearb	-0.069	-0.079	-0.022	-0.046
	(0.097)	(0.063)	(0.047)	(0.034)
femaleyearb	0.106	0.146*	0.026	-0.011
	(0.128)	(0.077)	(0.060)	(0.047)
Constant	-2.613***	-2.998***	-3.493***	-2.958***
	(0.337)	(0.167)	(0.143)	(0.111)
Observations	15,970	35,347	49,060	74,205
Impact on R.O.R.	0.167	0.404*	0.050	-0.015
	(0.203)	(0.214)	(0.119)	(0.066)
Female upper bound 25-34				
Logit		0.018**		
		(0.009)		
LPM comparison		0.013*		
		(0.006)		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

(1) Other controls include age finished education, GOR region and a dummy for whether born in UK
(2) yearb = 0 if observation year is 2006, yearb = 1 if observation year is 2008
(3) Estimation is by quasi-maximum likelihood.
(4) The impact on R.O.R. measures the change in the odds ratio of a female being on leave relative to a male

Table III: Decomposition of change in parental leave of females relative to males 2006-2008

	(1) 25-34	(2) 35-44
Fertility in 2006	0.088***	0.031***
γ_{FL}	(0.003)	(0.002)
Paternity in 2006	0.090***	0.051***
γ_{ML}	(0.003)	(0.002)
Maternity leave in 2006	0.623***	0.624***
λ_F	(0.017)	(0.024)
Paternity leave in 2006	0.057***	0.098***
λ_M	(0.008)	(0.011)
Change in fertility 2006-2008	0.010**	0.003
$\frac{d\gamma_{FL}}{d\theta}$ (LPM)	(0.004)	(0.002)
Change in paternity 2006-2008	0.012***	0.004
$\frac{d\gamma_{ML}}{d\theta}$ (LPM)	(0.004)	(0.003)
Change in maternity leave 2006-2008	0.066***	0.053
$\frac{d\lambda_F}{d\theta}$ (LPM)	(0.024)	(0.034)
Change in paternity leave 2006-2008	0.013	-0.020
$\frac{d\lambda_M}{d\theta}$ (LPM)	(0.012)	(0.015)
Due to Increase in fertility	0.006**	0.002
$\lambda_F \frac{d\gamma_{FL}}{d\theta}$	(0.003)	(0.001)
Due to Increase in maternity leave	0.006***	0.002
$\gamma_{FL} \frac{d\lambda_F}{d\theta}$	(0.002)	(0.001)
Aggregate Female Response	0.012***	0.004**
	(0.003)	(0.002)
Due to Increase in paternity (-ve effect)	0.001**	0.000
$\lambda_M \frac{d\gamma_{ML}}{d\theta}$	(0.000)	(0.000)
Due to Increase in paternity leave (-ve effect)	0.001	-0.001
$\gamma_{ML} \frac{d\lambda_M}{d\theta}$	(0.001)	(0.001)
Aggregate Male Response (-ve effect)	0.002*	-0.001
	(0.001)	(0.001)
Estimated increase in maternity-paternity leave gap	0.010***	0.004**
	(0.003)	(0.002)

(1) Independence between the male and female samples is assumed
(2) Robust standard errors reported
(3) GMM does not converge for age 16-25 men or for age 44-65 women

Table IV: Hourly Wages 2006-2007

VARIABLES	(1) 16-24	(2) 25-34	(3) 35-44	(4) 45-64
female	-0.103	-0.790***	-1.886***	-2.012***
	(0.101)	(0.127)	(0.157)	(0.149)
year	0.067	0.132	-0.192	-0.167
	(0.126)	(0.107)	(0.132)	(0.115)
femaleyear	-0.091	-0.288**	0.243	0.026
	(0.148)	(0.140)	(0.151)	(0.130)
Constant	1.391**	-0.861*	-5.266***	-6.136***
	(0.614)	(0.451)	(0.704)	(0.592)
Observations	4,702	11,213	14,675	20,948
R-squared	0.254	0.285	0.318	0.332

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

(1) Other controls include age finished education, region-industry fixed effects, a dummy for whether born in the UK, a dummy for whether working part time and a dummy for whether working in the public or private sector

(2) year = 0 if observation year is 2006, year = 1 if observation year is 2007

(3) Cluster robust standard errors with clustering at the region-industry level are reported

Table V: Employment Conditional on Participation 2006-2007 - Logit Model

VARIABLES	(1) 16-24	(2) 25-34	(3) 35-44	(4) 45-64
female	0.188***	-0.006	-0.152**	0.415***
	(0.057)	(0.066)	(0.064)	(0.061)
year	0.024	0.169**	0.102	0.008
	(0.053)	(0.067)	(0.068)	(0.053)
femaleyear	0.025	-0.150	0.003	-0.047
	(0.081)	(0.096)	(0.094)	(0.086)
Constant	-0.962***	-0.317	0.312	0.789***
	(0.242)	(0.242)	(0.235)	(0.196)
Observations	20,588	40,063	54,555	80,988
Impact on R.O.R.	0.030	-0.139	0.003	-0.070
	(0.100)	(0.088)	(0.081)	(0.128)
Female lower bound 25-34				
Logit		-0.006		
		(0.004)		
LPM comparison		-0.006		
		(0.004)		

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

(1) Other controls include age finished education, GOR region and a dummy for whether born in UK

(2) year = 0 if observation year is 2006, year = 1 if observation year is 2007

(3) Estimation is by quasi-maximum likelihood.

(4) The impact on R.O.R. measures the change in the odds ratio of a female being employed relative to a male

Table VI: Experienced redundancy in last 3 months 2006-2007 - Logit Model

VARIABLES	(1) 16-24	(2) 25-34	(3) 35-44	(4) 45-64
female	0.074	-0.330*	-0.289	-0.153
	(0.209)	(0.196)	(0.179)	(0.153)
year	0.082	-0.318*	-0.233	-0.071
	(0.181)	(0.171)	(0.151)	(0.126)
femaleyear	-0.163	0.596**	0.308	0.071
	(0.280)	(0.268)	(0.237)	(0.205)
Constant	-0.737	-4.494***	-4.634***	-4.767***
	(1.167)	(0.950)	(0.923)	(0.875)
Observations	18,191	38,764	53,149	79,506
Impact on R.O.R.	-0.162	0.586**	0.270	0.064
	(0.278)	(0.284)	(0.212)	(0.182)
Female upper bound 25-34				
Logit		0.002**		
		(0.001)		
LPM comparison		0.004**		
		(0.002)		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

(1) Other controls include age finished education, GOR region, a dummy for whether born in UK and industry
(2) year = 0 if observation year is 2006, year = 1 if observation year is 2007
(3) Estimation is by quasi-maximum likelihood.
(4) The impact on R.O.R. measures the change in the odds ratio of a female experiencing redundancy relative to a male

Table VII: Whether hired/changed job in last 3 months 2006-2007 - Logit Model

	(1)	(2)	(3)	(4)
VARIABLES	16-24	25-34	25-34	25-34
female	0.068	0.118**	-0.009	0.062
	(0.059)	(0.059)	(0.063)	(0.062)
year	0.073	0.086	0.074	-0.005
	(0.055)	(0.056)	(0.058)	(0.058)
femaleyear	-0.018	-0.041	0.051	0.015
	(0.079)	(0.080)	(0.084)	(0.084)
Constant	-1.801***	-2.074***	-2.252***	-3.587***
	(0.270)	(0.271)	(0.298)	(0.290)
Observations	19,113	39,558	53,911	80,172
Impact on R.O.R.	-0.019	-0.045	0.052	0.017
	(0.084)	(0.088)	(0.086)	(0.090)
Female lower bound 25-34				
Logit		-0.003		
		(0.005)		
LPM comparison		-0.002		
		(0.005)		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

(1) Other controls include age finished education, GOR region, a dummy for whether born in UK and industry
(2) Those hired and fired within 3 months treated as not hired
(3) year = 0 if observation year is 2006, year = 1 if observation year is 2007
(4) Estimation is by quasi-maximum likelihood.
(5) The impact on R.O.R. measures the change in the odds ratio of a female being hired/changing jobs relative to a male