University of Sussex
Business, Management & Economics

Economics Department Working Paper Series

No. 46-2012

A Re-examination of the Impact of the UK National Minimum Wage on Employment

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Abstract: A general consensus has emerged that while the UK National Minimum Wage (NMW) raised the pay of low wage workers it did little to harm their employment prospects. This is in contrast to the US and other countries where a debate over minimum wage effects still rages on. We re-examine the evidence on the introduction of the NMW and look at subsequent increases through the recession focusing on several groups in the labour market. We find a reduction in employment retention among part-time female workers, the group which is most affected by the NMW. These effects deepen in the recession.

JEL Classification: J08, J31, J38

Key Words: Minimum Wage, Employment, Wages, Recession

1. Introduction

Since a National Minimum Wage (NMW) was introduced in the UK in April 1999 much research has been conducted to evaluate its impact on various labour market outcomes. A general consensus has emerged that the NMW raised the wages of those workers directly affected by the legislation (Stewart, 2004), did little or no harm to employment rates among that group (Stewart, 2004), may have reduced hours somewhat among affected workers (Stewart and Swaffield, 2008) and may have reduced profits among firms who were most exposed to the minimum wage (Draca, Machin and Van Reenen, 2011). As such, the National Minimum Wage has largely been received as a successful policy tool and an important part of a strategy, alongside in-work benefits, to make work more attractive (Metcalf, 2008). This conclusion is somewhat in contrast to the US and other countries where a debate over minimum wage effects still rages on (See, for example, Card and Krueger, 1995, Neumark and Wascher, 2008).

When the UK NMW was first introduced it was set at a relatively modest £3.60 an hour. However, after a period of time the body charged with setting the minimum wage, the Low Pay Commission (LPC), implemented a series of larger increases that outstripped both inflation and average wage growth. These increases were implemented on the back of the evidence showing little impact on employment outcomes. As the UK entered recession towards the end of the 2000s the LPC took a much more cautious approach. The increases in the NMW in the past few years have been quite small. Most of the evidence we have on the UK minimum wage comes from a period of strong economic growth. To date we have little evidence on the labour market impacts of the minimum wage during the recession for the UK (Dolton and Bondibene, 2012).

In this paper we both re-examine some of the initial evidence of the impact of the NMW on employment and examine more recent impacts of the NMW through the recession. We utilize the approach of Linnerman (1982) that was adopted by Stewart (2004) in comparing individual outcomes among workers directly affected by the NMW, with some group of workers who are higher up the pay distribution and so not directly affected. Using national surveys, the New Earnings Survey (NES) and Labour Force Survey (LFS), we estimate difference in difference models for the introduction of the NMW and for subsequent up-ratings. Similarly to Stewart (2004) we find positive impacts on wage growth for affected workers. However, there is some evidence that wage growth has been lower in the recession among the affected group of workers, perhaps as wage differentials have been restored somewhat for higher paid workers.

It is our findings on employment retention that differ from previous research. We find no significant impacts for full-time male or female workers. However, we find that the introduction of the NMW had an adverse impact on employment retention of part-time

women, reducing their retention by about 3% points. This is the group that has the largest proportion of workers affected by the wage floor. Furthermore, we find that employment retention among part-time women fell further in the recession. This is despite the fact that the minimum wage was increased only modestly in the past few years. These findings appear at odds with the earlier research by Stewart (2004). However, he focused on full time workers only citing problems with the data on part-time workers. We acknowledge those concerns and conduct a series of robustness checks on our results for part-time women. We find that our main conclusions hold up.

The paper is organised as follows. In the next section we outline the difference in difference methodology. Section 3 details the data used and presents some descriptive statistics. Section 4 reports results on wage growth. Section 5 reports our main findings on employment retention and Section 6 offers some conclusions.

2. Methodology

The identification of the impact of the NMW on labour market outcomes is inherently difficult. There is no sizeable excluded group from the NMW legislation. Furthermore, there is a uniform minimum rate for adult workers across the UK which rules out the sort of variation one finds, for example, in the US where minimum rates vary by State. The typical approach to identification taken in the UK and much of Europe is the "differential impacts" approach (Dolado et al, 2004). This compares outcomes among groups of workers that are more or less affected by the minimum wage. For example, the degree to which workers are impacted by the minimum wage may vary across regions (Stewart, 2002, Dolton, Wadsworth, Bondibene, 2012). Or it may vary at different points in the wage distribution. One may expect those workers whose pay is directly affected by a change in the NMW to exhibit different labour market outcomes than those workers further up the pay distribution whose pay is not directly affected (Stewart, 2004, Stewart and Swaffield, 2008). This is the approach we take in this paper.

We utilise a difference-in-differences approach to study labour market outcomes of the NMW over time. The treatment group is defined as those paid below the new level of the NMW at time *t*, before it is enforced, and the comparison group is defined as those individuals paid within some range above the new NMW, before it is enforced. For example, those workers paid below the October 2009 rate of £5.80 in the months before enforcement are allocated to the treatment group and those workers paid up to 10% above £5.80 to the control group. Outcomes for these two groups of individuals are then compared at time *t+1* when the new NMW is in place. The policy effect is then measured as the difference in outcomes between the treatment and control group after the

change/introduction of the new NMW less the difference in outcomes between the treatment and control group in some period before the policy change. The choice of the base period can be crucial here. We will return to this issue below.

More formally, to estimate the effect of a change in the minimum wage we use as the basis of our analysis the model specified in equation (1):

$$Z_{i,t+1} = \propto_0 + \propto_1 D_i + \propto_2 T_t + \propto_3 (T_t * D_i) + \beta X_{it} + \varepsilon_{it} \quad (1)$$

where $Z_{i,t+1}$ is the outcome of interest after the minimum wage change. For example, the probability of being employed at time t+1, conditional on being in work at time t; $Prob(E_{i,t+1}, E_{i,t} = 1)$. D_i is a dummy variable equal to one if an observation belongs to the treatment group for evaluating the NMW change and zero otherwise. T_t is a dummy that indicates whether the observation is after the NMW change. The parameter $\propto_1$ measures the baseline average difference in outcomes between the treatment and control groups (the normal difference), $\propto_2$ common time effects and $\propto_3$ is the estimated treatment effect of the NMW change.

Using the standard difference-in-differences specification we examine separately the impact of the introduction of the NMW and each individual uprating since then. In addition to the examination of each individual uprating, we also estimate models pooled across all years to estimate the overall impact of the NMW since its inception. We do this in a number of ways. First we use a simple difference-in-differences model as presented above in equation (1). This just pools together each change in the NMW.

$$Z_{i,t+1} = \propto_0 + \propto_1 D_i + \propto_2 T_t + \propto_3 (T_t * D_{it}) + \beta X_{it} + \varepsilon_{it} \quad (2)$$

Here the time dummy T_t is the same as in equation (1) but now the treatment dummy, D_{it}, is defined in terms of whether the individual is in the treatment or control group for each uprating in year t and D_i is a dummy variable equal to one if the individual is in the treatment group for any uprating.. A potential drawback here is that this ignores the fact that the size of the minimum wage increase can be different from year to year. Consequently, we also experimented with a wage gap estimator which allows for variation in the NMW policy measure. The wage gap is defined as the % change in the wage required for someone to comply with the NMW legislation. For example, someone who is paid £5.73 in April 2009 requires a 1.2% pay rise to comply with the October 2009 NMW of £5.80. The results are broadly similar.

There are two key identification assumptions we require to hold for this approach to provide a valid estimate of the causal impact of the NMW. Firstly, in the absence of any change in the NMW, we require that the evolution of outcomes be the same in the treatment and control groups, i.e the change in employment retention would have been the same had the NMW not been introduced. This may be of concern as one looks at later increases in the NMW where the benchmark period is from a number of years back. We return to this issue below. Secondly, we require there to be no impact from the NMW on the control group. This may be violated if there are wage impacts from the minimum that affect workers further up the distribution. Stewart (2011) finds little evidence of so called "spillover" effects on workers above the minimum, but Butcher, Dickens and Manning (2011) do find some evidence of modest spillover effects.

3. Data sources and measurement issues

We use the New Earnings Survey 1994-2010 (NES) and the Labour Force Survey October 1996 - December 2010 (LFS) to estimate the impacts of the NMW.[1] There are a number of advantages and disadvantages of using the NES/ASHE and the LFS to evaluate the impacts of the NMW. Since the NES is employer reported, data issues of measurement error in wages are likely to be much less severe than in the LFS. This is crucial here since the identification of the treatment and control groups using this methodology will be imprecise if measurement error exists in the data. There is a significant amount of evidence that measurement error is much higher in the LFS data than in NES/ASHE and hence it is not particularly reliable for this sort of analysis (Dickens and Manning, 2004). The problem in the LFS lies largely in the construction of the hourly wage measure. This is derived from information on the last reported pay and length of the last pay period converted into weekly pay and then divided by weekly hours of work. About a third of respondents report their pay as an annual amount. This is then divided by 52 and then by weekly hours. This creates a significant degree of measurement error in the hourly wage. A direct question on hourly pay was introduced into the LFS in Spring 1999. This appears to provide a more robust estimate of hourly wages. Dickens and Manning (2004) show the existence of a spike at the NMW in this variable that doesn't exist in the alternative derived measure. Unfortunately, since this hourly rate variable was only introduced in March 1999 it does not allow for a pre-NMW period against which to benchmark our difference in difference results.

The NES/ASHE is not without its own sampling problems. The sampling structure of the NES/ASHE means it is likely to under-sample low paid workers. The NES/ASHE samples

[1] We use the NES rather than the Annual Survey of Hours and Earnings (ASHE) because the NES has a longer run of historical data, which proves useful for identifying NMW treatment effects.

all those individuals who have a National Insurance number that ends in two particular digits. Individuals' workplaces are traced from Inland Revenue tax records. A potential problem arises here with low paid workers who are not earnings enough to pay tax. An individual must have earned more than the weekly Pay-As-Your-Earn (PAYE) threshold at some point in the year with their present employer in order to appear in the tax records. This may be a particular problem for low paid, part-time workers. In addition, attrition rates from the NES tend to be quite high as low paid individuals may slip in and out of the tax system. If someone drops out of the NES for these reasons we cannot identify whether they have left employment or not.[2] We will return to this issue below.

In utilizing this type of estimation technique, where we compare those directly affected by changes in the NMW with some other group, the allocation of individuals to the treatment and control groups can be crucial. We experiment with different definitions of treatment and control groups and check the sensitivity of our results to this. Using the NES, an observation is allocated to the treatment group of an October uprating in a given year if in April of that year the wage is less than the upcoming October NMW, but greater than or equal to the October NMW of the previous year. An observation is allocated to the corresponding control group if the wage in April of a given year is greater or equal to the October NMW of that year, but not by more than 10%. For example, in October 2009 the NMW increased from £5.73 to £5.80. Individuals who are paid between £5.73 and £5.80 in April 2009 are allocated to the treatment group. Individuals paid between £5.80 and 10% above this are allocated to the control group. We then examine differences in outcomes in April 2010. For example, we examine whether those employed in the treatment and control group in April 2009 are still in work in April 2010. As a robustness check, we also estimate models where the control group is paid in a band 10-20% above the October NMW.[3]

[2] Attrition from the LFS is also high, with some 30% of individuals missing one year later. Furthermore, attrition is higher among the low paid and has increased substantially over the last decade or so as the LFS has found it increasingly hard to locate individuals from quarter to quarter.

[3] As a further robustness check, we also use treatment and control groups defined by particular percentiles of the wage distribution. In this approach individuals whose wages fall between the $Tmin^{th}$ and $Tmax^{th}$ percentiles of the wage distribution are allocated to the treatment group. Individuals whose wages fall between the $Cmin^{th}$ and $Cmax^{th}$ percentiles of the wage distribution are allocated to the control group. Tmin is the maximum value at which the $Tmin^{th}$ percentile is below the compliance NMW at all times (evaluated during the period when there is a NMW). Tmax is the minimum value at which the $Tmax^{th}$ percentile is above the compliance NMW at all times, which in most cases is less than the new NMW, which is not yet in place. Cmin is the minimum value at which the $Cmin^{th}$ percentile is above the new NMW at all times. Cmax is chosen to equalize the size of the treatment and control group. A further control group is chosen by moving a similar step up the wage distribution from Cmax . We don't report results for the percentile groups in this paper but the general story remains the same with these alternative groups (Dickens, Riley and Wilkinson, 2012).

This gives us our simple difference in outcomes between treatment and control. In order to estimate differences in differences we need to observe the same comparison between treatment and control groups in the period when the NMW is unchanging or not in force. Hence we define the treatment and control groups in the period before the introduction of the NMW; i.e. in the benchmarking period. In order to construct these groups we pool together the NES from 1994 to 1997. We deflate the April 1999 NMW of £3.60 using the average earnings index to determine who is in each of the treatment and control groups in the pre-NMW years.[4]

A potential drawback with the difference in difference approach outlined above is that as one looks at the later changes in the NMW, the benchmark period can be quite a few years back. A key identification assumption here is that, absent any change in the NMW, the evolution of labour market outcomes in the treatment and control groups would have been the same; the common trends assumption. The worry is that, while one may expect the common trends assumption to hold over relatively short time periods, it may be violated when we are comparing periods some 10 years apart. Hence, we experimented with a further robustness check by using the vertical difference-in-differences model developed in Stewart (2004) and used in Swaffield (2009). Here, one now compares differences in outcomes between the treatment and control groups with a set of groups from the ***same*** time period but from higher up the wage distribution. In estimating the vertical difference-in-differences model we use the groups paid 10-20% and 20-30% above the NMW. Essentially one is now comparing the difference between those below the NMW and up to 10% above it with the difference between those 10-20% and 20-30% above the NMW. Unfortunately we are not very confident in this approach. We find unusual wage impacts and also some very large negative employment effects across all groups of workers.

4. The impact of the NMW on wages

A key factor in the likelihood of whether the NMW will have any impacts on labour market outcomes is the extent to which it affects worker's pay. An important dimension is the size of the affected group of workers, or the size of our treatment groups. Figure 1 reports the percent of workers affected by introduction and each subsequent up-rating of the NMW. Data is reported for full time females and males and part time female workers.

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[4] We also estimated models where the treatment and control groups in the "before" period were chosen by deflating the NMW for a particular uprating to the "before" period. For example, in analysing the impact of the 2003 uprating, the 1997 'NMW' used to differentiate between the treated and controls in 1997 is calculated by scaling the October 2003 NMW with the ratio of the Average Earnings Index (AEI) April 1997 to the AEI April 2003. These estimates were not very different to those reported here.

The survey data here is from April each year. The NMW was introduced in April 1999, so the figure for 1998 refers to the percent of workers below the introductory rate. Subsequently the NMW was up-rated in October each year, from October 2000. The subsequent figures therefore refer to the percent of workers in April of year t, who will be directly affected by the October NMW for that year.

Impacts on full time workers are relatively modest. At most, only about 2% of full time male workers have their wages raised directly by the NMW at any point. The figures for full time females are somewhat larger, but still only 3% of these women at most experience a pay impact. The biggest impact of the NMW is on part time female workers. Some 8% of this group were affected by the introduction of the NMW in 1999. In the mid-2000s the Low Pay Commission implemented some larger increases in the NMW. Here we can see that a substantial group of part time female workers were directly affected by these increases, with some 12% of this group affected. As the UK went into recession, the Low Pay Commission acted more cautiously with some modest increases in the NMW. We see the percent of part time females affected by these increases declining. However, one must remember that this is against the backdrop of declining employment rates overall. The implication of this figure is that any significant impacts on labour market outcomes are likely to appear among the group of part time female workers who are by far the most affected group.

Let us now turn to examine the impact of these changes in the NMW on hourly wages. Much research has already been carried out on the impact of the NMW on wage levels and growth (Stewart, 2004 ; Swaffield, 2009). A consistent finding that emerges from this literature is that the NMW did indeed raise the wage of workers directly affected by its introduction and subsequent up-ratings. Here we replicate much of that earlier work for our different groups but we also examine wage growth in the recession years, when there were some relatively small increases in the NMW. We examine wage growth using a difference in difference framework outlined above, estimating equation (1) above with the dependent variable individual hourly relative wage growth between year t and t+1. We also report pooled models and a model with interactions with the recession years of 2008 and 2009. We benchmark the difference in difference estimates on the 1994-1997 period.

Table 1 reports our findings. Here we report regression estimates of the difference in difference coefficient for each year and for the pooled models. Since the data on individual level wage growth has some quite large outliers, we follow Stewart (2004) and Swaffield (2009) and use robust regression techniques. The introduction of the NMW is clearly associated with an increase in wage growth for those workers directly affected by the NMW across all groups; full time men and women and part time women. The results

suggest that on average wages grew some 4-7% faster at introduction among the directly affected group, compared to those with wages up to 10% above the NMW.

The larger upratings of 2001, 2003 and 2004 also appear to be associated with positive wage growth. However, for some years we do find evidence of negative wage growth effects, particularly in the recession years when increases in the NMW were more modest. It is important to be clear what this implies. These results are comparing wage growth between treatment and control groups for each uprating. This is *not* saying that the ***level*** of wages for low paid workers is less as a result of the NMW but rather that wage ***growth*** is slower for low paid workers in certain years. In other words, wage differentials between NMW workers and workers paid just above the NMW were restored during recession.

The pooled models generally show that on average wage growth for low paid workers in the treatment group has been higher as a result of the NMW for all groups considered The negative and significant interaction term of the treatment indicator with the recession years suggests that during recession, the NMW effect on wage growth was more muted than it was on average over the full period since its introduction.

In summary, we find a positive effect of the NMW on wage growth for all groups considered, which is particularly large upon introduction. We find some evidence to suggest that wage differentials between NMW workers and those paid just above the NMW were restored somewhat during the recent recession years.

5. The impact on job retention

Let us now turn to our main results of interest, the impact of the NMW on employment outcomes. We begin with a visual representation of changes in employment retention before and after the introduction of the NMW. Figures 2a – 2c present employment retention rates for full time male, full time female and part time female workers respectively. We report retention rates for our treatment and controls groups as defined above; where the control group includes those workers with an hourly wage up to 10% above the associated NMW. Employment retention rates are defined here as the probability of being employed at time t+1, conditional on being employed at time t; $Prob(E_{i,t+1}, E_{i,t} = 1)$; , it does not require the worker to be in the same job. Note that we cannot define retention rate for individuals in employment in April 1999 as there was no subsequent change in the NMW until October 2000. Retention rates are typically low among these low paid workers. We also see that retention rates tend to be lower in our treatment group than in the control group. This is why it is important to look at differences in differences that allow for a "normal" difference between the two groups.

The results for full time men show that the evolution of retention rates between treatment and control groups was similar prior to the introduction of the NMW. There is some sign that the retention rate fell more among the treatment group in 1998, but the differences do not look large. We see a dramatic fall in retention in 2006 that is associated with the temporary reduction in the sample size of the NES/ASHE. This affects both groups the same but one should perhaps not put too much weight on results from this year.

Turning to Figure 2b for full-time women, again we see little difference between the evolution of employment retention rates between the treatment and control group. Retention is lower among the lower paid treatment group but the trends are similar over time and over the period of the introduction of the NMW. However, Figure 2c for part time women shows some differences. Here we see that retention rates evolve similarly prior to the introduction of the NMW but then they drop quite sharply among the treatment group only. For example, retention rates hover around 70% among the treatment group in the years before the NMW but retention in 1998 falls to just over 65%. Employment retention rates remain lower (relative to the control group) in the period after introduction. This is suggestive of an adverse impact on employment retention from the introduction of the NMW for these part time women. Remember that this group are most affected by the wage floor and so we are most likely to see any impacts on employment among this group. We now turn to more formal estimation to check the validity of these findings.

The validity of the Difference in Difference approach

The difference in difference technique compares the difference in rates between treatment and control groups after the introduction of the NMW with those that existed prior to introduction. The key identification assumption is that the evolution of these rates would have been the same in the absence of the NMW, the *common trends* assumption. One can never test for common trends but we can explore the likely validity of this assumption by estimating trends in retention rates among the treatment and control groups prior to the introduction of the NMW. Essentially we estimate a 'difference-in-difference' model of equation (1) in years prior to the introduction of the NMW, interacting year dummies with the treatment dummy.

In Table 1 we report difference-in-differences (DID) coefficients for each year 1995-1997 in a regression with base year 1994. If the assumptions underlying the DID identification strategy are valid, these DID estimates should be statistically insignificant. We also report the Wald test of the joint significance of the year interaction terms. We report results for full time males and females and for part time females, both with and without control variables. These Wald statistics suggest there is no overall significance in the interaction

terms for any of our estimates. This is supportive of the assertion that there were common trends in employment retention prior to the introduction of the NMW. The findings are in line with those of Stewart (2004). The Table also reports the individual coefficients for each interaction. These are in line with the Wald tests reported. None of the individual year coefficients are statistically significant at conventional levels. The only result that is somewhat questionable is that we do find a positive interaction term for part time women in 1997 which is just outside of the 10% significance level.[5] Overall, these results are supportive of the common trends assumption that underlies our analysis.

Impacts of the NMW on employment retention

Let us now focus on our main results, the impact on employment retention from the introduction and subsequent up-ratings of the NMW. We report results separately for each year by estimating equation (1) above. This compares retention in the baseline period 1994-97 with that for each separate year. We present results for 1998, which captures the impact of introduction and then for each up-rating from 2000-2009. We also pool all of the up-ratings together and estimate equation (2) which captures an overall difference in difference effect.

Table 3 reports results on employment retention. We find little evidence of significant NMW impacts on employment retention for full-time workers. For both full time males and females most results are statistically insignificant. For a few of the years we do find a negative difference in difference coefficient that is significant but the results are not very robust. Furthermore, the pooled results for full time workers are insignificant. We also estimate models including a set of control variables but the findings here are not sensitive to this.

However, we do find evidence of a negative effect of the NMW on 12 month employment retention for low paid female part-time workers. These effects are particularly prevalent upon introduction, and there is some (albeit weak) suggestion that this effect worsened during recession in comparison to earlier years.[6] For example, we find that the introduction of the NMW reduced employment retention among this group by approximately 3% points. This finding is robust to the inclusion of controls. We also find negative impacts from the later increases in the NMW, particularly in the recession years towards the end of our sample period. We also find negative impacts for part-time women in our pooled models which capture the overall impact of the NMW.

[5] As a consequence we also estimate our main results excluding 1997 from the benchmark period with no substantive change in our findings.

The final two rows report the difference in difference coefficient that one obtains from including an interaction with a dummy variable for the recession years; 2008-09. This model allows for a difference in difference parameter for all years after the benchmarking period; 1994-97. But it also introduces an interaction term for the final two years; 2008 and 2009 . We find here no significant impacts for full-time men and women, but again a negative impact on retention for part-time women which deepens in the recession. This was in a period of modest increases in the NMW. We also experimented with estimating this pooled model with the recession interaction ***only*** in the time period post introduction (2000-2009) and also find a fall in retention in the recession. Here identification is coming only from the differential impacts in 2008-09.

We also estimate employment retention models using the LFS data. Here we examine the introduction and each subsequent up-rating, now benchmarked against the period 1996-98. Table 4 reports our results. The results for full time workers are very similar to the NES results where we find no significant effects. However, the results on part time female workers turn out to be very different from the NES findings. We find no significant employment effects in most of our estimates. Only for the 2008 up-rating for part-time women do we find a positive impact. However, given the wage results for the LFS we do not have a high degree of confidence in these results here as we are likely to be misclassifying individuals into the treatment and control groups.

At first glance these results are seemingly at odds with previous research on the employment effects of the introduction of the NMW. Our findings for full time workers are very much in line with Stewart (2004). He focuses on introduction and finds no adverse employment retention effects. However, Stewart's analysis excludes part-time workers on the grounds that these are more likely to fall below the Pay-As-You-Earn (PAYE) threshold, which may affect sampling and attrition in the NES. Here we chose to include analysis of this group as they are such a sizeable proportion of those affected by the NMW. However, we need to be somewhat careful in the analysis of part-time females for the reasons outlined by Stewart (2004). As such we conduct a number of robustness checks on our results with a focus on this group.

Robustness of the results

[6] These adverse effects of the NMW on employment retention for female part-time workers are also evident when we use the percentile control groups.

Firstly, we check the whether the choice of treatment and control groups are appropriate. The results reported above are all based on a control group of workers whose wage lies between the new NMW and 10% above this figure. We also estimated the same set of results using a control group whose wage is between 10-20% above the NMW. One reason for looking at this higher wage control group is if there are potential spillover effects from the NMW on wages further up the pay distribution (Stewart, 2012 and Butcher, Dickens and Manning, 2011). We report these results in Table A1 in the appendix. The results of this exercise are qualitatively similar to those reported above. We find no significant effects for any of the full time workers but again find negative impacts for part time women at both introduction and later on in the recession years

A further way to assess the allocation of individuals to the various groups is to provide a visual analysis of employment retention for different hourly wages. The results above somewhat arbitrarily assign individuals to treatment and control groups. In Figure 3 we present kernel density estimates of employment retention for different hourly wages, centered around the introductory NMW of £3.60 an hour. We report estimates for the period prior to introduction; 1994-97 and for the year that spans introduction; 1998. Similar patterns emerge for other years. We see that retention is increasing with the hourly wage, so that average retention for our lower paid treatment group is lower than for those in our control group. Looking at differences before and after introduction of the NMW, retention rates for those above the NMW are unchanged. In fact, there is nothing very special about our choice of control group of workers in the range £3.60-£3.90. Although retention rates are higher among the alternative control group (10-20% above NMW) there is little difference between the pre and post NMW rates, which is why our results are not very sensitive to the choice of control group.

Perhaps the most striking result from this figure is the decline in retention rates among the treatment group after introduction of the NMW. This decline begins immediately when the wage falls below the new minimum. The decline is greater the larger the wage gap from the NMW, so that those workers who have to have their wage raised the most to comply with the NMW experience the largest fall in employment retention. This figure implies that the estimation results above are not just an artifact of our choice of treatment and control groups, but that there appears to be a clear break in changes in retention between the treatment and control groups.

A key concern with the use of the NES data is the under-sampling and potential attrition of part-time workers. This poses some potential problems for the analysis of the NMW. As Figure 1 on coverage showed, this is the group we may be most interested in as they are by far the most affected by the NMW. However, as discussed above, the alternative datasets for this analysis have serious drawbacks. The Labour Force Survey and similarly the

British Household Panel Surveys have significant measurement error in their hourly wage measures which throws doubt on the identification of the treatment and control groups (Dickens and Manning, 2004). Since the NES is employer reported data measurement issues are much less severe but we do face potentially significant sampling problems.

The issue here is that the sample frame for the NES is drawn from tax records. However, for an individual to appear in the tax records they must have earned more than the weekly Pay-As-Your-Earn (PAYE) threshold at some point in the year with their present employer. Because part-time workers have lower weekly pay, they are more likely to fall below the PAYE threshold. The potential problem with our results is that low paid workers fall below the PAYE threshold and do not appear in the NES. If this occurs we would treat them as exiting from employment, when actually they may still be in employment but not earning enough to pay tax and hence appear in the NES.

For this to be a serious problem for our results, we would need the change in the proportions falling below the PAYE limit to differ between our treatment and control groups in the period before and after the NMW. If our negative retention results are driven by the PAYE issue it implies that there is an increase in the proportion falling below the PAYE threshold that is greater in the treatment group than control group over the period of introduction of the NMW.

While we can't directly identify those who drop out of the NES for these sampling reasons, we can get some handle on their likely behaviour as a substantial proportion of those in the NES/ASHE do have weekly pay below the PAYE threshold. This can occur for two main reasons. First, weekly pay is measured in the survey week in April, but to enter onto the tax records one only has to earn above the threshold in one week of the preceding tax year. Second, larger employers directly report information for those workers with the relevant NI numbers which bypasses the sampling problems outlined above. Since the PAYE threshold is a weekly amount and the NMW is an hourly rate we can look at the prevalence of workers below the PAYE threshold in our sample and see if any of our results on retention rates are different for this group.

At the time of the introduction of the NMW about 25% of our NES sample of part-time women report weekly wages below the PAYE threshold. This proportion is much higher among the low paid, with over 60% of our treatment group and over 40% of our control group below this threshold in 1998. The concern with our estimates is that attrition increases among the treatment group at the time of the introduction of the NMW. If someone drops out of the NES we can never be sure that they are no longer in employment. We do not believe that changes in attrition is a major driving factor in our results for part time females. Attrition rates do fall steadily over our time period of analysis but there is no sudden jump at introduction.

We can formalize the potential attrition of those above and below the PAYE threshold in estimating our retention models. Given that attrition will appear as (the lack of) job retention we can allow for different retention rates for those above or below the PAYE threshold. We therefore estimate the following model which is the same as the difference in difference model of equation (1) but now includes a dummy variable, $PAYE_{it}$, which is

equal to 1 if the individual has weekly pay below the PAYE threshold, and an interaction of this dummy with the post-NMW dummy; T_t. This allows for differential retention rates

among those above and below the PAYE threshold and for this difference to change post NMW.

$$Z_{i,t+1} = \propto_0 + \propto_1 D_i + \propto_2 T_t + \propto_3 (T_t * D_i) + \alpha_4 PAYE_{it} + \alpha_5 (T_t * PAYE_{it}) + \beta X_{it} + \varepsilon_{it}$$

The first two columns of Table 5 present estimates of the difference in difference parameter on the NMW from this model. Here we focus just on part-time females and report results both with and without controls. As expected, the coefficient on the PAYE dummy (not reported) suggests that job retention is low among the group with earnings below the PAYE threshold. However, the key results on the introduction of the NMW are largely unchanged with the addition of these extra interaction terms. We still find a significant negative impact from the NMW at introduction. We also find negative impacts in the recession years as above.

In the next two columns we check the robustness of these results further still by including an interaction term between the PAYE dummy and the NMW Treatment dummy, post introduction. This allows for any potential impacts of the NMW to vary between those above and below the weekly PAYE threshold. The qualitative nature of the results remains unchanged. We do find in some years a significant positive interaction term which suggests that the impact of the NMW is lower in magnitude among those treated individuals whose pay is also below the PAYE limit.

Note that these results here attempt to identify those who are below the PAYE threshold in period t by assigning individuals according to their weekly pay. They do not identify those who drop below the PAYE threshold in period t+1 as we would ideally like to. However, they do provide a proxy for this group of individuals. We find that when we control for different retention (attrition) rates among this group our results for part time females largely remain unchanged.

6. Conclusion

This paper has re-examined some of the existing evidence on the introduction of the National Minimum Wage with a focus on particular groups of workers. We have also provided an analysis of the impact of the minimum wage in more recent years when the UK entered recession. Our broad findings are not significantly out of line with the large existing literature. However, we do find some evidence of a fall in employment retention among part time female workers at introduction and also for some of the subsequent increases in the NMW. These later impacts appear largest in the recession years. In estimation we subject our results to a range of robustness checks. We also focus on the particular problem inherent in the NES data with regard to the attrition of workers below the tax threshold. Our findings are robust to the additional checks that we implement.

We have most confidence in our results on the introduction of the NMW and also those results in the recession. As one moves further away from the time period of introduction it becomes less clear what these results might be picking up. Firstly, there is an issue of whether the common trends assumption would have held over this longer time period. But also there is a problem of interpretation as to whether one is measuring the impact of changes in the NMW or the existence of the wage floor. The analysis of introduction is perhaps more clear cut than that of the subsequent up-ratings. The retention rates presented in Figure 2c showed that employment retention declined in the treatment group after the introduction of the NMW. So when one compares this with the pre-NMW period one finds a negative employment effect. If the retention rate declined due to the existence of the NMW then the later results for the years 2000-2009 are just picking up this wage floor effect rather than any impact from each increase in the NMW. One way to think of this is that we only have one policy change of introduction to examine rather than 10 policy changes of each up-rating.

The results for the recession are showing that retention worsened among the treatment group as the UK entered recession. This is despite some modest increases in the NMW. These findings are additional to the fall in retention observed at introduction. The results on recession are robust to excluding the pre NMW benchmark period and focusing on 2000-09. Here we identify differential effects among the treatment group in 2008-09 from the previous years and are less prone to problems of common trends violation.

A general consensus has emerged that the UK National Minimum Wage has done little to harm employment prospects. Previous work by Stewart (2004) and numerous studies carried out for the UK Low Pay Commission found little in the way of significant employment effects. Much of that earlier work did not look at different groups within the labour market. The evidence we present here suggests that among the group that are most affected by the NMW, part time female workers, we see some evidence of falls in employment retention.

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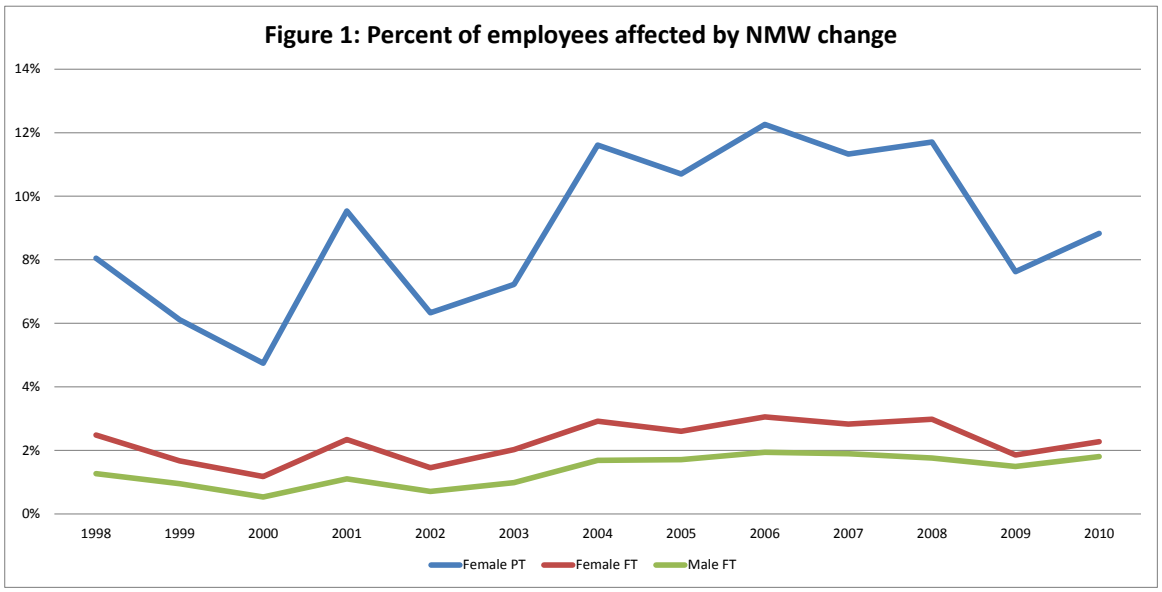
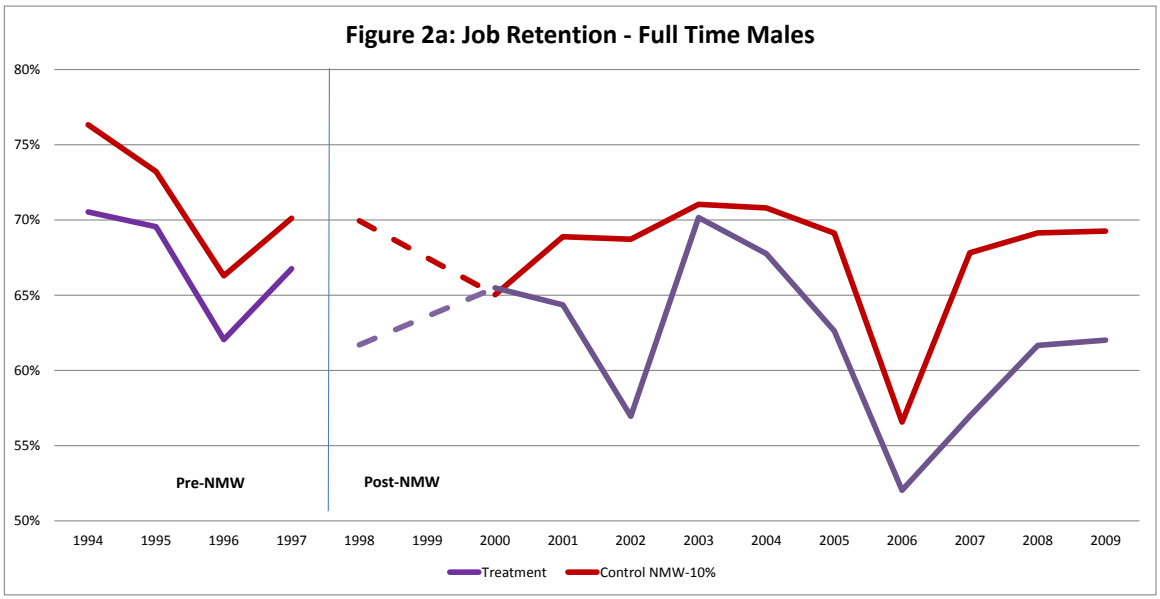
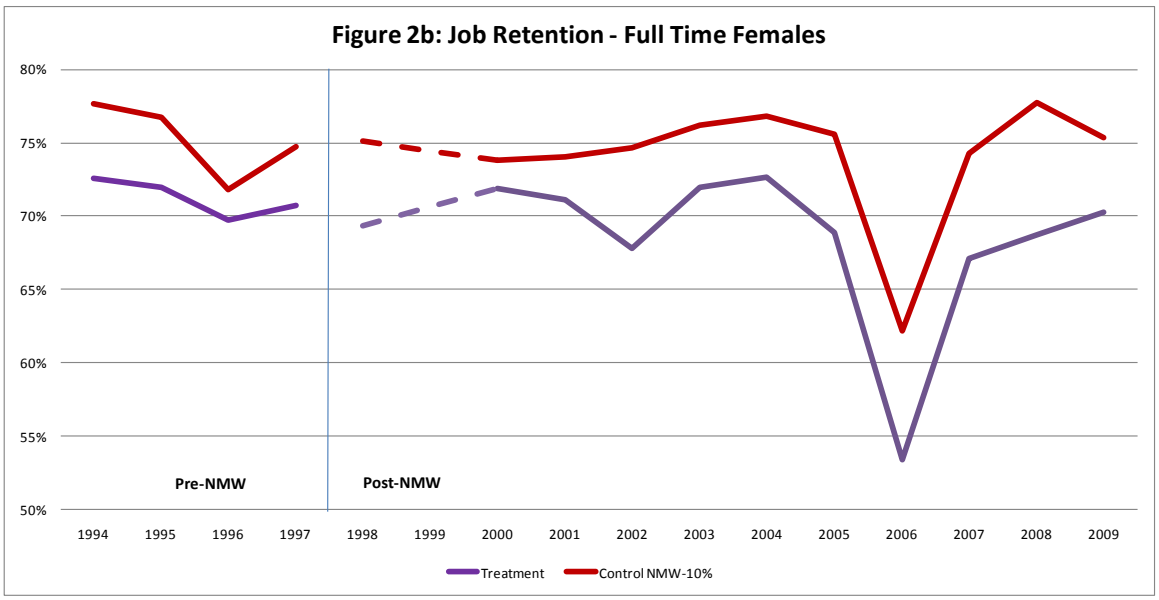


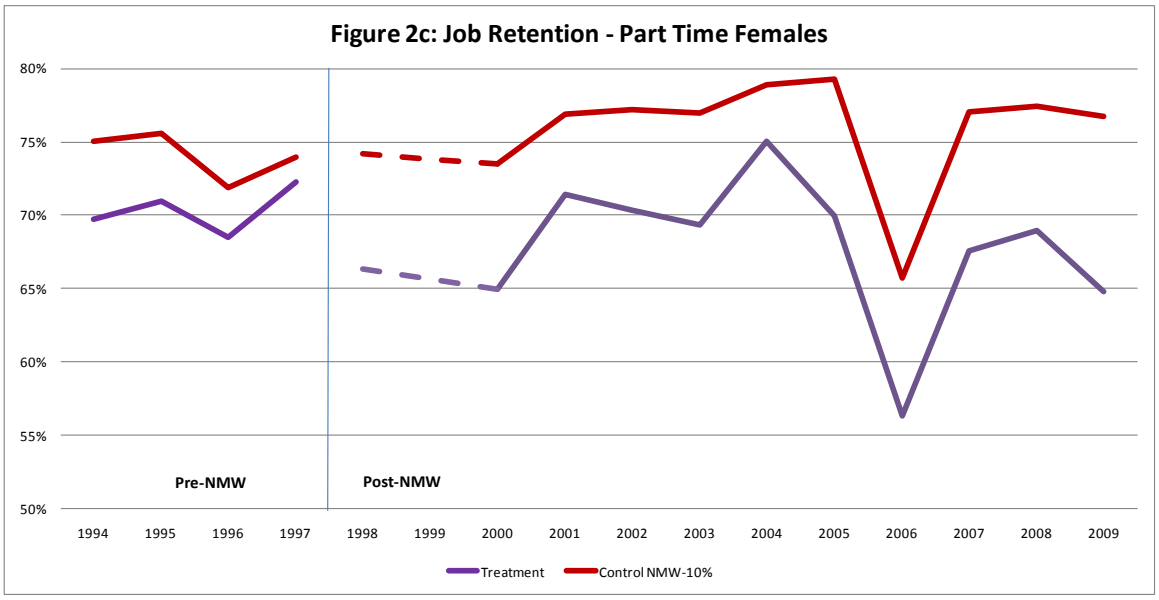
Source: NES 1998-2010.



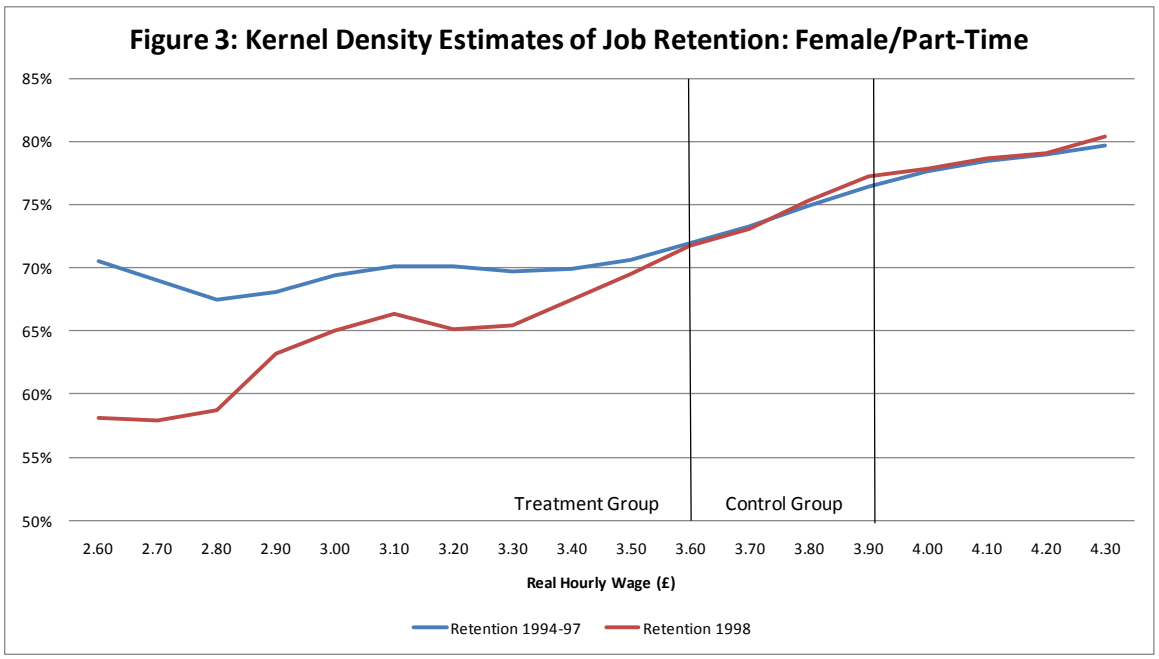
Notes: Dashed figures for 1998-2000 are because retention is not defined for 1999.



Notes: Dashed figures for 1998-2000 are because retention is not defined for 1999.



Notes: Dashed figures for 1998-2000 are because retention is not defined for 1999.



Notes: Kernel density estimates of employment retention using Epanechnikov kernel. NES 1994-1998.

Table 1: Annual Wage Growth NES

Sex	Female		Female		Male	
Hours	Full-time		Part-time		Full-time	
Control variables	No	Yes	No	Yes	No	Yes
Year:						
1998	0.042***	0.045***	0.060***	0.061***	0.069***	0.068***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
2000	-0.010	-0.008	-0.010*	-0.008	0.011	0.010
	(0.007)	(0.007)	(0.005)	(0.005)	(0.009)	(0.009)
2001	0.023***	0.025***	0.022***	0.022***	0.035***	0.034***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.006)	(0.006)
2002	-0.007	-0.005	-0.028***	-0.026***	0.011	0.011
	(0.007)	(0.007)	(0.005)	(0.005)	(0.008)	(0.008)
2003	0.013**	0.016***	0.014***	0.015***	0.039***	0.036***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
2004	0.018***	0.020***	0.008**	0.008**	0.045***	0.041***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
2005	-0.012***	-0.010**	-0.009**	-0.008**	0.019***	0.015***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
2006	0.012***	0.014***	0.010***	0.011***	0.036***	0.033***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
2007	-0.001	-0.000	-0.007**	-0.007*	0.025***	0.020***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
2008	0.001	0.002	-0.007**	-0.006*	0.033***	0.030***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
2009	-0.004	-0.004	-0.009**	-0.008**	0.020***	0.016***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
Pooled NMW effect						
Pooled	0.008***	0.010***	0.006***	0.006***	0.031***	0.028***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Pooled upratings	0.006**	0.007***	0.003	0.003*	0.030***	0.027***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Pooled NMW effect - Intreraction with recession years						
Pooled	-0.020***	-0.021***	-0.021***	-0.022***	-0.013***	-0.016***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Pooled upratings	-0.016***	-0.018***	-0.018***	-0.018***	-0.011***	-0.013***
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)

Notes: NES 1994 - 2010; Pooled models that concern the upratings only exclude 1998; Difference-in-differences estimates using 1994-1997 as the pre-period; Control variables include quadratic in age, indicator of whether in same job as last year, and a cubic in the real wage.

Table 2: Employment Retention in the pre-NMW Period

Sex	Female		Female		Male	
Hours	Full-time		Part-time		Full-time	
Control variables	No	Yes	No	Yes	No	Yes
1995	0.003	0.007	0.004	0.007	0.016	0.015
	(0.021)	(0.021)	(0.017)	(0.016)	(0.022)	(0.022)
1996	0.023	0.025	0.017	0.018	0.017	0.015
	(0.019)	(0.019)	(0.016)	(0.015)	(0.022)	(0.022)
1997	0.010	0.011	0.026	0.026	0.024	0.022
	(0.022)	(0.021)	(0.016)	(0.015)	(0.023)	(0.023)
Observations	141,245	141,245	80,156	80,156	256,389	256,389
Wald test	1.53	1.75	3.29	3.22	1.07	0.92

Notes: NES 1994 - 1998; Wald test is a test of joint significance of the difference-in-differences coefficients DID95-DID97 (Chisq-statistic: DF=3); Significance at the ***1, **5, and *10 per cent levels; Standard errors in parentheses. Controls include a quartic in age, a dummy if the individual has been in the same job for more than a year and a cubic in the real wage

Table 3: Annual Employment Retention NES

Sex	Female		Female		Male	
Hours	Full-time		Part-time		Full-time	
Control variables	No	Yes	No	Yes	No	Yes
Year:						
1998	-0.013	-0.018	-0.033**	-0.032**	-0.028	-0.027
	(0.019)	(0.019)	(0.015)	(0.015)	(0.021)	(0.021)
2000	0.014	0.009	-0.039**	-0.038**	0.030	0.039*
	(0.023)	(0.023)	(0.018)	(0.018)	(0.023)	(0.022)
2001	0.008	0.005	-0.016	-0.017	0.001	0.008
	(0.017)	(0.017)	(0.014)	(0.013)	(0.019)	(0.018)
2002	-0.022	-0.027	-0.029*	-0.029*	-0.054**	-0.045*
	(0.023)	(0.024)	(0.016)	(0.016)	(0.027)	(0.027)
2003	-0.000	-0.005	-0.034**	-0.038**	0.023	0.030*
	(0.019)	(0.019)	(0.015)	(0.015)	(0.018)	(0.017)
2004	-0.003	-0.006	-0.003	-0.003	0.007	0.014
	(0.017)	(0.017)	(0.012)	(0.012)	(0.017)	(0.016)
2005	-0.022	-0.025	-0.058***	-0.056***	-0.014	0.001
	(0.018)	(0.018)	(0.014)	(0.014)	(0.017)	(0.016)
2006	-0.030*	-0.027	-0.043***	-0.038***	0.000	0.012
	(0.018)	(0.018)	(0.013)	(0.013)	(0.017)	(0.016)
2007	-0.023	-0.020	-0.051***	-0.040***	-0.047**	-0.033*
	(0.018)	(0.018)	(0.014)	(0.013)	(0.019)	(0.019)
2008	-0.042**	-0.040**	-0.043***	-0.042***	-0.020	-0.005
	(0.019)	(0.018)	(0.014)	(0.014)	(0.018)	(0.017)
2009	-0.011	-0.003	-0.073***	-0.065***	-0.019	-0.010
	(0.018)	(0.018)	(0.015)	(0.015)	(0.018)	(0.017)
Pooled NMW effect						
Pooled	-0.016	-0.016	-0.039***	-0.034***	-0.013	-0.003
	(0.010)	(0.010)	(0.008)	(0.008)	(0.011)	(0.011)
Pooled upratings	-0.015	-0.015	-0.037***	-0.032***	-0.009	0.002
	(0.010)	(0.010)	(0.008)	(0.008)	(0.011)	(0.011)
Pooled NMW effect - Intreraction with recession years						
Pooled	-0.015	-0.006	-0.028***	-0.022***	-0.013	-0.008
	(0.011)	(0.011)	(0.008)	(0.008)	(0.011)	(0.010)
Pooled upratings	-0.017	-0.009	-0.035***	-0.030***	-0.020*	-0.016
	(0.011)	(0.011)	(0.008)	(0.008)	(0.011)	(0.011)

Difference-in-differences estimates using 1994-1997 as the pre-period; Control variables include quadratic in age, indicator of whether in same job as last year, and a cubic in the real wage.

Table 4: Annual Employment Retention LFS

Sex	**Female**		**Female**		**Male**	
Hours	**Full-time**		**Part-time**		**Full-time**	
Control variables	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**
Year:						
1998	-0.003	-0.006	-0.011	-0.009	0.011	0.015
	(0.021)	(0.021)	(0.021)	(0.019)	(0.020)	(0.016)
2000	0.016	0.011	0.042*	0.033	-0.010	-0.012
	(0.025)	(0.024)	(0.016)	(0.016)	(0.039)	(0.037)
2001	-0.016	-0.015	-0.024	-0.018	-0.033	-0.027
	(0.031)	(0.029)	(0.027)	(0.025)	(0.039)	(0.037)
2002	0.000	-0.004	-0.023	-0.027	-0.015	0.004
	(0.033)	(0.031)	(0.033)	(0.033)	(0.051)	(0.037)
2003	0.005	0.006	0.011	0.006	-0.040	-0.038
	(0.024)	(0.022)	(0.022)	(0.022)	(0.045)	(0.044)
2004	0.015	0.012	0.010	0.010	-0.036	-0.025
	(0.020)	(0.020)	(0.021)	(0.019)	(0.039)	(0.034)
2005	0.011	0.011	0.004	0.005	-0.039	-0.037
	(0.022)	(0.019)	(0.024)	(0.022)	(0.044)	(0.042)
2006	0.006	0.001	0.024	0.012	-0.035	-0.028
	(0.024)	(0.024)	(0.021)	(0.022)	(0.041)	(0.037)
2007	0.038*	0.033	0.003	-0.004	-0.055	-0.034
	(0.013)	(0.013)	(0.025)	(0.025)	(0.047)	(0.040)
2008	0.015	0.016	0.052**	0.050**	0.008	0.008
	(0.023)	(0.020)	(0.017)	(0.015)	(0.030)	(0.014)
2009	0.04	0.032	0.041	0.034	0.014	0.019
	(0.015)	(0.017)	(0.023)	(0.022)	(0.030)	(0.026)
Pooled NMW effect						
Pooled	0.009	0.007	0.005	0.002	-0.019	-0.013
	(0.014)	(0.013)	(0.013)	(0.012)	(0.023)	(0.021)
Pooled upratings	0.014	0.012	0.011	0.008	-0.025	-0.018
	(0.013)	(0.013)	(0.012)	(0.012)	(0.025)	(0.022)

Notes: LFS 1996 - 2010; Pooled models that concern the upratings only exclude 1999; Difference-in-differences estimates using 1996Q4-1998Q1 wave 1 as the pre-period; Evaluation of April 1999 NMW introduction includes wave 1 observations 1998Q2-1999Q1; Control variables include highest educational qualification, temporary job, public sector job, quadratic in age, ethnic minority, marital status, quadratic in job tenure, presence of children under 5, region, and a cubic in the real wage; HOURPAY.

Table 5: Employment Retention with PAYE effects: Part-time females

Model		PAYE Dummy		PAYE interaction with Treatment	
Control variables		No	Yes	No	Yes
Year:					
1998	**DiD**	-0.039***	-0.038**	-0.056***	-0.050**
		(0.015)	(0.015)	(0.020)	(0.020)
	DiD*PAYE			0.025	0.018
				(0.017)	(0.017)
2000	**DiD**	-0.049***	-0.045**	-0.057**	-0.050**
		(0.019)	(0.018)	(0.025)	(0.024)
	DiD*PAYE			0.013	0.008
				(0.025)	(0.025)
2001	**DiD**	-0.027*	-0.025*	-0.041**	-0.036**
		(0.014)	(0.014)	(0.017)	(0.017)
	DiD*PAYE			0.029*	0.023
				(0.017)	(0.017)
2002	**DiD**	-0.039**	-0.035**	-0.058***	-0.052**
		(0.017)	(0.016)	(0.021)	(0.021)
	DiD*PAYE			0.035*	0.031
				(0.021)	(0.021)
2003	**DiD**	-0.046***	-0.048***	-0.048***	-0.050***
		(0.016)	(0.016)	(0.018)	(0.018)
	DiD*PAYE			0.005	0.005
				(0.021)	(0.020)
2004	**DiD**	-0.013	-0.010	-0.038**	-0.032**
		(0.012)	(0.012)	(0.015)	(0.015)
	DiD*PAYE			0.056***	0.048***
				(0.014)	(0.014)
2005	**DiD**	-0.061***	-0.058***	-0.068***	-0.064***
		(0.014)	(0.014)	(0.016)	(0.016)
	DiD*PAYE			0.015	0.014
				(0.017)	(0.016)
2006	**DiD**	-0.055***	-0.049***	-0.072***	-0.066***
		(0.014)	(0.014)	(0.016)	(0.016)
	DiD*PAYE			0.040***	0.041***
				(0.015)	(0.015)
2007	**DiD**	-0.060***	-0.046***	-0.075***	-0.059***
		(0.015)	(0.014)	(0.017)	(0.016)
	DiD*PAYE			0.034**	0.028*
				(0.017)	(0.017)
2008	**DiD**	-0.055***	-0.051***	-0.084***	-0.077***
		(0.014)	(0.014)	(0.017)	(0.017)
	DiD*PAYE			0.062***	0.057***
				(0.014)	(0.014)
2009	**DiD**	-0.084***	-0.073***	-0.096***	-0.080***
		(0.015)	(0.015)	(0.018)	(0.018)
	DiD*PAYE			0.027	0.015
				(0.018)	(0.019)

Notes: NES 1994 - 2010; Difference-in-differences estimates using 1994-1997 as the pre-period; Control variables include quadratic in age and a cubic in the log real wage.

Table A1: Annual Employment Retention NES (10-20% Control Group)

Sex	Female		Female		Male	
Hours	Full-time		Part-time		Full-time	
Control variables	No	Yes	No	Yes	No	Yes
Year:						
1998	0.008	0.006	-0.042***	-0.044***	-0.012	-0.011
	(0.016)	(0.016)	(0.014)	(0.014)	(0.017)	(0.017)
2000	0.013	0.012	-0.068***	-0.063***	0.004	0.015
	(0.022)	(0.021)	(0.019)	(0.019)	(0.025)	(0.024)
2001	0.004	0.003	-0.024*	-0.023*	-0.020	-0.007
	(0.017)	(0.017)	(0.014)	(0.014)	(0.019)	(0.018)
2002	-0.014	-0.017	0.008	0.007	-0.054**	-0.042*
	(0.022)	(0.022)	(0.014)	(0.013)	(0.026)	(0.025)
2003	0.022	0.019	-0.004	-0.010	0.032**	0.040***
	(0.016)	(0.016)	(0.013)	(0.014)	(0.016)	(0.015)
2004	-0.018	-0.019	0.028***	0.027**	-0.001	0.007
	(0.017)	(0.017)	(0.011)	(0.011)	(0.016)	(0.015)
2005	-0.035*	-0.036**	-0.010	-0.009	-0.031*	-0.013
	(0.018)	(0.018)	(0.012)	(0.012)	(0.017)	(0.016)
2006	-0.022	-0.015	0.009	0.009	0.011	0.022
	(0.017)	(0.016)	(0.012)	(0.012)	(0.015)	(0.014)
2007	-0.051***	-0.048**	-0.022*	-0.014	-0.050***	-0.032*
	(0.019)	(0.019)	(0.013)	(0.013)	(0.019)	(0.018)
2008	-0.028	-0.025	-0.039***	-0.036**	-0.011	0.006
	(0.018)	(0.017)	(0.014)	(0.014)	(0.017)	(0.015)
2009	-0.012	-0.003	-0.056***	-0.046***	-0.026	-0.012
	(0.018)	(0.017)	(0.015)	(0.014)	(0.018)	(0.017)
Pooled NMW effect						
Pooled	-0.015	-0.013	-0.015**	-0.011	-0.016	-0.003
	(0.010)	(0.010)	(0.007)	(0.007)	(0.010)	(0.010)
Pooled upratings	-0.016	-0.014	-0.011	-0.008	-0.014	0.000
	(0.010)	(0.010)	(0.007)	(0.007)	(0.011)	(0.010)
Pooled NMW effect - Intreraction with recession years						
Pooled	-0.015	-0.006	-0.030***	-0.023***	-0.014	-0.009
	(0.011)	(0.011)	(0.008)	(0.008)	(0.011)	(0.010)
Pooled upratings	-0.017	-0.009	-0.038***	-0.031***	-0.020*	-0.017
	(0.011)	(0.011)	(0.008)	(0.008)	(0.011)	(0.011)

Difference-in-differences estimates using 1994-1997 as the pre-period; Control variables include quadratic in age, indicator of whether in same job as last year, and a cubic in the real wage.