The Impact of the National Minimum Wage on Employment Retention, Hours and Job Entry

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Summary

Studies of the effects of the National Minimum Wage (NMW) carried out before the economic downturn began in 2008 found almost no evidence of significant adverse impacts on employment and only a little evidence of a negative impact on weekly hours worked. Likewise, the few studies carried out after the start of recession have concluded that the NMW has not had adverse effects, except (again) possibly on hours. Given the prolonged nature of the downturn it is important to maintain the evidence base on the potential impacts of the NMW on labour market outcomes. In this report we estimate the impacts of the NMW during the recession years (2008-2011), and compare them with impacts estimated for the preceding years beginning in 1999 (or 2000). We focus on three outcomes: employment retention, changes in working hours among employees, and the job finding probability of the unemployed.

Data and methods

The analysis uses (i) difference-in-difference (DID) methods applied to data from the Labour Force Survey (LFS), and (ii) methods based on the geographical bite of the NMW, derived from the Annual Survey of Hours and Earnings (ASHE) and matched into LFS.

The DID method involves comparing outcomes for a treatment group of individuals that is directly affected by the NMW with those for a control group of similar individuals earning just above the NMW. To classify individuals into these groups, we use actual wages for employees and predicted hiring wages for the unemployed. We use three variants of the DID method: (i) we examine changes over time in the outcomes of the treatment and control group (horizontal DID); (ii) we look at differences between the treatment and control group with respect to two additional groups further up the wage distribution (vertical DID); and (iii) we combine horizontal and vertical DID into a triple DID estimator. The two methods embody different assumptions about the effects of macroeconomic trends on the treatment and control groups and so provide sensitivity checks on the results. Within the DID framework, we also allow the NMW impact to vary with the 'intensity' of treatment, as captured by the gap between the current wage and forthcoming NMW (for the employed) and the probability of being hired at the NMW (for the unemployed).

The geographical method involves comparing outcomes across 140 local areas that are differently affected by the NMW according to their local wage structures. The NMW will have a large bite in low pay areas but a much smaller bite in high pay areas. We measure the bite as the proportion of employees affecting by the NMW or alternatively as the ratio of the NMW to median hourly earnings in an area (the Kaitz index). The NMW bite can also be considered as a measure of treatment intensity.

Samples and outcomes

For employees entitled to the adult NMW rate (22+ years until October 2010, 21+ years thereafter), we estimate the impact of the NMW on job retention and changes in basic usual hours (both measured over 6 months) of all the separate NMW upratings from 2000-10. We also estimate the average effects during 2000-7 (the pre-recessionary period) and 2008-11 (the recession years).

For unemployed adults, we estimate the impact of the NMW on the probability of moving from unemployment to employment over a 6 month period. As for job retention and hours changes, we obtain DID estimates for each year during 2000-11, and for the combined years 2000-7 and 2008-11. From the geographical analysis we obtain estimates for 1999-2011 and for the combined years 1999-2007 and 2008-11.

For youths (aged 18-21 years), owing to relatively small sample sizes, we estimate the impact on job retention and changes in basic hours only for the two combined periods 2000-7 and 2008-11. As we have insufficient observations to predict hiring wages for young people, we do not estimate the impact of the NMW on their job finding probabilities using DID methods, but we do use geographical methods (Kaitz index). We produce estimates for each year 1999-2011 and for the combined years 1999-2007 and 2008-11.

Impact of NMW on employment retention

We do not find robust evidence that the NMW upratings had an adverse effect on the employment retention of adults in either the pre-recession or recession periods taken as a whole. Some estimates indicate that the NMW increased employment retention for men during the recession period, but they are sensitive to the choice of model specification. There is also some evidence that the NMW upratings had an impact for men in particular years. Most notably the large 2001 uprating may have reduced employment retention and the 2006 and 2011 upratings may have increased it, but these findings are also sensitive to changes in method and specification. For women, we find little evidence of any impact of the NMW on employment retention in any year. For young people, there is some evidence that the NMW may have reduced employment retention in the pre-recession years, especially for workers whose wages were raised most by NMW upratings, but not in the recession period (sample sizes prevent us from obtaining year-specific results).

Impact of NMW on changes in basic hours

We find no systematic effect of the NMW upratings on the basic hours of adults across the years. Especially for men, there is some evidence of impacts (both positive and negative) in particular years but they are not generally consistent across model specifications. Our tentative finding in Bryan et al (2012) that the 2010 uprating may have reduced weekly hours is not confirmed by the full 2010/11 LFS data release, and there is no evidence that the NMW had adverse effects on hours during the recession period as a whole.

For young people, using longer runs of data than in Bryan et al (2012), we see less evidence than previously that the NMW reduced hours. The estimates during the recession period are negative but not statistically significant and they do not differ significantly from the positive (but insignificant) pre-recession effects. As noted in Bryan et al (2012), the estimates for young people are based on relatively small sample sizes and so should be treated with caution.

Impact of NMW on job finding probabilities of the unemployed

We do not find strong evidence that the NMW affected the job entry probabilities of the unemployed. The geographical analysis indicates that individuals in areas with bigger NMW bites had higher job entry rates in the middle years of the 2000s, consistent with previous geographical studies that focussed on aggregate unemployment rates. There is some evidence that this positive NMW effect on job finding then reversed during the downturn. However, the other two methods either show no effect of the NMW on job entry in any year (using predicted hiring wages) or a positive effect during the recession on the job entry (of those women most likely to be hired on the NMW). Estimates within each method are not always robust to changes in model specification and given the additional differences we see across methods, we conclude that there is no empirical support for the hypothesis that the NMW has had an impact on job entry.

Conclusion

Our results add to the small number of studies which have examined the impact of the NMW during the current downturn. Consistent with these studies, the latest findings broadly indicate that there have not been adverse effects of the NMW on labour market outcomes. Future research should aim to refine methods of investigating the impact of the NMW on the unemployed and to add to this small evidence base.

Acknowledgements

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1 Introduction

The National Minimum Wage (NMW) was introduced in 1999 and until the onset of the recession in 2008 it operated in the context of a buoyant labour market when real wages were rising, employment rates were high and unemployment rates were low. Numerous studies examined the effects of the NMW on employment and working hours during this period. They found little evidence that either the introduction of the NMW or its upratings had an adverse effect on employment (Stewart 2002, 2004a, 2004b, Dickens and Draca 2005, Dickens et al 2009, Dolton et al 2009), although the NMW may have slowed employment growth (Galindo-Rueda and Pereira 2004). Studies of working hours found either no impact from the NMW (Connolly and Gregory 2002) or evidence of relatively modest hours reductions (Stewart and Swaffield 2008, Dickens et al 2009).

In mid-2008 the economy entered a year-long recession, followed by a partial recovery but then a second recession from late 2011 until mid-2012. According to the latest data (2012q3), GDP is still 3% lower than its pre-recession peak. Under these conditions, the NMW may have been a more severe constraint on firms than in boom times to the extent that they were less able to absorb cost increases because of falling demand. However, relatively little is still known about the effects of a minimum wage in a recession. The Low Pay Commission (LPC) commissioned a set of projects to analyse employment and working hours in the early part of the downturn (Bryan et al 2012, Dickens et al 2012, Dolton et al 2012) and previous studies looked at past downturns (Dickens and Dolton 2011, Dolton and Rosazza Bondibene 2011). The most recent analyses did not indicate that the NMW was having more severe impacts on employment or hours than before the recession, although there was some evidence of possible negative effects on women's full-time hours (Dickens et al 2012) and on men and women's hours in 2010 (Bryan et al 2012). However, these studies were mainly restricted to examining the 2008 and 2009 NMW upratings because the 2010/11 data had only been partly released.

As well as affecting employment retention and weekly hours worked, the minimum wage could affect the chances of the potentially low-paid unemployed entering work by limiting the ability of firms to create low-paid jobs. There is evidence that hiring wages are more sensitive to the business cycle than existing wages (Martins et al. 2010, Pissarides 2009) which suggests that the minimum wage may be a particular constraint for firms hiring during a recession. However, the vast minimum wage literature has almost entirely focused on the effects on already employed workers, with little or no attention given to NMW impact on the probability of the unemployed entering work. Dolton et al (2009) and Dickens et al (2012) looked at the impact of the NMW on local unemployment rates, but such an aggregate analysis does not distinguish between entries and exits from unemployment. Dickens et al. (2009) took an individual-level approach, investigating the effect of the NMW upratings on job entry, specifically on the probability that someone was not employed at time t-1 conditional on being employed at time t. However, rather than estimating the impact of the NMW on leaving unemployment, this effectively amounts to investigating whether the NMW upratings affected the proportion of new (versus previous) hires among minimum wage workers. Bryan et al (2012) focussed more directly on the probability of leaving unemployment among potentially low-paid individuals.

Building on Bryan et al (2012), this report has two main aims:

1. We update our previous analysis of the effects of the NMW on employment retention, working hours and job entry, using the latest data. In particular, this allows up to obtain a more reliable estimate of the 2010 uprating and to obtain first estimates for the 2011 uprating.
2. We add to the very small evidence base on the impact of the NMW on the unemployed with two methodological extensions. First, rather than relying on a yes/no indicator of predicted NMW status, we allow for the probability that a person will be hired on the NMW rather than being hired higher in the wage distribution. This continuous indicator of 'treatment intensity' allows us to distinguish between individuals likely to be hired only in NMW jobs and those with a good a chance of finding higher paid employment. The second extension is to exploit geographical variation in the bite of the NMW over time. The local area bite of the NMW can also be seen as a measure of treatment intensity, allowing us to compare the probability of leaving unemployment for individuals in areas with a large bite compared to areas with a small bite. These estimates of the impact of the NMW on job entry add to the first evidence on individual-level job entry provided in Bryan et al (2012).

Our analysis uses quarterly data from the Labour Force Survey (1997q2-2012q1) with local area characteristics matched in from the Annual Survey of Hours and Earnings 1997–2011 (Office for National Statistics 2012). We examine the effects of the NMW in Britain during the downturn, focussing on the job retention, hours and earnings of the employed and the job finding probability of the unemployed. Officially the first recession began in the second quarter of 2008 and ended in the second quarter of 2009, and the economy re-entered recession in the fourth quarter of 2011. Therefore the 2008 uprating of the NMW was made during a recession, the 2009 and 2010 upratings were made in an economy undergoing a very weak recovery, and the 2011 uprating occurred just as the economy entered the second recession. In order to assess whether these weak economic conditions affected the impact of the NMW on job retention, hours, earnings and job entry, we compare our estimates for the period 2008–2011 to estimates from 2000–2008.

Overall we find little evidence that the NMW upratings affected employment retention in either the pre-recessionary period or during the recession. Our tentative finding in Bryan et al (2012) that the 2010 uprating may have reduced weekly hours is not confirmed by the full 2010/11 LFS data release, and using longer runs of data than we had previously, we see less evidence that the NMW reduced the hours of young people. Finally, we do not find systematic evidence that the NMW affected the job entry probabilities of the unemployed.

The plan of this report is as follows. In section 2 we explain the importance of studying the effects of the NMW during a recession and present the research questions. Section 3 describes the methodologies used, and Section 4 gives details of the data and variables used. We report the results in Section 5 and draw conclusions in Section 6.

2 Background

In 2008 the UK entered its most severe economic downturn since the 1930s, with output falling by around 7% over a recession that lasted 5 quarters (2008q2–2009q2). This was followed by a weak recovery but in late 2011 the economy re-entered recession for a further 3 quarters (2011q4–2012q2). One possible response of firms to a slump in demand is to try to contain labour costs through reductions or freezes in wages. Data on median pay settlements summarised by LPC (2010) show that the level of pay rises fell sharply from 3–4% in late 2008 to around 1–2% in mid 2009 (with a large proportion of pay freezes), before rebounding somewhat to reach 2-3% in 2011.[1] Data on average weekly earnings (which include additions to basic wages such as overtime payments) tell a similar story: annual increases averaged 4.2% from 2000 to 2007, fell to only 0.2% by 2009 and then recovered to 2.0% in 2010-11.[2]

In the context of overall wage moderation, increases in the minimum wage raise the relative cost of low-wage workers by more than they would during a period of rapid wage growth, and in a recession firms are less likely to be able to absorb such cost increases. Economic theory predicts that in a perfectly competitive labour market, firms will react to increases in the cost of low-wage workers by reducing low-wage employment. If the labour market is not perfectly competitive, there is an offsetting effect because a higher minimum wage attracts more workers into the labour market, and firms may maintain or even expand employment if it is still profitable to do so. Because of the conflicting predictions of theory, the impact in reality is an empirical question. To assess the relative bite of the NMW, Table 1 shows how the annual NMW upratings have compared to annual growth in average weekly earnings (AWE) since 2000. There were some large increases in the NMW in its early years, for example the adult rate increased by nearly 11% between 2000 and 2001, while average weekly earnings rose by only 5%. Rises in the NMW have been smaller in recent years, but even during the downturn adult rates have exceeded AWE growth (with the one minor exception of October 2010 when the NMW increase was 0.01% points less than AWE growth[3]). In particular, the adult rate rose 1% point faster than AWE in the year to October 2009. The picture for the development rate is more mixed: the upratings were higher than AWE growth in 2008 and, especially, 2009, but then fell behind in 2010 and 2011. Particularly for workers aged 21+, the figures suggest that the NMW upratings may have been a real factor influencing firms' employment, and so it is important to know whether they resulted in job losses.

As well as the possible effects on the employment of existing workers, the presence of a minimum wage may also affect the hiring of new workers. There is evidence that the

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[1] Pay rises in 2011 took place against the backdrop of resurgent inflation (rising from -2% in 2009 to 5% in 2011 according to the retail price index), suggesting a reduction in real wage costs to firms.
[2] Increases to October each year. Authors' calculations from ONS average weekly earnings series KAB9.
[3] Authors' calculations based on ONS series KAB9.

wages of new entrants to the labour market are more sensitive to the business cycle than the wages of those already in work (Martins et al. 2010, Pissarides 2009). This implies that a minimum wage may be a particular constraint for firms seeking to recruit employees during a recession, as it forces them to pay a higher wage than they would freely choose. Hence the NMW and its upratings may reduce the chances of the low-skilled unemployed entering work during a recession.

The aim of this study is to assess the impact of the NMW upratings during the downturn, comparing our findings to estimates of the impact of the NMW in the years preceding the recession. This comparison will show whether or not the impacts of the NMW upratings are different during a recession, and so whether the LPC should be more cautious in recommending upratings during fragile economic conditions. We address the following specific questions:

- Do employers react to NMW upratings during a recession by reducing hours or employment more than they otherwise would have?
- What are the implications of the NMW upratings and any resulting hours changes for workers' earnings?
- What are the effects of the NMW upratings during the recession on the probability of the non-employed entering work?

3 Methodology

3.1 Employed

We take advantage of new data to update the analysis in Bryan et al (2012) following the same methodology therein. In particular, we identify the effect of the NMW upratings on employment retention and hours worked from 2000 to 2010, emphasising the comparison between the pre-recession and post-recession periods. We use quarterly data from the Labour Force Survey and adopt a difference-in-difference (DID) approach which closely follows previous studies, and in particular Dickens et al. (2009), Swaffield (2009) and Stewart and Swaffield (2008).

We apply two different versions of the DID approach. The first compares the change in outcome of workers directly affected by the NMW uprating (treated group) with the change in outcome of workers who are slightly higher up the wage distribution (control group). The latter are not affected by the uprating but deemed to be similar in all other respects to those that are affected. This is the standard DID method which compares changes in outcomes over time for a treated and a control group. We label it **horizontal DID.** This approach is based on the assumption that the treated and the control group share a common trend, i.e. that in the absence of the minimum wage uprating the outcomes of the two groups would have evolved in the same way.

To see whether our results hinge crucially on this particular assumption, we then use a second version of the DID approach, which can be understood in two stages. The first stage focuses on a specific point in time and compares differences in outcomes between the Treated and Control groups with the differences in outcomes between two additional control groups taken from further up the wage distribution. We label this first stage **vertical DID.** The assumption underlying vertical DID is that the difference in outcomes between the two additional control groups mirrors that between the treated and the control group in the absence of the treatment. Because this assumption may not be met in practice, we incorporate information from a second stage. The second stage consists of a similar vertical DID exercise performed before the NMW uprating. It offers a direct estimate of the difference-in-differences between the four groups in the absence of treatment (which the standard vertical DID assumed to be zero). Subtracting the second stage estimate from the first stage, we obtain a **triple DID** estimate which arguably provides the most robust evidence of the NMW effect. We now describe each of these approaches in more detail.

3.1.1 Horizontal difference-in-difference

We identify the impact of the NMW upratings on two different outcomes: the change in hours and employment transitions, both over a 6-month (or 2-quarter) period. The annual minimum wage upratings occur in October (i.e. at the beginning of quarter 4 (q4)). We divide individuals into the treated group and the control group based on their wages at a particular point in time t at the beginning of the transition. Employees whose wages at time t lie between the current and upcoming minimum wage are included in the treated group, while those whose wage lies in some range above the upcoming minimum wage are in the control group.

Changes in hours and employment are categorised according to the quarters in which they start. Changes from q4 of the previous year to q2 in the current year, and from q1 to q3 within the current year are observed following the previous NMW uprating, but before the forthcoming uprating, and so are defined as taking place in the "before" period. Changes measured between q2 and q4 of the current year, and between q3 of current year and q1 of the subsequent year, straddle the NMW uprating, and are defined as taking place in the "after" period.

Figure 1 provides an illustration to help understand the logic underpinning the horizontal DID approach. For simplicity, we take the 2004 uprating as an example and focus on employment retention. The vertical solid line on the right shows the point in time (horizontal axis) when this uprating came into effect. On the two vertical dotted lines we report the wage distribution at the time indicated on the horizontal axis. The horizontal markers on the dotted line indicate the level of wages we use to define the treated (T) and control (C) groups. We experiment by using different definitions (as explained below) but for simplicity we only refer to a particular case here. The group of workers whose wages fall between the 2003 MW (the diamond marker) and the 2004 MW (the full circle marker) are the treated group. Those with wages between the 2004 MW and 1.1 times the 2004 MW (the arrow marker) are the control group. That is, we compare outcomes for those affected by the NMW uprating with outcomes for those earning between the NMW and 10% above the NMW. The picture illustrates that we classify individuals based on their wage in a given quarter (say 2003q4) and then observe whether they are still in employment two quarters later (2004q2). All transitions which complete before the vertical solid line are not affected by the minimum wage uprating and therefore falls within our "before" time period. Those straddling the uprating (such as those beginning in 2004q2) fall within the "after" period. The horizontal DID approach looks at the change in outcomes between the before and the after period for each of the treated and control groups. It then takes the difference between these two changes and interprets this as the effect of the treatment (the MW uprating) based on the assumption that in the absence of treatment the change in outcome over time for the two groups would have been the same[4].

[4] This discussion makes clear that our identification strategy relies on the assumption that a given minimum wage uprating does not affect transitions that begins in later quarters, i.e. that an uprating has no effect on transitions occurring in the "before" period of the following uprating. This is admittedly a strong assumption as it is difficult to rule out entirely lagged effects of earlier uprating in the "before" period of the following uprating. Unfortunately, the timing of the upratings and the characteristics of the data at hand do not provide the opportunity to address this point fully. Dickens et al. (2009) presents some evidence that this might not be an empirically important issue, but this is based on simple comparisons of outcomes between the treated and the control groups – a method that also suffers from many clear shortcomings.

Formally, and following the notation adopted in Dickens et al. (2009) we base our analysis on the following model:

$$
\begin{aligned}
y_{t+1} = \; & X'_{it}\beta + \bar{\alpha} + \bar{\gamma} Post_{t+1} + (\alpha_1 + \gamma_1 Post_{t+1}) \cdot I(w_{it} < NMW_t) \\
& +(\alpha_2 + \gamma_2 Post_{t+1}) \cdot I(NMW_t \leq w_{it} < NMW_t^{*})G_i \\
& +(\alpha_3 + \gamma_3 Post_{t+1}) \cdot I\big(NMW_t^{*} \leq w_{it} < NMW_t^{*}(1 + c_1)\big)G_i \\
& +(\alpha_4 + \gamma_4 Post_{t+1}) \cdot I(NMW_t^{*}(1 + c_1 + c_2) \leq w_{it})
\end{aligned}
\qquad \textbf{(Eq 1)}
$$

where $Post$ is a variable which takes the value one if the minimum wage uprating has come into effect at time t+1, and zero if not; I(.) is an indicator function taking value 1 if the expression in brackets is true and 0 otherwise; $w_{it}$is the wage at the beginning of the transition, NMW_t is the minimum wage in place at t, and NMW_t^{*} is the upcoming minimum wage; and c_1 and c_2 determine the width and the position of the comparison group. The vector X includes a set of personal and job characteristics to adjust for systematic differences across workers in job retention probabilities. G_i is a measure of the difference between the existing wage of individual i and the upcoming level of the minimum wage, NMW_t^{*}. We return to this latter variable below.

The coefficient of interest is γ_2 which identifies the effect of the minimum wage uprating on those directly affected by it. Note that the omitted group for comparison is that of workers whose wage at t falls in the interval $[NMW_t^{*}(1 + c_1), NMW_t^{*}(1 + c_1 + c_2)]$. If $c_1 = 0$, this is the group with the wage just above the level of the upcoming minimum wage, but below a certain multiple of it $(1 + c_2)$. For example, a value of $c_2 = 0.1$ means that the highest wage included in the control group is 10% higher than the upcoming NMW. When $c_1 > 0$, an additional group is introduced to allow for the possibility that the NMW uprating indirectly affects even workers whose wage is just above the new level, perhaps because employers raise their wages to maintain differentials relative to the lowest paid workers. γ_3 is the coefficient capturing the effect of the uprating on this "spill-over group". If there are spill-over effects, including this latter group within the control group will bias the results. The other two groups identified by equation (1) are those paid less than the NMW (perhaps because they belong to groups which are not entitled to it, for example participants in some work experience programmes); and the large group of higher paid workers who are excluded from the control group since they not considered to be comparable to minimum wage workers.

We report estimates of γ_2 with $c_1 = 0$ and $c_2 = 0.1$ (no spill-over group), and $c_1 = 0.1$ and $c_2 = 0.2$ to maximise comparability with previous studies (see, for example, Dickens et al. 2009), but the results have proven robust to alternative choices of c_1 and c_2.

Following previous work, to take into account the different size of different upratings, we also estimate a model which introduces an interaction between the treatment indicator and a measure of the difference between the individual's wage and the upcoming level of the NMW. This latter variable is defined as $G_i = \ln\left(\frac{NMW_t^{*}}{w_{it}}\right)$.

We use the model in equation (1) to estimate the effect of all the upratings which took place between 2000 and 2011. To do so, we estimate the model pooling data from all years together, restricting the effects of the control variables (the β parameters) to be the same across years, but allowing all the other parameters (including those capturing the effect of the upratings themselves) to differ from year to year. We therefore allow for differences across treatment and control groups which change over time and identify the effect of each annual uprating.

We estimate separate models for workers entitled to the adult minimum wage and for the youth group entitled to the development rate[5] (until 2009, 18–21; from 2010, aged 18–20). For adults, the models are estimated separately by gender, to allow for differences in the experiences of men and women in the labour market. For both genders, we also obtain estimates of the average effect of the upratings in the years before the recession (2000-2007) and in years affected by the recession (2008-2011). These estimates are obtained from models where the differences between groups are still allowed to vary from one year to the other. For the youth, although we pool men and women together the sample sizes remain too small to obtain reliable estimates of the effect of each annual uprating. We therefore compute only the estimates of the average effect of NMW upratings before and during the recession. These models include year dummies, but restrict the differences between the treated and control groups to be constant over time.

The DID approach rests on two key assumptions: first, that the control group is unaffected by upratings to the NMW, and second, that in the absence of the uprating, the change in outcomes of the treated and the control groups would be the same. The first assumption will not be valid if increases in the NMW affect the wages of higher paid workers. As discussed above, we allow for this possibility by incorporating spillover groups of various sizes into the analysis. The second assumption, sometimes known as the common trend assumption, implies, for example, that in the absence of changes in the minimum wage, the job retention probability of workers paid very close to the minimum wage (the treated group) and those paid slightly above it (the control group) would be affected in the same way by the economic cycle. This hypothesis cannot be tested with the available data because we cannot observe changes in outcomes of the two groups over a period of time with no changes in the minimum wage. We therefore turn to a different but related approach to verify whether the results that we obtain hinge on the common-trend assumption.

3.1.2 Vertical and triple difference-in-difference

The identification problem we face is the lack of a counterfactual scenario in which we can observe both the treated and the control group in the absence of treatment, that is when there are no increases in the minimum wage. In the vertical DID approach the

[5] The adult NMW was payable to employees 22 and older until October 2010, and to those 21 and older thereafter. We however define adults as employees 22 and older consistently across all years.

counterfactual is provided by the difference in outcomes between two additional control groups taken at the same point in time from higher up the wage distribution (Stewart 2004b).

Figure 2 illustrates the vertical DID approach. Here we are only interested in transitions straddling the NMW uprating, so as an illustration we focus on those starting in 2004q2 and 2004q3. For each quarter, we now define four groups. The first two, the treated and control group, are defined as in the horizontal DID. The two additional control groups (C_2 and C_3 are taken from higher up the wage distribution; that is, they are located somewhere in the higher part of the vertical dotted line in the figure). In this approach, we look at the difference in job retention between the treated and the control group and then compare that to the difference in job retention between the two additional control groups. We interpret this as the effect of the MW uprating under the assumption that in the absence of the uprating these two differences would have been the same.

To see clearly the parallels between the two DID approaches, notice that in both approaches four different groups are involved. In the horizontal DID those are the treated group before the uprating, the control group before the uprating, the treated group after the uprating and the control group after the uprating. In the vertical DID, the time dimension is replaced with the position of the wage distribution. Neither of the two additional control groups taken higher up the wage distribution are affected by the NMW. They are therefore the equivalent of the two groups in the "before period" of the horizontal DID. Similarly, the two original treated and control groups are the equivalent of the two groups "after treatment" in the horizontal DID. So, mirroring the *Post* dummy in the horizontal DID exercise, in the vertical DID we can define a binary variable for these two "lower-wage" groups together. Let T_i and C_{1i} be the binary indicators for the original treated and control groups, the lower-wage binary indicator is:

$$LW_i = I(T_i = 1 \vee C_{1i} = 1)$$

Following this logic, it is now clear that in the vertical DID, the treated group and the first of the two additional control groups higher up the wage distribution play the same role as the treated group before and after treatment in the horizontal DID. In the standard representation used for the horizontal DID exercise, a binary variable is included to indicate the treatment group (before and after treatment). In the vertical DID, we can define a binary variable following the same logic. If C_{i2} and C_{i3} are binary variables for the two additional control groups, a binary indicator for T_i and C_{i2} mirroring the treatment indicator in the horizontal DID is:

$$T_i^V = I\left(T_i \;= 1 \vee \; C_{2i} \;\right)$$

This now allows us to write a simple model which resembles the familiar representation of the horizontal DID.

$$y_i = \alpha + \beta_1 T_i^V + \beta_2 LW_i + \beta_3 T_i^V * LW_i + \varepsilon_i$$

It is now straightforward to see that β_3 is a DID estimator obtained in a setting where differences over time (as in the standard horizontal DID) are replaced by differences over the wage distribution.

We estimate this model including demographic controls and by pooling all years together but allowing the coefficients associated with the group dummies T_i^V and LW_i (and their interaction) to vary from year to year. This, as in the horizontal-DID, allows us to estimate the effect of each individual uprating separately. Because the vertical DID focuses on a point in time when treatment is in place, only transitions starting in quarters 2 and 3 of each year (and therefore straddling the uprating at beginning of quarter 4) are included in the estimation sample.

The fundamental identification assumption is that the difference in average outcomes between the two additional control groups is the same as would be observed between the treated and the control group if there was no NMW uprating. Unlike the identification assumption underlying the horizontal DID approach, this allows the economic cycle to have a different effect on the treatment group compared to the control group, provided that this difference is mirrored in the two additional control groups.

If the gap in outcomes between the treatment and control groups (T_i^V and C_{1i}) differed from the gap between the two additional control groups (C_{i2} and C_{i3}) in the absence of a NMW uprating, then the vertical DID estimate would be biased. To relax this "common gap" assumption, we can construct a further counterfactual estimate. For every NMW uprating, we observe both transitions/changes which happen entirely before the uprating and transitions/changes which straddle the uprating. We therefore can obtain a vertical DID estimate from a period when there was no minimum wage uprating. This estimate can then be subtracted from the vertical DID estimate from the period with the uprating, to allow for a difference in outcomes between groups T_i^V and C_{1i} relative to groups C_{i2} andC_{i3}. This is effectively what is often referred to in the literature as a triple DID, where differences are taken both over different groups in the wage distribution (as in the vertical DID) and over time (as in the horizontal DID). In this report we present results from this model in addition to the horizontal DID results.

Figure 3 illustrates the triple-DID approach again by focusing on the 2004 uprating and restricting attention to employment retention. It is immediately clear that this approach is just a combination of the previous two. We define four groups in each period (as in the vertical DID) but then also follow them over time (as in the horizontal DID). The triple DID can therefore be seen as the difference between two vertical DID estimators or, alternatively, as the difference between two horizontal DID estimators.

Throughout this part of the analysis, the treated and the control group are constructed as described in the section on the horizontal DID. The two additional groups are constructing following three different methods, as suggested by Stewart (2004b) and Swaffield (2009):

1. The two groups are taken from wage bands of the same relative width as the first control group – that is if the control group includes workers with wage such that $NMW_t^* \leq w_{it} < NMW_t^*(1.1)$, the next control group includes workers with wage such that $NMW_t^*(1.1) \leq w_{it} < NMW_t^*(1.2)$ and so on.

2. The two groups are selected to be of equivalent sample size as the first control group[6].

3. The two groups are selected to ensure that the differences in median wage between consecutive groups are constant.

We check the robustness of the results to these three different ways of constructing the additional control groups.

3.2 Unemployed

As well as examining the impact of the NMW upratings on employment and hours, we also look at the effect of the NMW on the probability of the unemployed entering work. The identification of the effect of the NMW on the unemployed is hindered by the fact that one cannot know precisely which group of workers is affected by it in the absence of information about their job opportunities. This is an even more salient issue if one tries to look specifically at the effect of the successive marginal upratings of the NMW. In addition, from a substantive point of view, one might argue that what really matters for the unemployed is the presence of a wage floor, while whether this is a few percentage points higher or lower is relatively unimportant. In this part of the analysis we therefore focus on the effect of the NMW as a wage floor (as opposed to the effect of its upratings) and resort to three different methods that offer alternative ways of selecting the unemployed who are most likely to be affected by it.

The first method is the one previously adopted by Bryan et al (2012), which we apply here with new data from 2000 to 2011. Restricting the analysis to 2000-11 allows us to use the preferred measure of the hourly wage (stated by the respondents to the survey rather than derived by ONS) which is available in the LFS from 1999 only (more details in Section 4.1). We therefore implement a **vertical difference-in-difference** approach in which the groups are defined using the *predicted* wage distribution rather than the actual wage distribution. As explained in more detail below, we predict the wage that we would expect an unemployed person to earn based on a model of hiring wages estimated on the subsample of individuals who get jobs. Once the predicted wage distribution is obtained we can then split workers into treated and control groups based on how far from the NMW they are.

[6] To do so, we sort workers by their wage and then by a random number.

This method, therefore, operates a sharp distinction between unemployed workers who are deemed to be affected by the NMW and those who are not. The other two methods we use relax this assumption.

The second approach builds on the idea that potentially every unemployed person could be hired at the minimum wage, although with varying probability. We therefore use a first-stage regression to **predict the probability that an unemployed person is hired at the NMW** and then use that as a measure of the *treatment intensity* in a job entry equation. Because the probability of being hired on the NMW is estimated using socio-demographic characteristics, this approach effectively exploits the variation in the intensity of treatment across demographic groups and over time.

The credibility of this approach in identifying the effect of the NMW hinges on our ability to control effectively for differences between individuals with high and low probability of being at the bottom of the wage distribution that existed even before the introduction of the NMW. We therefore need to use data from 1997 and 1998 as well as data from 1999 onwards, which in turn implies that we can only use the ONS derived measure of hourly pay, in spite of its known limitations (see Section 4.1).

The third and final method enables us (i) to avoid having to classify the unemployed into strictly defined treated and control groups and (ii) to avoid the use of a measure of the hourly wage which suffers from measurement error. In particular, we follow Stewart (2002), Dolton et al. (2012) and Dickens et al. (2012) and exploit the variation in the bite of the NMW across different geographical areas of the UK.

The next subsections discuss the details of each of these three methods which are then applied to study the effect of the NMW on the job entry probability of the unemployed over 6 months (2 quarters)[7].

3.2.1 Vertical difference-in-differences for the unemployed

We use a three-step procedure to study the effect of the NMW on the job entry probability of the unemployed.

First, we estimate a hiring wage equation on the sample of new hires in the LFS, that is the sample of people who move from unemployment to employment over the period of time they remain in the survey. We focus on the sample of new hires rather than on the entire employed population to ensure that our predictions are obtained using a population that is as similar as possible to the unemployed. For example, the unemployed and the employed might differ in ways we do not observe and which might also affect their wages when in employment. This would imply that the wage that we would observe for an unemployed person upon entering employment would be different from the average wage observed for already-employed people with similar

[7] This is done to maximise consistency with the analysis for the unemployed. Note that in Bryan et al (2012) the dependent variable was the job entry probability over two consecutive quarters instead.

observable characteristics. This issue has been recognised in the literature, but is rarely addressed.[8]

The hiring wage equations are estimated using a tobit model which takes into account the left-hand censoring (or spike) of the wage distribution caused by the NMW. We estimate models separately for each four consecutive quarters covered by the same level of the NMW[9]. The variables included in these models include both standard demographic characteristics and information collected specifically about the unemployed, such as previous occupations, and search method used etc.

In the second step of this method, we use the estimates from the tobit models to predict a hiring wage for each unemployed individual in our sample, based on their demographic characteristics and unemployment history etc. Individuals are then assigned to treated or control groups depending on where on the predicted wage distribution they fall. The treated group comprises the unemployed with a predicted hiring wage between 95% and 105% of the NMW in place at a given point in time[10]. The first control group includes the unemployed with a predicted hiring wage between 105% and 115% of the NMW[11]. The two additional control groups necessary to perform the vertical DID exercise are selected using the first two methods described for the employed[12].

The third and final step consists in estimating the effect of the NMW on the job entry probability of the unemployed using the same regression model described for the employed. Because of the presence of a generated regressor in this model, we obtained the standard errors for these equations by bootstrap.

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[8] One exception is Neumark and Adams (2003). After acknowledging that there is no credible way to identify the selection mechanism, they check the robustness of their results when predicted wages for the unemployed are reduced by a certain percentage, on the grounds that it seems reasonable to assume that the unemployed would command lower wages than the employed.

[9] In practice this is done in a way that has to take into account how the LFS data are collected. In fact, employed workers are only asked about their wage in their first (wave 1) and last (wave 5) interview. Because we need to focus on people whom we observed unemployed at some point in the past, we must restrict attention to wave 5. So we take everyone with a wage in wave 5 in each quarter and then check whether in any of the previous four waves they appear unemployed. We then take the explanatory variables used to estimate the hiring wage equation from the last wave when that individual was recorded as unemployed.

[10] Arguably, even those with a predicted wage below 95% of the NMW should be included in the treated group. In fact, given that the wage floor is legally binding, they too would be hired at the NMW and are affected by it. However, people with very low predicted wages can also be seen as much less likely to enter work. Their inclusion in the treated group could therefore yield a difference in outcomes (job finding probability) between the treated and the control group which is unlikely to be reflected in the difference between the two additional control groups. This would then violate the identification assumption underlying the vertical DID approach adopted here.

[11] We have experimented with different thresholds to define the control groups and the results are not substantively affected.

[12] Cell sizes were too small for the third method (groups selected to ensure fixed difference in median wages) to be used.

3.2.2 Using the predicted probability of being hired at the NMW

To estimate the effect of the probability of being hired on the NMW on the job entry probability, we implement the following two steps.

We first use the same samples of new hires described for the V-DID approach to estimate the probability that an unemployed person is hired at a wage which is less than 105% of the current NMW[13]. Similarly to what we do for the tobit models described in the previous section, these probits are estimated separately for each set of consecutive quarters covered by the same level of the NMW. As explained below, we also use data from before the introduction of the NMW in this approach. For 1997 and 1998, in order to obtain the level of wage at which we classify a new hire, we deflate the 1999 NMW by average earnings growth. The results from these models are then used to predict the probability of being hired at the NMW for all the unemployed individuals in our sample.

In the second step, we estimate the following linear probability model:

$$\Pr\left(E_{iq+2} = 1 \middle| U_{iq}\right) = \alpha + \beta_1 \widehat{\Pr}(NMW)_{iq} + \sum_y \delta_y D_y + \sum_y \gamma_y D_y \widehat{\Pr}(NMW)_{iq} + X'_{iq}\beta$$

where the dependent variable is the probability of being in employment at quarter q+2 given unemployment at quarter q, $\widehat{\Pr}(NMW)_{iq}$ is the predicted probability of being hired at a wage lower than 105% of the NMW, and D_y are time dummies which take value 1 for each set of four consecutive quarters[14] covered by the same level of the minimum wage (i.e. from quarter 4 of a given year to quarter 3 of the following one). The estimation period goes from 1997q2 to 2012q1 and therefore includes a sub-period (1997q2-1999q1) when the minimum wage was not in place. It follows that this can be seen as a difference-in-difference exercise where β_1 captures the average difference in job entry probability between high- and low-pay unemployed which existed even before the minimum wage was introduced. The interaction terms of the time dummies and the estimated probabilities then pick up the additional difference in job entry arising *after* the introduction of the NMW between unemployed individuals with a high and low probability of being hired at the bottom of the wage distribution. The coefficients γ_y are therefore the focus of interest for our analysis.

3.2.3 Using geographical variation in the bite of the NMW

For this method, we build on Stewart (2002), Dolton et al. (2012) and Dickens et al. (2012). We combine data from two sources: we use the Annual Survey of Hours and

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[13] Unlike in the case of the predictions based on tobit models, we do not distinguish between workers hired below the NMW and at the NMW here. This is because while in the approach using tobit predictions we can account for differences between these two groups by defining suitable dummies, in this case if we predicted the probability of being hired at some interval around the NMW, we would end up effectively treating in the same way individuals with lower probabilities but on the two different sides of the interval of interest.

[14] The first of these dummies is actually one for the quarters from 1999q2 through 2000q3 – which were all covered by the same level of the NMW.

Earnings to construct two alternative measures of the geographical bite of the minimum wage (discussed in more detail in Section 4.2), and the Labour Force Survey, again, to look at individual transitions out of unemployment.

We then estimate this equation on data from 1997q2 to 2012q1:

$$\Pr\left(E_{q+2} = 1 \middle| U_q\right) = \alpha + \beta_1 \text{Bite}_{gy} + \sum_y \delta_y D_y + \sum_y \gamma_y D_y \text{Bite}_{gy} + X'_{iq}\beta + \eta_g$$

where η_g is an area fixed-effect and Bite_{gy} is the measure of the bite in area g in year y, and the time dummies D_y identify each set of four quarters covered by the same level of the NMW. This exercise can therefore be interpreted as a difference-in-differences method where β_1 captures differences between high and low paid areas which existed even before the introduction of the NMW, while the $\gamma's$ are the coefficients of interest which pick up the additional differences arising after the introduction of the NMW. The identification comes from variation in the bite of the NMW within areas over time (given the presence of area fixed effects) It is therefore important to control for factors that might be correlated with features of the wage distribution (which determine the bite) and also affect the probability that an unemployed person enters work. We therefore also augment the equation above with controls for local labour market characteristics (i.e. the share of manufacturing, construction and private service employment, and the share of employment in small and medium firms). In addition, we use the (lagged) change in the local employment rate to account for changes in the economic cycle. This is important to prevent the coefficients of interest from being biased by any possible correlation between the intensity of the economic expansion or contraction and the general level of pay in an area (for example, if the recession disproportionally hits low/high pay areas, therefore affecting the chances of the local unemployed to find work).

We exploit the differences in the time of collection of LFS and ASHE to strengthen our identification strategy. The ASHE data are collected in April each year, therefore providing us with a measure of the wage distribution in each area at the beginning of quarter 2 of year y. We use this information to measure the bite of the forthcoming NMW, due to come into effect at the beginning of quarter 4 of year y[15]. The bite at q2 is then used to explain all transitions from unemployment over 2 quarters starting before

15 For the period before the introduction of the NMW, Bite_{gy} is constructed using the information on the wage distribution from April 1997 and April 1998 and the level of the first NMW (introduced in April 1999) deflated using the average earning index. The use of ASHE 1999 is more problematic since it coincides with the introduction of the NMW (April 1999). One could therefore argue that the wage distribution measured at that point is already affected by NMW whose bite we want to use to explain subsequent transitions and therefore should not be used. For this reason, we have obtained both estimates that treat ASHE 1999 as all other available waves of ASHE, and another set of estimates that disregard ASHE 1999 and use the wage distribution from ASHE 1998 to compute the bite for the 1999 NMW. Because the results do not differ substantively, we report here only those that do not use ASHE 1999.

the next measure of the wage distribution becomes available in the second quarter of year y+1. Note that all these transitions are therefore affected by the NMW which comes into effect in quarter 4, either because they straddle the uprating or because they start after it. Also, they all end before the next uprating. The fact that our bite measure is constructed from a different survey and it is measured before the transitions that it is supposed to explain minimises the concerns that it might be affected by reverse causality.

Stewart (2002) uses a similar model to study the effect of the introduction of the NMW on job retention among the employed. Dolton et al. (2012) and Dickens et al. (2012) have used (versions of) this approach to look at the effect of the NMW on aggregate local unemployment. While such an approach is certainly informative, it does not identify the effect of the minimum wage on the chances that an unemployed person finds a job, since the level of unemployment is the net result of inward and outward fluxes of which that towards employment is only one.

4 Data, sample sizes, specification

4.1 Labour Force Survey

We use data from the Secure Data Service edition of the Labour Force Survey (LFS) (Office for National Statistics 2011), which contains more information, especially for the unemployed, than the standard edition of the LFS. For the employed, we focus on the effects of the minimum wage upratings, given the large existing literature on the effects of the introduction of the NMW. Therefore we use data from 1999q4 to 2012q1. For the unemployed, we also make use of data from before the introduction of the NMW, back to 1997q2.

We use data from the quarterly cross-sectional datasets and match them across quarters to follow individuals over time. Each individual remains in the LFS sample for 5 consecutive quarters at most. As explained in section 3, we look at labour market transitions and changes in hours worked from quarter q to quarter q+2.

LFS data have already extensively been used in the analysis of the effects of the NMW. Two wage measures are available, namely a self-reported hourly wage (hrrate) and derived hourly pay (hourpay). The relative merits of the two have been widely discussed (see for example Stewart 2004a,b Dickens and Manning 2004, Dickens and Draca 2005 and Dickens et al. 2009). We follow most of the literature in preferring hrrate on the grounds that its distribution exhibits a much clearer spike at the minimum wage and is generally regarded as being less affected by measurement error. However, for one of the approaches we take to study the unemployed, we require a measure of the hourly wage for those unemployed who do find a job in the period before the introduction of the NMW. Unfortunately for such a period of time, the only available measure is the ONS derived hourpay.

As explained in section 3, the first two methodologies we use to study the effects of the NMW on the job finding probability of the unemployed require a first stage estimation of the hiring wage (in a tobit model) or the probability of being hired at the minimum wage respectively (in a probit model). We define as "new hires" individuals we observe employed (that is, who report a wage) at a given quarter after having being unemployed at some earlier quarter. Note that we restrict attention to those unemployed according to the ILO definition of unemployment. Inactive individuals are excluded from the sample. We estimate a hiring wage equation separately for each of the four consecutive quarters covered by the same level of the NMW. For example, quarters from 2004q4 to 2005q3 are covered by the same level of the NMW and are pooled together and labelled as 2005 for convenience in our analysis. New hires are included in the year in which they were last observed as unemployed. In the LFS, individuals are asked about their wage in wave 1 (that is, upon entering the survey) and in wave 5 (that is, the last quarter they are interviewed). Since we need to observe newly employed workers before they were employed in order to be able to predict the wage for the unemployed, we necessarily restrict our attention to workers in wave 5 in each quarter. In order to maximise sample sizes, we pool men and women together when estimating the hiring wage equations (see section 4.1.1).

Both the tobit and probit models include the following demographic variables: age, gender, marital status, highest level of education, region, ethnicity, number of children, age left education, and whether the respondent has health problems. In addition, we include the following variables relating to their unemployment spell: whether ever worked, previous occupation if ever worked, whether worked as an employee, method of job search and activity before actively looking for work. The second stage equations for the unemployed (and the equation exploiting variation in the geographical bite) all include this same set of controls.

The controls we use in the DID equations for the employed include all the demographic variables listed for the unemployed plus additional job characteristics: in particular industry, public sector, occupation, and tenure.

In the regressions that exploit the variation across regions in the bite of the NMW, we also include a region-level control for the economic cycle which uses data from the LFS. This is the change in the employment rate within a region. We have used both the change between quarter q and q-1 and that between q-1 and q-2 and since the use of one or the other does not have substantively change the estimates of interest, we only report results obtained using the former definition. To ensure that the cell sizes used to compute the employment rates always remain of a reasonable size, we use the overall employment rate for both genders combined, even when we estimate equations for men and women separately.

4.1.1 Treated and control group sample sizes

Table 2 reports sample sizes for the treated and control group in the horizontal DID exercise. These refer to the employed and are presented separately by gender and for the before and after periods. The table shows that cell sizes are generally larger for adult women than for adult men, reflecting the fact that more women are found at the bottom of the wage distribution.

Cell sizes for youth are too small to provide reliable estimates for the individual upratings and therefore, following previous studies, we present results based on samples pooled across gender and across groups of years (recession vs pre-recession) for those aged below 22. Even with this level of aggregation, we only have 230 individuals in the treated group before the recession, and fewer than 130 in after the start of the recession.

Table 3 reports cell sizes for the additional control groups constructed for the vertical DID exercise. See section 0 for the details on how such groups are constructed under different methods. The table shows that methods 1 and 2 return satisfactory cell sizes, while method 3 – groups defined to maintain a fixed distance between their median wages – leads to some small or even empty groups, especially for the more recent years.

(This is because there are fewer workers in the region above the NMW, as compared to the spike around the NMW itself, and the difference between the median wages of the original treatment and control groups may be very small.)

Table 4 reports cell sizes for the sample of new hires which are used in the first stage estimations when the unemployed are studied. New hires are classified as such only if the wage information is non-missing. Two different wage variables are used because the self-reported hourly rate (hrrate) is known to be more accurate, but is not available before 1999, while the ONS derived hourly rate (hourpay) is more affected by measurement error, but it is available from 1997 (which is necessary for the method using the predicted probability of being hired at the minimum wage). The table shows that sample sizes are larger when hourpay is used reflecting the fact that this variable has fewer missing values, particularly among workers who are not actually paid an hourly rate (who are more likely to be away from the bottom of the wage distribution).

Table 5 reports sample sizes for the treated and control group for the vertical DID exercise for the unemployed. As described in section 3, the unemployed are assigned to different groups based on the wage predicted by tobit models run on the sample of new hires (based on hrrate) in every year.

4.2 Annual Survey of Hours and Earnings

To derive measures of the geographical bite of the NMW, subsequently matched into the LFS data, we use the Annual Survey of Hours and Earnings (ASHE), 1997-2011. ASHE is based on a one-percent sample from Inland Revenue PAYE records, containing about 170,000 individuals (temporarily reduced to about 140,000 in 2007-8).[16] The same individuals are followed from year to year and every April their employers provide information on their age, sex, wages, hours of work, and other job related characteristics. ASHE was introduced in 2004 but data going back to 1997 were derived by applying the ASHE methodology to its predecessor, the New Earnings Survey (NES). The 2004 introduction of ASHE, which included supplementary survey information and introduced imputation and weighting, represented a significant structural break and there are other discontinuities stemming from changes applied in 2006 and 2007-8. However, ONS (2007) reported that the effect of the 2006 changes was small overall, and LPC (2012, Figure 2.7, p30) indicates that the 2004 and 2006 changes had only a small effect on median wage measures.[17]

[16] In 2004, it was supplemented by a sub-sample drawn from the Inter Departmental Business Records to cover businesses registered for VAT but not PAYE.

[17] The discontinuities arise from: (i) the addition of supplementary information (surveys) at the introduction of ASHE in 2004, to improve coverage of low paid workers; (ii) a small correction to the weights in 2006 to allow for higher response among firms with special electronic reporting arrangements; (iii) changes to the calculation of weights in 2006 owing to the introduction of automatic occupation coding; (iv) the sample cut of about 30,000 returns in 2007-8.

ASHE includes geographical indicators at different levels. We use indicators of the county, Unitary Authority (Council Area in Scotland) or group of London boroughs (Inner or Outer London) in which a workplace is located. We can match this geography of 140 areas to LFS over the full period 1997-2011.[18]

We use data on all employees aged 18 or over, dropping those whose pay was affected by absence (7%) and those with missing area codes (0.2%). We use the ASHE measure of average hourly earnings, excluding overtime, for the reference period (variable hexo). We consider two alternative measures of NMW bite: the proportion of employees in the local area affected by the forthcoming NMW; and the ratio of the forthcoming minimum wage to median hourly earnings in the local area (the Kaitz index).

To calculate the first measure, we define a minimum wage worker as an employee earning, in April of a given year, less than the forthcoming minimum wage in October of that year (plus 5 pence, following LPC 2012).[19] For each employee we use the age-specific NMW rate (adult or development). As ASHE does not contain information on apprentice status, it is not possible to distinguish employees on the apprentice rate (or other adult trainees receiving the development rate). The bite is calculated as the proportion of minimum wage workers in a local area, and we produce separate measures for all employees, women and men.

For the Kaitz index, we calculate median earnings using the 18+ sample and then derive separate measures based on the adult rate and development rate; for adults we also derive separate Kaitz indices based on the median hourly earnings of men and women. Both the NMW proportion and the Kaitz index are weighted to reflect population totals across occupations, age, sex and region.[20] Finally, we derive some local area characteristics to use as controls in the main analysis: the (weighted) share of employment in manufacturing, private services and construction, and in small firms (1-49 employees) and medium companies (50-249 employees). For all these measures it is important to have sufficient cell sizes in ASHE. For the measures based on all employees in an area (including the employment shares), the median cell size is 522 (with a minimum of 57). For those measures using data on men or women only, the median cell sizes are 261 (men) and 258 (women), with minima of 25 (men) and 26 (women).

4.2.1 Variation in geographical bite

Identification of NMW effects on unemployment transitions in the geographical analysis requires sufficient variation in the NMW bite both across areas and over time. Figure 5 shows box plots of the NMW proportion (top panel) and Kaitz index (lower panel) for

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[18] For compatibility with the LFS, we combine Orkney, Shetland and Eilean Siar into one area.

[19] We omit 1999 because the NMW was introduced in April 1999, coinciding with ASHE data collection. For the pre-NMW years of 1997 and 1998, we construct a “potential bite” measure using the 1999 level of the NMW (deflated by the average earnings index). In the analysis, this variable controls for a general low pay effect on unemployment transitions in the absence of the NMW.

[20] The NMW proportion is weighted using low-pay weights produced by ONS, while the Kaitz index is weighted using the standard calibration weights.

each year since the introduction of the NMW.[21] Both measures show that the bite increased reasonably sharply in the early 2000s, corresponding to large increases in the NMW relative to average earnings growth. The bite then stabilised in the second half of the decade, although with somewhat more variation in the NMW proportion than the Kaitz index. Overall the median NMW proportion rose from just over 0.04 in 1999 to 0.07 in 2011, and the median Kaitz index rose from about 0.52 to 0.59. In all years there is considerable variation over local areas, for instance in 2011 the inter-quartile range of the NMW proportion is (0.06, 0.09) and of the Kaitz index it is (0.55, 0.62). Similar plots (not reported) for men and women separately show comparable patterns over time, but with the bite being markedly higher among women owing to their lower average hourly earnings: in 2011 the median NMW proportion is 0.09 for women and 0.05 for men; the median Kaitz is 0.65 for women and 0.53 for men.

After confirming that there is substantial variation in the bite measures, we now check whether a larger NMW bite in a given year compresses the lower part of the wage distribution in the next year, as we would expect if the NMW operates as intended. In Table 5 we report the results of regressions of changes in lower tail inequality on the initial NMW proportion.[22] We use two measures of inequality, the log of the ratio of median hourly earnings to the 5th percentile and the ratio of the median to the 10th percentile. The regressions in columns (1) and (4) include no controls, columns (2) and (5) control for observed area characteristics, and columns (3) and (6) also include area fixed effects. We only use data covering the upratings from 2000 onwards because the 1999 NMW introduction coincided with ASHE collection (i.e. the 1998-1999 change in inequality would not straddle the NMW).

The results indicate that in areas/years with a larger bite, there was a subsequent reduction in lower tail wage inequality. The exception is for the 50th/5th percentile ratio when area fixed effects are included (column (3)). However, this may not be surprising because, as shown in Figure 4, from 2004 onwards more than 5% of employees are affected by the NMW in most areas (2009 excepted). The 5th percentile already falls within the NMW in these areas, reducing geographical differences over time as the NMW increases. By contrast the impact of the bite on the 50th/10th percentile ratio is robust across specifications (columns (4)-(6)). With area fixed effects included, we conclude that a 5 percentage point increase in the proportion affected by the NMW leads to a 0.290*0.05 = 1.5% reduction in the growth of the 50th/10th percentile ratio.

[21] The boxes indicate the inter-quartile range of the bite, the centre line is the median, the ends of the lines are the adjacent values, and the dots are outside values.

[22] We do not run similar regressions using the Kaitz index as median hourly earnings enter the calculation of both the Kaitz index and the inequality measure, resulting in a mechanical correlation between the two.

5 Results

5.1 Employed

5.1.1 Employment retention

We first present the results for the probability of remaining in employment over a 6 month period obtained from the horizontal DID exercise. We report the coefficients obtained using a linear probability model (LPM), but the estimates are substantively the same when the marginal effects from a logit model are considered. The tables report estimates obtained when using both a simple treatment indicator and when using its interaction with the wage gap measure, as described in section 3. In addition, we show results with and without controls, and with and without a spill-over group separate from the reference control group. For adults, we report estimates for each individual uprating (Table 7 for men and Table 8 for women) and also the average effect over 2000-2007 and 2008-2011 separately (Table 9). These latter estimates are also accompanied by the results of a Wald test for the null hypothesis that the two effects are the same. For the youth group , we only report estimates for the two aggregate periods due to small sample sizes (Table 9).

Table 7 reports the estimated impact on employment of the NMW upratings for adult men. The first four columns show results obtained using a simple binary treatment indicator, while in the remaining four columns this is interacted with a wage gap variable. The estimated coefficients are generally small and statistically insignificant and do not seem to be affected systematically by the inclusion of controls or by allowing a spill-over group. Notable exceptions to this pattern of small and insignificant estimates are the 2001 and 2006 upratings. The estimates associated with the 2001 uprating indicate a negative impact on job retention, of between 9 and 18 percentage points depending on model specification. This negative impact on job retention may be due to the larger than average increase in the NMW in that year, which was more than double that of average wage growth (see Table 1). Our estimates are consistent with, if larger than, those reported in previous studies (e.g. Dickens et al 2009). The estimates associated with the 2006 uprating indicate a positive impact on job retention in excess of 10 percentage points across the specifications. Again, this is consistent with previous research – for example Dickens et al. (2009) also found some evidence of a positive (but smaller) effect of this specific uprating. However we find little evidence of negative employment retention effects of the NMW during the recent recession – from 2008 onwards the estimated coefficients are relatively small and imprecisely estimated. The exception is for 2011, where again there is evidence of positive retention effects although these are not robust to the inclusion of a spill-over group in the model.

The top panel of Table 9 shows that for adult men we detect some positive employment retention effects of the NMW upratings during the recessionary period since 2008, of between 5 and 7 percentage points. We also find some weak evidence that the average effects of upratings differ between the pre-recession and recession periods. For example, in specification (2) the Wald test for equality of coefficients rejects the null hypothesis that the estimates for pre-recession and recession periods are the same. However this is not robust across specifications, particularly in those including a spill-over group.

Table 8 shows that the estimated impacts on employment of the NMW upratings for adult women are generally smaller than those for men and are nearly always statistically insignificant across model specifications. The middle panel of Table 9 indicates that there are no statistically significant differences in the impact of NMW upratings in the pre-recession period compared to during the recession.

The bottom panel of Table 9 focuses on the estimated impact of the youth NMW upratings (which applies to 18-21 year olds). For this group of workers, we find some evidence that the NMW upratings had negative employment retention effects in the pre-recessionary period of between 10 and 14 percentage points. These emerge most notably in specifications in which the treatment indicator is interacted with the wage gap measure (specifications 5-8), suggesting that the largest NMW upratings had the biggest (negative) impacts on job retention. This pattern does not emerge during the recessionary period. Furthermore, Wald tests for equality of coefficients across the two time periods do not reject the null hypothesis that the estimated impact of the NMW upratings were equal prior to and during the recession.

The horizontal DID estimates are based on the underlying assumptions that the employment retention probability of the treated and control groups would follow the same trends in the absence of the treatment (the NMW uprating). In order to check the robustness of our results to these underlying identification assumptions, we turn to the estimates from the vertical DID models. In particular, we report results from the triple DID where we compare the DID estimates over time for the treated and control group with the DID estimate for two additional control groups from higher up the wage distributions. We therefore relax the assumption that the DID estimate would have been zero in the absence of the minimum wage uprating and instead assume that the counterfactual for what would have happened in the absence of treatment is represented by these two additional control groups. Three different methods are adopted to define these groups (see section 3.1.2), with the results presented in Table 9, 10 and 11. Each pair of columns in these tables is obtained using the same group definitions[23], with the first column in the pair reporting results for the model with controls and the second column those without controls.

The results of the triple DID exercise are largely consistent with those from the horizontal DID. For men (Table 10) we obtain estimates which are generally small and statistically insignificant. The estimates for 2001 under methods 2 and 3 when

[23] When using method 3, we obtain very small (and even empty) cells for some of the years under consideration. This leads to the missing estimates in columns 5 and 6 of these tables.

introducing additional controls (columns 4 and 6) stand out as they are statistically significant at the 5% level and very large – suggesting a negative impact on employment retention of 16–18 percentage points. The estimates for the same year obtained using method 1 to construct the control group (shown in column 2) are considerably smaller and statistically insignificant. Nevertheless we find further evidence suggesting that the relatively large NMW uprating in October 2001 had negative impacts on employment retention. However the estimates from the triple DID indicate that the 2006 uprating had no statistically significant impact on employment retention, unlike the evidence from Table 7. Overall, therefore, the support for significant positive effect in 2006 is weaker than that provided by the horizontal DID estimates. Hence estimates of the effect of NMW upratings on employment retention are sensitive to assumptions about the counterfactual. The last row of Table 10 shows that the positive retention effect in 2011 is larger in the triple DID than in the horizontal DID exercise, at least when using methods 1 and 2 (columns 1-4). These indicate that the NMW uprating in 2011 was associated with a 25 percentage point increase in employment retention. However the estimates using method 3 are small and statistically insignificant, making it difficult to draw robust conclusions.

Table 12 reports the average impact of the NMW upratings during the pre-recession years 2000-2007 and the average impact during the recession years 2008-2011, together with a Wald test of whether the impacts are equal. Estimates in the top panel of the table indicate that significant differences emerge in the average effect of the NMW upratings before and during the recession – as confirmed by the results of the Wald tests in the same table. In particular, in the pre-recession period between 2000 and 2007, we find that the NMW upratings had no statistically significant impacts on employment retention among men. This is consistent with the estimates from the horizontal DID exercise presented in Table 9. However we find positive and relatively large and positive impacts on employment retention among men during the recession period, from 2008 to 2011. These are of the order of 8-11 percentage points, and are statistically significant when using methods 1 and 2 (columns 1-4). These are also consistent, but more robust across specifications, with estimates from the horizontal DID.

Table 11 shows estimated coefficients for women that are mostly small and statistically insignificant across models. There is some weak evidence of a negative – and sizeable effect – in 2005. Using method (3) to define the counterfactual yields estimates which suggest the NMW upratings reduced job retention rates among women by more than 6 percentage points, but these are statistically significant only at the 10% level (columns 5 and 6). There is also evidence of a positive effect in 2009, but this only emerges when defining the counterfactual using method 1, and is statistically significant only in the absence of other controls. The estimates in Table 12 indicate that for women the average effects for the pre-recession and the recession periods remain predominantly positive across models, are very small in magnitude, and are statistically insignificant.

Furthermore the Wald tests do not reject the null hypothesis of equal coefficients in the pre-recession and recession years.

Taking all the results together, we see little evidence that for women the NMW affected employment retention in either the pre-recessionary period or during the recession. Generally the estimates are small and imprecisely estimated. For men, we find a number of patterns worth commenting on. Firstly, the relatively large uprating of the NMW in 2001 reduced employment retention. This emerges across specifications and estimation methods. Secondly, the uprating of the NMW in 2011 appears to have increased job retention, although this finding is not fully robust across model specifications and estimation methods. Thirdly, we find some evidence that the NMW upratings during the recession period had positive impacts on job retention relative to those in the pre-recession period. However this latter effect is predominantly driven by the impact of the NMW uprating in 2011, hence it is difficult to generalise this across the recession more generally.[24]

5.1.2 Basic hours

Table 13 through Table 15 report the results for the horizontal DID estimates for the effect of the NMW upratings on the change in weekly usual basic hours over a 6 month period.

The results for the sample of men reported in Table 13 show that the estimated effects of the upratings across years vary in statistical significance, in magnitude and in sign. For example, there is some evidence that the 2001 uprating was associated with a fall in basic weekly hours of 2-3 hours per week, while the 2004 uprating was associated with an increase in basic hours of around 2 hours per week. Furthermore, this latter effect was significantly larger for those whose wage required a larger adjustment to comply with the new level of the NMW. This is shown by the positive estimates in columns 5 to 8 where the treatment indicator is interacted with a wage gap measure. In contrast, the estimates for 2002 and 2006 are all negative but in general are not statistically significant at conventional levels. The estimates for the recession period are generally small, not statistically significant and of varied sign. There is some evidence of a positive impact on hours in 2008 of about 1.6 hours per week, but this is not robust across model specifications. Bryan et al (2012) found preliminary evidence that the 2010 uprating had reduced men's weekly hours but this is not confirmed using the latest LFS data that include all 6-month transitions straddling the 2010 increase: estimates are generally small and insignificant.

[24] Caution should also be taken when focusing on individual statistically significant coefficients in a context in which many regressions are estimated. This is because, for any given level of significance level, it is to be expected that some estimates will be statistically significant even if the true effect is in fact zero.

The estimates presented in Table 15 suggest that before the recession the NMW upratings had generally negative impacts on basic hours, although these effects are small and statistically insignificant. In the recession period, the estimated effects are positive, although again largely small and statistically insignificant. The exceptions are in specifications (3) and (4), where we include a treatment indicator with spillover groups. Here we find the NMW upratings increase basic hours by about 1 hour per week, which is statistically significant at the 10% level. However the Wald tests presented show that there are no systematic differences between the average effects of the upratings on hours before and during the recession among adult men.

Table 14 reports the results for women's basic hours. The estimated effects of the NMW upratings on hours are all small (less than an hour and in most cases less than half an hour) and statistically insignificant. Furthermore there is no consistent pattern in the direction of the effect. The average effects reported in the second panel of Table 15 are generally positive prior to the recession and negative during the recession, but again these are not statistically significant either from zero or from each other.

The estimates for 18-21 year olds are shown at the bottom of Table 15 and do not suggest that the NMW upratings impacted on their weekly hours. Bryan et al (2012) found evidence of negative effects on youth hours, in particular during the downturn, but they cautioned that the results were based on small sample sizes. These updated estimates include three more years of data (2000-2002) before the recession and one more year afterwards (2011). Although we find that the estimated effects during the recession are all negative, reducing basic hours by up to 1.8 hours per week, these effects are not statistically significant. The Wald tests, however, indicate that the effects pre-recession differ significantly (at the 10% level) from those during the recession in specifications that include controls (columns 2, 4, 6 and 8). Hence when adjusting for other characteristics, we find some weak evidence that for young people, the NMW upratings reduced basic hours during the recession relative to the pre-recession period.

Table 16 through Table 18 report the estimates obtained by the triple difference approach. For men the results in Table 16 are broadly consistent with those from the horizontal DID exercise (shown in Table 13). In particular, there is some support for negative effects of the 2001 and 2006 upratings on hours worked, and a positive effect for the 2004 uprating. The triple difference estimates tend to be larger in magnitude. For example, these suggest that the 2001 uprating reduced basic hours by up to 4 hours per week (columns 3 and 4), while the 2006 uprating reduced hours by up to 5 hours per week. Estimates during the recession period are almost all positive but they are generally not statistically significant (with the exception of method 3 in 2008). The estimated average effects pre-recession and during recession presented in Table 18 reveal larger positive effects of NMW upratings during the recession than in the earlier period. Estimates from method 3 (shown in columns 5 and 6) suggest that during the recession the NMW upratings increased basic hours by almost 3 hours per week, and these effects are significantly different from the near zero estimates for the pre-recession period. However no statistically significant differences between the pre-recession and the recession period emerge using the other methods.

For women, estimates from the triple DID exercise shown in Table 17 also portray a picture similar to that of the horizontal DID (Table 14). In fact, the estimates are generally small and statistically insignificant, with the exception of those for 2002, 2004, 2006 and 2011 when using method 3. For 2002 and 2011, the estimates from method 3 indicate that the NMW upratings had a positive effect on hours worked by between 1-2 hours per week, while for 2004 and 2006 indicate negative effects of similar magnitude. However the estimated effects using other methods are smaller and not statistically significant. The average estimates in Table 18 are negative for both the pre-recession and the recession. They appear slightly larger than that from the horizontal DID, but are not found to differ significantly between the pre-recessionary and recessionary periods, as indicated by the Wald tests.

Overall, we find little robust evidence that the upratings affected job retention during the recession taken as a whole for men, women or young people.[25] In the pre-recession period, there is some evidence that the upratings may have reduced job retention among the youth group, but no effects among adult men and women. However, there is some evidence of impacts (both positive and negative) in specific years. In particular, we find some evidence that the relatively large uprating of 2001 reduced hours and employment retention among men, while the uprating in 2011 increased job retention among men.

5.2 The effect of the NMW on the job entry probability of the unemployed

5.2.1 Vertical DID using predicted hiring wages

The estimates of the effect of the NMW on the probability of the unemployed entering work are presented in Table 19 to Table 21. All of these estimates are from linear probability models where the dependent variable is the probability of moving from unemployment to employment over a 6 month period. The results are from a vertical DID exercise where the unemployed are grouped into treated and control groups based on their predicted hiring wages. This prediction is from a hiring wage equation which is estimated separately for each year using a tobit model to take into account the left censoring induced by the minimum wage. These first-stage tobit models always include all the controls discussed in section 3.2.1. We report results from the second-stage linear probability models with and without controls, as indicated at the bottom of the tables. Two different methods (discussed in section 3) are adopted to define the treated and the control groups. Results for the two methods (with and without controls) are reported in columns 1–2 and 3–4 of the tables respectively. To account for the

[25] Some model specifications indicate that the upratings increased job retention among men during the recession, but this result appears mainly to reflect a particularly large estimated effect in 2011.

additional variation induced by the presence of the first step tobit models, the standard errors presented here have been bootstrapped[26].

Table 19 presents the results for men. The estimated coefficients are never statistically significant and there is no clear pattern to the estimates before or during the recession, although the estimates are consistently negative for 2010 and 2011 (ranging from -1.2 percentage points to -3.5 percentage points). The lack of clear differences before and during the recession is clearly confirmed by the average estimates for men over these periods reported in the top panel of Table 21.

Table 20 shows the results for women. As for men, we find no statistically significant effect across years, methods of constructing the control groups and specifications, and no clear pattern across the years (with predominantly negative coefficients in 7 years and positive coefficients in 5 years). The average effects for the periods before and during the recession reported in the bottom panel of Table 21 are always small and statistically insignificant and the Wald tests fail to reveal any significant difference between the pre-recession and the recession estimates.

5.2.2 Predicted probability of being hired at the NMW

Table 22 reports the results for the effect of the predicted probability of being hired at the minimum wage on the job finding probability of the unemployed. Predicted probabilities are obtained from a first stage probit (not reported here) and the standard errors in this table are bootstrapped to account for the presence of this generated regressor. The reported coefficients are from a linear probability model estimated over the period 1997q2 to 2012q1 and the predicted probability of being hired at the NMW (Pr(NMW)) enters the equation as a main effect and then interacted with dummies grouping consecutive quarters covered by the same level of the NMW. Hence, the coefficient on Pr(NMW) reported in the first row of Table 22 effectively captures the differences in job entry probabilities between unemployed with low and high probabilities of being hired at the bottom of the wage distribution *before* the introduction of the NMW itself. When no individual controls are included, it is found that both men (column 1) and women (column 3) at the lower end of the wage distribution are less likely to find a job (over a 6 month period) before the NMW came into force. The coefficients imply that someone with a very high probability (approaching 1) of having a hiring wage at the bottom of the wage distribution is about 20 percentage points less likely to find a job than someone whose hiring wage would be at the bottom of the wage distribution with a very low probability (approaching 0). These differences, however, become statistically insignificant when demographic controls are included.

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[26] We performed a cluster bootstrap in STATA with 500 repetitions with clusters defined at the individual level. The clustering was necessary because an individual can be observed repeatedly in our sample.

When we look at the changes which occurred after the introduction of the NMW, we see that for men most coefficients are negative and statistically insignificant. There are, however, some exceptions. The introduction of the NMW seems to have been associated with a decrease in the probability of entering work of about 7 percentage points (after controlling for demographic characteristics) for individuals expected to receive a wage at very bottom of the wage distribution. A statistically significant negative effect is also picked up in 2004. Finally, we note that there is a prevalence of positive (and statistically insignificant) coefficients after the start of the recession when controls are not included, but less so when controls are included. This leads to the finding reported in Table 23 that the average coefficient over 2008-2011 is either positive or negative depending on the specification, but in both cases substantively small and statistically insignificant.

For women, there is even less evidence of any effect before the onset of the recession. In fact, the coefficients for the period up to 2007 tend to be relatively small, of conflicting signs and always statistically insignificant. From 2008 onwards, the variables of interest attract positive and weakly statistically significant coefficients in the region of a 10 percentage point effect when controls are not included. Statistical significance is mostly lost and the magnitude of the effect is more than halved when controls are included in column 4. Table 23 shows, however, that the positive average effect of 5 percentage points over the 2008-2011 period does attain statistical significance at the 10% level when controls are included.

5.2.3 Geographical variation in the bite of the NMW

We now turn to estimates of the impact of the NMW on job entry based on geographical variation in the bite of the NMW. In Table 24 to Table 26 the bite is measured as the proportion of employees in April of each year earning less than or the same as the forthcoming NMW in October, and in Table 27 to Table 29 the bite is defined as the Kaitz index in April each year. We show estimates for men and women separately and for each sex we present results both for all unemployed individuals (columns (1) and (2) in each table) and for low-educated individuals only (those with GCSE equivalent qualifications or less; columns (3) and (4)). As in Section 5.2.1, the dependent variable is the probability of leaving unemployment for employment over a 6 month period. All models include year dummy variables to capture macro trends in labour demand and area fixed effects to allow for local differences (constant over time) in job market conditions. After controlling for time and area effects, the estimates show how the probability of job entry is associated with local variations in the NMW bite that depart from the national trend. Because there may be other factors, such as a temporary local boom, that affect both job entry and the pay structure in an area, we also report specifications that include a set of local area controls (the lagged change in local employment rate, calculated from LFS, and the shares of employment in manufacturing, private services and construction, and in small firms and medium firms, calculated from ASHE). Columns (1) and (3) in the tables exclude these controls and columns (2) and (4) include them (coefficients not reported).

The tables report the coefficients on the NMW bite and on the NMW bite interacted with a dummy variable for each of the NMW years 1999–2011. The coefficient on the bite (the main effect) gives the association of job entry with low pay at the local area level in the base period of 1997-8 (as noted in Section 4.2, the "bite" in 1997-8 is defined using the level of the NMW at its introduction in 1999, deflated by the average weekly earnings index). This is the relationship between living in a low pay area and getting a job that would exist even in the absence of a minimum wage: generally low pay areas are associated with lower employment (Dolton et al 2012). The coefficients on the interactions of each NMW year with the bite then give the additional impact which can be attributed to the NMW via its effect in compressing the bottom of the wage distribution. These interaction coefficients are the estimates of interest.

We first consider the bite as measured by the proportion of men or women employees affected by the upcoming NMW. Table 24 shows the results for unemployed adult men. The coefficient on the NMW proportion is positive, contrary to expectations, however it not significant. The remaining coefficients show almost no evidence that the NMW affected job entry. Only one coefficient is significant (and at only the 10% level), providing weak evidence that the NMW reduced transitions from unemployment to employment among low-skilled men in 2002. Table 26 shows the results when the pre-recession are grouped together and compared to the recession years. In neither period is there evidence that the NMW affected job finding and a Wald test indicates that there is no difference between the periods.

Table 25 shows the estimates for unemployed adult women. The bite coefficient for the period before the NMW is negative but, as for men, not significant. However, three of the interaction coefficients reach significance at least at the 10% level. They suggest that the NMW increased women's probability of finding a job from unemployment in 2005 and in 2007 (although there is no estimated effect when considering only low educated women, columns (3) and (4)). The estimates are larger when local area controls are included. The largest coefficients (0.5) imply that a 1 percentage point increase in the proportion affected by the NMW would raise the job finding probability by 0.5*0.01=0.5 percentage points. This should be compared to an average 6-month job finding rate in the sample of 14%. Although we find significant effects in particular years for women, when looking at the pre-recession and recession periods as a whole (Table 26), we see no evidence that the NMW affected job entry.[27]

Table 27 shows the estimates for adult men using the Kaitz index as the bite measure. In contrast to the men's results using the NMW proportion, we see evidence of positive effects on job entry in 2004 and 2007 which are robust to including local area controls (but which disappear when the sample is restricted to low-educated men), and negative effects for low-educated men in 2011. However, Table 29 shows no evidence of overall

[27] In estimates that combine men and women (not reported), we find more robust evidence of positive effects, especially from 2004-7, and an overall positive effect before the recession.

effects in either the pre-recession period or the recession. For adult women (Table 28), we see positive and significant estimates in 2005 and 2006, but they are not fully robust across specifications and samples. By contrast, for low-educated women, all coefficients in the recession years are negative and they reach statistical significance in 2010. Table 29 indicates that during the recession period taken as a whole, the NMW may have reduced job entry among low-educated women (and this effect differed significantly from the pre-recession period, for which we do not see an impact). A typical Kaitz index coefficient of approximately 0.2 implies that a 1 percentage point change in the Kaitz index leads to a 0.2 percentage point change in the probability of finding a job.

We examine the impacts on young workers using a Kaitz index defined as the ratio of the development rate to median hourly earnings for all adults.[28] Consistent with the broad pattern for adults, Table 30 indicates that the NMW increased job entry in 2004 and 2006. Combining and comparing the pre-recession and recession years, Table 31 suggests that the positive impact applied across the full pre-recession period. A Wald test indicates that the impact did not differ during the recession period, although the (positive) estimates during the recession are not significant.

Taking our geographical analysis as a whole, there is evidence that the NMW raised job entry rates during the middle years of the 2000s and some weak evidence that the NMW reduced job entry during the recession (only for adults). The pre-recession results are consistent with the geographical analysis of aggregate unemployment rates by Dickens et al (2012) and Dolton et al (2010). They found evidence that the NMW reduced the unemployment rate over the period 2003-7 (Dolton et al also found that the NMW increased unemployment rates over 1999-2002). One explanation is that the labour market is constrained by the supply side during a boom and the NMW acts as an incentive to take up available jobs. During a recession employment is constrained by demand and thus the NMW may restrict job creation. However we do not push these results given the somewhat different evidence from the analysis using predictions of the hiring wage and the probability of being hired on the NMW.

[28] Local area cell sizes are too small for reliable estimates of the median hourly earnings of young workers only or to calculate the proportion of young NMW workers in an area. A Kaitz index based on the youth rate relative to adult median earnings will under-estimate the absolute level of the bite experienced by young workers. However, we are assuming that it still provides a reasonable proxy of how the bite varies between areas and over time.

6 Summary and conclusions

In this report we have analysed the impact of the NMW from 1999 (or 2000) until 2011 on employment retention, changes in working hours, and on the job finding probability of the unemployed. To examine job retention and hours, we have used DID methods applied to the LFS, looking at changes over time in the outcomes of a treatment and control group (horizontal DID), and at changes over time combined with differences with respect to two additional groups further up the wage distribution (triple DID). To examine job entry, we have exploited geographical variation of the NMW bite (calculated using ASHE and matched into the LFS); used predicted hiring wages in a vertical DID framework comparing a treatment and control group with respect to two additional groups further up the wage distribution; and exploited the probability of receiving the NMW when hired.

The variety of different methods used, as well as alternative specifications within methods (inclusion or not of controls, multiple definitions of comparison groups), enables us to assess how robust the results are to confounding factors and assumptions about the incidence of the NMW. In general horizontal and triple DID lead to qualitatively similar estimates, suggesting that our results are not sensitive to assumptions about how macroeconomic trends affect the treatment and control groups respectively. On the other hand, there are some differences in estimated effects when comparing across alternative definitions of comparison groups, and for the unemployed the pattern of estimates of the NMW impact on job entry differs across the three measures of treatment.

To be sure that the NMW had an impact we would need to see statistically significant results that are similar across methods and specifications, or show a plausible pattern across years (e.g. larger effects in years of large upratings). In the event, few of the estimates are statistically significant and they do not form a consistent pattern. Furthermore they do not point systematically in the direction of adverse effects – significant results include both positive and negative signs. Thus our overall conclusion is that the NMW did not have adverse impact over the period considered. However, for some groups in particular years we do see more consistent evidence that the NMW had an impact, as we now summarise for each outcome.

We do not find robust evidence that the NMW upratings had an adverse effect on the employment retention of adults in either the pre-recession or recession periods taken as a whole. Some estimates indicate that the NMW increased employment retention for men during the recession period, but they are sensitive to the choice of model specification. There is also some evidence that the NMW upratings had an impact for men in particular years (most notably reducing job retention following the large uprating of 2001), but these findings are also sensitive to changes in method and specification. For women, we find little evidence of any impact of the NMW on employment retention in any year. For young people, there is some evidence that the NMW may have reduced employment retention in the pre-recession years, especially for workers whose wages were raised most by NMW upratings, but not in the recession period.

We find no systematic effect of the NMW upratings on the basic hours of adults across the years. Especially for men, there is some evidence of impacts (both positive and negative) in particular years but they are not generally consistent across model specifications. Our tentative finding in Bryan et al (2012) that the 2010 uprating may have reduced weekly hours is not confirmed by the full 2010/11 LFS data release, and there is no evidence that the NMW had adverse effects on hours during the recession period as a whole.

For young people, using longer runs of data than in Bryan et al (2012), we see less evidence than previously that the NMW reduced hours. The estimates during the recession period are negative but not statistically significant and they do not differ significantly from the positive (but insignificant) pre-recession effects. However, the estimates for young people are based on relatively small sample sizes and so should be treated with caution.

We do not find strong evidence that the NMW affected the job entry probabilities of the unemployed. The geographical analysis indicates that individuals in areas with bigger NMW bites had higher job entry rates in the middle years of the 2000s, and there is some evidence that this positive NMW effect on job finding then reversed during the downturn. However, the other two methods either show no effect of the NMW on job entry in any year (using predicted hiring wages) or a positive effect during the recession on the job entry (of those women most likely to be hired on the NMW). Estimates within each method are not always robust to changes in model specification and given the additional differences we see across methods, we conclude that there is little empirical support for the hypothesis that the NMW has had an impact on job entry.

Our results add to the small number of studies which have examined the impact of the NMW during the current downturn. Consistent with these studies, the latest findings broadly indicate that there have not been adverse effects of the NMW on labour market outcomes. Future research should aim to refine methods of investigating the impact of the NMW on the unemployed and to add to this small evidence base.

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8 Figures

Figure 1- Illustration of the horizontal DID approach

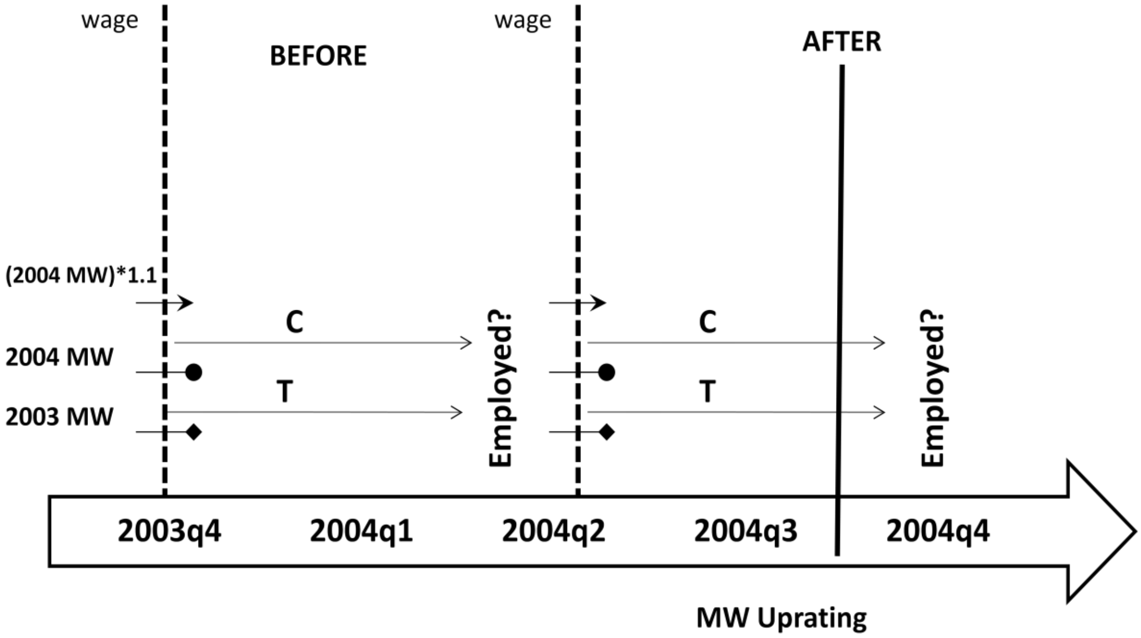


Figure 2 - Illustration of the vertical DID approach

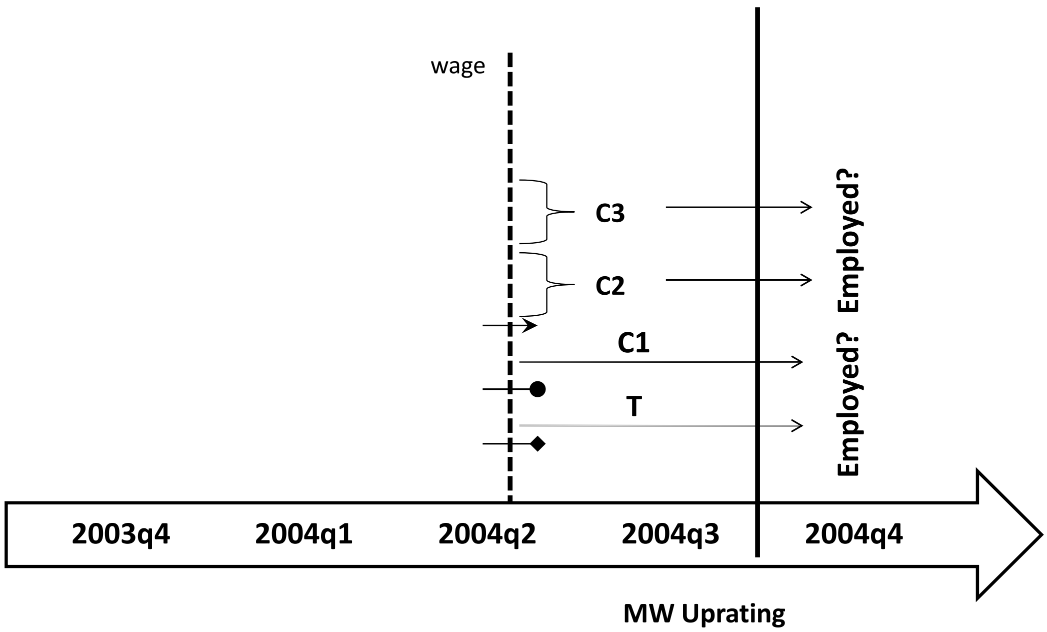


Figure 3 - Illustration of the triple-DID approach

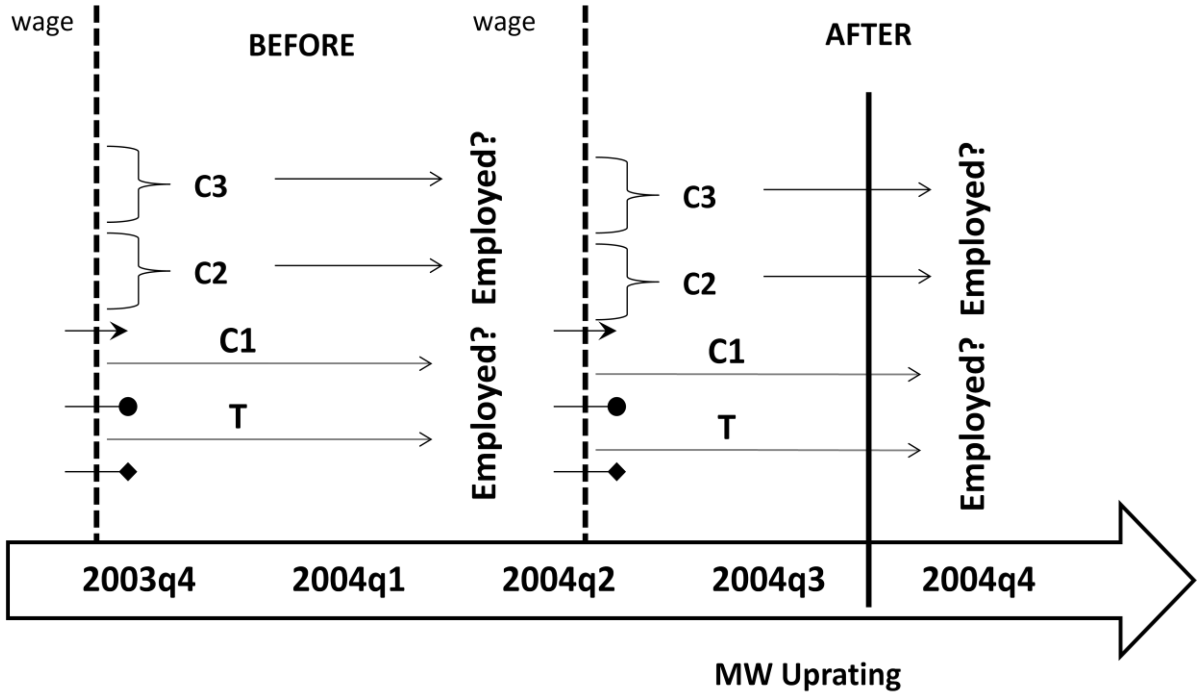
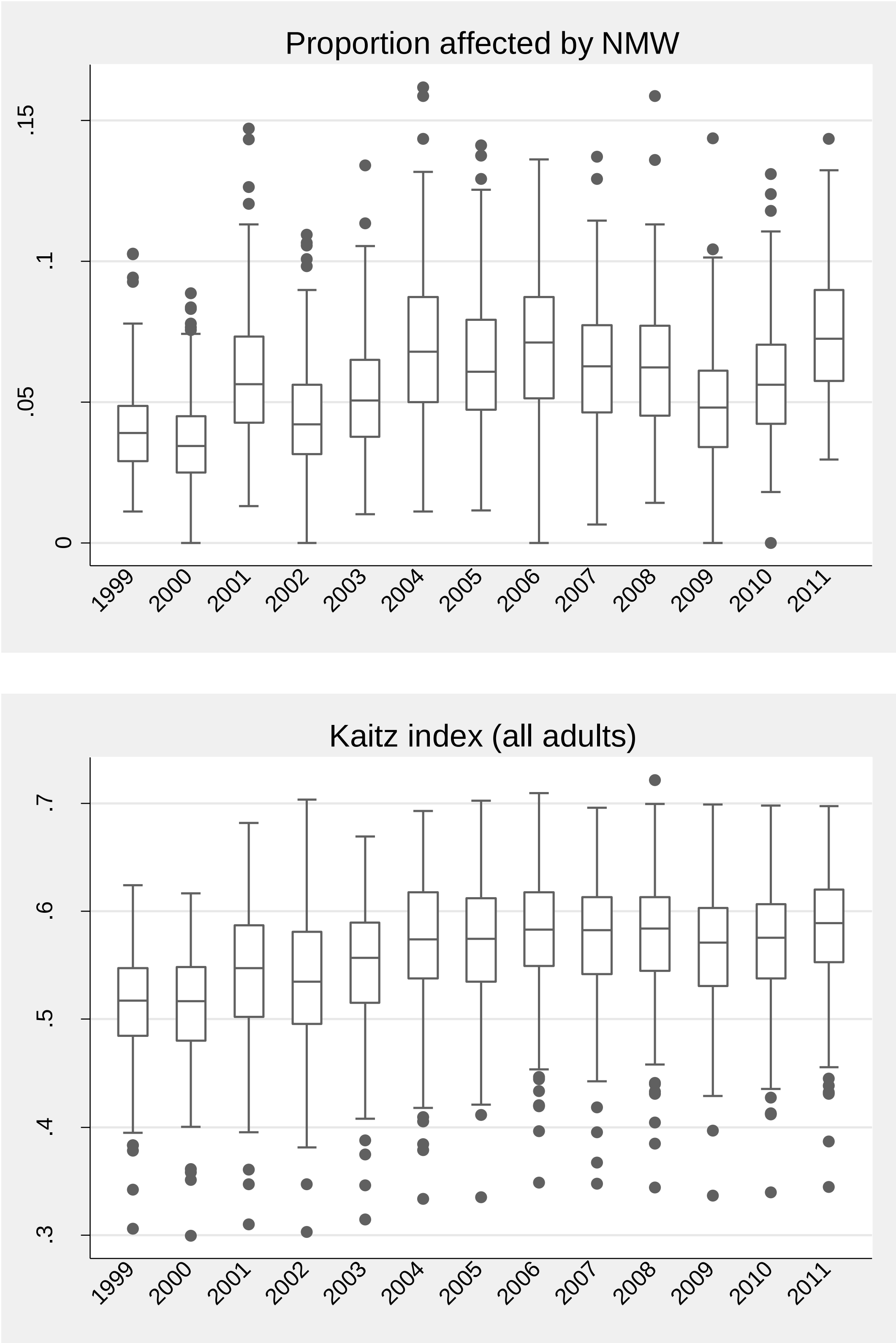


Figure 4 – Variation in NMW bite across areas and years



9 Tables

Table 1: NMW increases compared to growth in average weekly earnings (AWE)

		Adult rate			Youth development rate		
Date	AWE growth (%)	NMW hourly rate	NMW increase (%)	NMW increase minus AWE growth (%)	NMW hourly rate	NMW increase (%)	NMW increase minus AWE growth (%)
Apr 99		£3.60			£3.00		
Oct00		£3.70	2.78		£3.20	6.67	
Oct 01	4.66	£4.10	10.81	6.15	£3.50	9.37	4.72
Oct 02	2.67	£4.20	2.44	-0.23	£3.60	2.86	0.19
Oct 03	4.05	£4.50	7.14	3.10	£3.80	5.56	1.51
Oct 04	5.28	£4.85	7.78	2.50	£4.10	7.89	2.62
Oct 05	3.96	£5.05	4.12	0.17	£4.25	3.66	-0.30
Oct 06	4.31	£5.35	5.94	1.63	£4.45	4.71	0.39
Oct 07	4.38	£5.52	3.18	-1.20	£4.60	3.37	-1.01
Oct 08	3.50	£5.73	3.80	0.31	£4.77	3.70	0.20
Oct 09	0.23	£5.80	1.22	1.00	£4.83	1.26	1.03
Oct 10	2.25	£5.93	2.24	-0.01	£4.92	1.86	-0.38
Oct 11	1.98	£6.08	2.53	0.55	£4.98	1.22	-0.76

Source: NMW rates from LPC (2012). AWE from ONS series KAB9 (from 2000 only), annual changes calculated to October each year (revised from Bryan et al 2012, using latest AWE series published 18 July 2012).

Table 2 - Sample sizes for the horizontal DID for the employed, LFS data.

	Male adults				Female adults			
	Treated		Control		Treated		Control	
	Before	After	Before	After	Before	After	Before	After
2000	70	72	107	97	341	242	384	450
2001	54	114	76	132	291	452	224	495
2002	74	66	107	127	369	268	483	473
2003	82	78	113	108	336	339	446	408
2004	155	112	182	177	507	394	476	507
2005	147	111	147	158	438	389	428	507
2006	128	119	145	147	487	423	377	416
2007	146	172	177	167	430	396	460	460
2008	129	89	189	200	346	353	463	483
2009	69	77	163	153	196	172	483	473
2010	88	87	178	201	225	250	478	464
2011	114	150	132	137	263	344	332	338

Table 3 - Sample sizes of additional control groups for the vertical DID for the employed, LFS data.

Males												
	Method 1				Method 2				Method 3			
	Control 2		Control 3		Control 2		Control 3		Control 2		Control 3	
	Before	After	Before	After	Before	After	Before	After	Before	After	Before	After
2000	103	127	140	151	107	97	107	97	200	246	86	64
2001	72	132	131	209	76	132	76	132	126	454	174	62
2002	188	178	144	127	107	127	107	127	102	28	260	296
2003	194	200	155	181	113	108	113	108	226	262	68	88
2004	164	164	180	190	182	177	182	177	66	52	248	224
2005	170	168	127	157	147	158	147	158	100	76	172	224
2006	163	173	114	124	145	147	145	147	164	160	2	30
2007	136	155	143	140	177	167	177	167	280	314	72	12
2008	157	123	157	145	189	200	189	200	68	36	204	190
2009	119	130	155	168	163	153	163	153			164	152
2010	121	121	123	135	178	201	178	201	18		176	102
2011	145	135	100	94	132	137	132	137	72	14	160	192

Females												
	Method 1				Method 2				Method 3			
	Control 2		Control 3		Control 2		Control 3		Control 2		Control 3	
	Before	After	Before	After	Before	After	Before	After	Before	After	Before	After
2000	275	387	269	391	384	450	384	450	414	750	98	56
2001	163	346	203	449	224	495	224	495	214	362	220	620
2002	508	518	266	288	483	473	483	473	142	156	710	724
2003	473	554	238	320	446	408	446	408	556	728	68	82
2004	367	368	324	326	476	507	476	507	184	126	492	578
2005	369	372	213	282	428	507	428	507	170	160	406	392
2006	334	332	208	228	377	416	377	416	78	320	514	54
2007	320	335	186	202	460	460	460	460	340	306	66	214
2008	241	270	221	219	463	483	463	483	94	112	310	344
2009	276	301	247	268	483	473	483	473			362	350
2010	270	237	197	172	478	464	478	464	28	42	248	348
2011	250	299	171	161	332	338	332	338	84	84	248	332

Table 4 - Sample sizes of new hires used in the first stage estimations

	New hires with hrrate			New hires with hourpay		
	Males	Females	Total	Males	Females	Total
1997	.	.	.	329	297	626
1998	.	.	.	632	739	1,371
1999-2000	429	558	987	828	970	1,798
2001	215	318	533	409	466	875
2002	269	367	636	500	567	1,067
2003	244	324	568	479	512	991
2004	225	269	494	439	428	867
2005	194	240	434	361	387	748
2006	183	264	447	337	400	737
2007	176	278	454	329	410	739
2008	168	264	432	299	381	680
2009	162	212	374	309	336	645
2010	238	215	453	406	361	767
2011	150	189	339	275	282	557

Table 5 - Sample sizes for the vertical DID exercise for the unemployed

Males								
	Method 1				Method 2			
	Treated	Control 1	Control 2	Control 3	Treated	Control 1	Control 2	Control 3
2000	216	341	473	549	216	341	432	427
2001	194	395	563	708	194	395	452	519
2002	528	603	661	534	528	603	679	692
2003	271	316	386	418	271	316	334	364
2004	251	299	389	340	251	299	339	346
2005	422	547	436	401	422	547	512	534
2006	439	579	539	474	439	579	610	703
2007	310	308	300	288	310	308	350	361
2008	531	571	588	535	531	571	596	658
2009	656	607	586	494	656	607	655	657
2010	589	611	591	510	589	611	602	600
2011	446	477	513	487	446	477	484	510
Females								
	Method 1				Method 2			
	Treated	Control 1	Control 2	Control 3	Treated	Control 1	Control 2	Control 3
2003	385	438	392	402	385	438	347	352
	466	460	502	404	466	460	403	336
	493	549	465	351	493	549	473	460
	332	326	360	310	332	326	308	278
	269	289	290	224	269	289	249	242
2004	355	401	368	337	355	401	436	414
2005	430	457	393	259	430	457	426	333
2006	274	220	151	154	274	220	178	167
2007	515	482	453	317	515	482	457	395
2008	547	502	396	343	547	502	454	452
2009	406	371	374	310	406	371	380	382
2010	381	387	411	331	381	387	380	354

Table 6 – Association of NMW bite with change in lower tail inequality, 2000-2011

	Δ log (50^{th} / 5^{th} percentile)			Δ log (50^{th} / 10^{th} percentile)		
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged prop NMW	-0.100**	-0.102*	-0.017	-0.144***	-0.167***	-0.290***
	(0.046)	(0.053)	(0.088)	(0.044)	(0.050)	(0.083)
Manufacturing share		-0.010	0.126**		0.006	0.120**
		(0.020)	(0.054)		(0.019)	(0.052)
Construction share		0.032	0.086		0.020	0.074
		(0.068)	(0.108)		(0.064)	(0.102)
Private service share		-0.009	-0.131***		-0.012	-0.097**
		(0.015)	(0.041)		(0.014)	(0.039)
Share in firm size 1-49		-0.030	-0.119**		-0.010	-0.058
		(0.024)	(0.059)		(0.023)	(0.057)
Share in firm size 50-249		0.004	-0.061		-0.031	-0.062
		(0.044)	(0.068)		(0.042)	(0.065)
N observations	1540	1540	1540	1540	1540	1540
N areas	140	140	140	140	140	140
Area fixed effects	No	No	Yes	No	No	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes

OLS estimates. The dependent variable is the change from April(t-1) to April(t) in the natural log of 50^{th}/5^{th} (50^{th}/10^{th}) percentile of hourly earnings for all adults (18+) at county/UA/London area level. The bite measure is the proportion of all adults in April(t) earning less than the new NMW (plus 5p) introduced in October (t). Local area controls are shares of employment.

* p<0.10 ** p<0.05 *** p<0.01"

Table 7 - Effects of NMW upratings on job retention for adult males.

Horizontal DID estimates from linear probability models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Treatment Dummy				**Treatment Dummy * Wage Gap**			
2000	-0.044	-0.040	0.018	0.024	-0.053	-0.051	0.003	0.006
	(0.076)	(0.075)	(0.067)	(0.066)	(0.074)	(0.073)	(0.065)	(0.064)
2001	-0.131**	-0.177**	-0.153**	-0.179***	-0.090*	-0.125**	-0.112**	-0.137***
	(0.060)	(0.057)	(0.055)	(0.050)	(0.050)	(0.044)	(0.046)	(0.040)
2002	0.068	0.069	0.125**	0.117**	0.060	0.059	0.107**	0.098*
	(0.063)	(0.064)	(0.053)	(0.054)	(0.062)	(0.063)	(0.052)	(0.053)
2003	-0.092	-0.106	-0.076	-0.077	-0.097	-0.107*	-0.088	-0.088
	(0.066)	(0.066)	(0.057)	(0.058)	(0.064)	(0.063)	(0.056)	(0.056)
2004	0.005	-0.033	0.022	-0.002	0.024	0.002	0.039	0.025
	(0.050)	(0.053)	(0.043)	(0.045)	(0.048)	(0.051)	(0.043)	(0.045)
2005	-0.004	-0.002	-0.005	0.007	-0.009	-0.024	-0.009	-0.014
	(0.052)	(0.056)	(0.046)	(0.050)	(0.039)	(0.042)	(0.035)	(0.038)
2006	0.118**	0.103**	0.128**	0.120**	0.094**	0.085*	0.106**	0.102**
	(0.050)	(0.050)	(0.045)	(0.045)	(0.048)	(0.048)	(0.044)	(0.044)
2007	0.049	0.057	-0.028	-0.020	0.046	0.059	0.002	0.015
	(0.049)	(0.049)	(0.042)	(0.042)	(0.041)	(0.042)	(0.037)	(0.037)
2008	0.065	0.098*	-0.019	0.008	0.032	0.070	-0.025	0.008
	(0.058)	(0.058)	(0.050)	(0.050)	(0.052)	(0.051)	(0.046)	(0.045)
2009	0.009	0.009	-0.032	-0.036	0.007	0.005	-0.020	-0.024
	(0.059)	(0.063)	(0.056)	(0.060)	(0.055)	(0.059)	(0.052)	(0.055)
2010	0.000	0.008	0.043	0.049	0.005	0.013	0.037	0.042
	(0.057)	(0.057)	(0.053)	(0.053)	(0.056)	(0.055)	(0.052)	(0.052)
2011	0.153**	0.147**	0.071	0.067	0.117**	0.112**	0.056	0.053
	(0.057)	(0.059)	(0.051)	(0.053)	(0.052)	(0.054)	(0.047)	(0.049)
N	38526	37514	38526	37514	38507	37495	38507	37495
Spill-over group	no	no	yes	yes	no	no	yes	Yes
Additional controls	no	yes	no	yes	no	yes	no	Yes

Coefficients from a linear probability model.
Robust Standard errors in parentheses
* p<0.10 ** p<0.05 *** p<0.001

Table 8 - Effects of NMW upratings on job retention for adult females.

Horizontal DID estimates from linear probability models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Treatment Dummy				**Treatment Dummy * Wage Gap**			
2000	0.018	0.012	0.002	0.000	0.022	0.017	0.006	0.003
	(0.034)	(0.033)	(0.030)	(0.030)	(0.033)	(0.033)	(0.030)	(0.029)
2001	-0.023	-0.028	-0.005	-0.005	-0.004	-0.006	-0.001	-0.000
	(0.028)	(0.028)	(0.026)	(0.026)	(0.025)	(0.025)	(0.023)	(0.023)
2002	0.019	0.018	0.018	0.019	0.023	0.023	0.027	0.026
	(0.029)	(0.029)	(0.026)	(0.026)	(0.028)	(0.027)	(0.025)	(0.024)
2003	-0.007	-0.010	0.025	0.024	-0.003	-0.006	0.012	0.012
	(0.030)	(0.030)	(0.025)	(0.025)	(0.028)	(0.028)	(0.025)	(0.024)
2004	-0.003	-0.010	0.006	0.001	0.009	0.005	0.005	0.002
	(0.025)	(0.026)	(0.022)	(0.022)	(0.022)	(0.023)	(0.020)	(0.020)
2005	-0.008	-0.016	-0.017	-0.018	-0.003	-0.002	-0.004	-0.000
	(0.025)	(0.025)	(0.023)	(0.023)	(0.018)	(0.019)	(0.017)	(0.017)
2006	0.002	0.001	-0.019	-0.019	0.007	0.008	-0.006	-0.005
	(0.026)	(0.026)	(0.024)	(0.024)	(0.023)	(0.023)	(0.021)	(0.021)
2007	0.044	0.037	0.047*	0.039	0.036	0.034	0.043*	0.040*
	(0.027)	(0.027)	(0.027)	(0.027)	(0.023)	(0.023)	(0.022)	(0.022)
2008	-0.018	-0.017	0.005	0.006	0.002	-0.005	0.021	0.014
	(0.027)	(0.027)	(0.026)	(0.026)	(0.025)	(0.025)	(0.023)	(0.023)
2009	0.041	0.037	0.016	0.017	0.034	0.032	0.013	0.014
	(0.034)	(0.034)	(0.033)	(0.033)	(0.032)	(0.032)	(0.030)	(0.030)
2010	0.009	0.013	0.023	0.020	-0.000	0.007	0.010	0.013
	(0.028)	(0.028)	(0.027)	(0.027)	(0.027)	(0.027)	(0.026)	(0.025)
2011	-0.016	-0.014	-0.002	-0.008	-0.013	-0.010	-0.001	-0.005
	(0.031)	(0.030)	(0.030)	(0.030)	(0.027)	(0.027)	(0.026)	(0.026)
N	57276	55998	57276	55998	57253	55975	57253	55975
Spill-over group	no	no	yes	yes	no	no	yes	Yes
Additional controls	no	yes	no	yes	no	yes	no	Yes

Coefficients from a linear probability model.

Robust Standard errors in parentheses

* p<0.10 ** p<0.05 *** p<0.001

Table 9 - Average effects of NMW upratings on job retention before and during the recession.

	Horizontal DID estimates from linear probability models							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Treatment Dummy				Treatment Dummy * Wage Gap			
					Male adults			
2000-2007	0.007	-0.004	0.007	0.002	0.007	-0.002	0.007	0.003
	(0.020)	(0.021)	(0.018)	(0.018)	(0.018)	(0.018)	(0.016)	(0.016)
2008-2011	0.062**	0.072**	0.019	0.026	0.046*	0.058**	0.015	0.024
	(0.029)	(0.030)	(0.027)	(0.027)	(0.027)	(0.027)	(0.025)	(0.025)
Wald test for equality	2.388	4.500	0.136	0.526	1.451	3.250	0.073	0.499
P-value	0.122	0.034	0.713	0.469	0.228	0.071	0.787	0.480
					Female adults			
2000-2007	0.006	0.001	0.007	0.005	0.010	0.009	0.010	0.009
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
2008-2011	0.001	0.002	0.010	0.008	0.004	0.003	0.011	0.009
	(0.015)	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)	(0.013)
Wald test for equality	0.060	0.004	0.026	0.025	0.171	0.121	0.013	0.001
P-value	0.806	0.952	0.871	0.873	0.679	0.727	0.910	0.970
					18-21 year olds[a]			
2000-2007	-0.095	-0.117*	-0.080	-0.077	-0.131**	-0.138**	-0.109**	-0.100*
	(0.058)	(0.070)	(0.052)	(0.063)	(0.053)	(0.065)	(0.049)	(0.059)
2008-2011	-0.007	-0.043	0.007	-0.002	-0.080	-0.118**	-0.058	-0.083
	(0.061)	(0.077)	(0.055)	(0.070)	(0.050)	(0.055)	(0.048)	(0.052)
Wald test for equality	1.899	0.853	1.923	0.912	0.797	0.107	0.761	0.074
P-value	0.168	0.356	0.166	0.340	0.372	0.744	0.383	0.785
Spill-over group	no	no	yes	yes	no	no	yes	yes
Additional controls	no	yes	no	yes	no	yes	no	yes

a: constant group differences across time

Coefficients from a linear probability model. Robust Standard errors in parentheses.

* p<0.10 ** p<0.05 *** p<0.001

Table 10 - Effects of NMW upratings on job retention for adult males.

	Triple DID estimates from linear probability models					
	(1)	(2)	(3)	(4)	(5)	(6)
	Method 1		**Method 2**		**Method 3**	
2000	-0.132	-0.115	-0.108	-0.082	-0.100	-0.098
	(0.090)	(0.088)	(0.094)	(0.091)	(0.090)	(0.089)
2001	-0.072	-0.116	-0.130	-0.179**	-0.114	-0.157**
	(0.077)	(0.074)	(0.082)	(0.078)	(0.072)	(0.069)
2002	0.078	0.057	0.099	0.082	0.075	0.072
	(0.076)	(0.076)	(0.082)	(0.082)	(0.086)	(0.084)
2003	-0.137*	-0.142*	-0.100	-0.108	-0.096	-0.110
	(0.078)	(0.078)	(0.084)	(0.084)	(0.083)	(0.083)
2004	0.020	-0.017	0.028	-0.005	0.070	0.035
	(0.062)	(0.065)	(0.062)	(0.065)	(0.074)	(0.077)
2005	-0.036	-0.040	-0.011	-0.013	-0.026	-0.015
	(0.067)	(0.072)	(0.068)	(0.073)	(0.071)	(0.077)
2006	0.051	0.026	0.063	0.039	-0.007	-0.056
	(0.069)	(0.069)	(0.068)	(0.067)	(0.085)	(0.085)
2007	0.068	0.069	0.037	0.049	-0.006	-0.007
	(0.065)	(0.065)	(0.062)	(0.062)	(0.101)	(0.101)
2008	0.078	0.095	0.065	0.089	0.049	0.099
	(0.071)	(0.069)	(0.068)	(0.067)	(0.082)	(0.082)
2009	0.030	0.027	0.017	0.013	0.009	
	(0.078)	(0.082)	(0.076)	(0.079)	(0.059)	
2010	0.051	0.059	0.018	0.026	0.138	0.142
	(0.070)	(0.069)	(0.066)	(0.065)	(0.113)	(0.111)
2011	0.267***	0.236**	0.249**	0.226**	0.078	0.008
	(0.080)	(0.082)	(0.077)	(0.079)	(0.103)	(0.079)
N	38538	37525	38538	37525	38538	37525
Additional controls	no	yes	no	yes	no	yes

Coefficients from a linear probability model.
Robust Standard errors in parentheses
For the definitions of the methods see page 18.
* p<0.10 ** p<0.05 *** p<0.001

Table 11 - Effects of NMW upratings on job retention for adult females.

	Triple DID estimates from linear probability models					
	(1)	(2)	(3)	(4)	(5)	(6)
	Method 1		**Method 2**		**Method 3**	
2000	-0.008	-0.018	0.023	0.014	0.062	0.043
	(0.042)	(0.041)	(0.041)	(0.040)	(0.043)	(0.041)
2001	-0.014	-0.027	0.010	-0.003	-0.001	-0.014
	(0.041)	(0.040)	(0.039)	(0.038)	(0.039)	(0.039)
2002	0.012	0.011	0.015	0.018	0.013	0.011
	(0.038)	(0.038)	(0.037)	(0.036)	(0.042)	(0.042)
2003	0.028	0.020	0.039	0.030	0.051	0.037
	(0.038)	(0.038)	(0.038)	(0.038)	(0.039)	(0.039)
2004	0.037	0.028	0.014	0.001	0.034	0.027
	(0.035)	(0.037)	(0.032)	(0.033)	(0.040)	(0.042)
2005	-0.031	-0.035	0.005	0.008	-0.065*	-0.065*
	(0.036)	(0.036)	(0.032)	(0.032)	(0.037)	(0.037)
2006	-0.014	-0.020	-0.011	-0.017	-0.017	-0.017
	(0.039)	(0.038)	(0.035)	(0.035)	(0.058)	(0.057)
2007	0.048	0.047	0.057	0.053	0.052	0.058
	(0.043)	(0.043)	(0.036)	(0.036)	(0.049)	(0.050)
2008	-0.047	-0.044	0.020	0.027	-0.042	-0.052
	(0.043)	(0.043)	(0.036)	(0.035)	(0.051)	(0.051)
2009	0.080*	0.064	0.033	0.036	0.041	
	(0.046)	(0.046)	(0.041)	(0.041)	(0.034)	
2010	0.046	0.050	0.032	0.027	-0.001	0.007
	(0.042)	(0.042)	(0.034)	(0.034)	(0.063)	(0.061)
2011	0.018	0.013	0.029	0.032	0.056	0.049
	(0.045)	(0.045)	(0.040)	(0.040)	(0.052)	(0.051)
N	57295	56015	57295	56015	57295	56015
Additional controls	no	yes	no	yes	no	yes

Coefficients from a linear probability model.
Robust Standard errors in parentheses
For the definitions of the methods see page 18.
* p<0.10 ** p<0.05 *** p<0.001

Table 12 - Average effects of NMW upratings on job retention before and during the recession.

	Triple DID estimates from linear probability models					
	(1)	(2)	(3)	(4)	(5)	(6)
	Method 1		**Method 2**		**Method 3**	
			Male adults			
2000-2007	-0.016	-0.029	-0.007	-0.017	-0.028	-0.043
	(0.026)	(0.026)	(0.026)	(0.026)	(0.030)	(0.030)
2008-20011	0.106**	0.105**	0.086**	0.089**	0.083	0.090
	(0.037)	(0.038)	(0.036)	(0.036)	(0.057)	(0.055)
Wald test for equality	7.284	8.576	4.408	5.567	2.973	4.526
P-value	0.007	0.003	0.036	0.018	0.085	0.033
			Female adults			
2000-2007	0.004	-0.001	0.016	0.012	0.007	0.003
	(0.014)	(0.014)	(0.013)	(0.013)	(0.015)	(0.015)
2008-20011	0.021	0.017	0.027	0.029	0.002	-0.003
	(0.022)	(0.022)	(0.019)	(0.019)	(0.032)	(0.031)
Wald test for equality	0.410	0.495	0.246	0.602	0.017	0.024
P-value	0.522	0.482	0.620	0.438	0.898	0.876
Additional controls	no	yes	no	yes	no	yes

Coefficients from a linear probability model. Robust
Standard errors in parentheses.

For the definitions of the methods see page 18.

* p<0.10 ** p<0.05 *** p<0.001

Table 13 - Effects of NMW upratings on changes in basic hours for adult males.

Horizontal DID estimates from linear models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Treatment Dummy				**Treatment Dummy * Wage Gap**			
2000	-2.106	-1.857	-0.905	-0.982	-2.023	-1.776	-0.960	-0.986
	(1.816)	(1.824)	(1.722)	(1.751)	(1.828)	(1.844)	(1.726)	(1.759)
2001	-2.896*	-2.591	-1.093	-1.035	-3.170**	-2.969*	-2.036	-1.993
	(1.698)	(1.708)	(1.546)	(1.546)	(1.533)	(1.539)	(1.433)	(1.434)
2002	-1.766	-1.578	-0.717	-0.674	-1.745	-1.566	-0.763	-0.812
	(1.835)	(1.830)	(1.528)	(1.561)	(1.839)	(1.846)	(1.553)	(1.591)
2003	1.041	0.530	-0.585	-1.031	1.362	0.764	-0.076	-0.561
	(1.618)	(1.588)	(1.432)	(1.417)	(1.530)	(1.470)	(1.358)	(1.312)
2004	1.965**	2.231**	0.826	0.738	2.329**	2.572**	1.518*	1.444*
	(0.975)	(1.041)	(0.835)	(0.856)	(0.884)	(0.949)	(0.783)	(0.819)
2005	1.338	0.650	0.740	0.359	0.362	-0.053	0.180	-0.089
	(1.317)	(1.330)	(1.083)	(1.155)	(1.188)	(1.318)	(1.067)	(1.213)
2006	-1.820*	-1.735*	-0.714	-0.326	-1.077	-1.194	-0.428	-0.338
	(0.998)	(0.933)	(0.809)	(0.742)	(0.816)	(0.794)	(0.680)	(0.652)
2007	-0.176	-0.199	0.414	0.522	0.276	0.220	0.456	0.474
	(1.239)	(1.249)	(1.092)	(1.099)	(1.123)	(1.128)	(1.033)	(1.036)
2008	0.445	0.469	1.670*	1.625*	0.410	0.408	1.210	1.191
	(1.048)	(0.997)	(0.978)	(0.921)	(0.957)	(0.965)	(0.907)	(0.908)
2009	-0.331	-0.480	0.451	0.722	-0.210	-0.263	0.357	0.630
	(1.295)	(1.362)	(1.129)	(1.193)	(1.219)	(1.283)	(1.069)	(1.128)
2010	0.758	0.825	1.456	1.499	0.636	0.665	0.893	0.927
	(1.715)	(1.834)	(1.773)	(1.891)	(1.665)	(1.804)	(1.673)	(1.809)
2011	-0.002	0.177	0.349	0.437	-0.343	-0.170	0.254	0.333
	(1.174)	(1.199)	(0.979)	(0.995)	(1.020)	(1.045)	(0.886)	(0.906)
N	35167	34289	35167	34289	35152	34274	35152	34274
Spill-over group	no	no	yes	yes	no	no	yes	yes
Additional controls	no	yes	no	yes	no	yes	no	yes

Coefficients from linear models.
Robust Standard errors in parentheses
* p<0.10 ** p<0.05 *** p<0.001

Table 14 - Effects of NMW upratings on changes in basic hours for adult females.

Horizontal DID estimates from linear models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Treatment Dummy				**Treatment Dummy * Wage Gap**			
2000	0.554	0.448	-0.141	-0.230	0.563	0.478	-0.020	-0.118
	(0.697)	(0.696)	(0.618)	(0.621)	(0.688)	(0.688)	(0.618)	(0.621)
2001	-0.070	0.033	0.313	0.325	0.063	0.141	0.335	0.364
	(0.616)	(0.615)	(0.517)	(0.512)	(0.487)	(0.486)	(0.428)	(0.425)
2002	0.967*	0.578	0.635	0.272	0.883	0.568	0.483	0.200
	(0.581)	(0.548)	(0.533)	(0.497)	(0.559)	(0.538)	(0.507)	(0.483)
2003	-0.498	-0.478	-0.093	-0.072	-0.631	-0.606	-0.220	-0.196
	(0.625)	(0.616)	(0.536)	(0.532)	(0.566)	(0.554)	(0.486)	(0.477)
2004	-0.070	-0.205	0.279	0.159	0.134	0.032	0.364	0.248
	(0.471)	(0.488)	(0.417)	(0.440)	(0.397)	(0.414)	(0.357)	(0.376)
2005	-0.113	-0.230	-0.054	-0.271	0.206	0.044	0.271	0.054
	(0.500)	(0.515)	(0.471)	(0.478)	(0.374)	(0.383)	(0.354)	(0.359)
2006	-0.375	-0.439	-0.049	-0.127	-0.470	-0.522	-0.245	-0.306
	(0.500)	(0.493)	(0.460)	(0.458)	(0.444)	(0.439)	(0.405)	(0.403)
2007	0.003	0.132	0.516	0.633	0.263	0.396	0.552	0.677*
	(0.515)	(0.512)	(0.484)	(0.480)	(0.428)	(0.422)	(0.404)	(0.397)
2008	-0.096	-0.104	-0.277	-0.324	-0.539	-0.554	-0.571	-0.621
	(0.513)	(0.511)	(0.495)	(0.495)	(0.413)	(0.411)	(0.394)	(0.393)
2009	-0.751	-0.860	-0.569	-0.611	-0.660	-0.754	-0.428	-0.478
	(0.654)	(0.668)	(0.656)	(0.667)	(0.595)	(0.604)	(0.587)	(0.595)
2010	0.079	0.223	-0.478	-0.381	0.463	0.640	0.038	0.162
	(0.716)	(0.724)	(0.693)	(0.699)	(0.636)	(0.646)	(0.610)	(0.619)
2011	0.705	0.804*	0.139	-0.010	0.585	0.714	0.190	0.146
	(0.484)	(0.476)	(0.482)	(0.472)	(0.481)	(0.482)	(0.465)	(0.465)
N	52420	51304	52420	51304	52398	51282	52398	51282
Spill-over group	no	no	yes	yes	no	no	yes	yes
Additional controls	no	yes	no	yes	no	yes	no	yes

Coefficients from linear models.
Robust Standard errors in parentheses
* p<0.10 ** p<0.05 *** p<0.001

Table 15 - Average effects of NMW upratings on basic hours before and during the recession.

	Horizontal DID estimates from linear models							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Treatment Dummy				**Treatment Dummy * Wage Gap**			
	Male adults							
2000-2007	-0.281	-0.368	-0.104	-0.164	-0.182	-0.283	-0.086	-0.193
	(0.486)	(0.488)	(0.423)	(0.430)	(0.455)	(0.466)	(0.409)	(0.422)
2008-2011	0.195	0.236	0.947*	1.030*	0.093	0.141	0.675	0.754
	(0.631)	(0.646)	(0.572)	(0.583)	(0.578)	(0.601)	(0.531)	(0.550)
Wald test for equality	0.358	0.556	2.183	2.717	0.140	0.310	1.288	1.864
P-value	0.550	0.456	0.140	0.099	0.708	0.578	0.256	0.172
	Female adults							
2000-2007	0.030	-0.037	0.175	0.086	0.102	0.049	0.206	0.133
	(0.196)	(0.196)	(0.177)	(0.176)	(0.167)	(0.167)	(0.152)	(0.152)
2008-2011	0.001	0.027	-0.254	-0.305	-0.100	-0.061	-0.231	-0.252
	(0.289)	(0.289)	(0.282)	(0.282)	(0.255)	(0.256)	(0.246)	(0.247)
Wald test for equality	0.007	0.034	1.664	1.379	0.439	0.129	2.283	1.764
P-value	0.934	0.854	0.197	0.240	0.508	0.719	0.131	0.184
	18-21 year olds[a]							
2000-2007	0.031	2.084	0.171	1.950	0.727	2.249	0.801	2.100
	(1.411)	(1.659)	(1.257)	(1.477)	(1.364)	(1.628)	(1.246)	(1.480)
2008-2011	-1.708	-1.750	-1.206	-1.540	-0.634	-1.475	-0.217	-1.402
	(1.644)	(1.864)	(1.517)	(1.715)	(1.903)	(2.096)	(1.786)	(1.971)
Wald test for equality	1.155	4.640	0.727	3.858	0.491	3.141	0.278	2.793
P-value	0.283	0.031	0.394	0.050	0.484	0.076	0.598	0.095
Spill-over group	no	no	yes	yes	no	no	yes	yes
Additional controls	no	yes	no	yes	no	yes	no	yes

a: constant group differences across time

Coefficients from linear models. Robust Standard errors in parentheses.

* p<0.10 ** p<0.05 *** p<0.001

Table 16 - Effects of NMW upratings on basic hours for adult males.

	3-ple DID estimates from linear models					
	(1)	(2)	(3)	(4)	(5)	(6)
	Method 1		**Method 2**		**Method 3**	
2000	-0.176	-0.103	-1.018	-0.846	-3.809*	-3.781*
	(2.224)	(2.228)	(2.369)	(2.369)	(2.094)	(2.096)
2001	-2.989	-2.745	-4.253**	-3.993*	-1.074	-0.796
	(1.998)	(2.006)	(2.046)	(2.046)	(1.896)	(1.904)
2002	-1.400	-1.095	-0.042	0.280	-1.469	-1.277
	(2.029)	(2.026)	(2.174)	(2.170)	(2.121)	(2.117)
2003	0.415	0.134	0.229	-0.698	0.251	-0.020
	(1.855)	(1.825)	(2.016)	(1.971)	(1.909)	(1.888)
2004	2.792**	2.746**	2.302*	2.316*	1.897	1.729
	(1.361)	(1.385)	(1.348)	(1.365)	(1.555)	(1.601)
2005	1.080	0.332	1.446	0.750	2.311	1.794
	(1.534)	(1.576)	(1.533)	(1.568)	(1.600)	(1.647)
2006	-2.456*	-2.176*	-2.494*	-2.120*	-5.031**	-4.217**
	(1.296)	(1.235)	(1.313)	(1.248)	(1.796)	(1.982)
2007	1.405	1.392	1.550	1.546	-0.086	-0.088
	(1.490)	(1.501)	(1.458)	(1.469)	(2.113)	(2.101)
2008	0.853	1.032	1.722	1.856	3.371*	3.333*
	(1.387)	(1.348)	(1.326)	(1.288)	(1.897)	(1.915)
2009	1.210	1.026	1.070	0.891		
	(1.579)	(1.658)	(1.543)	(1.626)		
2010	1.078	1.495	-0.325	0.071	1.504	1.452
	(1.741)	(1.805)	(1.608)	(1.671)	(2.388)	(2.679)
2011	0.787	1.000	0.185	0.344	2.694	2.706
	(1.418)	(1.450)	(1.412)	(1.442)	(1.869)	(1.904)
N	35792	34903	35792	34903	35792	34903
Additional controls	no	yes	no	yes	no	yes

Coefficients from a linear model. Robust Standard errors in parentheses.

For the definitions of the methods see page 18.

* p<0.10 ** p<0.05 *** p<0.001

Table 17 - Effects of NMW upratings on basic hours for adult females.

	3-ple DID estimates from linear models					
	(1)	(2)	(3)	(4)	(5)	(6)
	Method 1		**Method 2**		**Method 3**	
2000	1.270	1.073	1.275	1.093	1.817	1.723
	(0.894)	(0.890)	(0.846)	(0.843)	(1.192)	(1.197)
2001	0.269	0.144	0.021	-0.033	0.055	0.029
	(0.841)	(0.831)	(0.825)	(0.820)	(0.803)	(0.797)
2002	0.557	0.122	1.001	0.554	2.050**	1.642*
	(0.757)	(0.727)	(0.752)	(0.721)	(0.931)	(0.906)
2003	-0.615	-0.531	-0.534	-0.480	-0.631	-0.232
	(0.826)	(0.815)	(0.811)	(0.804)	(1.073)	(1.003)
2004	-0.645	-0.700	-0.543	-0.623	-1.539*	-1.797**
	(0.691)	(0.727)	(0.643)	(0.670)	(0.796)	(0.846)
2005	-0.324	-0.547	-0.042	-0.447	-0.435	-0.340
	(0.738)	(0.767)	(0.647)	(0.664)	(0.750)	(0.774)
2006	-1.242	-1.177	-0.499	-0.508	-1.586	-1.571
	(0.808)	(0.795)	(0.702)	(0.695)	(1.071)	(1.077)
2007	-0.111	0.148	0.629	0.793	-0.256	-0.004
	(0.790)	(0.790)	(0.694)	(0.688)	(0.770)	(0.773)
2008	-0.559	-0.548	-0.672	-0.680	0.292	0.211
	(0.783)	(0.786)	(0.659)	(0.660)	(0.939)	(0.954)
2009	-0.794	-0.780	-0.720	-0.828		
	(0.874)	(0.884)	(0.784)	(0.792)		
2010	-0.733	-0.740	-1.011	-0.938	-0.531	-0.341
	(0.788)	(0.791)	(0.690)	(0.689)	(0.929)	(0.943)
2011	0.655	0.785	0.728	0.785	1.674*	1.563*
	(0.763)	(0.750)	(0.674)	(0.664)	(0.861)	(0.807)
N	53343	52205	53343	52205	53343	52205
Additional controls	no	yes	no	yes	no	yes

Coefficients from a linear model. Robust Standard errors in parentheses.

For the definitions of the methods see page 18.

* p<0.10 ** p<0.05 *** p<0.001

Table 18 - Average effects of NMW upratings on basic hours before and during the recession.

	Triple DID estimates from linear models					
	(1)	(2)	(3)	(4)	(5)	(6)
	Method 1		**Method 2**		**Method 3**	
			Male adults			
2000-2007	0.147	0.008	0.134	-0.037	-0.279	-0.414
	(0.595)	(0.599)	(0.601)	(0.604)	(0.675)	(0.685)
2008-20011	1.004	1.174	0.724	0.867	2.685**	2.683**
	(0.763)	(0.780)	(0.736)	(0.752)	(1.161)	(1.190)
Wald test for equality	0.785	1.409	0.385	0.880	4.871	5.084
P-value	0.376	0.235	0.535	0.348	0.027	0.024
			Female adults			
2000-2007	-0.115	-0.185	0.162	0.054	-0.027	-0.029
	(0.278)	(0.279)	(0.258)	(0.258)	(0.317)	(0.319)
2008-20011	-0.360	-0.322	-0.456	-0.451	0.610	0.568
	(0.403)	(0.404)	(0.350)	(0.350)	(0.546)	(0.541)
Wald test for equality	0.251	0.078	2.022	1.351	1.018	0.903
P-value	0.616	0.780	0.155	0.245	0.313	0.342
Additional controls	no	yes	no	yes	no	yes

Coefficients from a linear model. Robust Standard errors in parentheses.

For the definitions of the methods see page 18.

* p<0.10 ** p<0.05 *** p<0.001

Table 19 - Effects of NMW upratings on job entry for male unemployed.

Vertical DID estimates from linear probability models

	(1)	(2)	(3)	(4)
	Method 1		**Method 2**	
2000	-0.001	0.013	0.015	0.029
	(0.053)	(0.052)	(0.055)	(0.055)
2001	-0.007	-0.021	0.007	-0.012
	(0.046)	(0.044)	(0.050)	(0.049)
2002	0.044	0.042	0.027	0.023
	(0.049)	(0.048)	(0.046)	(0.045)
2003	0.065	0.066	0.082	0.078
	(0.059)	(0.059)	(0.059)	(0.058)
2004	0.061	0.052	0.041	0.033
	(0.058)	(0.056)	(0.058)	(0.057)
2005	-0.010	-0.009	-0.026	-0.028
	(0.049)	(0.047)	(0.050)	(0.049)
2006	0.060	0.063	0.008	0.008
	(0.046)	(0.044)	(0.047)	(0.045)
2007	-0.026	-0.026	-0.043	-0.052
	(0.056)	(0.055)	(0.054)	(0.053)
2008	-0.007	-0.007	0.006	0.003
	(0.045)	(0.044)	(0.045)	(0.045)
2009	0.008	0.009	-0.001	-0.001
	(0.038)	(0.037)	(0.038)	(0.037)
2010	-0.030	-0.035	-0.026	-0.029
	(0.043)	(0.042)	(0.039)	(0.040)
2011	-0.012	-0.012	-0.017	-0.020
	(0.046)	(0.046)	(0.046)	(0.046)
N	68118	68118	68118	68118
Additional controls	no	yes	no	yes

Coefficients from a linear probability model.
Bootstrapped Standard errors in parentheses
For the definitions of the methods see page 18.
* p<0.10 ** p<0.05 *** p<0.001

Table 20 - Effects of NMW upratings on job entry for female unemployed.

Vertical DID estimates from linear probability models

	(1)	(2)	(3)	(4)
	Method 1		**Method 2**	
2000	-0.063	-0.059	-0.022	-0.014
	(0.061)	(0.060)	(0.060)	(0.058)
2001	-0.068	-0.053	-0.090	-0.073
	(0.057)	(0.054)	(0.055)	(0.052)
2002	0.030	0.038	0.006	0.015
	(0.062)	(0.061)	(0.060)	(0.058)
2003	-0.020	-0.025	-0.022	-0.027
	(0.061)	(0.059)	(0.060)	(0.057)
2004	0.089	0.077	0.113	0.095
	(0.072)	(0.071)	(0.072)	(0.070)
2005	-0.057	-0.046	-0.040	-0.029
	(0.065)	(0.062)	(0.063)	(0.060)
2006	0.064	0.075	0.067	0.077
	(0.063)	(0.062)	(0.061)	(0.060)
2007	0.057	0.066	-0.012	-0.001
	(0.091)	(0.090)	(0.090)	(0.087)
2008	-0.000	-0.000	-0.014	-0.013
	(0.057)	(0.056)	(0.054)	(0.054)
2009	-0.019	-0.011	-0.003	0.009
	(0.050)	(0.049)	(0.047)	(0.046)
2010	0.026	0.037	0.011	0.023
	(0.056)	(0.055)	(0.056)	(0.054)
2011	-0.039	-0.022	-0.041	-0.022
	(0.050)	(0.049)	(0.049)	(0.049)
N	51255	51255	51255	51255
Additional controls	no	yes	no	yes

Coefficients from a linear probability model.
Bootstrapped Standard errors in parentheses
For the definitions of the methods see page 18.
* p<0.10 ** p<0.05 *** p<0.001

Table 21 - Average effects of NMW upratings on job entry before and during the recession.

Vertical DID estimates from linear probability models

	(1)	(2)	(3)	(4)
	Method 1		**Method 2**	
		Male adults		
2003-2007	0.025	0.025	0.012	0.008
	(0.018)	(0.018)	(0.019)	(0.018)
2008-2010	-0.010	-0.011	-0.009	-0.011
	(0.022)	(0.022)	(0.021)	(0.020)
Wald test for equality	1.617	1.718	0.607	0.543
P-value	0.204	0.190	0.436	0.461
		Female adults		
2003-2007	-0.004	0.002	-0.004	0.003
	(0.023)	(0.022)	(0.023)	(0.022)
2008-2010	-0.009	0.000	-0.011	-0.001
	(0.027)	(0.027)	(0.026)	(0.026)
Wald test for equality	0.013	0.002	0.046	0.011
P-value	0.908	0.968	0.830	0.916
Additional controls	no	yes	no	yes

Coefficients from a linear probability model.
Bootstrapped Standard errors in parentheses
For the definitions of the methods see page 18.
* p<0.10 ** p<0.05 *** p<0.001

Table 22 - Effect of the predicted probability of being hired at the NMW on job entry probability.
Linear probability models.

	(1)	(2)	(3)	(4)
	Men		**Women**	
Pr(NMW)	-0.193***	0.003	-0.230***	-0.032
	(0.025)	(0.023)	(0.030)	(0.024)
99or00*Pr(NMW)	-0.135**	-0.068**	-0.075	0.007
	(0.047)	(0.034)	(0.059)	(0.042)
2001*Pr(NMW)	-0.032	-0.032	-0.024	-0.038
	(0.066)	(0.050)	(0.071)	(0.048)
2002*Pr(NMW)	-0.093	-0.069	-0.033	-0.037
	(0.070)	(0.049)	(0.064)	(0.050)
2003*Pr(NMW)	-0.037	0.031	0.048	0.040
	(0.052)	(0.043)	(0.052)	(0.041)
2004*Pr(NMW)	-0.151**	-0.075*	-0.006	0.028
	(0.050)	(0.042)	(0.053)	(0.046)
2005*Pr(NMW)	-0.077	-0.039	-0.067	-0.057
	(0.053)	(0.041)	(0.057)	(0.042)
2006*Pr(NMW)	0.073	-0.028	0.060	0.004
	(0.049)	(0.036)	(0.047)	(0.037)
2007*Pr(NMW)	-0.019	-0.042	0.061	0.040
	(0.051)	(0.038)	(0.056)	(0.041)
2008*Pr(NMW)	-0.020	-0.045	0.100*	0.063
	(0.051)	(0.038)	(0.057)	(0.042)
2009*Pr(NMW)	0.010	-0.056**	0.117**	0.055
	(0.038)	(0.028)	(0.048)	(0.035)
2010*Pr(NMW)	0.079**	0.011	0.103**	0.054*
	(0.033)	(0.027)	(0.042)	(0.032)
2011*Pr(NMW)	0.054	0.028	0.087*	0.035
	(0.040)	(0.034)	(0.048)	(0.033)
N	94422	94422	70745	70745
Controls	no	yes	no	yes

Estimates from linear probability models. Bootstrapped standard errors in parenthesis.
* p<0.10 ** p<0.05 *** p<0.001

Table 23 - Effect of the predicted probability of being hired at the NMW on job entry probability before and during the recession. Linear probability models.

	(1)	(2)	(3)	(4)
	Men		**Women**	
(200-2007)*Pr(NMW)	-0.049	-0.042*	0.002	0.002
	(0.030)	(0.023)	(0.034)	(0.025)
(2008-2011)*Pr(NMW)	0.041	-0.013	0.103**	0.051*
	(0.029)	(0.024)	(0.036)	(0.027)
Wald Test for equality	18.362	3.102	16.969	7.098
P- Value	0.000	0.078	0.000	0.008
N	94422	94422	70745	70745
Controls	no	yes	no	yes

Estimates from linear probability models. Bootstrapped standard errors in parenthesis.
* p<0.10 ** p<0.05 *** p<0.001

Table 24 - Effects of local area bite of NMW on job entry for male unemployed 1997-2011 (proportion of employees affected by forthcoming NMW)

	(1)	(2)	(3)	(4)
	All men		**Low educated men**	
NMW proportion	0.142	0.217	0.117	0.379
	(0.309)	(0.334)	(0.385)	(0.442)
1999 * NMW prop	-0.467	-0.640	0.312	-0.038
	(0.502)	(0.513)	(0.639)	(0.667)
2000 * NMW prop	-0.641	-0.749	0.035	-0.271
	(0.759)	(0.799)	(0.982)	(1.036)
2001 * NMW prop	-0.751	-0.796	-0.281	-0.517
	(0.465)	(0.489)	(0.570)	(0.590)
2002 * NMW prop	-0.673	-0.746	-1.051	-1.299*
	(0.614)	(0.636)	(0.715)	(0.774)
2003 * NMW prop	0.355	0.291	0.117	-0.109
	(0.612)	(0.637)	(0.720)	(0.761)
2004 * NMW prop	0.135	0.078	-0.031	-0.248
	(0.612)	(0.629)	(0.563)	(0.592)
2005 * NMW prop	0.143	0.092	0.082	-0.075
	(0.415)	(0.452)	(0.633)	(0.692)
2006 * NMW prop	0.484	0.416	0.067	-0.128
	(0.445)	(0.470)	(0.494)	(0.546)
2007 * NMW prop	-0.260	-0.340	-0.129	-0.301
	(0.499)	(0.521)	(0.634)	(0.664)
2008 * NMW prop	0.074	-0.043	0.312	0.048
	(0.463)	(0.482)	(0.580)	(0.614)
2009 * NMW prop	0.193	0.069	0.486	0.231
	(0.368)	(0.385)	(0.454)	(0.485)
2010 * NMW prop	0.471	0.417	0.043	-0.098
	(0.384)	(0.411)	(0.541)	(0.581)
2011 * NMW prop	-0.119	-0.190	-0.372	-0.557
	(0.459)	(0.487)	(0.657)	(0.689)
N observations	78014	75794	45892	44503
N local areas	140	140	140	140
Local area controls	No	Yes	No	Yes

Coefficients from a linear probability model. Standard errors in parentheses, clustered at local area level.
Local areas are counties, UAs, or inner/outer London. All specifications include year dummies and local area fixed effects.
* p<0.10 ** p<0.05 *** p<0.001

Table 25 - Effects of local area bite of NMW on job entry for female unemployed 1997-2011 (proportion of employees affected by forthcoming NMW)

	(1)	(2)	(3)	(4)
	All women		**Low educated women**	
NMW proportion	-0.168	-0.294	-0.132	-0.181
	(0.211)	(0.209)	(0.198)	(0.215)
1999 * NMW prop	-0.092	0.003	-0.309	-0.257
	(0.194)	(0.192)	(0.205)	(0.228)
2000 * NMW prop	-0.053	0.070	-0.102	-0.036
	(0.322)	(0.318)	(0.372)	(0.373)
2001 * NMW prop	-0.285	-0.165	-0.337	-0.276
	(0.198)	(0.207)	(0.208)	(0.226)
2002 * NMW prop	-0.162	-0.062	-0.076	-0.012
	(0.310)	(0.306)	(0.299)	(0.296)
2003 * NMW prop	0.122	0.247	-0.268	-0.200
	(0.345)	(0.339)	(0.303)	(0.301)
2004 * NMW prop	0.167	0.296	0.163	0.249
	(0.240)	(0.232)	(0.231)	(0.239)
2005 * NMW prop	0.374*	0.509**	0.001	0.101
	(0.222)	(0.231)	(0.253)	(0.271)
2006 * NMW prop	0.239	0.383	0.255	0.360
	(0.242)	(0.247)	(0.222)	(0.245)
2007 * NMW prop	0.330	0.505*	0.235	0.368
	(0.272)	(0.285)	(0.308)	(0.336)
2008 * NMW prop	-0.147	0.016	-0.231	-0.113
	(0.233)	(0.237)	(0.236)	(0.266)
2009 * NMW prop	0.076	0.304	-0.042	0.121
	(0.406)	(0.390)	(0.367)	(0.353)
2010 * NMW prop	-0.025	0.163	-0.426	-0.299
	(0.270)	(0.269)	(0.309)	(0.306)
2011 * NMW prop	0.012	0.223	-0.232	-0.052
	(0.324)	(0.325)	(0.453)	(0.444)
N observations	56306	54922	36675	35692
N local areas	140	140	140	140
Local area controls	No	Yes	No	Yes

Coefficients from a linear probability model. Standard errors in parentheses, clustered at local area level.
Local areas are counties, UAs, or inner/outer London. All specifications include year dummies and local area fixed effects.
* p<0.10 ** p<0.05 *** p<0.001

Table 26 - Effects of local area bite of NMW on job entry for unemployed 1997-2011 (proportion of employees affected by forthcoming NMW)

	(1)	(2)	(3)	(4)
	All men		**Low educated men**	
NMW proportion	0.179	0.254	0.117	0.381
	(0.308)	(0.334)	(0.387)	(0.442)
1999-2007 * NMW prop	-0.168	-0.254	-0.036	-0.265
	(0.366)	(0.396)	(0.455)	(0.512)
2008-2011 * NMW prop	0.161	0.062	0.185	-0.031
	(0.321)	(0.349)	(0.465)	(0.510)
Wald test for equality	1.204	1.128	0.395	0.479
P-value	0.273	0.288	0.530	0.489
	All women		**Low educated women**	
NMW proportion	-0.174	-0.297	-0.126	-0.171
	(0.223)	(0.222)	(0.198)	(0.212)
1999-2007 * NMW prop	0.044	0.158	-0.085	-0.018
	(0.174)	(0.173)	(0.153)	(0.172)
2008-2011 * NMW prop	-0.051	0.109	-0.259	-0.152
	(0.238)	(0.239)	(0.223)	(0.224)
Wald test for equality	0.438	0.106	0.838	0.469
P-value	0.508	0.745	0.360	0.494
Local area controls	No	Yes	No	Yes

Coefficients from a linear probability model. Standard errors in parentheses, clustered at local area level.
Local areas are counties, UAs, or inner/outer London. All specifications include year dummies and local area fixed effects.
* p<0.10 ** p<0.05 *** p<0.001

Table 27 - Effects of local area bite of NMW on job entry for male unemployed 1997-2011 (Kaitz index)

	(1)	(2)	(3)	(4)
	All men		**Low educated men**	
Kaitz	-0.088	-0.114	0.086	0.133
	(0.101)	(0.103)	(0.132)	(0.142)
1999 * Kaitz	0.060	0.075	0.064	0.030
	(0.080)	(0.081)	(0.093)	(0.094)
2000 * Kaitz	0.085	0.103	0.016	-0.017
	(0.107)	(0.106)	(0.103)	(0.115)
2001 * Kaitz	-0.003	0.012	0.082	0.045
	(0.086)	(0.087)	(0.096)	(0.106)
2002 * Kaitz	0.052	0.065	-0.053	-0.095
	(0.079)	(0.083)	(0.120)	(0.137)
2003 * Kaitz	0.111	0.121	0.012	-0.025
	(0.079)	(0.088)	(0.084)	(0.098)
2004 * Kaitz	0.211*	0.220**	0.026	-0.002
	(0.107)	(0.106)	(0.096)	(0.100)
2005 * Kaitz	0.031	0.044	-0.000	-0.020
	(0.073)	(0.080)	(0.149)	(0.166)
2006 * Kaitz	0.145*	0.152*	0.100	0.074
	(0.075)	(0.079)	(0.115)	(0.130)
2007 * Kaitz	-0.020	-0.016	-0.111	-0.142
	(0.100)	(0.099)	(0.112)	(0.124)
2008 * Kaitz	-0.031	-0.031	-0.130	-0.163
	(0.078)	(0.088)	(0.115)	(0.127)
2009 * Kaitz	0.083	0.076	0.066	0.024
	(0.064)	(0.072)	(0.111)	(0.121)
2010 * Kaitz	0.152	0.157	0.031	0.010
	(0.106)	(0.117)	(0.155)	(0.169)
2011 * Kaitz	-0.049	-0.048	-0.223*	-0.248*
	(0.086)	(0.094)	(0.124)	(0.137)
N observations	78014	75794	45892	44503
N local areas	140	140	140	140
Local area controls	No	Yes	No	Yes

Coefficients from a linear probability model. Standard errors in parentheses, clustered at local area level.
Local areas are counties, UAs, or inner/outer London. All specifications include year dummies and local area fixed effects.
* p<0.10 ** p<0.05 *** p<0.001

Table 28 - Effects of local area bite of NMW on job entry for female unemployed 1997-2011 (Kaitz index)

	(1)	(2)	(3)	(4)
	All women		**Low educated women**	
Kaitz	-0.006	-0.081	-0.037	-0.088
	(0.111)	(0.120)	(0.110)	(0.124)
1999 * Kaitz	-0.062	-0.025	-0.079	-0.042
	(0.057)	(0.059)	(0.094)	(0.099)
2000 * Kaitz	-0.062	-0.017	-0.019	0.024
	(0.106)	(0.109)	(0.103)	(0.102)
2001 * Kaitz	-0.033	0.017	-0.053	-0.008
	(0.067)	(0.071)	(0.070)	(0.069)
2002 * Kaitz	-0.113	-0.068	-0.029	0.016
	(0.090)	(0.094)	(0.090)	(0.086)
2003 * Kaitz	-0.001	0.049	-0.105	-0.056
	(0.118)	(0.120)	(0.074)	(0.074)
2004 * Kaitz	-0.014	0.038	0.026	0.082
	(0.073)	(0.075)	(0.077)	(0.079)
2005 * Kaitz	0.115	0.172**	-0.042	0.018
	(0.071)	(0.073)	(0.071)	(0.078)
2006 * Kaitz	0.108	0.167**	0.144**	0.205**
	(0.074)	(0.077)	(0.071)	(0.072)
2007 * Kaitz	0.070	0.136	0.080	0.150
	(0.083)	(0.087)	(0.084)	(0.090)
2008 * Kaitz	-0.088	-0.020	-0.104	-0.035
	(0.059)	(0.063)	(0.086)	(0.098)
2009 * Kaitz	-0.126	-0.049	-0.139	-0.067
	(0.105)	(0.111)	(0.084)	(0.087)
2010 * Kaitz	-0.065	0.006	-0.220**	-0.153*
	(0.067)	(0.070)	(0.080)	(0.087)
2011 * Kaitz	-0.008	0.067	-0.118	-0.042
	(0.106)	(0.108)	(0.167)	(0.161)
N observations	56306	54922	36675	35692
N local areas	140	140	140	140
Local area controls	No	Yes	No	Yes

Coefficients from a linear probability model. Standard errors in parentheses, clustered at local area level.
Local areas are counties, UAs, or inner/outer London. All specifications include year dummies and local area fixed effects.
* p<0.10 ** p<0.05 *** p<0.001

Table 29 - Effects of local area bite of NMW on job entry for unemployed 1997-2011 (Kaitz index)

	(1)	(2)	(3)	(4)
	All men		**Low educated men**	
Kaitz	-0.091	-0.120	0.056	0.104
	(0.098)	(0.099)	(0.123)	(0.132)
1999-2007 * Kaitz	0.073	0.085	0.025	-0.008
	(0.056)	(0.058)	(0.069)	(0.083)
2008-2011 * Kaitz	0.051	0.051	-0.036	-0.068
	(0.060)	(0.072)	(0.109)	(0.120)
Wald test for equality	0.075	0.182	0.634	0.650
P-value	0.784	0.670	0.426	0.420
	All women		**Low educated women**	
NMW Kaitz	-0.007	-0.089	-0.026	-0.082
	(0.109)	(0.116)	(0.108)	(0.121)
1999-2007 * Kaitz	-0.008	0.038	-0.016	0.031
	(0.061)	(0.066)	(0.043)	(0.045)
2008-2011 * Kaitz	-0.081	-0.022	-0.154**	-0.094
	(0.066)	(0.074)	(0.059)	(0.063)
Wald test for equality	4.219	2.436	8.754	6.372
P-value	0.040	0.119	0.003	0.012
Local area controls	No	Yes	No	Yes

Coefficients from a linear probability model. Standard errors in parentheses, clustered at local area level.
Local areas are counties, UAs, or inner/outer London. All specifications include year dummies and local area fixed effects.
* p<0.10 ** p<0.05 *** p<0.001

Table 30 - Effects of local area bite of NMW on job entry for 18-21 year olds 1997-2011 (Kaitz index)

	(1)	(2)
Kaitz	-0.088	-0.114
	(0.101)	(0.103)
1999 * Kaitz	0.060	0.075
	(0.080)	(0.081)
2000 * Kaitz	0.085	0.103
	(0.107)	(0.106)
2001 * Kaitz	-0.003	0.012
	(0.086)	(0.087)
2002 * Kaitz	0.052	0.065
	(0.079)	(0.083)
2003 * Kaitz	0.111	0.121
	(0.079)	(0.088)
2004 * Kaitz	0.211*	0.220**
	(0.107)	(0.106)
2005 * Kaitz	0.031	0.044
	(0.073)	(0.080)
2006 * Kaitz	0.145*	0.152*
	(0.075)	(0.079)
2007 * Kaitz	-0.020	-0.016
	(0.100)	(0.099)
2008 * Kaitz	-0.031	-0.031
	(0.078)	(0.088)
2009 * Kaitz	0.083	0.076
	(0.064)	(0.072)
2010 * Kaitz	0.152	0.157
	(0.106)	(0.117)
2011 * Kaitz	-0.049	-0.048
	(0.086)	(0.094)
N observations	78014	75794
N local areas	140	140
Local area controls	No	Yes

Coefficients from a linear probability model. Standard errors in parentheses, clustered at local area level and local area fixed effects.
Local areas are counties, UAs, or inner/outer London. All specifications include year dummies and local area fixed effects.
* p<0.10 ** p<0.05 *** p<0.001

Table 31 - Effects of local area bite of NMW on job entry for 18-21 year old unemployed 1997-2011 (Kaitz index)

	(1)	(2)
Kaitz	-0.130	-0.165
	(0.118)	(0.116)
1999-2007 * Kaitz	0.096*	0.115**
	(0.051)	(0.051)
2008-2011 * Kaitz	0.042	0.059
	(0.086)	(0.088)
Wald test for equality	1.239	1.195
P-value	0.266	0.275
Local area controls	No	Yes

Coefficients from a linear probability model. Standard errors in parentheses, clustered at local area level.
Local areas are counties, UAs, or inner/outer London. All specifications include year dummies and local area fixed effects.
* p<0.10 ** p<0.05 *** p<0.001