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ABSTRACT

Employment, Inequality and the UK National Minimum Wage over the Medium-Term*

This paper assesses the impact of the National Minimum Wage (NMW) on employment and inequality in the UK over the decade since its introduction in 1999. Identification is facilitated by using variation in the bite of the NMW across local labour markets and the different sized year on year up ratings of the NMW. We use an 'incremental differences-in-differences' (IDiD) estimator which allows us to estimate the effects of the NMW in each year since its introduction. We find that an increased bite of the NMW is associated with falls in lower tail wage inequality. Moreover, while the average employment effect of the NMW over the entire period is broadly neutral, there are small but significant positive employment estimates from 2003 onward, when the average bite of the NMW was at its highest since its introduction.

JEL Classification: J0, J2, J3

Keywords: minimum wage, employment, inequality

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I. INTRODUCTION

It is now more than ten years since the National Minimum Wage (NWM) was introduced in the UK in April 1999. This rather extended length of time since implementation affords us an opportunity to take a retrospective look at the impact of the NMW. Most existing UK studies, (Stewart, 2002, 2004a, 2004b) have focused on the impact of the introduction of the NMW, finding, broadly, that the employment effects of the introduction were negligible. Aside from adjustment along other dimensions such as productivity, profits, hours or prices, or simply that the initial rate was too low in the wage distribution, another possible reasons for this, arguably counter-intuitive employment effect is that any longer-run effects have not been captured by previous studies. Since in the short-run the costs of adjusting inputs tend to be high, the response of employment to NMW increases might not be immediate. As recently pointed out by Neumark and Washer (2007): "Most of the existing research on the United Kingdom has been limited to estimating short-run effects, and in our view, the question of the longer-run influences of the national minimum wage on UK employment has yet to be adequately addressed". In this paper we take a medium to long run look at the impact of the NMW in the UK and its up-ratings and try to assess whether this has had a differential impact across heterogeneous geographical areas.

Since inception, the UK minimum wage has been administered on a national basis, with both adult and youth rates applying to all parts of the country. However, the issue of whether a national minimum adequately reflects putative regional variation in productivity has recently been mooted in government and in the media.[1] The longstanding geographic variation in wage rates across the UK does indeed have consequences for the bite of the national minimum wage, (NMW) in different areas. As Stewart (2002) points out, the NMW reaches further up the wage distribution in certain parts of the country than in others. We therefore make use of both this geographical variation and the variation in the real level that the NMW has been set at over time

[1] Daily Telegraph 23 July 2007, http://www.telegraph.co.uk/news/uknews/1558174/Gordon-Brown-to-vary-minimum-wage-over-UK.html

in order to see how changes in the local area NMW incidence over several years of the minimum wage's existence are correlated with changes in local area performance. Since the level of the NMW is typically announced six months in advance of any uprating, we also explore issues of advance implementation of employment changes in the dynamic specifications that follow.

While there are a large number of studies on the labour market impact of the NMW, especially on the impact on employment, (see Brown et al. (1982) and Card and Krueger (1995) for extensive reviews of the literature), only a few studies evaluate the impact of the NMW by exploiting geographical variation in local or regional labour markets, (See Card (1992) or Neumark and Washer (1992) for the United States, Stewart (2002) for the UK). This paper builds on that small literature by examining the impact of the NMW in the UK over the period 1997-2007, comparing the period two years before its introduction with the subsequent history of the NMW and its up-ratings. This enables us to provide an additional insight by distinguishing effects between those in a NMW policy off period compared to each incremental up-rating of the NMW in subsequent years. Hence instead of using a simply policy on - policy off, difference-in-difference model, we examine a model in which each year's change in the NMW is considered as a separate interaction effect. This 'Incremental Diff-in-Diff' (IDiD) estimator is a logical corollary of the econometric model suggested by Wooldridge (2007) and Bertrand et al. (2004) in that it introduces a yearly interaction for each up-rating of the NMW so that we may gauge the impact of each change in the NMW. We use this IDiD procedure to evaluate the year on year impact of the uprating of the NMW on both employment and inequality.

Secondly, we seek to assess whether the definition of the variable used to capture the impact of the NMW makes a notable difference to the analysis. In the empirical literature there is some debate over the exact definition of which variable to use to measure (or instrument for) the NMW. In our work, three different minimum wage variables are used and compared. Two measures focus on the proportion of workers directly affected by increases in the minimum wage: the minimum wage “share” (the proportion paid at or below the minimum wage) and the “spike” (the proportion paid at the minimum). The third measure is the Kaitz index, the ratio of the minimum wage to average wages in the local area.

Thirdly, we examine whether the definition of the geographical unit used for the analysis matters. Since the definition of what constitutes a 'local labour market' in Great Britain is still open to discussion, the analysis is undertaken at three different levels of geographical aggregation. As in Stewart (2002), the data can be divided into 140 areas comprising Unitary Authorities and Counties. However, the same analysis can be done using 406 Unitary Authorities and Districts. We also look at how the results change if we use the definition of 67% of people living and working in the same geography to capture a local labour market, as now used by the UK national statistics office to define a “travel to work area”, (TTWA). We remain agnostic as to what the correct definition of a 'local labour market' is and let the data tell us whether such definitional difficulties matter.

Finally, the paper examines the robustness of our results with regard to the specification issues associated with: dynamic specification to incorporate the lagged effects of the impact of the NMW, fixed effects for geographical areas, time and interaction effects, and we also assess whether the estimates differ if we include young people (those aged 16-25) or just use them separately for the analysis. In this testing we suggest that much of the previous literature is sometimes presented as if the results are in stark contrast to each other. Our take on this literature is that it often estimates fundamentally different parameters and that this explains a large degree of the differences in results.

Previous research in the UK focused mainly on the employment effects of the NMW and for the most part found mainly no impact. In our companion report and discussion paper Dolton et al. (2008) we broaden our examination of the labour market effects of the NMW to include both unemployment and hours. However since, one of the motivations of the introduction of the minimum wage was to help reduce the trend of rising wage inequality which characterised the British labour market in the 80s and 90s, (Low Pay Commission 1998), we show how changes in the local area minimum wage incidence are related to the extent of wage inequality in the locality along with our employment estimates.

The paper is organised as follows. Section II describes the datasets used and the characteristics of the data and contains a description of the maps of the incidence of the minimum wage and the measures of local area performance in each local area. Section III outlines the methodology for the analysis. The main results of the analysis are presented in section IV. Section V concludes.

II. DATA

The central idea of this paper is to see whether geographic variation in the "bite" of the minimum wage is associated with geographic variation in employment and wage inequality. Geographical variation in wages in the UK is exploited in order to evaluate the impact of the NMW on a series of indicators of local area performance. The data used in this study are drawn primarily from three sources. Data on earnings and a restricted number of covariates all disaggregated by geography is provided by the New Earnings Survey (NES) from 1997 to 2003 and by the Annual Survey of Hours and Earnings (ASHE), which replaced the NES in 2004. In both surveys, conducted in April of each year, employers are asked to provide information on hours and earnings of the selected employees. The geographic information collected for the full sample period used in the paper is based on workplace rather than residence. This is the only dataset that has hourly wage information from 1997 to 2007 at the various levels of geographical disaggregation used in this paper. Alongside the hourly wage, the ASHE data enable us to compute different measures of wage inequality at the same geographic level, (we use the $50^{th}/5^{th}$,

$50^{th}/10^{th}$ percentiles of the wage distribution. See Appendix II for a detailed description of the limitations affecting ASHE\NES dataset).

The geographic variation in wages will reflect the demographic and industrial composition of each local labour market. The changing industrial composition of an area and the extent to which industries are low and high paying will affect the changing incidence of the minimum wage working in a locality. Likewise the skill, age and gender composition of the local workforce. To a certain extent we can control for variation in these factors with a set of time varying local labour market control variables, drawn from either ASHE or matched in from complementary Labour Force Survey (LFS) data. However, the choice of what constitutes a local labour market is open to discussion, therefore the analysis is conducted at three different levels of aggregation. First, the analysis is conducted at Unitary Authority and District level which includes 32 London boroughs, 238 districts[2], 36 Metropolitan districts and the 46 Unitary Authorities in England. This geography also includes the 22 Unitary Authorities in Wales and the 32 Unitary Councils in Scotland, resulting in 406 local areas in Great Britain. The median ASHE sample cell size is 311 and the smallest cell is 37. The second level of analysis is conducted at Unitary Authority and County level including 34 English counties, 6 English metropolitan counties, 46 English Unitary authorities, Inner and Outer London and finally 52 Unitary authorities in Scotland and Wales. [3] This results in 140 local areas in Great Britain. Here the median sample cell size is 575 and the smallest cell is 42. The final level of our analysis is to use a general definition of a TTWA, by aggregating up from district level to create areas in which 67% of people living and working in the same geography. Since TTWAs are not available for the entire period considered in this study the only option was to attempt to replicate our

[2] The London borough City of London and the district Isles of Scilly are excluded from the analysis due to small sample sizes.

[3] The Orkney Islands, Shetland Isles and Western Isles are aggregated together. The 36 English metropolitan districts are combined into 6 English Metropolitan Counties. London Boroughs are aggregated into Inner and Outer London. This allows to have matched geographies in the LFS and in the ASHE/NES, using the definition of the variable "uacnty" in the LFS.

results for the most 'reasonable' definition of a TTWA that we could manually reconstruct from the data available. (The mechanics of how to do this are described in an appendix which is available to interested readers on request). This gave us 138 new geographical areas for which we repeated all our analysis. Some of the estimated effects using TTWA instead of the formal geographical administrative areas are given in the Appendix.

We then match local area employment data from the LFS with the minimum wage covariates generated from ASHE. There is an important feature of the timing of data collection which we exploit in order to try and make sure that our employment variable is measured after the up-rating of the NMW. The ASHE and NES estimates for hourly earnings and therefore the minimum wage variables used in this paper are recorded in April of each year. Since the minimum wage was first introduced in April 1999 but then up-rated in October of each following year, the NMW variables are therefore generally recorded six months after each NMW up-rating. There are however two exceptions: April 1999 which is contemporaneous to the introduction of the minimum and April 2000, which is one year from the introduction of the minimum. To reduce simultaneity concerns, the wage data in April of year *t* is regarded as having absorbed any effect of the NMW upgrade in October of year *t-1*. This is in turn matched to employment data taken from June to August of year *t*, while data on unemployment is collected from May to September of year t.[4] This means that the estimated impact effect we identify is a mixture of the impact of the up-rating in year *t-1* and any changes from the already announced anticipation of the effect of the new NMW level in year t. As a robustness check we have varied our timing assumptions and our results suggest that any anticipation effect is negligible.[5]

[4] For 1997 and 1998, data on employment rates are collected from March 1997 to February 1998 and from March 1998 to February 1999. Quarterly data is not available for these two calendar years. Since LFS Local Area data is only available in seasonal quarters, it is only possible to use the June-August quarter and not a longer period (eg. from May to September) unlike say the monthly claimant count unemployment data.

[5] Swaffield (2008) shows that there is little early upward adjustment in wages in the six months prior to October over several years of data.

Data on employment at these levels of aggregation derived from the LFS are available via NOMIS for yearly data for 1997 and 1998. For the period 1999 to 2005 we use employment rates calculated from the quarterly LFS local area data. For the years 2006 and 2007 we use the quarterly LFS Special License data to calculate the employment rate. According to the US literature, young workers are considered to be the most exposed to the possible negative effects of a uniform NMW. While the UK has always set a lower youth minimum, it seems worth looking for any differential effects of the NMW across age groups. Data availability means that we can do our analysis separately for three age groups: All workers from 16 years old to retirement age (65 years for men and 60 for women); Adults workers, from 25 years old to retirement age[6]; Younger workers aged 16 to 24.

Measures of the National Minimum Wage

One of the most widely used variables in the literature is the Kaitz index, defined as the ratio of the minimum wage to some measure of the average wage. We use the median wage in our study. The closer the Kaitz index to unity the "tougher" the bite of minimum wage legislation in any area. However, the denominator can be influenced by factors other than the level of the NMW and so the median wage is arguably more endogenous in an employment regression. For example, a positive correlation between the employment rate and the median wage might be generated by an exogenous labour demand shift. This will create a negative correlation between the Kaitz index and the employment rate. In view of these problems with the Kaitz index, two other minimum wage variables are used in this study. These two measures focus on the proportion of workers directly affected by increases in the minimum wage: the minimum wage "share" proportion paid at or below the minimum wage, and the "spike" (proportion paid at the minimum). The larger the spike or the shares, the more likely the impact of the minimum wage

[6] Due to the presence of age bands in the area-level LFS, it is not possible to analyse the impact of the NMW on adults from 22 years up that the actual coverage of the adult rate of the NMW would require. Analysis is therefore restricted to persons from 25 years up.

on the local wage. The "shares" and the "spike" should exclude the variation in real minimum wages that results from inflation or other aggregate factors (Neumark and Washer, 2007).

The logic of our identification strategy is evident in the descriptive statistics in Figures 1 to 3. Figure 1 highlights the temporal variation in the NMW, comparing the nominal hourly wage level of the adult NMW over time with the notional level which would have been achieved if the NMW were indexed to average earnings. The Figure shows how the NMW started off by being lower than the average rise in earnings and then rose more steeply than this series. Most marked is the rise in this level in both real and nominal terms since 2003. The largest rises in the NMW are in 2001, 2004 and 2006. This is mirrored in the rising level of the Kaitz Index over the same years shown in Figure 2.

As well as temporal variation in the NMW, there are clear geographic differences in the bite of the NMW. The 95% range for the Kaitz index is around 20 percentage points and the spread for the share estimate is around 5 points. This pattern does not change much over the 1997-2007 period. While the average value of the Kaitz has risen, there is less evidence that these spreads have risen or fallen consistently over time. Figure 3 plots how these patterns of geographical low pay vary across the UK at the inception of the NMW in 1999 alongside the changes in the NMW share over the period 1999-2007. The bite of the minimum wage in the region around London tends to be lower than in the rest of the country. Areas particularly affected are the rural periphery of the country and the formerly industrialised urban areas. Over time the map shows that the "bite" of the minimum wage has increased across more areas. The biggest changes in the bite occur in parts of the Midlands, Hampshire, Wiltshire and Dorset and parts of Lancashire and the North East. As we show below, these changes are associated with changes in the local area levels of wage inequality. The tougher the NMW bites, the bigger the effect on local measures of wage inequality.

III. METHODOLOGY AND IDENTIFICATION

To understand any of the estimation results relating to the impact of the NMW one must be clear about the exact form of the econometric specification and which parameters the model aims to identify in the model.

Among the first to use panel data to address the question of the impact of the MW were Neumark and Wascher (1992) who used US state data from 1973-1989. They estimated the model:

$$E_{jt} = \alpha + \gamma T_t + J_j + \beta MW_{jt} + \delta X_{jt} + \varepsilon_{jt} \qquad (1)$$

Where E_t is employment at time t in State j , MW_{jt} is the level of the MW (adjusted for coverage) at time t in State j, X_t is a set of controlling regressors at time t in State j, T_t is a set of year effects and J_j is a set of State fixed effects. Fixed effect estimation identifies potential causal inferences based on changes in the regressor and regressand given the assumption that the unobserved heterogeneity across areas remains constant over time periods. Later Neumark and Wascher (2004) use the same specification to estimate the impact of the NMW laws across countries with the slight modification that now the MW_{jt} term is similar to the Kaitz index using the ratio of the NMW in country j at time t divided by the average wage in that year[7]. Neumark and Wascher in their various papers, whether at the US State level or at the level of countries, also find a negative employment effect of the NMW.

The logical critique of this panel model is that it still suffers from potentially all the same sources of potential heterogeneity bias as the simple time series model. Indeed it could even be argued that using geographical States as the unit of observation could potentially have even more problems - if for example - one state legislature's decision to implement or change a MW is heavily influenced by another neighbouring state's policy decision. This concern is less of a

[7] Usually the Kaitz index is also weighted by some measure of 'coverage' of the NMW in the sense of the fraction of the labour force that the NMW applies to.

problem in the UK context as there is a national NMW rather than a state MW - in which case the actual level (and change) in the NMW is not under the control of the authorities in any particular location.

A related methodological departure focused on identification is suggested by Card (1992) and Stewart (2002) in which a 'structural' econometric model consists of two equations. The first is a form of labour demand equation which suggests that any change in the employment rate in area *j* is a movement along the labour demand curve which results from a change in the wage level in area *j*.

$$\Delta E_j = \gamma_0 + \eta \Delta W_j + u_{1j} \tag{2}$$

The second equation is a form of identity suggesting that the wage increase in area j is a function of the proportion in the area who are 'low paid', P_j.

$$\Delta W_j = \alpha_1 + \lambda P_j + u_{2j} \tag{3}$$

Substituting equation (3) into equation (2) we get:

$$\Delta E_j = \gamma_0 + \beta P_j + \varepsilon_j \tag{4}$$

Where $\beta = \eta\lambda$, with λ assumed to be positive, implying that β has the same sign as η which basic economic theory would suggest is negative if the demand for labour falls as wages rise. According to Stewart (2002) the precondition for identification is that the proportion in the area that are 'low paid', P_j is a predetermined instrument for the endogenous wage change.

The central idea of our paper is also to see whether geographic variation in the "bite" of the minimum wage is associated with geographic variation in employment. However, we also allow the effect of any treatment to vary over time, given the differential pattern of upratings that we observe in the data. This can be done by pooling over the eleven year period and letting the treatment be the measures of the "bite" of the NMW in each area at time t, $P_{jt,}$ so that the model estimated is:

$$E_{jt} = \gamma_0 + J_j + \sum_{t=1999}^{2007} \gamma_t Y_t + \theta_0 P_{jt} + \sum_{t=1999}^{2007} \theta_t^{IDID} Y_t P_{jt} + + \delta X_{jt} + \varepsilon_{jt} \tag{5}$$

Where E_{jt} is a measure of area labour market performance in area j at time t, J_j are area effects, and Y_t is a set of year effects. Area fixed effects are included to control for omitted variables that vary across local areas but not over time such as unmeasured economic conditions of local areas economies that give rise to persistently tight labour markets and high wages in particular areas independently of national labour market conditions. Time fixed effects control for omitted variables that are constant across local areas but evolve over time.

The Incremental Difference-in-Difference coefficients θ_t^{IDiD} on the interaction of the year dummies and the measure of the bite, capture the average effect of the up-rating of the NMW in each year, starting from the introduction of the policy in 1999 all relative to the 'off period' of 1997 and 1998, provided of course that the proportion in the area who are 'low paid', P_{jt} is a valid instrument for the endogenous wage change. The advantage of using the IDiD estimation procedure is that it facilitates the estimation of year on year incremental effects of each year's up-rating. So even if the average effect over all years is insignificantly different from zero, this does not mean that the effect of any individual year's change in the NMW is also zero. Note that one cannot deduce the longer run effect of all the changes in the NMW by simply summing all the year-on-year IDiD coefficients.[8] The long run effect can only be measured in aggregate by using one DiD coefficient for the whole period. We therefore present both short run IDiD and medium run DiD estimates in what follows.

[8] This is because some additional (untestable) assumption relating to independence of effects over time would be necessary. In addition, since we use a dummy variable interaction term, rather than a normalised metric on how large each increment was then this also makes aggregation of the individual interaction term estimates difficult.

The literature is silent on how to untangle autocorrelation in panel data with very short time series like ours. An additional concern is the obvious one of spatially contiguous areas giving rise to heteroskedastic errors. With regard to the latter problem one approach is to model the form of these spatial relations. As all our geographical areas have bordering areas then it may well be that there is a clear relationship between these contiguous areas. The complex nature by which these neighbouring states have local labour markets which are inter-related and how to model these effects is left for future work. In the absence of an appropriate spatial model, we calculate standard errors robust to heteroskedasticity and serial correlation of unknown form, Wooldridge (2002 p.275), which gives consistent, if inefficient, estimates. Another alternative is to simply cluster the data by local area. [9]

Identification Issues

One important question to ask is how long it should take the introduction (or changes) in the NMW to have its full effects on employment and other economic indicators (especially since some of the variables in the data are already measured with a lag). From an empirical point of view, this raises the specification issue about including a lagged effect of the minimum wage variable in the regression. The debate is on this question is still ongoing. On the one hand, employers might react relatively quickly to increases in minimum wages. Employers might even adapt before the implementation of the minimum wage. Brown et al. (1982), regarding employment, argue that: ”One important consideration is the fact that plausible adjustment in employment of minimum wage workers can be accomplished simply by reducing the rate at which replacements for normal turnover are hired.”, (p.496). Clearly the size of any change to the existing wage bill generated by the NMW matters here. Another reason given by the authors is that minimum wages increases are announced months before they are implemented – typically six months in the UK - therefore firms may have begun to adapt before the increase of the

[9] Clustering by local area rather than using the general robustness correction makes little or no qualitative difference to our conclusions.

minimum wage come effectively into force. On the other hand, it might take time for employers to adjust factors inputs to changes in factors prices. Hamermesh (1995) points out that in the short run capital inputs might be costly to adjust. If firms adjust capital slowly following an increase of the minimum wage, the adjustments of labour input might be slowed as well. The use of a lagged minimum wage measure as well as the inclusion of fixed effects in the regression also helps to decrease the possible endogeneity of the minimum wage variable which occurs from correlation of either the proportion paid at the minimum or, in case of the Kaitz index, the minimum wage and the median wage with labour market conditions or productivity.

A further issue of identification arises from the 'common trends assumption' which, in our context, is that the effect of market conditions will be the same across all geographic units in the absence of the introduction of the NMW. One way of examining this is to consider whether the employment rate has the same underlying trend across all our geographical units before the introduction of the NMW. In our case we cannot do this because the small geography LFS data which we use to construct the employment rate does not go back before 1997. However, it is possible to have a longer off-period starting from 1994 and using 95 areas, which correspond to the coding used on the NES up to 1996.[10] The results of the test give us some confidence about the internal validity of the model, being unable to reject the null of a common trend at 10% level for all the age group considered in the study. [11] Whilst this is no proof of the presence of common trends in our data, this gives us some confidence about the internal validity of our model for the full set of more detailed geographies.

The NMW was not the only labour market policy instrument in operation over the period that varied by area and time. It may be that identification of a NMW effect is also compromised by any

[10] The areas comprise all existent counties, the counties abolished with the 1996 local government reform and the London boroughs. The "City of London" was deleted from the dataset due to small sample size and the Scottish Islands were excluded from the analysis because they are not present in the data across all years.

[11] For adult workers (25 years to retirement) we cannot reject the null of a common trend at the 10% level ($F(94, 285) = 1.41$). For young workers (16 to 24 years) we cannot reject the null of a common trend at the 5% level ($F(94, 285) = 1.15$). For all workers (16 years to retirement) we cannot reject the null of a common trend at the 10% level ($F(91, 276) = 1.45$) if we omit three areas, all with small sample sizes, (Scottish Borders, Gwynedd and Shropshire). However, omitting these areas from our IDiD regressions does not change our main results.

correlation of these other interventions with changes in the local bite of the NMW. The set of area and time varying covariates in the control vector X_{jt} help net out some of the concerns over these issues. [12]

IV. RESULTS.

We begin with a summary of the association between the level of lower tail wage inequality and the bite of the NMW in the local area. If there appears to be an impact on the wage distribution then this might suggest there would be effects on other measures of local labour market performance. There is good reason to expect that imposition and then raising of the NMW will have positive effects in reducing wage inequality at the bottom end of the income distribution. If one truncates the income distribution from the left by forcing employers to pay the lowest earners at a specified minimum then automatically one expects that (unless there are large spillover effects) we would find that inequality would be reduced as the NMW rises, other things equal. Dickens and Manning (2002) report evidence of these effects in the UK around the introduction and other authors report similar findings from the US. (See DiNardo et al. (1996), Lee (1999) and Tuelings (2000)).

There are obvious endogeneity concerns here when regressing a measure of wage inequality on another variable linked to wages. For this reason we do not use the Kaitz index as an NMW toughness proxy and the remaining estimates should be seen as indicative only of correlations in the data. Table 1 presents our IDiD results using model (5) for the effects of the year on year upratings of the NMW on local area wage inequality as measured by the log 50-5 and the log 50-10 percentile ratio. The results are given for two different local labour market

[12] Employment rates for groups more or less likely to have been affected by the NMW within areas as a means to identification through a triple difference in difference, could, in principle be disaggregated by local area and industry or education from 2004 onward using the Annual Population Survey, though the level of area disaggregation would have to be larger than that used in the present study because of sample size limitations. Wages could be disaggregated by (macro) region and industry back to 1997.

definitions for all adults aged 16 and over. We have also performed our estimation for the TTWA as defined above. Our results with their TTWA robust counterparts can be summarised in a graphical representation of the estimates coefficients from the underlying regression model. Figure 4 graphs the estimated NMW coefficients along with the 95% confidence interval for both the 406 and TTWA area levels of aggregation. The coefficients of our incremental difference in differences regression are all negatively significant and increasingly so over time, indicating that lower tail wage inequality fell more in areas where the NMW bit most. It is also important to note that there is a clear overlap in all of the 95% confidence intervals for both these different geographies.

There are also smaller effects moving up wage distribution, again consistent with the idea that the NMW is driving the fall in inequality. The NMW coefficients for the 50-10 wage ratio are smaller than the equivalent coefficients using the 50-5 ratio. This may also indicate limited spillover effects of the NMW as the lower percentile used in the measure of inequality moves further away from the percentile at which the NMW bites. When we repeat the same exercise at 140 areas level of aggregation the results are qualitatively similar. Here the regression coefficients tend to be even more negative than the coefficients for the 406 areas, suggesting there may be a greater degree of attenuation bias in the 406 level of disaggregation.[13]

There is little difference between the estimates when wage inequality rate for all age groups (including young people) is used as the dependent variable or when only the adult (25 to retirement) rate is used, (results available on request). When the analysis is repeated for youth, ages 16 to 24, arguably the age group more likely to be at the margin of adjustment, the point estimates are similar to those for all workers, but are generally insignificant, no matter which measure of the bite of the NMW is used.

[13] If we use the 50-20 differential as the dependent variable, the NMW effects, available on request, are smaller still.

We next present estimates of the DID model (1) using (the log of) employment as the labour market outcome of interest to summarise the NMW effect on employment over the medium term, namely the average over nine years since its introduction relative to the base period of 1997/98. Table 2 outlines the estimated NMW coefficients . For each NMW toughness measure there are 4 columns. The first column is the estimate from a simple regression of the dependent variable on the NMW measure, effectively establishing the correlation between the two variables. The estimates confirm the long-established fact that employment is lower in low wage areas. The correlation is stronger when 140 areas are used rather than 406. In every regression the estimated coefficients based on the 406 areas are attenuated relative to the higher level of aggregation estimates. This again suggests the presence of a greater degree of measurement error among the more disaggregated data. There is little difference between the estimates when total employment is used as the dependent variable or when the adult (25 to retirement) rate is used. The addition of year specific time dummies makes little difference to the estimates, but the addition of area fixed effects removes the positive association between low wages and low employment. Since any effect is now identified through variations in the NMW bite over time across areas, this suggests no overall difference in employment growth rates between areas where the NMW bites most compared to areas where the NMW has less impact. The further addition of time and varying area-level covariates has little effect. The estimates for youth employment, (not shown but available on request), beginning in 1999 due to data limitations, show similar patterns.

Table 3 presents the results of the Incremental Diff-in-Diff estimates for several samples based on the model (5), with a full set of controls along with time and area fixed effects. The results suggest that the average estimate of no association between the NMW bite and employment obscures significant changes over the sample period. Indeed over time, the positive association between low pay and NMW toughness becomes negative, so that in the latter sample period, areas where the NMW bit most experienced higher employment growth. These positive estimates are larger and most significant for the sample of all individuals aged to 16 to retirement, but in 2004 and 2006 there are positive, significant estimates of the NMW bite on employment for two of the three NMW measures. These point estimates effects are small in magnitude,[14] but it is clear that they are masked if the simple DiD Policy-On Policy -Off variable is used. If the standard assumptions of Diff-in-Diff relating to the Stable Unit Treatment are applicable (namely that no other systematic factors are varying across geography and over time) then we can interpret this as a causal impact of the up-ratings to the NMW. On this basis, if anything, employment rate appears to have risen more in areas where the NMW has more relevance.[15]

Figure 5 plots the individual year employment estimates for the 16 to retirement group for both the 406 areas and the TTWA areas. The regression estimates are given in Table A4. Here again we can see clearly that whichever geography is used there are grounds to believe that there were positive employment effects for 2004 and for 2006 with a reasonable possibility that the positive effect also exists for 2003 and 2005. Figure 1 suggests that these are all the years in which the uprating of the NMW kept it above the general rise in average earnings.[16]

V. ROBUSTNESS CHECKS.

Table 4 offers a set of robustness checks for the employment estimates. To address concerns over measurement error in the construction of the minimum wage variables, we use instead the mandated minimum plus 5 or 10 pence to generate the share, spike and Kaitz variables. This

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[14] For example the point estimate of 0.026 for 2004 implies that employment growth in that year was 0.26% higher in an area where 10% of employees were paid at or below the NMW compared to areas where no-one was paid the NMW compared to the respective growth rates in 1997/98.

[15] One concern with the timing of the effects we have found is that the post 2003 period coincides with the change in the sampling frame of ASHE. However, it would seem to us that there is no way to test this.

[16] The IDiD results using claimant count unemployment rate as a dependent variable – see the discussion paper version of this study - are essentially a mirror image of the employment results, in that we find unemployment rates falling further in areas where the NMW bit most in the latter half of the sample period

makes very little difference to the estimates, nor does using the mean rather than the median as the denominator for the Kaitz index. A weighted least squares regression, based on the sample sizes of the local areas used to calculate wages also makes little difference to the overall impression that while the full sample period there is little association between the bite of the minimum and employment, there are years toward the end of the sample period when there is a positive association between the bite of the NMW and employment.

An alternative way to eliminate fixed unobserved area characteristics and obtain consistent estimates is to estimate the model in differences. Table A1 compares within group estimates of the NMW effect estimated in Table 2, averaged over the nine years, with the estimates in differences. In both models time fixed effects are added to control for omitted variables that are constant across local areas but evolve over time. Both models suggest no overall difference in employment growth rates between areas where the NMW bites most compared to areas where the NMW has less impact. Similarly using different dynamic specifications, outlined in Table A2, make little differences to the conclusions drawn from Table 2.

The results of the Incremental Difference-in-Difference estimates measured the additional incremental effect of the up-rating of the NMW in each year relative to the off-period of 1997/98. In Table A3, we run separate difference-in-differences regressions year by year, measuring the effect of the up-ratings of the minimum wage in each year relative to the year before. The estimates for the years 1997-1998 (before the NMW was introduced), effectively test how our difference-in-differences model performs on a 'placebo', fictitious law. The estimated coefficients are not significantly different from zero, independently on the minimum wage measures used and the level of geographical aggregation, giving us confidence about the internal validity of our model. The results for the other year pairings are generally insignificant, excepting the negative and significant estimate of the introduction of the NMW in 1999 using the proportion paid at the NMW. In general then, it seems that the positive employment results we find above are driven mainly by comparisons with local area conditions in the run-up to the introduction to the NMW.[17]

In Table A5 we present our iDiD results using as a base year either 1997 only or 1998 only. This is mainly because in 1998 there might be already an anticipation of the effect of the introduction of the NMW. The results using either 1997 or 1998 as a base year are similar to our main regressions results, suggesting that the anticipation effect of the introduction of the NMW in 1998 was limited. The coefficients of the interactions between the NMW measure and 1998 as well as 1997 are insignificant.

The regression estimates of Table A6 show our incremental difference-in-differences estimates using a longer off-period from 1993 to 1998 and compares them with our previous estimates. Due to the changing in coding reflecting the local government reorganisation of the mid-1990s, the geography used in previous sections of the paper cannot be used for a longer period estimation. Instead we use the same 95 areas used to test for common trends. The results in Table A6 again show that the average estimate of no association between the NMW bite averaged over the entire sample period obscures significant changes at different points. Comparing the regression results of the 408 and 140 areas with the ones of the 95 areas, over time, the initial (insignificant) negative association between employment and NMW toughness is now statistically significant and then becomes positive and statistically significant.

A further robustness check of interest is to what extent the employment effect is concentrated in some local labour markets rather than others. An alternative way to 'cut the data' is to repeat our analysis by how tight the labour market is. So we anlaysed our data again by three types of market according to the level of tightness. We chose the middle year of our analysis - namely 2003 and categorised our geographical areas according to whether they have

[17] Dickens, Riley and Wilkinson (2009) have also recently used an area based approach over the latter half of our sample period. They find statistically insignificant NMW effects on employment growth over this period. This again seems to suggest that the base period is an important reference point underlying the results.

<1.7%, 1.7-2.7% or greater than 2.7% covered. These thresholds ensure that a third of the sample is in each group. Our results indicate that most of the employment effects we find in the IDiD are in the middle third of the sample where the coverage is between 1.7% and 2.7% and for the years 2004 and 2006 - although there are some other year effects for the less than 1.7% coverage group. (Our full results are again available on request.)

VI. CONCLUSIONS

Our starting point was that much of the US debate over the employment effects of the NMW has generated a 'lot of heat but not much light'. This conclusion is warranted to the extent that our examination of the empirical literature made it clear that much of the US controversy and debate over whether the effects on employment are negative or positive is actually arguing about different estimated parameters in the sense that they use different estimation strategies, with different types of data, on widely different samples of people of different ages. The truth is that most of the papers in this literature are estimating different marginal effects.

Our identification strategy was to use two sources of variation to try and identify the effect of the NMW. The first is to exploit a natural variation in how the NMW bites in different geographical locations. In our UK case the MW is set nationally and so there is no decision to be made at the local level (in sharp contrast to the US case). This means that the natural variation in the way the NMW works must be different at each geographical area. Our second source of variation was to examine the effect of changes in the up-rating of the NMW over the years since it was introduced. This estimation is based on an Incremental Diff-in-Diff method which allows us to estimate the marginal (interaction) effect of each years change in the NMW. The combination of these two different methods of identification along with the rigorous use of different robustness checks means that we can be more confident about the estimated effect of the impact of the NMW. Our conclusions are all the more credible in the sense that we got substantially the same results even though we reanalyzed the data in three completely different ways using completely different definitions of the geographical units of analysis.

The conclusion from our estimates is that overall there seems to be no significant association of the NMW on employment when we use a conventional Diff-in-Diff estimation for the whole policy-on/ policy off effect. However, when we use of Incremental Diff-in-Diff estimation method we retrieve significant positive effects on employment in recent years. Most specifically in the period 2004 to 2006. These findings are interesting as they are firstly consistent with much of the recent literature focusing on the introduction of the NMW (i.e. since they also get zero or small positive effects) but also because they explain why it may be possible to get both zero and positive effects. What drives these results is open to interpretation and subject to our ability to identify a NMW effect. It may be a realisation that the effects of the NMW on the wage bill may not warrant widespread employment losses, particularly given the level of demand and the ability of UK firms to adjust to labour cost shocks through a combination of hours, prices, productivity and profits documented elsewhere (summarized in Metcalf (2008)).

In relation to our findings on inequality it is clear, as one might expect, that raising the NMW is associated with reduced lower tail wage inequality in a systematic way each year since its introduction.

Figure 1 Change in the Nominal Hourly Wage Level of the National Minimum Wage

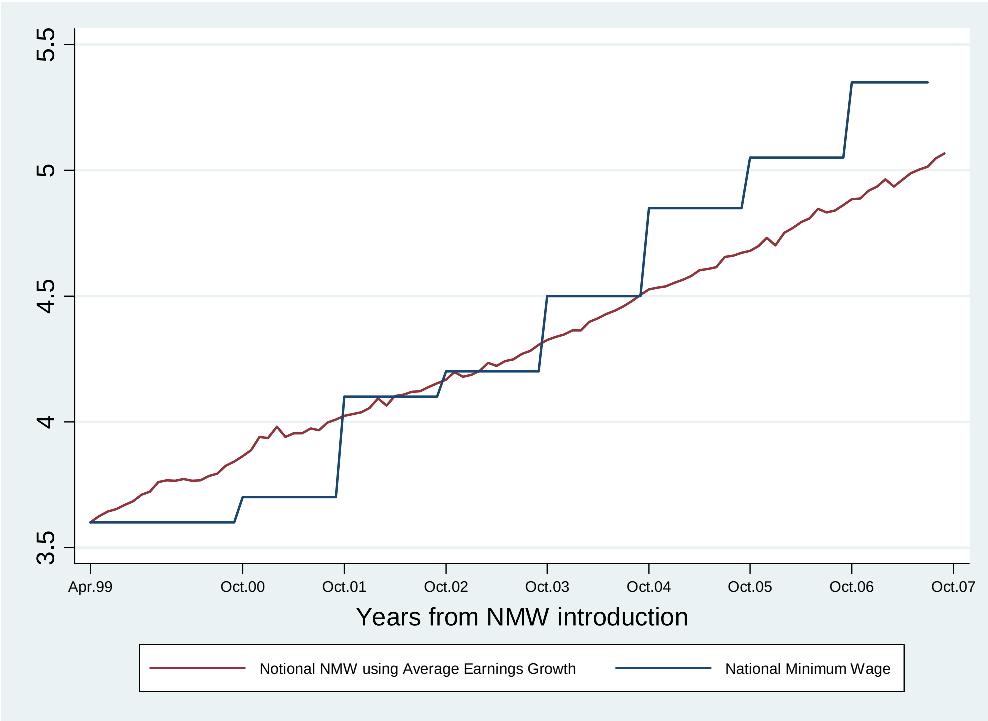
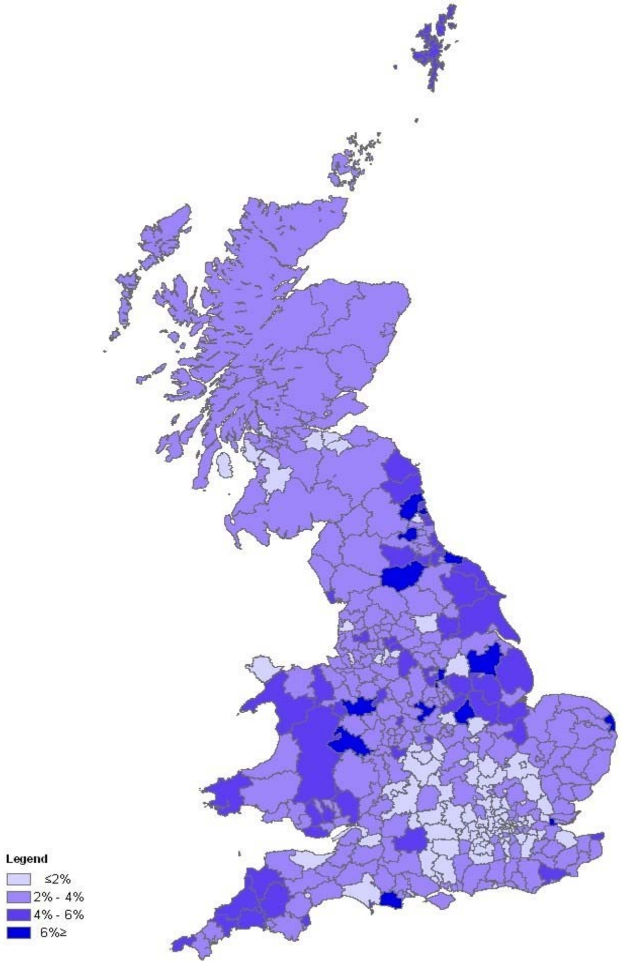


Figure 2. Change in Estimated NMW & Kaitz Index Over Time, 1997-2007.

Figure 3. Geographical Variation in the Minimum Wage Share (persons of working age)

Level 1999-2001



Change 1999- 2007

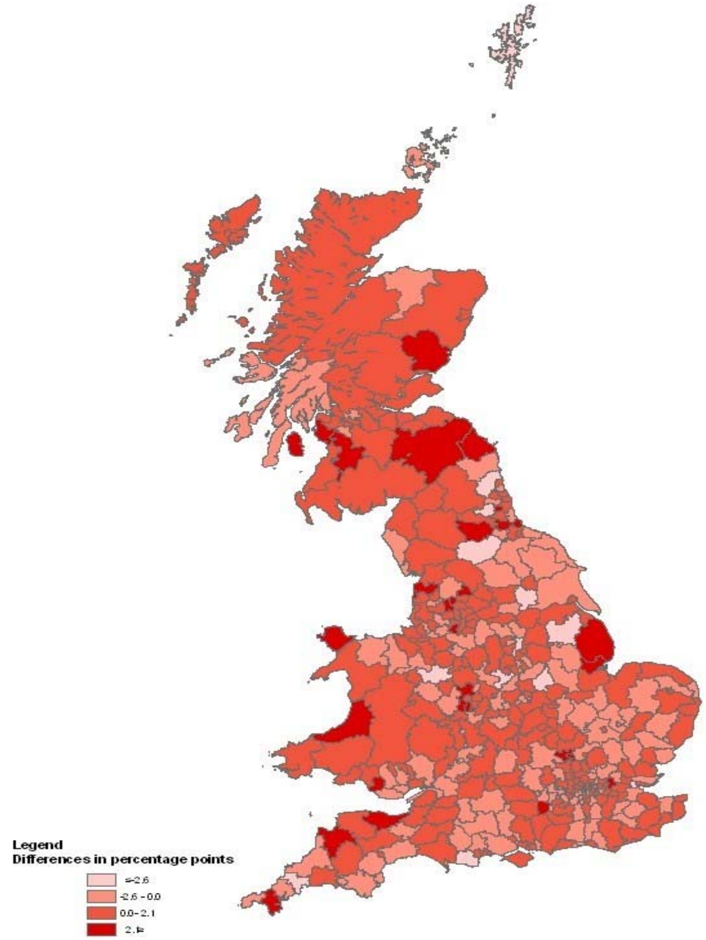


Figure 4. Incremental differences-in-differences wage inequality estimates, Age 16+

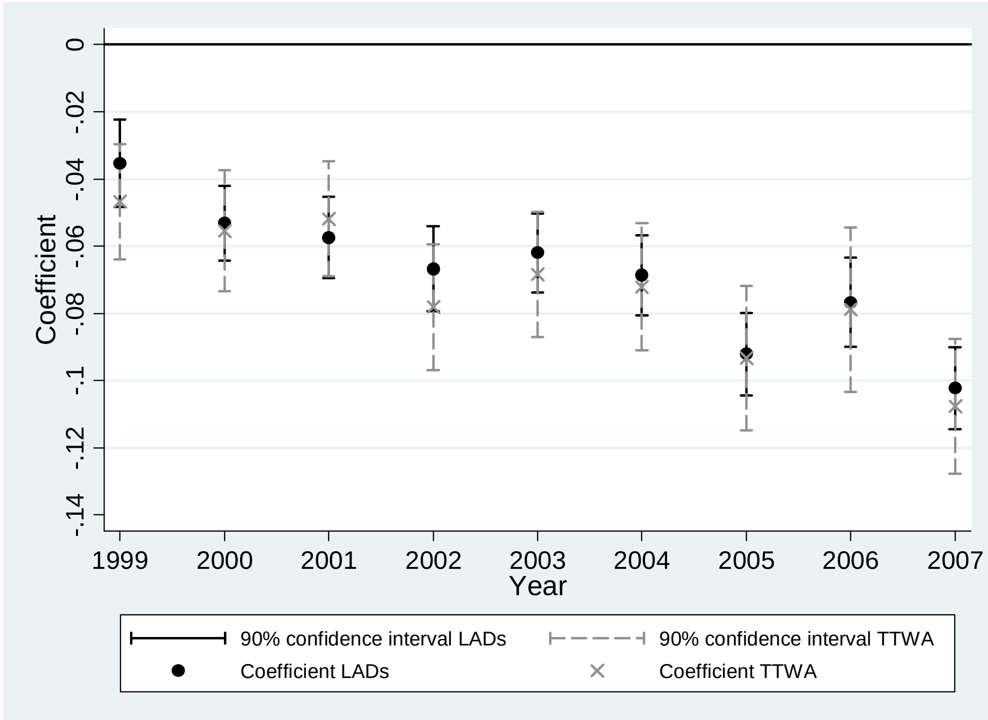


Figure 5. Incremental differences-in-differences employment estimates, Age 16+

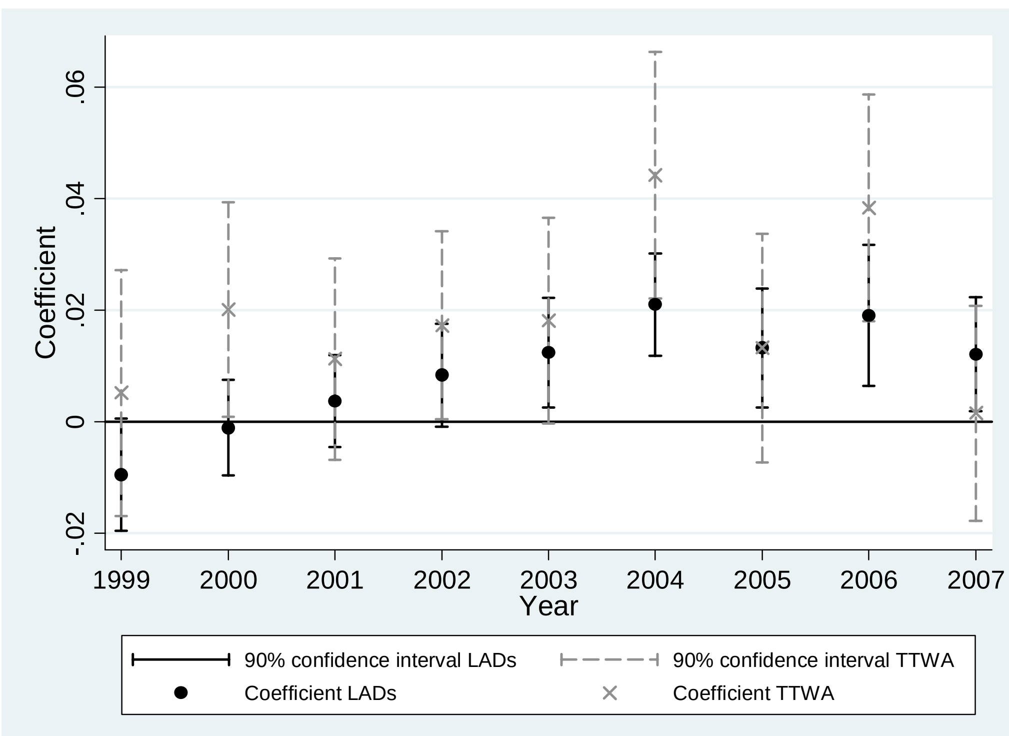


Table 1. Incremental Difference in-Difference Wage Inequality Estimates

	Proportion paid at or below NMW				*Proportion Paid at NMW*			
	Total 16-ret. 406 areas	Total 16-ret. 140 areas	Total 16-ret. 406 areas	Total 16-ret. 140 areas	Total 16-ret. 406 areas	Total 16-ret. 140 areas	Total 16-ret. 406 areas	Total 16-ret. 140 areas
	50-5		50-10		50-5		50-10	
NMW	0.092**	0.095**	0.054**	0.060**	-0.002	0.004**	-0.001	0.001
Base year	(0.006)	(0.010)	(0.005)	(0.007)	(0.006)	(0.002)	(0.005)	(0.002)
NMW*1999	-0.035**	-0.052**	-0.009	-0.007	0.007	-0.006	0.014**	0.008
	(0.008)	(0.012)	(0.006)	(0.004)	(0.007)	(0.004)	(0.006)	(0.004)
NMW*2000	-0.053**	-0.050**	-0.027**	0.028**	0.006	0.005	0.008	0.005
	(0.007)	(0.010)	(0.006)	(0.009)	(0.007)	(0.004)	(0.006)	(0.005)
NMW*2001	-0.057**	-0.061**	-0.031**	-0.043**	0.002	-0.011**	0.004	-0.011**
	(0.007)	(0.011)	(0.006)	(0.009)	(0.008)	(0.004)	(0.006)	(0.004)
NMW*2002	-0.067**	-0.079**	-0.032**	-0.048**	-0.011	-0.014**	-0.003	-0.009**
	(0.008)	(0.012)	(0.006)	(0.010)	(0.007)	(0.005)	(0.006)	(0.005)
NMW*2003	-0.062**	-0.064**	-0.036**	-0.041**	0.001	-0.011**	0.001	-0.014**
	(0.007)	(0.011)	(0.006)	(0.009)	(0.008)	(0.004)	(0.007)	(0.004)
NMW*2004	-0.069**	-0.073**	-0.043**	-0.049**	-0.005	-0.017**	-0.004	-0.011**
	(0.007)	(0.011)	(0.005)	(0.008)	(0.007)	(0.004)	(0.006)	(0.004)
NMW*2005	-0.092**	-0.094**	-0.055**	-0.053**	-0.015**	-0.017**	-0.009	0.009
	(0.007)	(0.012)	(0.006)	(0.009)	(0.017)	(0.006)	(0.006)	(0.005)
NMW*2006	-0.077**	-0.097**	-0.047**	-0.056**	-0.009	-0.025**	-0.007	-0.017**
	(0.007)	(0.012)	(0.006)	(0.010)	(0.007)	(0.006)	(0.006)	(0.006)
NMW*2007	-0.102**	-0.116**	-0.064**	-0.077**	-0.028**	-0.036**	-0.021**	-0.027**
	(0.007)	(0.013)	(0.006)	(0.011)	(0.007)	(0.008)	(0.006)	(0.007)

Note: All regressions contain year, area effects + controls. HAC robust fixed effect estimates in brackets. ** significant at 5% level.

Table 2. Employment Estimates of the NMW over the Medium Term, 1997-2007

	Proportion paid at or below NMW				*Proportion Paid at NMW*				*Kaitz Index*			
Total 16-ret. 406 areas	-0.021** (0.002)	-0.020** (0.003)	0.001 (0.002)	0.001 (0.002)	-0.012** (0.002)	-0.012** (0.002)	0.001 (0.002)	0.001 (0.002)	0.013 (0.012)	0.015 (0.013)	-0.021 (0.023)	-0.012 (0.024)
Total 16-ret. 140 areas	-0.039** (0.004)	-0.043** (0.005)	0.009* (0.004)	0.008* (0.005)	-0.024** (0.003)	-0.030** (0.003)	-0.001 (0.002)	0.002 (0.002)	-0.109** (0.021)	-0.150** (0.024)	0.031 (0.048)	0.035 (0.030)
Adult 25-ret 406 areas	-0.023** (0.002)	-0.022** (0.002)	-0.001 (0.002)	0.001 (0.002)	-0.013** (0.002)	-0.016** (0.002)	0.001 (0.002)	0.001 (0.002)	-0.014 (0.010)	-0.026** (0.011)	-0.008 (0.022)	-0.006 (0.023)
Adult 25-ret 140 areas	-0.038** (0.004)	-0.042** (0.004)	0.003 (0.004)	0.002 (0.004)	-0.026** (0.003)	-0.034** (0.003)	-0.002 (0.002)	0.003 (0.002)	-0.102** (0.018)	-0.151** (0.020)	0.066 (0.041)	0.047 (0.042)
Year Effects	N	Y	Y	Y	N	Y	Y	Y	N	Y	Y	Y
Area Effects	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
Controls	N	N	N	Y	N	N	N	Y	N	N	N	Y

Note: see Table 1.

Table 3. Incremental Difference-in-Difference Employment Estimates

	Proportion paid at or below NMW				*Proportion Paid at NMW*				*Kaitz Index*			
	Total 16-ret. 406 areas	Total 16-ret. 140 areas	Adult 25-ret 406 areas	Adult 25-ret 140 areas	Total 16-ret. 406 areas	Total 16-ret. 140 areas	Adult 25-ret 406 areas	Adult 25-ret 140 areas	Total 16-ret. 406 areas	Total 16-ret. 140 areas	Adult 25-ret 406 areas	Adult 25-ret 140 areas
NMW	-0.006*	-0.002	-0.004	-0.006	0.009	-0.001	0.012*	-0.001	-0.041	-0.034	-0.032	-0.009
Base year	(0.003)	(0.007)	(0.003)	(0.006)	(0.006)	(0.002)	(0.007)	(0.002)	(0.026)	(0.050)	(0.025)	(0.045)
NMW*1999	-0.009	-0.011	-0.006	-0.006	-0.025**	-0.021**	-0.023*	-0.013**	-0.029	0.023	-0.009	0.023
	(0.006)	(0.011)	(0.006)	(0.011)	(0.007)	(0.006)	(0.007)	(0.006)	(0.022)	(0.040)	(0.032)	(0.036)
NMW*2000	-0.001	0.002	0.002	0.008	-0.013*	-0.007	-0.014	-0.006	0.020	0.078**	0.022	0.090**
	(0.005)	(0.010)	(0.005)	(0.010)	(0.007)	(0.005)	(0.008)	(0.006)	(0.021)	(0.038)	(0.020)	(0.034)
NMW*2001	0.004	0.002	0.003	-0.001	-0.009	-0.017**	-0.008	-0.013**	0.010	0.038	0.006	0.035
	(0.005)	(0.010)	(0.005)	(0.010)	(0.007)	(0.005)	(0.007)	(0.005)	(0.019)	(0.042)	(0.018)	(0.037)
NMW*2002	0.008	0.002	0.007	0.001	-0.010	-0.007	-0.009	-0.004	0.048*	0.068*	0.048**	0.036
	(0.006)	(0.010)	(0.006)	(0.010)	(0.007)	(0.005)	(0.008)	(0.005)	(0.020)	(0.035)	(0.021)	(0.034)
NMW*2003	0.012**	0.010	0.007	0.013	-0.008	0.004	-0.013*	0.005	0.074**	0.184**	0.054**	0.128**
	(0.006)	(0.012)	(0.006)	(0.011)	(0.007)	(0.006)	(0.008)	(0.006)	(0.024)	(0.044)	(0.022)	(0.039)
NMW*2004	0.021**	0.026**	0.012**	0.021**	-0.003	0.008	-0.011	0.003	0.078**	0.115**	0.050**	0.079**
	(0.006)	(0.010)	(0.006)	(0.009)	(0.007)	(0.006)	(0.008)	(0.006)	(0.025)	(0.044)	(0.022)	(0.037)
NMW*2005	0.013**	0.023*	0.006	0.017*	-0.004	0.013**	-0.004	0.008	0.072**	0.132**	0.031	0.067**
	(0.006)	(0.011)	(0.006)	(0.010)	(0.007)	(0.006)	(0.007)	(0.005)	(0.028)	(0.036)	(0.023)	(0.032)
NMW*2006	0.019**	0.033**	0.013*	0.023**	-0.001	0.011*	-0.004	0.006	0.077**	0.177**	0.063**	0.142**
	(0.008)	(0.011)	(0.007)	(0.010)	(0.008)	(0.006)	(0.009)	(0.007)	(0.031)	(0.036)	(0.028	(0.035)
NMW*2007	0.012*	0.020*	0.005	0.012	-0.003	0.011	-0.008	0.002	0.058**	0.143**	0.049**	0.116**
	(0.006)	(0.011)	(0.006)	(0.009)	(0.007)	(0.008)	(0.009)	(0.007)	(0.026)	(0.048)	(0.024)	(0.042)

Note: see Table 1. All regressions contain year, area effects + controls

Table 4. Employment Robustness Checks.

	Proportion paid at or below NMW				*Proportion Paid at NMW*				*Kaitz Index*			
	Original	5p	10p	Cell size	Original	5p	10p	Cell size	Original	5p	10p	Cell size
NMW	-0.006*	-0.005	-0.006	-0.009**	0.009	-0.002	-0.006**	0.004	-0.041	-0.041	-0.042	-0.037
Base year	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.003)	(0.003)	(0.007)	(0.026)	(0.026)	(0.026)	(0.028)
NMW*1999	-0.009	-0.011*	-0.010	-0.011*	-0.025**	-0.010**	-0.007	-0.019**	-0.029	-0.029	-0.029	-0.035*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)	(0.0047)	(0.008)	(0.022)	(0.022)	(0.022)	(0.021)
NMW*2000	-0.001	-0.011*	-0.002	0.001	-0.013*	-0.001	-0.001	-0.010	0.020	0.021	0.021	0.027
	(0.005)	(0.006)	(0.005)	(0.006)	(0.007)	(0.004)	(0.004)	(0.008)	(0.021)	(0.021)	(0.021)	(0.022)
NMW*2001	0.004	0.003	0.001	-0.001	-0.009	0.003	-0.002	-0.010	0.010	0.010	0.010	0.010
	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)	(0.005)	(0.004)	(0.008)	(0.019)	(0.019)	(0.019)	(0.017)
NMW*2002	0.008	0.006	0.007	0.010	-0.010	0.006	0.005	-0.002	0.048**	0.048**	0.049**	0.058**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)	(0.004)	(0.009)	(0.020)	(0.021)	(0.021)	(0.024)
NMW*2003	0.012**	0.012**	0.015**	0.001	-0.008	0.006	0.010**	-0.013	0.074**	0.074**	0.074**	0.007
	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)	(0.005)	(0.004)	(0.010)	(0.024)	(0.024)	(0.024)	(0.036)
NMW*2004	0.021**	0.021**	0.023**	0.021**	-0.003	0.013**	0.019* *	-0.001	0.078**	0.078**	0.079**	0.065**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)	(0.005)	(0.009)	(0.025)	(0.025)	(0.025)	(0.029)
NMW*2005	0.013**	0.012*	0.013*	0.021**	-0.004	0.007	0.010*	0.004	0.072**	0.073**	0.073**	0.097*
	(0.006)	(0.007)	(0.007)	(0.010)	(0.007)	(0.006)	(0.005)	(0.009)	(0.028)	(0.028)	(0.028)	(0.050)
NMW*2006	0.019**	0.023**	0.021**	0.028**	-0.001	0.018**	0.019**	0.008	0.077**	0.077**	0.078**	0.100**
	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)	(0.006)	(0.008)	(0.031)	(0.032)	(0.032)	(0.037)
NMW*2007	0.012*	0.012*	0.015**	0.012	-0.003	0.010*	0.14**	-0.002	0.058**	0.058**	0.059**	0.077
	(0.006)	(0.007)	(0.007)	(0.008)	(0.007)	(0.006)	(0.005)	(0.009)	(0.026)	(0.026)	(0.027)	(0.050)

All regressions contain year, area effects + controls

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APPENDIX

I. DEFINITION OF KEY VARIABLES

Dependent variable

Employment rate

Total number of employees, self-employed, unpaid family workers and participants in government-supported training and employment programs in working age as a proportion of people in working age in each local area.

This variable has been generated also for adult workers (25 to retirement age) and for young workers (16 to 24).

Data on employment used in this paper is taken from June to August of each year.

Source: Labour Force Survey. Residence based analysis.

Wage Inequality:

In this study two different measures of wage inequality are used:

- The median wage divided by the 5^{th} percentile of the wage distribution in each local area
- The median wage divided by the 10^{th} percentile of the wage distribution in each local area.

This variable has been computed also for adult workers (25 to retirement age).

Source: ASHE, data recorded in April of each year. Workplace based analysis.

Independent variables

Minimum wage shares

Proportion of workers paid at or below the minimum wage in each local area.

The shares are generated for three age bands in each local area:

- <u>16 to 24 years old</u>

Starting from 1999, the shares are a weighted average of the minimum wage shares of persons from 18 to 21 years and of persons from 22 to 24.

From 2004, with the introduction of the new development rate for young between 16 and 17 years, the shares are a weighted average of the minimum wage shares of persons of 16 and 17 years, of persons from 18 to 21 years and of persons from 22 to 24.years.

- <u>16 to retirement age</u>

Starting from 1999, the shares are a weighted average of the minimum wage shares of persons from 18 to 21 years and of persons from 22 to retirement age.

From 2004, with the introduction of the new development rate for young between 16 and 17 years, the shares are a weighted average of the minimum wage shares of persons of persons of 16 and 17 years, of persons from 18 to 21 years and of persons from 22 to retirement age.

- 25 to retirement age

Source: ASHE, data recorded in April of each year. Workplace based analysis.

Spike of the minimum wage

Proportion of workers paid at the minimum wage in each local area.

The spikes are generated for three age bands in each local area:

- 16 to 24 years old

Starting from 1999, the spike is a weighted average of the spike of persons from 18 to 21 years and of persons from 22 to 24.

From 2004, with the introduction of the new development rate for young between 16 and 17 years, the spike is a weighted average of the spike of persons of 16 and 17 years, of persons from 18 to 21 years and of persons from 22 to 24 years.

- 16 to retirement age

Starting from 1999, the spike is a weighted average of the spike of persons from 18 to 21 years and of persons from 22 to retirement age.

From 2004, with the introduction of the new development rate for young between 16 and 17 years, the spike is a weighted average of the spike of persons of 16 and 17 years, of persons from 18 to 21 years and of persons from 22 to retirement age.

- 25 to retirement age

Source: ASHE, data recorded in April of each year. Workplace based analysis.

Kaitz Index

Kaitz Index, generated as the ratio of the NMW to the median hourly wage in each local area.

The Kaitz index is generated for three age bands in each local area:

- 16 to 24 years old

Starting from 1999, the Kaitz index is a weighted average of the Kaitz index of persons from 18 to 21 years and of persons from 22 to 24.

From 2004, with the introduction of the new development rate for young between 16 and 17 years, the Kaitz index is a weighted average of the Kaitz index of persons of 16 and 17 years, of persons from 18 to 21 years and of persons from 22 to 24.years.

- <u>16 to retirement age</u>

Starting from 1999, the shares are a weighted average of the minimum wage shares of persons from 18 to 21 years and of persons from 22 to retirement age.

From 2004, with the introduction of the new development rate for young between 16 and 17 years, the shares are a weighted average of the minimum wage shares of persons of persons of 16 and 17 years, of persons from 18 to 21 years and of persons from 22 to retirement age.

- <u>25 to retirement age</u>

Source: ASHE, data recorded in April of each year. Workplace based analysis.

II. PROBLEMS WITH THE ASHE/NES DATASETS

Even if ASHE is considered to give reliable wage figures though payroll records and it has a relatively large sample size, there are some limitations of this dataset which affect this study.

a) <u>Possible measures of hourly earnings</u>

The Low Pay Commission recommended construction of the hourly pay variable on the NES/ASHE data involves dividing gross pay (excluding overtime, shift and premium payments) by basic paid hours. This variable closely matches the definition of National Minimum Wage. However, the variable is available in the panel only from 2000. It is therefore necessary to use another measure of hourly earnings in this study which covers the period 1997 to 2007.

The variable used is a “basic hourly wage rate”, defined as gross weekly earnings excluding overtime, and divided by normal basic hours. As a result this variable will be slightly larger than the true hourly wage and the measurement error will tend to be larger, the higher shift and premium payments are. This might therefore result in an under-statement of the number of low paid workers.

b) <u>Discontinuities in NES/ASHE dataset across years</u>

Time series analysis has been complicated when the ASHE replaced the NES in 2004 and also by several changes in the ASHE methodology from 2004 to 2007.

First of all, the coverage of employees for the ASHE is greater than that of the NES. The NES surveys employees taken from HM Revenue & Customs PAYE record, excluding the majority of those whose weekly earnings fall below the PAYE deduction threshold. Moreover, this survey does not cover employees between sample selection for a particular year and the survey reference week in April. Thus, mobile workers who have changed or started new jobs between the drawing of the sample and the reference week are excluded. In conclusion, NES understate the proportion on NMW as it does not record the earnings of many low paid workers, especially part-time and mobile workers. In 2004, ASHE survey was introduced to improve on the representation of the low paid: it improved coverage of employees including mobile workers who have either changed or started new jobs between sample selection and the survey reference in April. Also, the sample was enlarged by including some of the employees outside the PAYE system.

In 2005 a new questionnaire was introduced. In particular, the definition of incentive/bonus pay changed to only include payments that were paid and earned in April. Also, a new question including "pay for other reasons" was introduced. This implies respondents might include earnings information which was not collected in the past. Even if results for 2004 have been reworked to exclude irregular bonus/incentive payments and to allow for this missing pay, results from 1997 to 2003 remain inconsistent with the ones from 2004 onwards.

Given that the main source of information on hourly pay in this study includes shift and premium payments and from 2004 "pay for other reasons", estimations of measures of minimum wage and wage inequality might be affected by this discontinuity, with an increase of the average measurement error and the dispersion in the measurement error from 2004 onwards.

Finally, in 2007 the sample size of ASHE was reduced by 20%. ASHE results for 2007 are based on approximately 142,000 returns, down from 175,000 in 2006. The largest sample cuts occurred principally in industries where earnings are least variable, affecting the randomness of the sample.

Consistent series which takes into account of the identified changes has been produced going back from 2006 to 2004 and from 2007 to 2006. For 2004 results are also available that exclude supplementary information, to be comparable with the back series generated by imputation and weighting of the 1997 to 2003 NES data. Unfortunately, it is not possible to get consistent datasets for the entire period concerning this study (1997-2007).

Table A1. Employment estimates in differences.

	Proportion paid at or below NMW		*Proportion paid at the NMW*		*Kaitz Index*	
	Within Group	Differences	Within Group	Differences	Within Group	Differences
Total 16-ret	0.001	0.001	0.001	0.001	-0.012	0.004
406 areas	(0.002)	(0.002)	(0.002)	(0.002)	(0.024)	(0.026)
Total 16-ret	0.008	0.004	0.002	-0.003	0.035	0.005
140 areas	(0.005)	(0.004)	(0.002)	(0.002)	(0.03)	(0.055)
Adult 25-ret	0.001	-0.001	0.001	-0.001	-0.006	-0.009
406 areas	(0.002)	(0.002)	(0.002)	(0.002)	(0.023)	(0.023)
Adult 25-ret	0.002	0.003	0.003	-0.003	0.047	0.011
140 areas	(0.004)	(0.004)	(0.002)	(0.002)	(0.042)	(0.048)
Years Effects	Y	Y	Y	Y	Y	Y
Areas Effects	Y	N	Y	N	Y	N
Controls	Y	Y	Y	Y	Y	Y

** significant at 5% level.

Table A2. Within Group Estimates of Dynamic Specifications of Minimum Wage Effects on Employment Rate (16 years to retirement age), 406 areas.

	Proportion at or below the NMW				Proportion at the NMW				Kaitz Index			
Independent Variables	*(1)*	*(2)*	*(3)*	*(4)*	*(1)*	*(2)*	*(3)*	*(4)*	*(1)*	*(2)*	*(3)*	*(4)*
Proportion paid at or below the NMW t			0.001	0.002								
			(0.002)	(0.002)								
Proportion paid at or below the NMW t-1	0.002	0.002	0.002	0.002								
	(0.002)	(0.002)	(0.002)	(0.002)								
Proportion paid at the NMW t							0.001	0.001				
							(0.002)	(0.002)				
Proportion paid at the NMW t-1					0.002	0.002	0.002	0.002				
					(0.002)	(0.002)	(0.002)	(0.002)				
Kaitz Index t											-0.014	-0.003
											(0.027)	(0.028)
Kaitz Index t-1									-0.015	-0.011	-0.011	-0.010
									(0.025)	(0.025)	(0.026)	(0.025)
Years Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Areas Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	4060	4060	4060	4060	4060	4060	4060	4060	4060	4060	4060	4060
R-squared	0.013	0.027	0.013	0.027	0.013	0.027	0.013	0.027	0.013	0.027	0.013	0.027

** significant at 5% level, .* significant at 10% level

Table A3. Differences-in-Differences year by year, Employment Estimates.

	Proportion paid at or below the NMW		*Proportion paid at NMW*		*Kaitz Index*	
	Total 16- ret, 408	Total 16- ret, 140	Total 16- ret, 408	Total 16- ret, 140	Total 16- ret, 408	Total 16- ret, 140
1997-1998						
NMW*1998	-0.002	0.001	0.012	0.004	-0.002	-0.010
	(0.005)	(0.010)	(0.008)	(0.003)	(0.018)	(0.035)
1998-1999						
NMW*1999	-0.007	-0.015	-0.025**	-0.014**	-0.034	0.008
	(0.007)	(0.011)	(0.012)	(0.007)	(0.021)	(0.035)
1999-2000						
NMW*2000	0.003	-0.003	0.010*	0.015**	0.051**	0.081*
	(0.008)	(0.015)	(0.006)	(0.006)	(0.025)	(0.042)
2000-2001						
NMW*2001	0.003	-0.005	0.003	-0.005	-0.004	-0.049
	(0.006)	(0.014)	(0.006)	(0.008)	(0.023)	(0.048)
2001-2002						
NMW*2002	0.011	0.006	-0.002	0.016**	0.040*	0.038
	(0.007)	(0.012)	(0.006)	(0.007)	(0.021)	(0.040)
2002-2003						
NMW*2003	0.011	0.021	0.005	0.011	0.029	0.115**
	(0.008)	(0.015)	(0.007)	(0.007)	(0.027)	(0.047)
2003-2004						
NMW*2004	0.008	-0.001	0.003	0.001	0.006	-0.077
	(0.008)	(0.012)	(0.006)	(0.009)	(0.026)	(0.053)
2004-2005						
NMW*2005	-0.006	0.009	0.001	0.007	-0.005	0.023
	(0.008)	(0.014)	(0.006)	(0.009)	(0.032)	(0.045)
2005-2006						
NMW*2006	0.007	0.011	0.009	0.003	0.002	0.030
	(0.009)	(0.013)	(0.007)	(0.008)	(0.041)	(0.037)
2006-2007						
NMW*2007	-0.009	-0.013	-0.006	-0.006	-0.027	-0.043
	(0.010)	(0.010)	(0.009)	(0.007)	(0.039)	(0.038)

Table A4. Incremental Difference-in-Differences, Employment Estimates: using only areas which correspond to TTWAs.

	Proportion paid at or below the NMW				*Proportion Paid at NMW*				*Kaitz Index*			
	Total 16-ret Base 406	Total 16-ret TTWA only (live)	Total 16-ret TTWA only (work)	Total 16-ret TTWA only (live+work)	Total 16-ret Base 406	Total 16-ret TTWA only (live)	Total 16-ret TTWA only (work)	Total 16-ret TTWA only (live+work)	Total 16-ret Base 406	Total 16-ret TTWA only (live)	Total 16-ret TTWA only (work)	Total 16-ret TTWA only (live+work)
NMW	-0.006*	-0.003	-0.004	0.004	0.009	0.002	0.011	0.003	-0.041	-0.117	-0.015	-0.029
Base year	(0.003)	(0.014)	(0.013)	(0.010)	(0.006)	(0.009)	(0.013)	(0.008)	(0.026)	(0.090)	(0.082)	(0.074)
NMW* 1999	-0.009	0.025	-0.020	-0.016	-0.025*	-0.010	-0.047*	-0.030*	-0.029	0.152*	-0.195*	-0.069
	(0.006)	(0.025)	(0.027)	(0.020)	(0.007)	(0.017)	(0.020)	(0.014)	(0.022)	(0.091)	(0.101)	(0.077)
NMW*2000	-0.001	0.035	0.058*	0.034	-0.013*	-0.003	0.013	0.007	0.02	0.078	0.147	0.072
	(0.005)	(0.031)	(0.028)	(0.022)	(0.007)	(0.016)	(0.019)	(0.013)	(0.021)	(0.108)	(0.109)	(0.063)
NMW*2001	0.004	0.028	0.030	0.014	-0.009	-0.002	-0.001	-0.002	0.01	0.032	0.024	-0.013
	(0.005)	(0.026)	(0.020)	(0.016)	(0.006)	(0.015)	(0.019)	(0.013)	(0.019)	(0.083)	(0.102)	(0.061)
NMW*2002	0.008	0.012	0.021	0.006	-0.01	-0.031*	-0.023	-0.019	0.048*	-0.004	0.117	0.028
	(0.006)	(0.030)	(0.022)	(0.019)	(0.007)	(0.015)	(0.018)	(0.013)	(0.02)	(0.090)	(0.097)	(0.062)
NMW*2003	0.012*	0.015	0.002	-0.005	-0.008	0.022	-0.003	0.005	0.074*	0.213*	-0.011	-0.008
	(0.006)	(0.031)	(0.017)	(0.015)	(0.007)	(0.016)	(0.017)	(0.012)	(0.024)	(0.098)	(0.087)	(0.057)
NMW*2004	0.021*	0.074*	0.047*	0.040*	-0.003	0.039*	0.003	0.016	0.078*	0.203*	-0.073	0.029
	(0.006)	(0.026)	(0.025)	(0.019)	(0.007)	(0.018)	(0.021)	(0.015)	(0.025)	(0.102)	(0.165)	(0.087)
NMW*2005	0.013*	0.022	-0.004	-0.002	-0.004	0.021	-0.005	0.006	0.072*	0.075	0.021	-0.013
	(0.006)	(0.028)	(0.022)	(0.020)	(0.007)	(0.014)	(0.022)	(0.013)	(0.028)	(0.128)	(0.126)	(0.093)
NMW*2006	0.019*	0.068*	0.023	0.012	-0.001	0.001	-0.024	-0.015	0.077*	0.006	-0.123	-0.097
	(0.008)	(0.024)	(0.020)	(0.017)	(0.008)	(0.017)	(0.021)	(0.014)	(0.031)	(0.112)	(0.146)	(0.090)
NMW*2007	0.012*	0.007	-0.017	-0.017	-0.003	-0.004	-0.027	-0.016	0.058*	0.129	-0.193*	-0.035
	(0.006)	(0.028)	(0.021)	(0.018)	(0.007)	(0.021)	(0.018)	(0.014)	(0.026)	(0.124)	(0.115)	(0.083)

All regressions contain year, area effects + controls. ** significant at 5% level, .* significant at 10% level

Table A5. Incremental Differences-in-Differences, Employment Estimates, 406 areas: pre-period 1997 only and 1998 only.

	Proportion paid at or below the NMW			*Proportion Paid at NMW*			*Kaitz Index*		
	Total 16-ret Base years '97-98	Total 16-ret Base year '97	Total 16-ret Base year '98	Total 16-ret Base years '97-98	Total 16-ret Base year '97	Total 16-ret Base year '98	Total 16-ret Base years '97-98	Total 16-ret Base year '97	Total 16-ret Base year '98
NMW	-0.006**	-0.007*	-0.010**	0.009	0.001	0.016*	-0.041	-0.048*	-0.051*
Base year	(0.003)	(0.004)	(0.005)	(0.006)	(0.004)	(0.009)	(0.026)	(0.029)	(0.028)
NMW*1997			-0.001			-0.076			-0.006
			(0.019)			(0.048)			(0.016)
NMW*1998		-0.003			0.015			-0.003	
		(0.006)			(0.010)			(0.022)	
NMW* 1999	-0.009	-0.009	-0.014	-0.025**	-0.017**	-0.138**	-0.029	-0.028	-0.008
	(0.006)	(0.007)	(0.023)	(0.007)	(0.006)	(0.048)	(0.022)	(0.025)	(0.017)
NMW*2000	-0.001	-0.000	0.019	-0.013*	-0.005	-0.074	0.020	0.022	0.034**
	(0.005)	(0.006)	(0.022)	(0.007)	(0.006)	(0.048)	(0.021)	(0.025)	(0.017)
NMW*2001	0.004	0.004	0.033	-0.009	-0.001	-0.062	0.010	0.012	0.021
	(0.005)	(0.006)	(0.022)	(0.006)	(0.006)	(0.047)	(0.019)	(0.023)	(0.016)
NMW*2002	0.008	0.009	0.050**	-0.010	-0.002	-0.064	0.048**	0.050**	0.050**
	(0.006)	(0.006)	(0.023)	(0.007)	(0.006)	(0.048)	(0.020)	(0.024)	(0.016)
NMW*2003	0.012**	0.013**	0.068**	-0.008	-0.000	-0.056	0.074**	0.075**	0.068**
	(0.006)	(0.007)	(0.026)	(0.007)	(0.006)	(0.049)	(0.024)	(0.027)	(0.018)
NMW*2004	0.021**	0.022**	0.092**	-0.003	0.005	-0.039	0.078**	0.080**	0.064**
	(0.006)	(0.006)	(0.023)	(0.007)	(0.006)	(0.048)	(0.025)	(0.028)	(0.018)
NMW*2005	0.013**	0.014**	0.065**	-0.004	0.004	-0.041	0.072**	0.073**	0.060**
	(0.006)	(0.007)	(0.026)	(0.007)	(0.006)	(0.049)	(0.028)	(0.030)	(0.019)
NMW*2006	0.019**	0.020**	0.074**	-0.001	0.007	-0.039	0.077**	0.078**	0.052**
	(0.008)	(0.008)	(0.030)	(0.008)	(0.007)	(0.051)	(0.031)	(0.034)	(0.021)
NMW*2007	0.012*	0.013*	0.048**	-0.003	0.005	-0.053	0.058**	0.059**	0.040**
	(0.006)	(0.007)	(0.024)	(0.007)	(0.006)	(0.047)	(0.026)	(0.029)	(0.018)

Table A6. Incremental Differences- in-Differences Employment Estimates: 95 areas regressions results, pre-period 1993-1997.

	Proportion paid at or below the NMW			*Proportion Paid at NMW*			*Kaitz Index*		
	Total 16-ret Base 406 areas Base '97-98	Total 16-ret Base 140 areas Base '97-98	Total 16-ret Base 95 areas Base '93-97	Total 16-ret Base 406 areas Base '97-98	Total 16-ret Base 140 areas Base '97-98	Total 16-ret Base 95 areas Base '93-97	Total 16-ret Base 406 areas Base '97-98	Total 16-ret Base 140 areas Base '97-98	Total 16-ret Base 95 areas Base '93-97
NMW	-0.006**	-0.002	0.001	0.009	-0.001	-0.001	-0.041	-0.034	0.050
Base year	(0.003)	(0.007)	(0.003)	(0.006)	(0.002)	(0.001)	(0.026)	(0.050)	(0.035)
NMW* 1999	-0.009	-0.011	-0.031**	-0.025**	-0.021**	-0.010**	-0.029	0.023	-0.092**
	(0.006)	(0.011)	(0.007)	(0.007)	(0.007)	(0.005)	(0.022)	(0.040)	(0.025)
NMW*2000	-0.001	-0.002	-0.008	-0.013*	-0.007	-0.010**	0.020	0.078**	0.004
	(0.005)	(0.010)	(0.006)	(0.007)	(0.005)	(0.005)	(0.021)	(0.038)	(0.028)
NMW*2001	0.004	0.002	-0.009*	-0.009	-0.017**	0.002	0.010	0.038	-0.032
	(0.005)	(0.010)	(0.005)	(0.006)	(0.005)	(0.004)	(0.019)	(0.042)	(0.021)
NMW*2002	0.008	0.002	-0.012	-0.010	-0.007	-0.004	0.048**	0.068*	0.001
	(0.006)	(0.010)	(0.008)	(0.007)	(0.005)	(0.005)	(0.020)	(0.035)	(0.031)
NMW*2003	0.012**	0.010	0.007	-0.008	0.004	0.001	0.074**	0.184**	0.017
	(0.006)	(0.012)	(0.008)	(0.007)	(0.006)	(0.005)	(0.024)	(0.044)	(0.033)
NMW*2004	0.021**	0.026**	0.014*	-0.003	0.008	0.013**	0.078**	0.115**	0.055
	(0.006)	(0.010)	(0.007)	(0.007)	(0.006)	(0.005)	(0.025)	(0.044)	(0.035)
NMW*2005	0.013**	0.023**	0.011	-0.004	0.013**	0.010	0.072**	0.132**	0.059
	(0.006)	(0.011)	(0.010)	(0.007)	(0.006)	(0.008)	(0.028)	(0.036)	(0.048)
NMW*2006	0.019**	0.033**	0.023**	-0.001	0.011*	0.021**	0.077**	0.177**	0.074*
	(0.008)	(0.011)	(0.009)	(0.008)	(0.006)	(0.009)	(0.031)	(0.036)	(0.045)
NMW*2007	0.012*	0.020*	0.007	-0.003	0.011	0.011	0.058**	0.143**	0.035
	(0.006)	(0.011)	(0.009)	(0.007)	(0.008)	(0.009)	(0.026)	(0.048)	(0.043)

All regressions contain year, area effects + controls. ** significant at 5% level, .* significant at 10% level.