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OF THE UK MINIMUM WAGE ON THE  
EMPLOYMENT PROBABILITIES  
OF LOW WAGE WORKERS**

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# **The Impact of the Introduction of the UK Minimum Wage on the Employment Probabilities of Low Wage Workers \***

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# **The Impact of the Introduction of the UK Minimum Wage on the Employment Probabilities of Low Wage Workers**

## **Abstract**

This paper uses longitudinal data from three contrasting datasets (matched Labour Force Surveys, the British Household Panel Survey and matched New Earnings Surveys) to estimate the impact of the introduction of the UK minimum wage (in April 1999) on the probability of subsequent employment among those whose wages would have needed to be raised to comply with the minimum. A difference-in-differences estimator is used, based on position in the wage distribution. No significant adverse employment effects are found for any of the four demographic groups considered (adult and youth, men and women) or in any of the three datasets used.

**Keywords:** Minimum wage, employment determination, labour demand, difference-in-differences estimator.

**JEL classifications:** J38, J23.

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## 1. Introduction

The employment effect of a minimum wage remains one of the most fiercely contested policy questions in economics. In the standard textbook theoretical model of the labour market the introduction of a minimum wage leads perfectly competitive employers to cut employment. The magnitude of the aggregate reduction in employment then depends on the wage rises required to comply with the minimum and on the slope of the labour demand schedule at the relevant point. In contrast a range of monopsony, efficiency wage and search models have been suggested in which a decline in employment may not result and employment may even increase.<sup>1</sup>

There is a vast research literature on minimum wages, particularly on their effects and particularly for the United States. A recent review of the literature is given by Brown (1999). A consensus seemed to have emerged by the 1980s that the effect of minimum wages on employment in the United States was negative although probably fairly small (see for example Brown et al. (1982) for a review). Most of the evidence was based on time series estimation and much of it on teenage employment, where the effects were felt to be largest. Research findings in the 1990s have blown this consensus apart. On the one hand a growing body of research finds zero or positive employment effects (e.g. Card and Krueger (1994, 1995, 2000) for the US, the US results in Abowd et al. (2000) and Machin and Manning (1994) and Dickens et al. (1999) for the UK). On the other hand there is also a body of recent research that finds significant (both statistically and numerically) negative effects (e.g. Kim and Taylor (1995), Currie and Fallick (1996), Burkhauser et al. (2000), Neumark and Wascher (2000) and Neumark et al. (2000) for the US and the French results in Abowd et al. (2000)). Thus the employment effect of minimum wages remains a highly contentious issue.

A new minimum wage was introduced in the UK in 1999 after a number of years with no minimum. The UK had statutory wage floors in many low wage sectors of the

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<sup>1</sup> See Card and Krueger (1995, chapter 11), Dickens et al. (1999, section 2) and Brown (1999, section 2) *inter alia*.

economy for most of the last century.<sup>2</sup> The Wages Council system, introduced in 1909, reached its height in terms of coverage in the 1950s, was allowed to wither on the vine in the 1980s and was finally abolished in 1993. There then followed a period without any statutory minimum (except in agriculture) until the introduction of the new minimum wage in 1999 following a change of government.

Based on the recommendations from the new Low Pay Commission (LPC, 1998), a minimum wage was introduced in the UK on 1 April 1999. The adult rate was set at £3.60 per hour, with a lower youth rate of £3.00 per hour for those aged 18-21 inclusive and a development rate of £3.20 per hour for adults in the first 6 months of a new job with accredited training. The youth rate subsequently rose to £3.20/hour in June 2000 and the adult rate to £3.70/hour in October 2000.

There is obviously a need to estimate the impact of the introduction of the minimum wage in a number of dimensions, including employment, as part of the policy evaluation. In addition in the context of the international minimum wage debate the recent UK experience can be viewed as providing an important “quasi-experiment”. Since its introduction followed a period without any minimum, the UK case allows direct examination of the crucial link between an individual’s position in the wage distribution and subsequent employment probabilities in the absence of a minimum wage and then examination of any post-intervention change in the relationship. As the Low Pay Commission point out, “this was a major intervention in the labour market” – they estimated that in excess of 1.5 million employees (6.4%) were entitled to a wage increase as a result of its introduction (Low Pay Commission, 2000). It therefore provides the opportunity to investigate the effect on employment of significant wage increases for a large group of workers. An additional advantage for the test to be conducted is that the evidence on the introduction of the UK minimum wage suggests a lack of spillover effects onto the wages of higher paid workers.

This paper uses individual-level longitudinal data from a number of different sources to estimate the impact of the introduction of the minimum wage on the employment

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<sup>2</sup> See Low Pay Commission (1998, Appendix 5) and Metcalf (1999) on the history of UK wage floors.

prospects of low wage workers whose wages would have had to be raised to comply with the new minimum – the group directly affected. The paper estimates a model of the individual employment to non-employment transition probability as a function of the individual's initial position in the wage distribution, building on the models used by Linneman (1982) and more recently Abowd et al. (2000).

The next section lays out the identification and estimation strategy used to investigate the minimum wage impact on individual employment probabilities. Three datasets are used in the empirical analysis: matched Labour Force Surveys, the British Household Panel Survey and the New Earnings Survey panel. Their advantages and disadvantages are discussed in Section 3. Results for the basic model specified in Section 2 using data from each of these three surveys are presented in Section 4. Various modifications, designed to examine the robustness of these findings, are presented in Section 5. Section 6 presents conclusions from the analysis.

## **2. Estimation Strategy**

This paper estimates the effects of the introduction of the minimum wage on the employment prospects of those affected. The central feature of the methodology employed is the use of individual-level longitudinal data to compare the employment experience of individual workers whose pay would have had to be increased to comply with the new minimum with that of a similar group who were not directly affected.

The introduction of the minimum wage is viewed in this methodology as what has come to be known as a “quasi-experiment” and a difference-in-differences estimator adopted to estimate its effect.<sup>3</sup> The approach is an intuitively appealing one given the sharp change in wages brought about at the bottom of the wage distribution by the introduction of the minimum wage.

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<sup>3</sup> See for example Meyer (1995) and Angrist and Kreuger (1999) for a fuller discussion of this type of approach. Heckman and Robb (1985) provide an extensive discussion of estimators for “interventions” of this type. Blundell et al. (1998) provide an illustration of this type of estimator in a different context.

The starting point of the approach is that, other things equal, one would expect the group of workers whose wages had to be raised to comply with the new minimum (i.e. those initially below the minimum) to be more affected than a group from higher up the wage distribution. A direct comparison of the two groups will not be appropriate to identify any causal effect since, even in the absence of a minimum wage, those at the bottom of the wage distribution have lower subsequent employment probabilities. This makes the difference-in-differences approach a natural one to take. The difference between the two groups in a period affected by the minimum wage can be compared with the equivalent difference in an earlier period when no minimum wage was in place.

For those directly affected by the introduction of the minimum wage one wants to ask the question, what would their employment position have been if the minimum wage had not been introduced? The objective is to find a suitable comparison to enable one to address a “what if” question of this type.

To be more precise about the estimation method, define  $e_{0it}$  to be the employment status of individual  $i$  in time period  $t$  in the absence of a minimum wage (= 1 if employed, = 0 if not employed) and  $e_{1it}$  to be that in the presence of a minimum wage. Thus only one of these is ever observed for a given individual  $i$  in a given time period  $t$ . Suppose that a minimum wage is introduced at a point in time,  $t^*$ , and that for observations prior to  $t^*$  no minimum wage is in place. Classify employees into a number of groups indexed by  $g$ . Then for a given group  $g$  in time period  $t$  there is direct information on the employment rate in the absence of a minimum wage,  $E[e_{0it} | g, t]$ , only for  $t < t^*$  and direct information on the employment rate in the presence of a minimum wage,  $E[e_{1it} | g, t]$ , only for  $t \geq t^*$ . The objective is to estimate the counterfactual  $E[e_{0it} | g, t, t \geq t^*]$ , i.e. what the employment rate in group  $g$  would have been if the minimum wage had not been introduced. This is done using comparisons across  $g$ . Suppose that:

$$E[e_{0it} | g, t] = \alpha_g + \gamma_t$$



where the first component is fixed over time and the second component is common across groups. This assumes that in the absence of a minimum wage the difference in the employment rate between two groups is the same in each time period, or equivalently that the growth in employment over time is the same for each group. This is the key identifying assumption in the simple difference-in-differences estimator and will be returned to below.

Suppose initially that the minimum wage has a constant effect on the employment probability for those in group  $g = 1$  and no effect on those in group  $g = 2$ :

$$\begin{aligned} E[e_{1it} | g = 1, t] &= E[e_{0it} | g = 1, t] + \theta \\ E[e_{1it} | g = 2, t] &= E[e_{0it} | g = 2, t]. \end{aligned}$$

Consider two time periods,  $t_1$  when no minimum wage was in place and  $t_2$  when one was. So  $t_1 < t^* \leq t_2$ . Then differencing across these two groups and across these two time periods gives  $\theta$ :

$$\begin{aligned} &\{E[e_{it} | g = 1, t = t_2] - E[e_{it} | g = 2, t = t_2]\} \\ &- \{E[e_{it} | g = 1, t = t_1] - E[e_{it} | g = 2, t = t_1]\} = \theta \end{aligned}$$

Thus the simple, or raw, difference-in-differences estimator is given by double differencing sample means.

Alternatively, under the above assumptions, the employment status of individuals in all groups and all time periods can be written:

$$e_{it} = \alpha_g + \gamma_t + \theta D_{it} + \varepsilon_{it} \quad (1)$$

where  $D_{it} = 1$  if individual  $i$  is affected by the minimum wage, i.e. if individual  $i$  is in group  $g = 1$  and if  $t \geq t^*$ ,  $D_{it} = 0$  otherwise, and where  $E[\varepsilon_{it} | g, t] = 0$ . Thus the raw difference-in-differences estimator is also given by a regression using micro data pooled across groups and time periods with additive group and time dummies plus an

interaction term between the “ $g = 1$ ” dummy and another dummy variable for all time periods with the minimum wage in place. If there are more than two groups, additional interaction terms of this type for groups  $g > 2$  will also be required.

This paper defines the groups indexed by  $g$  in terms of segments of the real wage distribution in the preceding period.<sup>4</sup> The first group ( $g = 1$ ) contains those directly affected, i.e. those with real wage (adjusted to April 1999 terms) below the appropriate (age-specific) minimum. The second group ( $g = 2$ ) is the “comparison” group and contains those between the minimum and some point slightly above the minimum. The remaining group covers the rest of the wage distribution.<sup>5</sup>

The simple difference-in-differences specification can be extended to produce a “regression adjusted” difference-in-differences estimator by adding a vector of individual characteristics,  $x_{it}$ , that are thought to affect the probability of employment as control variables to equation (1) to give:

$$e_{it} = x_{it}'\beta + \alpha_g + \gamma_t + \theta D_{it} + \varepsilon_{it} \quad (2)$$

The objective in adding these control variables to the equation is to sweep up any differences in characteristics between the “affected” or “treatment” group ( $g = 1$ ) and the “comparison” or “control” group ( $g = 2$ ) that are not picked up by the additive group and time effects.

The first key identifying assumption is still that interaction terms are zero in the absence of the minimum wage. This is now after controlling for differences in observable characteristics. The problem here is that even in the absence of the minimum wage introduction, employment transition rates may evolve differently in the different groups. The second key identifying assumption is that the minimum wage does not alter employment probabilities in group  $g = 2$ . There are two threats to

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<sup>4</sup> This approach has been used, *inter alia*, by Linneman (1982), Currie and Fallick (1996), Abowd *et al.* (2000) and Neumark *et al.* (2000).

<sup>5</sup> This third group can also be further subdivided, see Neumark *et al.* (2000).

this assumption.<sup>6</sup> First, there may be wage spillovers. Those paid slightly above the minimum wage may receive a pay boost to preserve differentials. Second, there may be substitution between groups as a result of the minimum wage introduction. (These two potential effects would be expected to act in opposition to one another.) These possible threats need to be kept in mind when interpreting results from this approach and when the robustness of results to modifications in the definition of the “comparison” group is investigated.

The empirical model used analyses the probability of being employed in the subsequent period as a function of the individual’s wage group in the current period, and therefore for those currently employed. It is therefore a model of the employment transition probability. This requires a slight adjustment to the specification above. In addition since a probability is being analysed, there are certain well known disadvantages to the use of a linear specification, and a logit model is adopted. The estimated model takes the form:

$$\Pr[e_{it+1} = 1 \mid e_{it} = 1] = \Lambda\{x_{it}'\beta + \alpha_1 g_{1it} + \alpha_3 g_{3it} + \gamma_0 d_{t+1} + \theta g_{1it} d_{t+1} + \phi g_{3it} d_{t+1} + \gamma_t\} \quad (3)$$

where  $g_{1it} = 1$  if  $w_{it} < m_i$  and  $= 0$  else, where  $w_{it}$  is the real wage of individual  $i$  in year  $t$  and  $m_i$  is the value of the minimum appropriate to individual  $i$ , where  $g_{3it} = 1$  if  $w_{it} \geq m_i(1+c)$  and  $= 0$  else, where the constant  $c$  defines the width of the comparison wage group,  $d_{t+1}$  is a binary variable taking the value 1 if the new minimum wage was in place at time  $t+1$ ,  $x_{it}$  is a vector of other factors that influence the probability of remaining in employment,  $\gamma_t$  are time effects for the remaining time periods and  $\Lambda$  is the logit transformation, i.e. the CDF of the logistic distribution,  $\Lambda(z) = [1 + e^{-z}]^{-1}$ . Thus group 2 [ $m_i \leq w_{it} < m_i(1+c)$ ] acts as the comparison group and  $\theta$  is still the parameter of interest leading to the difference-in-differences estimator. The question addressed is whether an individual whose wage would have had to be increased to comply with the new minimum, has a higher probability of losing their job than a *comparable* person in the wage group just above the new minimum.

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<sup>6</sup> Meyer (1995) lists and discusses in more detail the likely “threats” to an identification strategy of this

The methodology is a natural one. It looks at changes in employment status spanning the introduction of the minimum wage compared with changes prior to its introduction and it looks at the difference in this difference between a group directly affected and a group not directly affected. This type of model, with a control group from further up the wage distribution, has been used on individual-level data by several important studies in the minimum wages literature.<sup>7</sup>

An important defect of the simple difference-in-differences approach in the current context is that those with lower pay have a lower probability of remaining employed even in the absence of a minimum wage. There is thus a relationship between  $\Pr[e_{it+1} = 1 \mid e_{it} = 1]$  and  $w_{it}$  even before the introduction of a minimum wage.<sup>8</sup> Abowd et al. (2000) control for this effect by including a polynomial in the real wage among the variables in  $x_{it}$ . Neumark et al. (2000) employ a similar model, but use straight line segments for the function of the real wage.

### 3. Data

Results for the model outlined above are presented for three different datasets: the Labour Force Survey (LFS), the British Household Panel Survey (BHPS) and the New Earnings Survey (NES). Each has advantages and disadvantages for the task at hand. Thus each can be regarded as providing important checks on the results produced by each of the other datasets.

Suitable datasets for the estimation of the model specified above require a number of features. First, they must have a matched cross-section or panel element of at least two time periods: the model estimates the probability of employment at time  $t+1$  as a function of the wage (and other factors) at time  $t$ . Second, they must provide

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type.

<sup>7</sup> See footnote 4. Studies using this approach and this type of control group are reviewed by Card and Krueger (1995, Chapter 7, pp. 223-231) and Brown (1999, section 7, pp. 2139-2142). A control group from further up the wage distribution is also used at the establishment level by Card and Krueger (1994).

employment status information for the second time period (time  $t+1$ ) and information on the individual's hourly rate of pay at time  $t$ . Third, they need to be part of a series: there must be observations for which the time interval ( $t$  to  $t+1$ ) straddles the introduction of the minimum wage in April 1999 and other observations for which the entire interval falls before April 1999.

Fourth, they must provide information on other factors that influence the conditional probability of an individual being employed at time  $t+1$ , given employed at time  $t$ , to construct suitable control variables. Finally, they must provide reasonably large samples of individuals. The construction of the difference-in-differences estimator requires adequate cell sizes of individuals with real hourly wage rates below the April 1999 minimum and of individuals in the “just above the minimum” control group, both in periods providing panel intervals that straddle April 1999 and in intervals entirely prior to April 1999.

This is a fairly demanding set of criteria. The three datasets listed above meet them to varying degrees and have contrasting strengths and weaknesses. Using the combination of all three different datasets therefore provides the broadest possible evaluation of the impact of the introduction of the minimum wage on employment in the context of the estimation strategy outlined above.

### *3.1 Matched Labour Force Survey data*

The model outlined above can be estimated using matched LFS data. The LFS is a quarterly survey with individuals remaining in the sample for up to 5 quarterly waves. For estimation of the equation specified above, LFS data can be used only from 1997 quarter 1 onwards, when earnings questions were added to the wave 1 questionnaire (prior to this earnings questions had only been asked of the (outgoing) wave 5 respondents). The dataset constructed for the empirical analysis in this paper uses data from 11 quarterly LFSs: from 1997 quarter 1 (March – May) to 1999 quarter 3 (September – November). The estimates therefore capture employment effects up to 8

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<sup>8</sup> See Stewart (2000) for evidence on this for Britain and an econometric analysis of the relationship.

months after the introduction of the minimum wage. The estimation procedure uses wage and characteristic information from wave 1 and employment status information from wave 5 (12 months later).

In the current context the advantages of the LFS are that it provides a much better representation of low earnings workers than the NES and that it provides a much larger sample than the BHPS. The LFS also has a number of disadvantages. Compared with the BHPS, the LFS has only a limited panel dimension, makes extensive use of proxy responses, and does not consult respondent's payslips (something which is done in the BHPS). Compared with the NES, the LFS gives serious concerns about measurement error in the constructed hourly pay rates, particularly due to that in its hours variable.

### *3.2 British Household Panel Survey data*

Results are also presented based on waves 4 to 9 inclusive of the BHPS. Wave 4 was conducted in the autumn of 1994 and therefore is after the abolition of the Wages Councils. Wave 9 is the latest available. It was conducted in the autumn of 1999 and therefore enables examination of conditional employment probabilities after the introduction of the minimum wage. This six-year panel provides five years' worth of data to estimate the model for the probability of employment at wave  $t+1$  conditional on position in the wage distribution (and other characteristics) at wave  $t$ . Of these matched waves, the last provides the probability of employment after the introduction of the minimum wage (specifically autumn 1999) given the pre-minimum wage (autumn 1998) and the other four give a control sample for periods when there was no wage floor. The analysis starts in wave 4 to permit this role as a control sample. The estimates will capture employment effects up to 9 months after the introduction of the minimum wage.

The advantages of the BHPS are the proper panel nature of the data, the coverage of the complete earnings distribution (in contrast to the NES), the extensive range of information on individual characteristics relative to the NES, the lack of proxy respondents, and the consultation of individual's payslips wherever possible. The

main disadvantage, relative to both the LFS and the NES, is the much smaller sample size. The additional disadvantage relative to the NES is the potentially greater measurement error problems with the wage variable.

### *3.3 New Earnings Survey panel data*

The NES, conducted in April of each year, surveys employees with a particular final two digits to their National Insurance number so long as they are in employment. It can therefore provide data to estimate the model outlined in section 2 above by matching people across years. Non-employment can be inferred from an individual's absence from the survey in a particular year, although not observed directly.

In practice there are also other reasons why an individual may be missing from the survey in a particular year. Primary among these is that the NES excludes most of those whose weekly earnings falls below the PAYE deduction threshold (particularly in small organisations). Thus, for example, someone in a low-paying job who reduces their hours may as a result fall below the PAYE threshold and not appear in the survey in the subsequent year and be incorrectly classified as not in employment. At the time of the 1999 survey the PAYE tax threshold was £83.37 per week. Thus someone on exactly the minimum wage of £3.60/hour would have to work 23 hours/week or fewer to fall below the PAYE tax threshold. Those already part-time (predominantly women) are likely to be most affected by this.

Other potential misclassifications result from the fact that there is a lag (of about a month) between the drawing of the sample for a particular year and the reference week in which the survey is conducted. First, those unemployed or out of the labour force when the sample is located, but who enter employment before the survey date, are excluded. Second, those with one employer when the sample is located who have moved to another by the survey date and cannot be traced are also excluded. Thus there is some uncertainty about an individual's exact economic status for years when they do not appear in the NES. Certain groups such as domestic service workers and piece-rate homeworkers are also excluded from the survey.

The NES systematically under-represents the low paid in year  $t$  as a result of the exclusions just described. It also provides more limited information on personal characteristics than the other two datasets from which to construct control variables (since it is provided from employer records). Its great strengths are the likely accuracy of the wage data it provides, much of it direct from computerised payroll records, and the enormous samples, giving satisfactory cell sizes for the four key groups needed in the construction of the difference-in-differences estimator. As with the BHPS, and for the reasons discussed there, NES data for the years 1994 to 1999 inclusive are used to estimate the model.

The NES estimates will only capture immediate short-run effects, since the survey is conducted in April of each year and is currently only available in matched panel form up to 1999. It will therefore only classify as non-employed those leaving employment before the survey date – defined to be the pay period that includes April 14. Since most low-wage workers are paid weekly, for them the pay period will have been the week Monday 12 April to Friday 16 April. The NES estimates will therefore only capture those who are laid off prior to or immediately after the introduction of the minimum wage.<sup>9</sup>

## **4. Results for the basic specification**

### *4.1 Estimates using matched LFS data*

The sample is restricted to those aged at least 18 but below 60 who were employees at the time of the first wave interview, but excludes those who were full-time students or on government schemes. The dependent variable in the model to be estimated is the individual's employment status at time  $t+1$ . The employment category is defined to include both employees and self-employed (plus very small numbers of unpaid family workers and those on government employment and training programs). The base

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<sup>9</sup> The list of employees and their employers to whom NES questionnaires are to be sent is taken from Inland Revenue PAYE records in mid March (about a month before the survey pay period). Thus those who are not employees at this date will also be classified as non-employed by the construction here. This will include employees laid off before April 1 to avoid increasing their pay rate to the minimum wage.



group includes both the unemployed and those who have become economically inactive. The robustness of the results to variation in this and other variable definitions are considered in Section 5 below.

The wage variable construction used initially is that recommended by the Office for National Statistics (ONS) and used by the Low Pay Commission in their first report (LPC, 1998). The wage is constructed as gross pay in the most recent pay period, converted to a weekly basis and then divided by hours per week. Both pay and hours are for the individual's main job only. The hours variable is total usual hours, including paid overtime and excluding any unpaid overtime. The wage is converted to real terms (April 1999 prices) using the Retail Price Index (all items). Whether this is the most suitable construction of the wage variable and in particular whether usual hours should be used as opposed to actual hours is examined in the next subsection. Potential modifications to various other aspects of the construction of the wage variable are also considered in Section 5 below.

The top half of Table 1 presents sample raw probabilities of being in employment in year  $t+1$  (i.e. at the time of interview for wave 5) broken down by the wage group the individual was in at  $t$  (i.e. at the time of interview for wave 1, roughly 12 months previously) and by whether or not the minimum wage was introduced between these two dates. Employment probabilities are given for four demographic groups - male and female adults and youths - with the age division set at 22 to match that in the level of the minimum wage. The first two rows of the table give these probabilities for those whose real wage (in April 1999 prices) at time  $t$  was below the April 1999 minimum wage appropriate to their age. The first of the two rows is for observations where this period straddled the introduction of the minimum wage, i.e. it gives employment probabilities in April 1999 or later for those with a real wage below the minimum one year previously. For this group wages would have needed to be raised to comply with the minimum wage and by more than the rate of inflation. The next row gives the corresponding probabilities for earlier periods (i.e. for periods with both observation points prior to the introduction of the minimum wage). The next row then gives the differences in these probabilities between periods which saw the introduction of the minimum wage and periods which did not. For all four

demographic groups the probability of being in employment at  $t+1$  is smaller after the introduction of the minimum wage than for periods without a minimum.

The next three rows of the table present the equivalent statistics for those in the "control group", defined as those whose real wage (in April 1999 prices) at the time of interview at  $t$  was between the appropriate April 1999 minimum and the minimum plus 10%. For this group the probability of being in employment at  $t+1$  is smaller after the introduction of the minimum wage than for periods without a minimum for adult and young men and for young women, but is higher for adult women.

The raw (i.e. without control variables) linear difference-in-differences estimator is then given by subtraction. The results are given in the next row of the table. The results can be equivalently obtained by a linear regression as outlined in Section 2. The corresponding absolute "robust"  $t$ -ratios from the regression are given in parentheses. The difference-in-differences estimate is insignificantly different from zero for all four demographic groups. For both adult men and young women the estimate is also positive, implying a positive impact of the minimum wage on the employment probability for the group directly affected.

A logit form is estimated for the full model with control variables. Controls are included for age completed full-time education, a set of highest educational qualification indicators, labour market experience (a quartic), length of tenure with current employer (a quadratic), part-time status, marital status, ethnic status, an indicator if the job at time  $t$  is not permanent (fixed term contracts, seasonal, agency temping, or casual work), a public sector indicator, whether the individual has a health problem or disability (lasting more than 12 months) which limits the kind of paid work they can do, regional dummies, and year and month dummies. A cubic in the real hourly wage is also included in the  $x$ -vector to control for the relationship in the absence of a minimum wage as discussed in Section 2 above. Separate models are estimated for men and women and the main effects (and the intercept) are allowed to vary within these across the two age groups.

For the purpose of interpretation, the logit coefficient estimate is converted to a “marginal effect” of the dummy variable of interest evaluated at the sample proportion (or equivalently the sample means of the explanatory variables), i.e. a probability difference-in-differences. This can then be interpreted as the effect of the introduction of the minimum wage on the probability of employment. Two alternative methods are used to calculate this from the logit coefficient. The table gives those calculated by scaling the coefficient by  $p(1-p)$ , where  $p$  is the sample proportion. This is the standard partial derivative adjustment. In all cases the second method, which evaluates the difference in predicted probabilities, gives very similar estimates.<sup>10</sup> The absolute value of the robust asymptotic t-ratio of the coefficient in the logit model is given in parentheses in each case.

The full model difference-in-differences estimates are insignificantly different from zero for all four demographic groups. For three of the groups the effect is positive. For young men the effect, negative in raw probability terms, turns positive when the control variables are introduced. Only for adult women is there a negative (although insignificant) effect. The absolute t-ratio is also highest for this group (although only 1.49, giving a p-value of 0.14). However, the very large sample sizes involved should be kept in mind when evaluating these t-ratios. In very large samples it is often felt that standard 5% or 10% critical values are inappropriate and that the critical values used should rise with the sample size, i.e. we should be more demanding of our test statistics as the sample size increases. The Schwarz criterion, sometimes used in this context, implies using a critical value for the female sample of 3.17. The t-ratio for adult women is less than half this value.

For adult women the effect is exactly the same as the raw difference-in-differences estimate, i.e. the introduction of the control variables makes no difference, and it is worth noting that the raw estimate is mainly driven by the increased employment probability after the introduction of the minimum wage for those in the “just above” control group. However not too much should be read into this given the magnitudes of the t-ratios involved. The bottom line is that for all four demographic groups the

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<sup>10</sup> The difference in predicted probabilities is given by  $\Lambda(\alpha + \Lambda^{-1}(p)) - p$ , where  $\alpha$  is the appropriate logit coefficient in the specification above and  $\Lambda$  is the logit transformation:  $\Lambda(z) = [1 + e^{-z}]^{-1}$ .

estimated impact of the introduction of the minimum wage on the probability of remaining in employment is insignificantly different from zero.

#### *4.2 How should the wage be measured?*

The ONS recommended construction of the hourly pay variable on the LFS data (and that distributed with the data) involves dividing gross pay last time paid (converted to a weekly basis) by the number of paid hours usually worked (both referring to main job only). This is the wage variable used in the model estimates presented in the previous subsection. In this construction, there is a potential mismatch between the hours measure used and the hours worked in the period covered by the reported gross pay. Information is also collected in the LFS on hours actually worked in the reference week (in the main job). This does not necessarily correspond exactly to the pay period either. It should do so for those paid weekly at the end of each week, but will not do so for some other individuals. It may also be rather unrepresentative for some individuals with fluctuating hours, which will then cause measurement error when used to divide gross pay for a non-matching pay period. Never-the-less it may provide a preferable measure for the low paid who are more likely to be weekly paid, and either way provides a useful comparison and sensitivity test.

The difference-in-differences estimates based on this alternative wage measure are given in Table 2. Those for the ONS-recommended hourly pay variable are repeated to facilitate comparison. Both with and without control variables the difference-in-differences estimates remain insignificantly different from zero for all four demographic groups. They are also positive for all four groups. The negative (though insignificant) effect for adult women in the basic specification is not present when actual hours in the reference week are used to construct the hourly wage variable. To put it another way, the negative (though insignificant) effect for this group depends entirely on the use of usual hours (in conjunction with latest weekly pay) in the construction of the hourly wage rate, rather than hours in the reference week.

### 4.3 *British Household Panel Survey estimates*

Table 2 also presents the results from estimating the model using BHPS data. As far as possible the specification adopted is equivalent to that used in the analysis of the LFS data in Section 4.1 above. The sample is restricted to original sample members (and excludes the ECHP sub-sample) who provided full interviews and who were aged at least 18 but below 60 at time  $t$ . It is restricted to those who were employees at  $t$ , but excludes full-time students and those on government training schemes. The dependent variable, the individual's employment status at time  $t+1$ , is constructed on the basis of whether or not the individual had a job in the last week before the interview. The base group includes both the unemployed and those who were economically inactive. The wage variable is usual gross pay (converted to a weekly basis) divided by paid hours usually worked "in a normal week" and converted to real terms. (Both pay and hours are for the individual's main job only.)

The raw (i.e. without control variables) linear difference-in-differences estimates based on the BHPS are all insignificantly different from zero for each of the four demographic groups, as in those based on the LFS data. They are also positive for both male groups and for young women. The estimated effect for adult women is negative (although insignificantly different from zero) and of a similar magnitude to that for the LFS data.

As in the LFS analysis described in the previous section, a logit form is estimated for the full model with control variables. Similar control variables are also used as far as possible and are listed in the notes to Table 2. The logit coefficient estimate is converted to a "marginal effect", i.e. a probability difference-in-differences, as in the LFS analysis, and the absolute value of the "robust"  $t$ -ratio of the logit coefficient is given in parentheses below the difference-in-differences estimate.

For both male and female youth groups the samples for the below-minimum group and/or the comparison group post-introduction are empty or too small once observations with missing values for control variables are excluded. The with-controls difference-in-differences estimates are therefore given only for the two

adult groups. They are positive but insignificantly different from zero for both these groups. For women the difference-in-differences estimate switches from negative to positive when the control variables are added, in contrast to the results for the LFS data, although the estimates are insignificantly different from zero in both cases. In summary, there is no evidence of negative employment effects in the BHPS data on the basis of the basic specification.

#### *4.4 New Earnings Survey estimates*

Table 2 also presents the results from estimating the model using NES data. As far as possible the specification adopted is equivalent to that used in the analysis of the LFS and BHPS data. However the NES provides much less information on individual characteristics than the other two datasets and hence the model is able to control for far fewer control variables. The sample is restricted to those aged at least 18 but below 60 and excludes those who held more than one job and those who worked less than one hour in total in the reference week for year  $t$ . The dependent variable, the individual's employment status at time  $t+1$ , is inferred indirectly by presence as an employee in the survey for year  $t+1$  as explained above. The potential misclassifications as a result of this are also discussed above. (The overall mean employment probability is much lower on the NES than the LFS and BHPS due to the method of construction and the leakage of individuals inherent therein.) In an attempt to circumvent the misclassification of the employment status at  $t+1$  of those below the PAYE threshold referred to above, the female sample is restricted to those working at least 30 hours in the reference week for year  $t$ .

The wage variable is the basic hourly rate, constructed as total gross weekly earnings for the reference week (converted to a weekly basis if necessary) less overtime earnings, divided by normal basic hours (defined to be "basic hours for the employee in a normal week, excluding meal breaks and overtime") and converted to real terms.<sup>11</sup> Despite the enormous sample size, the NES raw difference-in-differences

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<sup>11</sup> Those for whom normal basic hours excluding meal breaks and overtime exceed 60 (0.2%) are excluded from the sample, as are those for whom the constructed hourly wage is less than 50p/hour (0.04%).

estimates are all insignificantly different from zero for each of the four demographic groups, as in those based on the LFS and BHPS datasets.

As in the LFS and BHPS analyses, a logit form is estimated for the full model with control variables. More limited control variables are available. Those used are listed in the Notes to Table 2. As with the LFS and BHPS analyses, the logit coefficient estimate is converted to a “marginal effect” and the absolute value of the robust t-ratio of the logit coefficient given in parentheses beneath it. The full model difference-in-differences estimates are insignificantly different from zero at conventional significance levels for all four demographic groups (and even less significant when judged against more suitable critical values for such large samples).

## **5. Evaluation and examination of the robustness of the estimates**

To add to the credibility of the results presented in the previous section, it is important to examine the reasonableness of the underlying methodological assumptions made and the robustness of the estimates to modifications in these assumptions. In particular various threats to the assumptions underlying the difference-in-differences approach need to be considered. The difference-in-differences methodology depends crucially on the one hand on the distinction between the periods before and after the intervention and on the other hand on the distinction between the “treatment” group and the untreated “comparison” group. Both are considered in more depth in this section. The latter distinction is defined in terms of the wage and therefore in turn depends crucially on the measurement of the wage, which is also considered below. A range of other robustness checks is also presented.

### *5.1 The “before” and “after” distinction*

The implementation of the difference-in-differences methodology in the previous section compares observations for periods which straddle the introduction of the minimum wage with observations for periods entirely prior to the introduction. It therefore assumes that there is a “pre-minimum wage” phase up to the end of March

1999 and a “post-minimum wage” phase from the beginning of April 1999, with a clear distinction between them. For example, individual observations of the employment status in March 1999 (as a function of the individual's real wage in March 1998) are prior to, and hence treated as unaffected by, the introduction. If instead a significant part of the necessary wage increases to comply with the minimum took place prior to the legally required implementation date of April 1st, then the “before” and “after” contrast would be weakened. The methodology requires that the wage adjustment took place at the legal due date for a sufficient number of employees.

A new direct question on the basic hourly rate of pay was added to the LFS for some respondents from the Spring quarter (March-May) of 1999. Since it is not available prior to this, it cannot be used as the wage measure in the model to estimate the effect of the introduction of the minimum wage. However since it provides, even if only for some individuals, a more accurate measure of their basic wage, it is useful for addressing the issue of the timing of implementation. Figure 1 shows the distribution of the basic hourly wage rate (in 10p bands) for hourly paid employees in the LFS in March 1999 (upper graph) and April 1999 (lower graph), restricted to those paid below £7/hour.

The lower end of the wage distribution shows a very sharp change between the two months. The April 1999 distribution shows a very pronounced “spike” at the adult minimum wage of £3.60. That for March 1999 does not. The 10p band starting at £3.60 increases in relative frequency from 4% to 13%, an increase of 9 percentage points. The bands below this show a reduction of 8 percentage points between them and none of the bands above that for £3.60 shows a change of more than one percentage point.

There was a marked shift in the distribution for this group of employees. However not all employees are included. Only those paid less frequently than monthly are asked whether they are paid a fixed basic hourly rate and only those responding “yes” are asked what it is. Figure 2 shows wage distributions for all employees, with the wage defined as the basic hourly rate for those who provide one and as usual weekly earnings divided by usual weekly hours (the main variable used in the model



estimated in this paper) for those who do not. This damps the relative magnitude of the £3.60 spike in April 1999 (still absent in March 1999), but the conclusion remains the same.

An alternative approach to this issue is to exclude from the analysis a period of time immediately prior to the April 1st legal implementation date, creating a “neutral zone” between the pre- and post-policy-introduction periods. The second row of Table 3 gives the difference-in-differences estimates for the full model when employment status observations in March 1999 are excluded. The estimates change very little and all remain insignificantly different from zero. The next row excludes February 1999 in addition to March. Again the difference-in-differences estimates change very little. All are insignificant and the only negative effect, that for adult women, weakens slightly.<sup>12</sup> The next row also excludes January 1999 and again the results are similar. The negative effect for adult women weakens still further and the estimated effects remain insignificantly different from zero for all four demographic groups. Excluding one, two or three months to create a “neutral zone” between the “before” and “after” periods has little effect on the results.

## 5.2 *Measurement of the wage*

Evaluating the impact of the introduction of the minimum wage by using a difference-in-differences estimator based on position in the wage distribution requires that those in the directly affected group received a bigger wage boost than those above the minimum and that this difference shows up in the wage variable used, rather than being swamped by measurement error. Figure 3 shows Nadaraya-Watson nonparametric kernel regression plots of the relationship between the percentage wage growth and the initial wage for observations entirely prior to the introduction of the minimum wage and for periods that span its introduction.<sup>13</sup>

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<sup>12</sup> This reduction in the size of the “before” sample results in 100% employment in the control group cell for young women, which prevents estimation of the logit model. The linear difference-in-differences estimate from a linear regression (with the same controls) is presented for this demographic group and put in square brackets to indicate the different estimation method used.

Panel (a) plots the relationships for the main LFS wage variable used in section 4 above, that based on “usual” hours. The difference in wage growth between “before” periods and periods spanning the introduction of the minimum wage is greatest for those with an initial wage below £3.60/hour. There is some evidence of a gap between the two kernel regression plots for those in the comparison group, but it is much less than that for those below £3.60 and at the lower end is partly the result of the kernel smoothing.

The significance of this difference between the group differences can be evaluated by applying the estimator used above on the employment model to an equation for the percentage wage growth. In these terms the difference-in-differences estimate is 3.98, with a t-statistic of 2.71 and a p-value of 0.007. That is to say, the difference between the annual rate of wage growth in the affected group and in the comparison group is 4 percentage points higher for periods which straddle the introduction of the minimum wage than for periods that do not.<sup>14</sup> The estimate is significant at conventional levels (although it does not reach the Schwarz threshold discussed above).

Panel (b) of Figure 3 shows the kernel regression plots when the alternative hourly pay variable based on using “actual” hours rather than “usual” hours is used. The gaps for both the treatment and comparison groups are reduced slightly, particularly that for the comparison group, and those further up the wage distribution increased slightly. The difference-in-differences estimate for wage growth rises slightly (to 4.79 percentage points), but the significance declines slightly ( $t = 2.63$ ,  $p = 0.009$ ). In these terms the wage based on “usual” hours would seem to have an advantage over that based on “actual” hours.

### *5.3 Definition of the comparison group*

The difference-in-differences methodology depends on there being a meaningful difference between the “treatment” group and the untreated “comparison” group. This

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<sup>13</sup> This figure is restricted to the sample of adults, i.e. those to whom the minimum wage rate of £3.60 applies, to make clearer the positioning of the “treatment” and “comparison” groups.

distinction may be threatened by spill-overs or by measurement error in the wage variable. In fact Dickens and Manning (2001) suggest that “the spill-over effect has been non-existent” (page 11) for the UK experience (in contrast to evidence for the US). Their analysis finds that “the national minimum wage has had virtually no impact on the pay of workers not directly affected” (page 3). Stuttard and Jenkins (2001) conclude similarly. However it seems worthwhile investigating the sensitivity of the results to the definition of the comparison group, given also the likely measurement error in the wage variable used. As Meyer (1995) points out, when examining the robustness of the estimates “the more comparison groups the better”.

There is a trade-off when selecting the comparison group. On the one hand, a fairly narrow definition is desirable to keep the comparison group as similar to the directly affected group (in terms of unobservables) as possible. Moving the comparison group further up the wage distribution makes the possibility of interactions more of a threat and increases the burden of adjustment through the real wage polynomial. On the other hand, widening the definition of the comparison group or moving it further away from the “treatment” group lessens the problem of misclassification due to measurement error and makes the underlying wage rates less similar, giving the test more leverage. Moving the comparison group further up the wage distribution reduces the impact of the “threats” to the identification strategy discussed in Section 2: both wage spillovers and substitution between the groups become less likely. An additional benefit of widening the definition is that it increases the size of the comparison group and (other things equal) the precision of estimation.

Difference-in-differences estimates for a series of alternative comparison groups are given in the lower block of Table 3. Extending the upper limit of the comparison group from the minimum +10% to the minimum +15% or +20% moves the difference-in-differences estimate for adult women slightly closer to zero and reduces its significance. The other three comparison groups examined introduce a gap between the minimum wage and the lower limit of the comparison group. In all cases the difference-in-differences estimate for adult women moves even closer to zero and

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<sup>14</sup> Measurement error in hours can give extreme values of this growth rate. The regression is estimated after suitable trimming.

experiences a further decline in its absolute t-ratio. The estimated effects become more negative for each of the other three demographic groups, but in no case does the estimate get even close to statistical significance. The results based on the use of a modified comparison group support the conclusions drawn in Section 4 on the basis of the original definition.

#### *5.4 Other robustness checks on the LFS results*

Another issue to be addressed concerns overtime hours and the payment of an overtime premium. The LFS analysis in section 4, in common with much of the literature, uses an average hourly earnings measure – averaged across all (usual) hours. These means that, in terms of providing an estimate of the basic hourly wage (as is suitable for comparison with the minimum), it implicitly assumes a zero overtime premium, i.e. that overtime hours are paid at the same hourly rate as basic hours.

The 1999 BHPS is a useful source of information on overtime premia. Of those paid by the hour for whom overtime is available 40% receive no overtime premium, i.e. their rate of pay for overtime hours is equal to their basic rate. Of those who receive a premium, time-and-a-quarter and time-and-a-half are dominant modal values and the median premium is a quarter.

Estimates using a wage variable constructed assuming a premium of 25% for all overtime hours are given in Table 4. The difference-in-differences estimate for adult women falls slightly (in absolute terms) and its statistical significance declines. The estimate for adult men is now negative, but still insignificant. Allowing for an overtime premium does not produce evidence of a significant employment effect.

The results are also robust to modification of the definition of employment used in the construction of the dependent variable. Unpaid family workers and those on government employment and training programmes are treated as employed in the basic specification. Table 4 gives the results if these groups are instead treated as non-employed. Very little changes as a result.

A very broad definition of non-employment is used in the basic specification, to include all those who are economically inactive (as well as those who are unemployed). This includes those who are not seeking work and would not accept it if offered. The three largest groups among the economically inactive in the data all fall into this latter category. They are those who do not wish to work because they are staying at home to look after a family, because they are retired, and because they are long-term sick or disabled. The next row of Table 4 gives the results when this group (economically inactive, not seeking work and do not wish to work) are excluded – on the grounds that they left employment for other reasons. There are small movements in the difference-in-differences estimates for some demographic groups, but all four estimates remain insignificantly different from zero, the negative effect for adult women declines slightly and the estimates for the other three groups remain positive.

The next modification considered concerns the price deflator used to construct the real wage. The all-items RPI is used in the basic specification.<sup>15</sup> The alternative specification examined deflates the wage using the RPI excluding mortgage interest payments.<sup>16</sup> The results are given in the next row of Table 4. Again the difference-in-differences estimates of the impact of the introduction of the minimum wage alter very little. The largest change in effect is for young women, where the positive effect increases further. The estimated effect remains insignificantly different from zero for all four demographic groups.

The next pair of modifications trim the wage distribution. Due to the method of construction of the hourly wage rate on the LFS, there are a small number of very small or large estimated rates. Symmetric trimming is used in both cases. In the first modification the lower limit is set at 50p per hour. This trims around 0.2% of individuals from the lower end of the distribution. The matching point to trim the same number from the upper end of the distribution is at £48/hour. The second modification trims 0.5% from each of the top and bottom of the distribution (the limits in this case are £1.10 and £34.28 per hour). Neither of these modifications

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<sup>15</sup> ONS series CHAW.

<sup>16</sup> ONS series CHMK.

changes the difference-in-differences estimates by much for any of the four demographic groups.

Much of the measurement error in the constructed hourly wage is due to that in reported “usual” hours. Overstatement of hours can lead to some individuals being wrongly classified as below the minimum and dilute the effect on the “treatment” group. The modification examined excludes those with reported usual hours per week greater than 60 (about 2% of the sample). However this has very little effect on the estimates.

The next modification uses weights in the estimation to compensate for differential non-response. This is a questionable procedure in the current context due both to the potential endogeneity of any non-response and to difficulties associated with the construction of the weights in relation to employment status. Nevertheless it is reassuring that the weighted estimates are very similar to the corresponding unweighted ones for all four demographic groups.

The final set of modifications examined concerns the use of proxy responses in the LFS. This is a well known problem, particularly in relation to the construction of an hourly pay measure, and has been highlighted by ONS and the Low Pay Commission among others. Of the sample used for estimation in the basic specification here, 28.5% are proxy responses. The majority of these (23.2%) are responses by the individual’s spouse or partner. ONS has developed “adjustments” for hourly earnings for proxy responses. Its “lower adjustment” increases hourly earnings by 1% for spouse/partner proxies and by 10% for other proxies. Its “upper adjustment” increases hourly earnings by 3% for spouse/partner proxies and by 13% for other proxies. Four modifications to the basic specification are given in Table 4. The ONS “lower” and “upper” adjustments are used in the first two of these respectively. Since these adjustments are rather large for the “other” proxies, and since these responses are viewed as much less reliable, the remaining two modifications exclude “other” proxy responses entirely and make the adjustments solely for the spouse/partner proxies.

In the first of these modifications the difference-in-differences estimates are very little changed for men and adult women. In the case of young women the estimate becomes negative, although it remains insignificantly different from zero. However this seems to be the result of the rather large adjustments to the “other” proxy response cases. When these are excluded, it reverts to being positive (and of course still insignificant). The use of the ONS “upper adjustment” produces estimates very similar to those in the basic specification.

In summary, this range of modifications confirms the main findings on the basis of the basic specification. The difference-in-differences estimate of the effect of the introduction of the minimum wage on the probability of employment is insignificantly different from zero for all four demographic groups. It is positive for both male groups and for young women in most specifications. It is generally negative (although insignificant) for adult women, but this depends on the construction of the hourly wage rate using “usual” hours, and is no longer the case if actual hours in the reference week are used.

### *5.5 Robustness of the BHPS and NES estimates*

A similar range of modifications was also examined for the other two datasets used above – the BHPS and the NES. The findings based on these two datasets are also robust to the various modifications considered. In the case of the BHPS, attention is restricted to the two adult groups for the same reasons as above. For both these groups the conclusions from the results given in Section 4 are robust to: (1) the use of an alternative comparison group, (2) assuming an overtime premium of 25%, (3) using an alternative definition of employment, (4) using an alternative price index, (5) trimming the wage distribution, (6) excluding those with very long hours, (7) using sampling weights, and (8) adjusting the wage according to whether or not a payslip was seen by the interviewer.

Turning to the NES, some of the robustness checks conducted on the other datasets are not needed (e.g. an assumption about an overtime premium is not required since separate basic and overtime pay is recorded and a wage rate for basic hours is used in

the model) and others are not possible (e.g. no weights are provided). However for the modifications considered the conclusions from the results given in Section 4, in particular the statistical insignificance of the difference-in-differences estimates for all four demographic groups, are robust. The modifications considered were: (1) the use of an alternative comparison group, (2) using an alternative price index, and (3) trimming the wage distribution.

## 6. Conclusions

This paper uses individual-level longitudinal data from matched Labour Force Surveys, the British Household Panel Survey and the New Earnings Survey panel to estimate the impact of the introduction of the UK minimum wage in April 1999 on the probability of subsequent employment among those whose wages would have to be raised to comply with the new minimum. A difference-in-differences estimator is employed using individuals from slightly higher up the wage distribution as the comparison group.

Using data from the LFS the estimated impact of the introduction of the minimum wage on the probability of remaining in employment is insignificantly different from zero for all four demographic groups (male and female adults and youths). This finding is robust to an extensive range of modifications considered. The estimated effect is also positive (although insignificant) for both male groups and for young women. The estimated effect is negative (although insignificant) for adult women, but this depends on the construction of the hourly wage rate using “usual” hours, and is no longer the case if actual hours in the reference week are used.

The estimated effect is positive, but insignificantly different from zero for both adult men and adult women on the basis of data from the BHPS. (The BHPS samples are too small to estimate the model for the two youth groups.) These findings too are robust to the modifications considered. Using data from the NES also gives an estimated effect that is insignificantly different from zero for all four demographic groups and the NES results too are robust to the various modifications considered.



Using a similar approach to that used here, Abowd et al. (2000) find significant negative effects for France, but insignificant effects for the US. The results in this paper for the UK are therefore closer to those for the US. This seems likely to partly reflect the differing levels at which minimum wages have been set in the three countries. In PPP terms the adult minimum wage rates in France and the US in November 1999 were £4.10 and £3.38 respectively (compared with a rate of £3.60 in the UK).<sup>17</sup> In relative terms the adult minimum was 38% of full-time median earnings in the US, 46% in the UK and 70% in France.<sup>18</sup> On both comparisons the UK minimum wage has been set between the two, but closer to that in the US than that in France. The results in this paper suggest that the estimated employment effects for the UK are also closer to those found for the US.

To sum up, the evidence for the UK presented in this paper indicates that the effect of the introduction of the minimum wage on the probability of employment is insignificantly different from zero for all four demographic groups and in all three datasets used.

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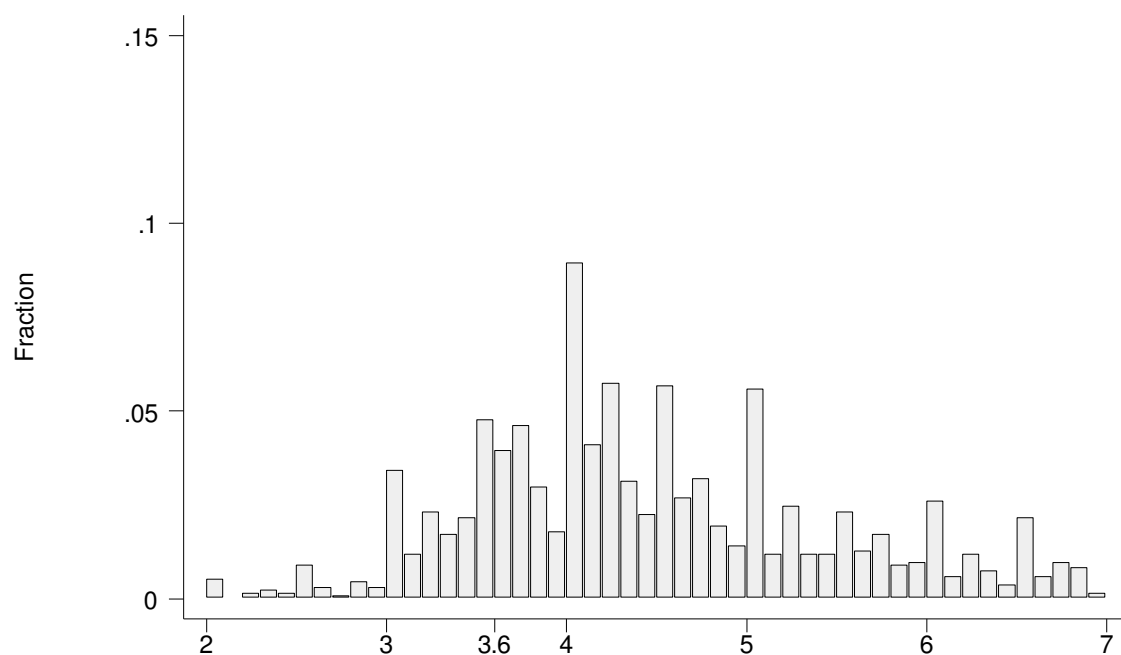
<sup>17</sup> Low Pay Commission (2000, Appendix 7).

<sup>18</sup> Some care needs to be taken with these calculations, since they are influenced by the stage of the uprating cycle at which they are measured.

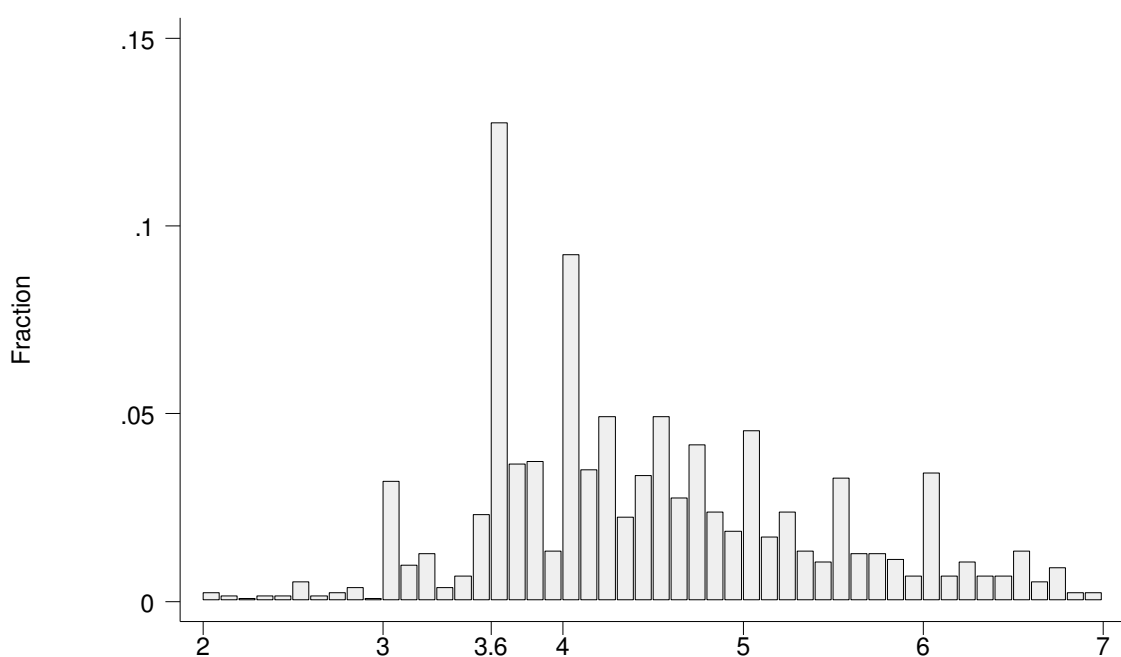
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Wage of hourly paid: LFS: March 1999



Wage of hourly paid: LFS: April 1999

Figure 1: Basic hourly wage rate, hourly paid employees, LFS

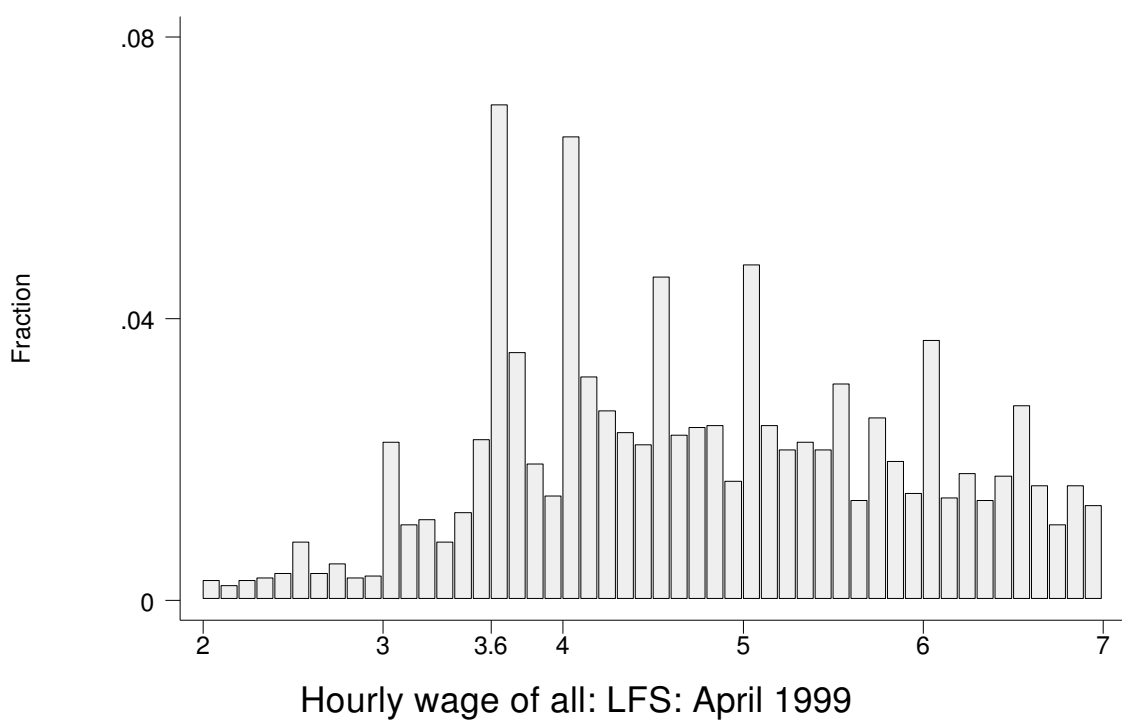
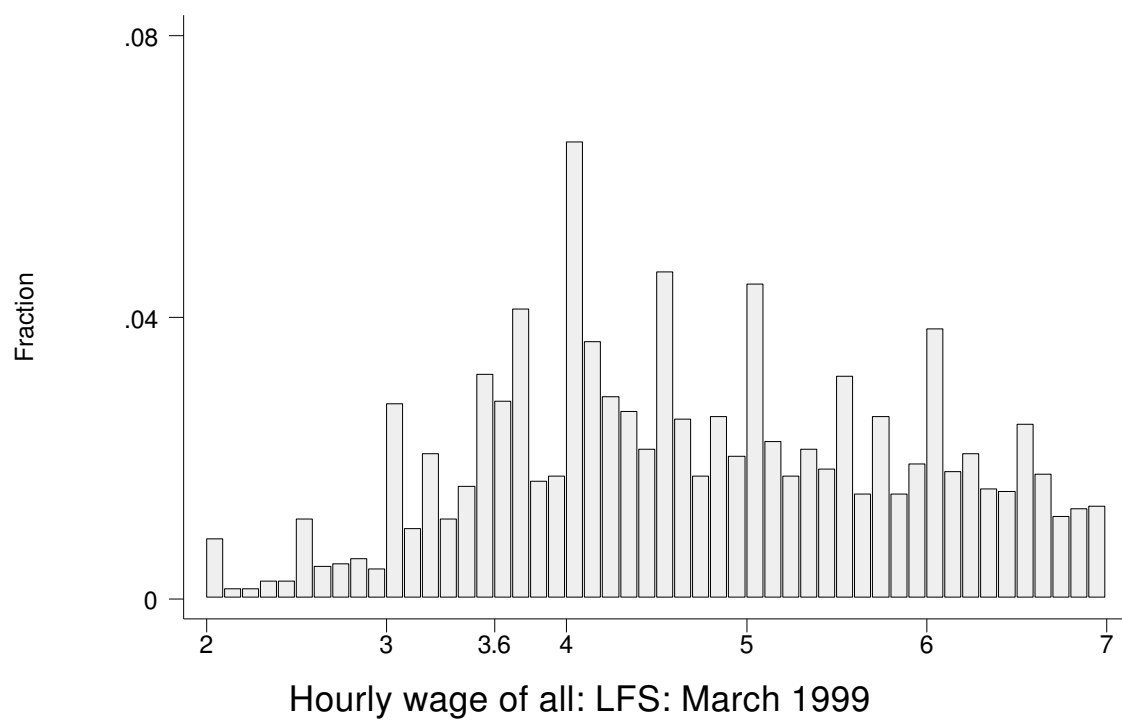
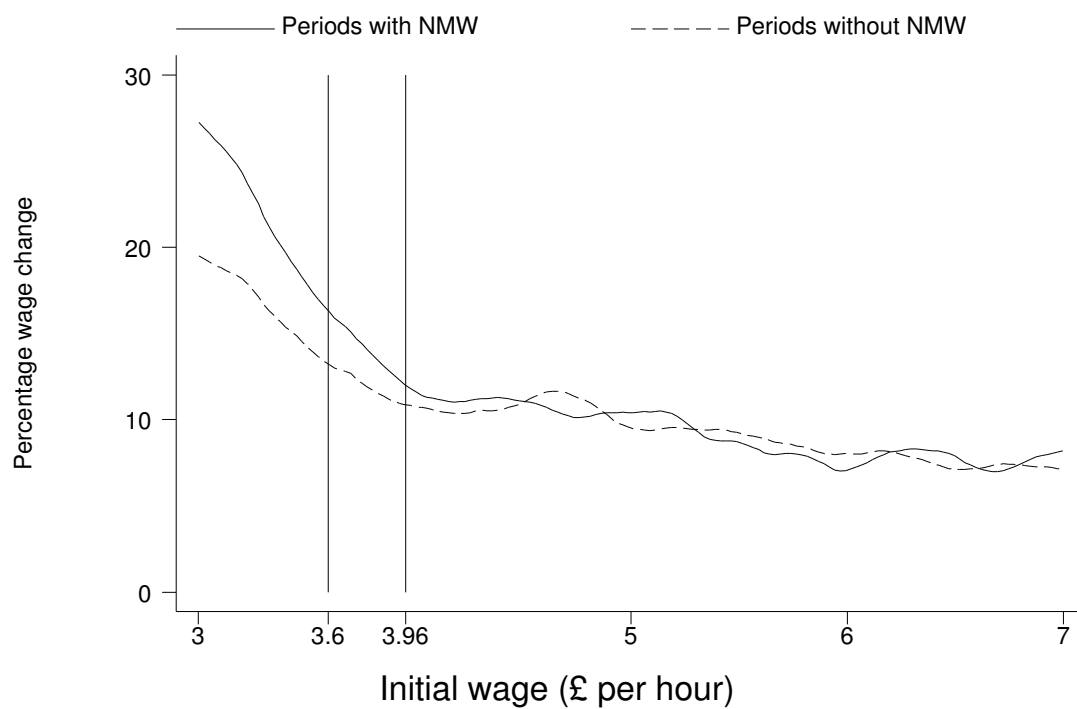
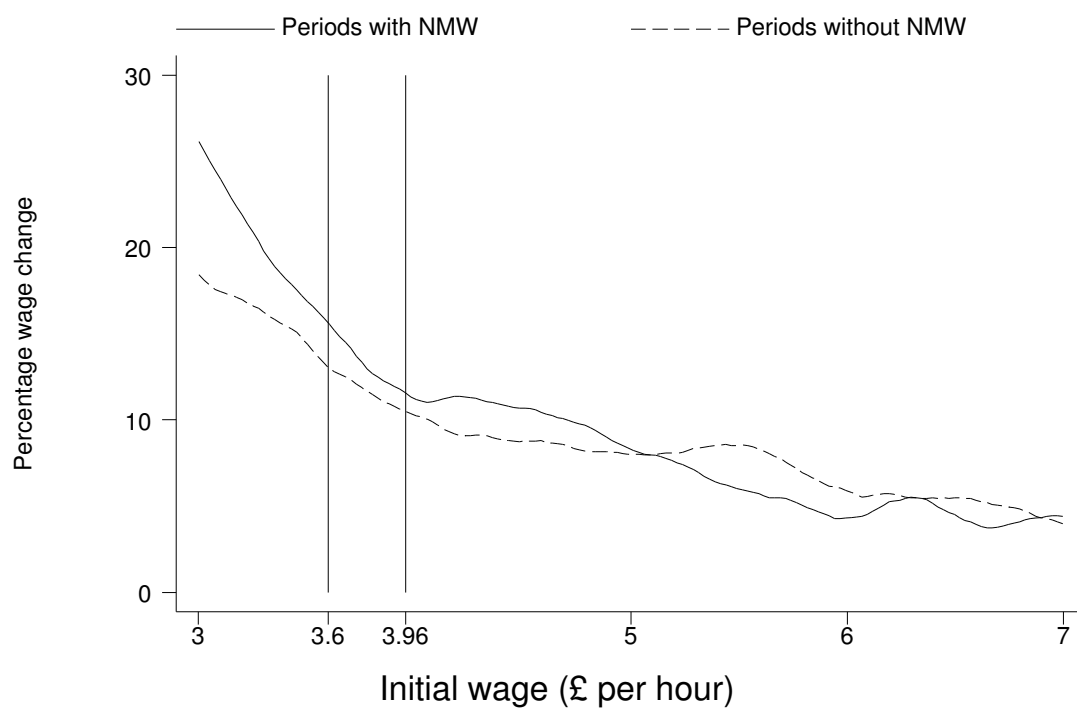


Figure 2: Hourly wage, all employees, LFS



**(a) Wage based on “usual” hours**



**(b) Wage based on “actual” hours**

**Figure 3: Kernel Regressions for Wage Growth (%): LFS**

Table 1

**Difference-in-differences estimates of the effect of the introduction of the minimum wage on the probability of subsequent employment.**

**Basic specification: Labour Force Survey data.**

	Adult men	Young men	Adult women	Young women
<u>Raw employment probabilities:</u>				
<i>Real wage &lt; minimum:</i>				
Period with introduction of minimum wage	.892	.870	.882	.855
Periods without	.908	.887	.885	.890
$\Delta$	-.016	-.017	-.003	-.035
<i>Real wage between min &amp; min + 10%:</i>				
Period with introduction of minimum wage	.913	.913	.923	.913
Periods without	.944	.922	.907	.970
$\Delta$	-.031	-.009	.016	-.057
Raw linear difference-in-differences estimate (Robust absolute t-ratio in regression)	.015 (0.43)	-.008 (0.09)	-.019 (0.99)	.022 (0.25)
Logit difference-in-differences estimate (Coefficient robust absolute asymptotic t-ratio)	.010 (0.66)	.054 (0.57)	-.019 (1.49)	.063 (0.60)

Notes:

1. Based on 11 Quarterly Labour Force Surveys: from 1997, quarter 1 (March-May) to 1999, quarter 3 (September-November).
2. Sample size = 45,991, made up of 22,353 men (920 young) and 23,638 women (807 young).
3. Logit model in final row contains as control variables: age completed full-time education, highest educational qualification dummies, labour market experience (quartic), length of tenure with current employer (quadratic), part-time, marital status, ethnic status, dummy for job at time t not permanent, public sector, health problem or disability limits kind of work can do, real hourly wage (cubic), regional dummies, year and month dummies.
4. Absolute values of "robust" t-ratios on model coefficients in parentheses.

Table 2

**Difference-in-differences estimates of the effect of the introduction of the  
minimum wage on the probability of subsequent employment.**

**Comparisons on LFS, BHPS and NES data.**

	Adult men	Young men	Adult women	Young women
<u>Raw linear difference-in-differences estimate</u>				
(Robust absolute t-ratio in regression)				
LFS estimates with ONS-recommended wage	.015 (0.43)	-.008 (0.09)	-.019 (0.99)	.022 (0.25)
LFS estimates with wage based on actual hours	.007 (0.19)	.053 (0.56)	.013 (0.62)	.030 (0.31)
BHPS estimates	.093 (0.81)	.084 (0.44)	-.017 (0.38)	.049 (0.27)
NES estimates	.010 (0.41)	-.004 (0.03)	-.011 (0.52)	.051 (0.69)
<u>Logit difference-in-differences estimate</u>				
(Coefficient robust absolute asymptotic t-ratio)				
LFS estimates with ONS-recommended wage	.010 (0.66)	.054 (0.57)	-.019 (1.49)	.063 (0.60)
LFS estimates with wage based on actual hours	.006 (0.34)	.141 (1.17)	.007 (0.50)	.024 (0.26)
BHPS estimates	.044 (1.29)	NA	.013 (0.43)	NA
NES estimates	.005 (0.32)	.001 (0.02)	-.011 (0.73)	.048 (0.79)

Notes:

1. LFS sample and control variables as in Table 1.
2. BHPS estimates based on 6 waves for 1994 (wave 4) to 1999 (wave 9) inclusive. Sample size = 16,796, made up of 7,851 men (561 young) and 8,945 women (518 young). Control variables: age completed full-time education, highest educational qualification dummies, labour market experience (quartic), length of time in current job (quadratic), part-time, marital status, ethnic status, dummy for job at time t not permanent, public sector, health limits kind of work can do, real hourly wage (cubic), regional dummies, year and month dummies.
3. NES estimates based on the New Earnings Surveys for 1994 to 1999 inclusive (April of each year). Sample size = 537,697, made up of 341,957 men (16,440 young) and 195,740 women (12,075 young). Control variables: age (quartic), dummy for more than 12 months in current job, part-time status, real hourly wage (cubic), regional dummies and year dummies.



**Table 3**

**Difference-in-differences estimates of the effect of the introduction of the minimum wage on the probability of subsequent employment.**

**Modifications to basic specification: Labour Force Survey data.**

	Adult men	Young men	Adult women	Young women
Basic specification	.010 (0.66)	.054 (0.57)	-.019 (1.49)	.063 (0.60)
<u>“Neutral zone” between “before” and “after” periods</u>				
Sample excludes March 1999	.014 (0.88)	.060 (0.63)	-.019 (1.45)	.062 (0.57)
Sample excludes February & March 1999	.018 (1.09)	.026 (0.28)	-.016 (1.20)	-.045 (0.57)
Sample excludes January – March 1999	.018 (1.09)	.039 (.039)	-.014 (1.08)	-.050 (0.63)
<u>Alternative comparison groups</u>				
Minimum to (minimum +15%)	.002 (0.17)	.054 (0.71)	-.014 (1.24)	.075 (0.93)
Minimum to (minimum +20%)	-.003 (0.22)	.007 (0.09)	-.013 (1.27)	.088 (1.13)
(Minimum +10%) to (minimum +20%)	-.014 (0.91)	-.037 (0.43)	-.006 (0.49)	.104 (0.99)
(Minimum +20%) to (minimum +30%)	-.010 (0.73)	-.024 (0.36)	-.001 (0.08)	-.007 (0.95)
(Minimum +10%) to (minimum +30%)	-.011 (1.01)	-.027 (0.45)	-.004 (0.39)	-.018 (0.30)

Note: Sample and control variables as in Table 1.

**Table 4**

**Difference-in-differences estimates of the effect of the introduction of the  
minimum wage on the probability of subsequent employment.**

**Modifications to basic specification: Labour Force Survey data.**

	Adult men	Young men	Adult women	Young women
Basic specification	.010 (0.66)	.054 (0.57)	-.019 (1.49)	.063 (0.60)
Assume overtime premium of 25% for all overtime hours	-.006 (0.43)	.007 (0.07)	-.014 (1.14)	.008 (0.06)
Treat unpaid family workers and those on government programmes as non-employed	.008 (0.53)	.069 (0.73)	-.019 (1.50)	.055 (0.52)
Exclude those who are inactive, not seeking and don't want work	.002 (0.16)	.061 (0.79)	-.012 (1.50)	.008 (0.09)
Wage deflated by RPI excl. mortgage interest payments	.019 (1.24)	.059 (0.62)	-.018 (1.41)	.114 (1.12)
Trim wage distribution at £0.50 & £48.00	.008 (0.55)	.029 (0.32)	-.018 (1.46)	.062 (0.58)
Trim wage distribution by 0.5% at top & bottom	.008 (0.53)	.021 (0.23)	-.018 (1.45)	.065 (0.61)
Exclude those with reported usual hours per week >60	.007 (0.42)	.054 (0.56)	-.018 (1.40)	.080 (0.77)
Use distributed sampling weights	.008 (0.56)	.039 (0.42)	-.020 (1.52)	.068 (0.64)
ONS "lower adjustment" to wage for proxy responses	.025 (1.63)	.043 (0.44)	-.018 (1.41)	-.026 (0.22)
ONS "upper adjustment" to wage for proxy responses	.033 (2.11)	.078 (0.84)	-.020 (1.52)	.035 (0.33)
ONS "lower adjustment" to wage for spouse proxies + exclude "other" proxy responses	.020 (1.21)	.048 (0.29)	-.018 (1.40)	.003 (0.02)
ONS "upper adjustment" to wage for spouse proxies + exclude "other" proxy responses	.027 (1.61)	.037 (0.22)	-.019 (1.47)	.003 (0.02)

Note: Sample and control variables as in Table 1.