Modelling the Employment Effects of the Minimum Wage

Final Report to the Low Pay Commission

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Executive summary

The report presents the results of an evaluation of the employment effects of the introduction of the minimum wage and the subsequent upratings in 2000 and 2001, based on matched Labour Force Survey data and the New Earnings Survey Panel Dataset.

<u>Results from the Labour Force Survey analysis:</u>

- The extended LFS dataset shows slightly less evidence of a negative effect of the introduction of the minimum wage on employment than was the case in the previous report. Various "wage gap" estimates are presented in addition to the difference-in differences estimates provided in the previous report and confirm the findings.

- In the evaluation of the impact of the 2000 uprating it is extremely difficult to find a suitable and adequate time period for the inter-temporal comparison required in the difference-in-differences estimator. Evidence is also presented that the differential impact of the overall slowdown in employment growth biases both the difference-in-differences and wage gap estimates. The "growth adjusted" versions of the estimators, which attempt to allow for this, show no significant negative employment effects of the 2000 uprating. Among the unadjusted estimates, important differences are found between the difference-in-differences and "wage gap" estimator results, although more in terms of t-ratios and significance than in terms of point estimates. Using the simple unadjusted estimators, there is some evidence of a negative effect, mainly for adult women, but it is sensitive to the choice of wage variable and estimation method. It is only significant for one of the four variable and estimator combinations.

- In the case of the 2001 uprating also, the "growth adjusted" versions of the estimators show no sign of significant negative employment effects. When the unadjusted estimators are used, there is some weak evidence of an adverse effect for women, but it is sensitive to the estimator and wage variable used: it is only significant for the combination of the “wage gap” estimator and the wage variable based on usual hours, and even then the evidence is not strong.

Results from the New Earnings Survey Panel Dataset analysis:

- The NES data and estimates show more change from the previous version of the dataset (used in the previous report) than one would have hoped and I still have reservations about the changes in the data. Unfortunately it is not possible to check the validity of these.

- There is some evidence of a negative effect of the introduction of the minimum wage for adult men, but only with NESPD-2001, not with NESPD-99 (even though only the same years are used) and only for the “wage gap” estimator, not the difference-in-differences estimator.

- There is no evidence of an adverse employment effect of the 2000 uprating on the basis of the “growth adjusted” versions of the estimators. When the simple unadjusted estimators are used there is some evidence of a negative effect for adult and young men, but only when the wage gap estimator is used. There is no such evidence from the difference-in-differences estimator.

- When specific sectors are examined (to the extent that sample sizes of low paid employees permit), no compelling evidence of a negative effect on employment is found. There is some evidence of a negative effect for female catering staff, but only if an occupation-based definition of the sector is used, not if an industry-based one is used, and only if a difference-in-differences estimator is used, not if a “wage gap” estimator is used. No evidence of a negative effect is found for the retail sector, for cleaners, or for child care workers and care assistants.

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1. Introduction

The research described in this report extends and updates the analysis in my earlier report to the Commission, "Estimation of the Individual-level Employment Effects of the Introduction of the National Minimum Wage" (April 2001), in a number of ways. The approach taken in that report was to use individual-level longitudinal data from three contrasting datasets (LFS, BHPS and NES) and a difference-in-differences estimator based on position in the wage distribution. The evidence presented in the report suggested zero, or if anything small positive, employment effects for adult men, young men and young women, but a less clear-cut conclusion for adult women. The current report uses extended and updated versions of the same datasets and focuses on the following extensions of the analysis presented in the earlier report.

1. The impact of the introduction of the minimum wage is re-examined with updated data that has become available since that report.

2. The methodology is extended to analyse the impact of the subsequent upratings to the minima.

3. The estimation methodology is extended to consider spline or "wage gap" specifications as well as the dummy variable specifications used for the difference-in-differences estimator.

4. "Growth adjusted" extensions of the estimators are also examined to address the difference in underlying employment conditions between the time periods being compared when the minimum wage upratings are being evaluated.

5. The effect on the results of adding sector-specific controls to capture the influences on the probability of employment of the stage of the economic cycle and the state of the labour market are considered.

6. To the extent that data (and in particular sample and cell sizes) permit, the analysis is also disaggregated by sector (defined in terms of industry and/or

occupation), focusing on those sectors likely to be most affected by the minimum wage, i.e. those employing the most low-paid workers.

The next section gives a restatement of the basic methodology and Section 3 describes the data used for the report. The results based on the matched Labour Force Surveys are given in Section 4 and those based on the New Earnings Survey Panel Dataset in Section 5. Sections 6 presents results disaggregated by sector. Conclusions are given in Section 7.

2. A restatement of the basic methodology

The basic methodology employed is laid out in my earlier report and in Stewart (2002). It is restated here for convenience and completeness. 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 treated like a "quasi-experiment" and a difference-in-differences estimator used to estimate its effect.[1] The sharp change in wages brought about at the bottom of the wage distribution by the introduction of the minimum wage makes this an obvious approach to take.

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 would 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.

Define e_{0it} to be the employment status of individual i in time period t in the <u>absence</u> of a minimum wage (= 1 if employed, = 0 if not employed) and e_{1it} to be that in the <u>presence</u> 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

[1] See for example Meyer (1995) and Angrist and Kreuger (1999) for a fuller discussion of this type of approach and estimator.

minimum wage, $E[e_{0it} \mid g, t]$, only for $t < t^*$ and direct information on the employment rate in the presence of a minimum wage, $E[e_{1it} \mid g, t]$, only for $t \geq t^*$. The objective is to estimate the counterfactual $E[e_{0it} \mid g, t, t \geq t^*]$, 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 the probability in the absence of the minimum wage can be specified as the sum of two components, where the first component is fixed over time and the second is common across groups:

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

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.

If 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, then:

$$E[e_{1it} \mid g = 1, t] \;=\; E[e_{0it} \mid g = 1, t] \;+\; \theta$$
$$E[e_{1it} \mid g = 2, t] \;=\; E[e_{0it} \mid g = 2, t].$$

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 θ:

$$\{E[e_{it} \mid g = 1, t = t_2] \;-\; E[e_{it} \mid g = 2, t = t_2]\}$$
$$-\;\{E[e_{it} \mid g = 1, t = t_1] \;-\; E[e_{it} \mid g = 2, t = t_1]\} \;=\; \theta$$

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

This estimator is also given by a simple regression. Under the above assumptions, the employment status of individuals in all groups and all time periods can be written:

$$e_{it} = \alpha_g + \gamma_t + \theta D_{it} + \varepsilon_{it} \tag{1}$$

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 $E[\varepsilon_{it} \mid g, t] = 0$. 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.

As in the previous report, this report defines the groups indexed by g in terms of segments of the real wage distribution in the preceding period. 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.[2]

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:

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

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.

[2] This third group can also be further subdivided, see Neumark et al. (2000).

The key identifying assumptions are that interaction terms are zero in the absence of the minimum wage (after controlling for differences in observable characteristics) and that the minimum wage does not alter employment probabilities in group g = 2.

The model estimated is for 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:

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

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, γ_t are time effects for the remaining time periods and Λ 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 θ 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.

A further addition to the model is required to allow for the fact that those with lower pay have a lower probability of remaining employed even in the absence of a minimum wage, meaning that 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.[3] 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. The two techniques tend to give very similar results for the parameters of interest. For the difference-in-differences estimates in this report I include a polynomial in the real wage. This specification is extended when splines are used as described later in the report.

[3] See Stewart (2000) for evidence on this for Britain and an econometric analysis of the relationship.

3. Data used in the report

The previous report used individual-level longitudinal data from three contrasting datasets (matched Labour Force Surveys, the New Earnings Survey Panel Dataset, and the British Household Panel Survey). In all three cases, data up to the most recent available at the time of the report were used:

- Matched Labour Force Surveys: 1997 quarter 1 (i.e. from March 1997) to 1999 quarter 3 (i.e. up to November 1999).

- New Earnings Survey Panel Dataset: 1994 to 1999 (April of each year). [The data file / release NESPD-99.]

- British Household Panel Survey: waves 4 (autumn 1994) to 9 (autumn 1999).

The starting date for the LFS data used was dictated by the fact that earnings questions were only added to the wave 1 questionnaire from 1997 quarter 1 onwards (prior to that they had only been included on the outgoing wave 5). The NESPD and BHPS data used were restricted to 1994 onwards so as to be entirely after the abolition of the Wages Councils.

The current report again uses data up to the most recent waves currently available. This involves:

- Matched Labour Force Surveys: 1997 quarter 1 (i.e. from March 1997) to 2001 quarter 4 (i.e. up to February 2002).

- New Earnings Survey Panel Dataset: 1994 to 2001 (April of each year). [The data file / release NESPD-2001.]

- British Household Panel Survey: waves 4 (autumn 1994) to 10 (autumn 2000).

Thus 27 months (9 quarters) have been added to the matched LFS dataset used, two years to the NESPD and one year to the BHPS.

The British Household Panel Survey data proved to be of little use to the extended analysis. Only one extra wave has become available since the previous report: wave 10, for which the interviews were mainly in autumn 2000. For this wave, given the timing of the uprating, only those interviewed in October 2000 and later can be viewed as suitable for the post-uprating category.[4] This renders the key cells for the post-uprating observations too small for reliable estimation, even for the two adult groups: only 14 in the "treatment" group and 9 in the "comparison" group for men and only 62 and 43 for women. Thus the results presented in this report are restricted to those for the LFS and the NESPD.

The results for the matched Labour Force Surveys are presented in Section 4 and those for the New Earnings Survey Panel Dataset in Section 5.

[4] In fact one should possibly also exclude those interviewed in early October from this category, so that the wage estimates refer to a period after the uprating. This would make the cells even smaller than the sizes quoted in the text.

4. Results for the matched Labour Force Surveys

4.1 Re-examination of the impact of the April 1999 introduction

4.1.1 Difference-in-Differences estimation

The results presented in my first report used matched LFS data up to 1999 quarter 3, i.e. up to November 1999. The LFS data used in this report extends the matched data by 9 quarters, taking this up to February 2002. The difference-in-differences estimator used in the report to estimate the impact of the introduction of the minimum wage looks at 12-month gaps between t and t+1, i.e. it looks at the impact on the probability of being in employment at the end of a 12-month window. It does this by comparing periods where t is prior to the introduction and t+1 after the introduction with periods where both t and t+1 are before the introduction. Thus for the examination of the minimum wage introduction it is appropriate to use data up to observations with t in March 1999 and t+1 in March 2000. This means that three months worth of data can be added to the sample used for the analysis.

The results are presented in Table 1, together with those from the previous report for comparison. A logit form is estimated for the 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. The parameter c is set to 10%, i.e. the comparison group used contains those whose wage at time t was between the minimum (in real terms) and the minimum plus 10%.

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 (and other tables in this report) gives marginal effects 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.) The absolute value of the robust asymptotic t-ratio of the coefficient in the logit model is given in parentheses in each case.

Concentrating on the estimates once other control variables are added (using the logit model), those based on the extended sample are very similar to the earlier estimates, with if anything slightly less evidence of any adverse employment effect. Looking first at the estimates using the wage variable based on usual hours, as in the previous report the estimated effect is positive for both male groups and for young women. The point estimates for these groups increase very slightly when the sample is extended. The estimated effect for adult women, which was -.019 with an absolute t-ratio of 1.49 in the previous report, i.e. on the shorter time period, declines both in absolute value (to -.010) and in significance (the absolute t-ratio falls to 0.93) when the additional data is incorporated.

An aside on the interpretation of the numerical value of the coefficient estimate may be useful here, despite its insignificance. The estimated effect of -.010 implies that the probability of remaining in employment for someone whose wage would have needed to be raised to comply with the introduction of the minimum (i.e. who had previously been paid below the minimum) was lower than it would have been in the absence of the minimum by one percentage point. Someone whose probability of remaining employed would have been 0.95 if the minimum wage had not been introduced would have had this decline to 0.94 as a result of the minimum wage's introduction. However it is important to remember that this difference is not significantly different from zero.

There is similarly little impact on the estimates using the wage based on actual hours, with the estimated effect on employment being positive but insignificant for all four demographic groups with or without the extended time period.

As discussed in my first report, there are difficulties associated with the construction of an appropriate hourly wage variable on the LFS, particularly relating to the choice of the appropriate hours measure to use. The ONS-recommended construction of the hourly pay variable for 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). In this construction, there is a potential mismatch between the hours measure used and the number of 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). The reference week does not necessarily correspond exactly to the pay period either. However it should do so for employees paid weekly at the end of each week. For this group using actual hours will provide a more accurate wage measure than that based on usual hours. The low paid, who are the focus of this report, are more likely to be weekly paid and for them the wage based on actual hours is likely to be preferable to that based on usual hours. While the wage constructed using usual hours may provide a more accurate measure for those higher up the wage distribution, my belief is that the wage variable based on actual hours will be a more accurate measure for those with low wages, who are the focus of attention here. However, since the wage based on usual hours is the ONS-recommended measure, I use the two in parallel in the results presented in this report. I return to this issue below.

4.1.2 Estimates based on using a "wage gap" variable

The difference-in-differences estimator uses a binary variable for the "at risk" wage group (i.e. those initially paid below the incoming minimum), together with its interaction with the post-minimum indicator, to measure the impact on the employment probability. Implicitly any effect on an individual's employment probability is assumed to be the same irrespective of how far below the minimum the person's wage initially was, i.e. by how much it would have had to be increased to comply with the new minimum. Any effect is taken to be the same for someone who would have needed say a 50p increase in their hourly wage as someone just 5p below the minimum.

An alternative method of estimation is to use a "wage gap" variable, as was done for example by Currie and Fallick (1996). This is constructed as the gap between the individual's wage at time t and the relevant minimum wage in place at time t+1. More explicitly

$$gap_{it} = \begin{cases} m_{t+1} - w_{it} & \text{if } m_{t+1} > w_{it} \\ 0 & \text{else} \end{cases} \tag{4}$$

The model used here is specified in the same way as for the difference-in-differences estimator, but with the dummy variable g_{1it} replaced by this "gap" variable. Equivalently this can be viewed as replacing g_{1it} by the product term $(m_{t+1} - w_{it})g_{1it}$ in equation (3) above.

It is important to note that there are advantages and disadvantages of this estimation method relative to a difference-in-differences estimator. Potentially precision is gained because those whose wage needed to be raised a lot are distinguished from those for whom only a small increase was required. Counter to this, the "gap" estimator will be more susceptible to the problems of measurement error at the very bottom of the distribution. For example a wage misreported at £1.50/hour instead of £3.50/hour has no effect on the difference-in-differences estimator, but will have an effect on the estimator using the wage gap variable. Since a not insignificant minority of those in the below-£3.60 category have wages below £2/hour, one suspects a number of these cases may be affected by measurement error. This would induce a bias in the gap estimator.

The results from using this construction to estimate the effect of the introduction of the minimum wage on the probability of subsequent employment are given in the top half of Table 2. Looking first at the wage based on usual hours, the estimated effects in the logit model with control variables included are all smaller (with same sign) than the corresponding dummy variable (i.e. difference-in-differences) estimates. The absolute t-ratios are also smaller, with the exception of young men, and in all cases indicate insignificance. The overall conclusion is the same when the wage gap construction is used as when the dummy variable construction is used. When the wage based on actual hours is used, the estimated effect is insignificant for three of the demographic groups, but significantly positive for young men. With both wage measures there is no evidence of any significant adverse effect.

The interpretation of the estimated effect is of course slightly different when this variable is used. It now measures the effect on the employment probability per unit "gap" (i.e. per £ below the minimum). For adult women, for example, a wage "gap" of £1 (i.e. a wage that needed to be raised from £2.60 to £3.60 to comply) is estimated to have reduced the employment probability by half a percentage point (but insignificantly different from zero). To put this another way, someone on £3.40/hour, 20p below the incoming minimum, with characteristics that in the absence of the minimum wage would have given them a 90% chance of remaining in employment, is estimated to have had this reduced to 89.9% as a result of the introduction of the minimum wage and the 20p/hour raise required to bring them up to the new minimum.

Alternatively we can interpret the estimates in terms of the implied elasticities. These are given in the next two rows of Table 2. To illustrate, an adult woman whose wage had to be raised by 10% to bring them up to the minimum would have had their employment probability reduced by 0.03%.

This gap variable can alternatively be viewed as a (negative) linear spline term with node at £3.60. A linear spline is a set of connected straight-line segments. Since this technique may be unfamiliar, a brief introduction to the use of splines is given in Appendix A. Taking m_{t+1} as the first node of the spline, the first spline term would be:

$$v_{it}^{(0)} = \begin{cases} w_{it} & \text{if } w_{it} < m_{t+1} \\ m_{t+1} & \text{if } w_{it} \geq m_{t+1} \end{cases}$$

From this it follows that $gap_{it} = m_{t+1} - v_{it}^{(0)}$. This equivalence with a spline specification means that there is something of an inconsistency in this model: the range of the wage distribution below £3.60 is represented by a spline, while that above £3.60 is modelled by dummies. A more consistent specification, one might argue, would be to represent the entire wage range by a spline with the set of nodes including m_{t+1} and $m_{t+1}(1+c)$ for comparability with the difference-in-differences estimator.

If only these two nodes are used (i.e. if three wage groups are considered as in the difference-in-differences specification above), then the two spline terms above the minimum are defined as

$$v_{it}^{(1)} = \begin{cases} 0 \text{ if } w_{it} < m_{t+1} \\ w_{it} - m_{t+1} \text{ if } m_{t+1} \le w_{it} < (1+c)m_{t+1} \\ cm_{t+1} \text{ if } w_{it} \ge (1+c)m_{t+1} \end{cases}$$

and

$$v_{it}^{(2)} = \begin{cases} 0 \text{ if } w_{it} < (1+c)m_{t+1} \\ w_{it} - (1+c)m_{t+1} \text{ if } w_{it} \ge (1+c)m_{t+1} \end{cases}$$

where c defines the upper limit of the comparison group used before (set to 10% here). This defines two connected straight-line segments above the minimum. Estimates of this linear spline specification are given in the lower block of Table 2. The estimates for all groups and both wage variables are similar to those in the top half of the table.

The nodes used in this specification give a very uneven spanning of the wage distribution. However the results are changed little by adopting a more even representation. The second block of Table 3 gives the corresponding estimates for the wage variable based on usual hours when the range above $(1+c)m_{t+1}$ is divided into five segments instead of one.[5] The previous estimates are repeated in the first block of the table to ease comparison. The results are again very similar.

[5] The nodes used are roughly equal to the quintiles for this range. £5, £7, £9 and £12 are used for adults and £3.80, £4.30, £5 and £6 for youths.

Both these specifications restrict the relationship with the real wage to be a series of connected straight-line segments. In the third block of Table 3 the linear spline is replaced by a cubic spline using the same nodes (and incorporating the cubic already in the control variables). Again the estimates are very similar. The central conclusion on the impact of the minimum wage's initial introduction is not sensitive to which of the above specifications is adopted.

4.2. The impact of the 2000 minimum wage uprating

The difference-in-differences estimator used above to estimate the impact of the introduction of the minimum wage can also be used to estimate the impact of the subsequent upratings. The first uprating took place in June 2000 for youths, with the rate rising from £3 to £3.20, and in October 2000 for adults, with the rate rising from £3.60 to £3.70. The second uprating took place in October 2001 for both groups, with the youth rate going from £3.20 to £3.50 and the adult rate from £3.70 to £4.10. The two upratings are examined separately. The phases involved in considering 12-month changes with the LFS data are laid out in Table 4 separately for adults and youths.

As above when evaluating the introduction of the minimum, the cell sizes in the "treatment" and "comparison" groups for both young men and young women for both upratings in the standard setup are rather small, and in these cases they are really too small for reasonable estimation. Two changes are made to combat this. First, the two youth groups are combined together into a single group. Second, the comparison group for youths is widened to cover wages up to the minimum plus 15% (i.e. the parameter c is increased from 0.10 to 0.15). Implementation of these changes increases the size of the smallest cell to 110 people in the most favourable case for the 2000 uprating. Nevertheless, the important cells are still rather small for reliable results (and get even smaller than this for certain comparisons), and so the main focus will be on the impact of the upratings of the adult rate.

The 2000 uprating is considered in this section, and the 2001 one in the next. The first difference-in-differences estimates compare the period covering the 2000 uprating with the pre-introduction phase, this latter being the same as that used in the estimates of the impact of the introduction. The estimates are given in the first row of each of the first two blocks of Table 5. The estimated effect of the uprating on the subsequent employment probability is insignificantly different from zero for all three groups and for both wage variables. For both wage variables it is positive for men and negative for women. For women it is smaller (in absolute value) and less significant when the wage based on actual hours is used. It is largest (in absolute terms) for youths with both wage variables, but it is negative using the wage based on usual hours and positive using that based on actual hours, but in both cases it is insignificantly different from zero.

If the wage gap estimator is used instead of difference-in-differences, the estimated effects are very similar. These estimates are given in the second row of each block of Table 5 with dummies above the minimum and in the third row with a linear spline above the minimum. Although the point estimates are similar to those using the difference-in-differences estimator, for the wage based on usual hours the absolute t-ratio on the coefficient for adult women strengthens markedly. It roughly doubles and now indicates a significant negative effect on the employment probability. However this does not happen when the wage based on actual hours is used. In this case the significance falls if anything and the estimated effect for adult women is insignificant and positive. Thus for this inter-temporal comparison the results are mixed, particularly for adult women, and the conclusions are sensitive to both the wage variable used and the estimation method used.

The central difficulty in the estimation of effects such as these is the finding of suitable comparison groups. This is crucial both for the comparison between "treated" and "untreated" groups in the period being studied and also for the comparison across time periods. In other words, in terms of the difference-in-differences estimator, we have to worry about the differencing in both dimensions. In the current context, when considering the 2000 uprating (and also when considering the 2001 one below), one of the key problems, alongside definition of the "treated" and "untreated" groups for the period spanning the uprating, is the finding of a suitable *inter-temporal* comparison. The period compared with needs to be one in which the level of the minimum did not change, but which is otherwise as similar to the period spanning the uprating as possible. This creates enormous difficulties for these evaluations. One obvious candidate is the pre-introduction phase (see Table 4), as used above. However this makes the time gap quite large for some observations. 2000 and particularly 2001 saw a slowdown in employment growth compared with the mid and late 1990s (i.e. the period with which the inter-temporal comparison is being made). This is problematic for these estimators and may partly be driving the results above.

What are the alternatives? For the analysis of the 2000 uprating one possibility is the period immediately before the uprating. Given the 12-month gap between t and t+1, this gives us a 6-month window for t in the case of adults (see Table 4): April to September 1999 inclusive. This produces a considerable drop in the sizes of the crucial cells, but is feasible. (For the youth group the window is only a 2-month one and the key cells become too small for meaningful analysis.) Note that this option is not open to us for analyzing the 2001 uprating for adults: there are no observations for which t and t+1 are 12 months apart and both lie after the 2000 uprating and before the 2001 one. (For the youth group the gap was wider, but the window is still only a 4-month one and again the key cells are too small.)

To overcome the small cells problem one possibility would be to combine this phase with the pre-introduction one used above, and this is considered below. If it is the case that the introduction of the minimum did indeed have no effect on employment probabilities as found above, then this period also could be used for comparison in conjunction with these other two. These comparisons are also considered in Table 5. The lower half of the first page of Table 5 gives estimates based on the comparison with the combination of the pre-introduction phase and the no-change phase described above. The top half of the second page of the table adds the phase spanning the introduction to the comparison period so that is covers all of the first three phases listed in Table 4. The lower half of the second page of the table then uses the no-change phase alone for the comparison period (only for adults), but it should be remembered when looking at the results for that comparison that some of the crucial cells become rather small in that case.

When the pre-introduction and no-change phases are combined as the comparison period, the results are very similar to those in the top half of the first page of the table. For adult women there is a significant negative effect of the 2000 uprating if the wage based on usual hours and the gap estimator are used, but not if either the wage based on actual hours or a difference-in-differences estimator is used. So the sensitivity of the results continues. For adult men and youths the effect is insignificant in all cases. The same pattern of results is repeated in the top half of the second page of the table where the period spanning the introduction is added to the comparison period so that it contains all of the three previous phases.

In the lower half of that page of the table where the no-change phase alone is used for the comparison period, the numerical estimates for adult women are similar to those above, but the t-ratios fall and the estimated effects are now insignificant. However the much-reduced cell sizes should be kept in mind. For adult men the estimated effect is negative for this inter-temporal comparison for both wage measures and all three estimators. When the wage based on usual hours and the difference-in-differences estimator are used, the estimate is significant at the 5% level, but not for the other estimators or if the wage based on actual hours is used.

A new wage question, asking directly about the hourly rate that respondents are paid (but only for those who are hourly paid), was introduced to the Labour Force Survey questionnaire in the spring quarter of 1999. The timing of this prevents it from being useful in the analysis of the impact of the introduction of the minimum wage around the same time, but the information provided may potentially be useful for the analysis of the effects of the upratings. Since the question was only asked for those paid by the hour, crucial cell sizes are reduced even further. Using only the no-change phase and any analysis of youths are both ruled out because of this. Assuming again no impact of the introduction of the minimum wage, the final block of Table 5 presents results based on a comparison with a combination of the phase that spans the introduction and the no-change phase. None of the estimates is significantly different from zero. For adult women the difference-in-differences estimate is positive and the wage gap estimates negative, for adult men it is the other way round.

Overall there is *some* evidence of a negative effect of the 2000 uprating on employment using these simple estimators, mainly for adult women, but it is sensitive to the choice of wage variable and estimation method, only being significant for one of the four variable and estimator combinations.

The possibility was raised above that the results on the evaluation of the effect of the 2000 uprating might be influenced by the overall slowdown in employment growth compared with the mid and late 1990s. If the slowdown, while affecting all wage groups, has an increasingly adverse effect as we go down the wage distribution, then this might bias estimates of the type used here towards a negative effect. One way to address this might be to look at a comparison between two groups from further up the wage distribution (and reasonably well clear of any impact of the minimum wage) and compare the results for this comparison with those presented thus far, and more formally to take the difference between them to give what will be referred to as "growth adjusted" estimates.

The results of an analysis of this type (for the two adult groups) are presented in Table 6. The artificial "treatment" group in this experiment is taken to be those between 20% and 30% above the new minimum (£4.44 - £4.81, in October 2000 terms, for the adult rate) and the "comparison" group to be those between 30% and 40% above (£4.81 - £5.18). The top half of Table 6 gives the estimates for this comparison: both difference-in-differences estimates and wage gap estimates for wage variables based on both usual and actual hours.

The estimates are all insignificantly different from zero, but in most cases are of a similar order of magnitude to the estimates presented above for the evaluation of the impact of the uprating. For example, for adult women, the estimated elasticity for the wage gap estimator and the wage based on usual hours is -.014 when comparing those below the minimum with those between it and 10% above and -.012 when making the comparison outlined above. However the former is based on a coefficient with an absolute t-ratio of 2.80, whereas the latter is based on one with an absolute t-ratio of 0.96. Never-the-less this suggests that the earlier evidence of an adverse effect of the

2000 uprating may, at least in part, be due to differential underlying employment growth.

As a more formal examination of this, one can take the difference between the estimates to give "growth adjusted" estimates of the impact of the 2000 uprating on the "affected" group, i.e. the group initially below the new minimum. For the difference-in-differences estimator this is straight-forward conceptually. The real wage distribution at time t is divided into 6 intervals (in October 2000 terms) as follows:

(1) Below £3.70 (i.e. below the minimum, *m*)
(2) £3.70 - £4.07 (*m* - 1.1*m*)
(3) £4.07 - £4.44 (1.1*m* - 1.2*m*)
(4) £4.44 - £4.81 (1.2*m* - 1.3*m*)
(5) £4.81 - £5.18 (1.3*m* - 1.4*m*)
(6) £5.18 and above (1.4*m* and above)

Dummy variables are then defined for the six groups: $g_{kit} = 1$ if the real wage of individual i in year t is in group k as defined above. The construction required is then most easily seen in terms of a linear model:

$$y_{it+1} = x_{it}'\beta + \alpha_1 g_{1it} + \alpha_3 g_{3it} + \alpha_4 g_{4it} + \alpha_5 g_{5it} + \alpha_6 g_{6it} + \gamma_0 d_{t+1} + \phi_1 g_{1it} d_{t+1} + \phi_3 g_{3it} d_{t+1} + \phi_4 g_{4it} d_{t+1} + \phi_5 g_{5it} d_{t+1} + \phi_6 g_{6it} d_{t+1} + \gamma_t + \varepsilon_{it}$$

where group (2) is the omitted comparison group and d_{t+1} is a binary variable taking the value 1 if the new minimum wage was in place at time t+1. With this construction the original (linear) difference-in-differences estimator of the effect of the uprating, equivalent to that considered above, is given by the estimate of ϕ_1, while the corresponding estimator for the artificial comparison from further up the distribution discussed above is the estimate of $\phi_4 - \phi_5$. The "growth adjusted" estimator is then given by the difference between these two comparisons, i.e. by the estimate of $\phi_1 - (\phi_4 - \phi_5)$. This can be got directly (together with appropriate standard error and t-ratio) by re-parameterizing the model as follows:

$$y_{it+1} = x_{it}'\beta + (\alpha_1 - \alpha_4 + \alpha_5) g_{1it} + \alpha_3 g_{3it} + \alpha_4 (g_{4it} + g_{1it}) + \alpha_5 (g_{5it} - g_{1it}) + \alpha_6 g_{6it} + \gamma_0 d_{t+1} + (\phi_1 - \phi_4 + \phi_5) g_{1it} d_{t+1} + \phi_3 g_{3it} d_{t+1} + \phi_4 (g_{4it} + g_{1it}) d_{t+1} + \phi_5 (g_{5it} - g_{1it}) d_{t+1} + \phi_6 g_{6it} d_{t+1} + \gamma_t + \varepsilon_{it}$$

The "growth adjusted" estimates constructed in this way (but using a logit model to control for other factors) are presented in the lower half of Table 6. They are insignificantly different from zero in all cases: for both estimators, for both wage variables and for both men and women. For men two of the estimates are positive and two negative. For women, three are negative, but the wage gap estimator using the wage variable based on usual hours, which was negative and significant when the ordinary unadjusted estimator was considered, is positive here. Among the estimates that are negative, the largest absolute t-ratio is only 0.84. Overall the evidence of an adverse effect of the 2000 uprating is far weaker when this "growth adjusted" estimator is used.

4.3. The impact of the 2001 minimum wage uprating

For the second uprating there are additional small cell problems due to the shorter post-uprating period observed with the data only currently available up to February 2002. This is particularly acute for the youths group. Results are therefore only presented for the two adult groups. The results for the 2001 uprating are given in Tables 7 – 9. For men the unadjusted estimated effects using either the difference-in-differences estimator or the wage gap estimator and using either of the wage variables are all positive, but not significantly different from zero.

For women, surprisingly given that this was a larger uprating, the unadjusted estimated effects are typically smaller and less significant than those for the first uprating. This adds to the suspicion, explored above, that there may be other forces influencing these unadjusted estimates, such as the impact of the economic slowdown. The unadjusted difference-in-differences estimator using the wage based on usual hours is negative but with an absolute t-ratio of only 0.37 and only about one third the size of that for the first uprating. It ceases to even be negative when either the "no-change" time periods are added to the sample or the wage based on actual hours is used.

When the "wage gap" estimator is used in combination with the wage based on usual hours, the estimate for women is negative with an absolute t-ratio of 1.81, implying a p-value of 7%, i.e. significant at the 10% level, but not at 5%. However, when the wage based on actual hours is used, both the estimate and t-ratio are much reduced (in absolute value).

As for the analysis of the 2000 uprating, there is a problem with these unadjusted estimates due to the rather different economic conditions in the two time periods being compared. This can be addressed using "growth adjusted" versions of the estimators constructed equivalently to those used above for the 2000 uprating. These are presented in Table 9. They are insignificantly different from zero in all cases: for both estimators, for both wage variables and for both men and women. For women three of the four combinations give a positive estimate, including both wage gap estimates. The only estimate that is negative has a very small t-ratio.

Overall there is some weak evidence of an adverse effect for adult women when the unadjusted "wage gap" estimator and the wage based on usual hours are both used, but its significance is weak and it is absent if either the wage based on actual hours is used or the difference-in-differences estimator or either of the "growth adjusted" estimators used. The balance of the evidence does not suggest an adverse employment effect of the 2001 uprating.

4.4. Industry-specific influences

Employment growth and the timing of cycles differ a lot from industry to industry. This section considers the role of industry-specific employment growth in a number of inter-linked ways. The first compares pre-minimum wage growth rates to test an underlying assumption of the difference-in-differences estimator that these are equal in the "treatment" and "comparison" groups in the absence of a minimum wage. The second looks at the use of (lagged) employment growth rates as additional control variables to capture any effects of the stage of the economic cycle and state of the labour market in the particular industry in which an individual is employed. The third looks at the hypothesis that the minimum wage will bite more in sectors where growth is low (or negative) and hence firms' room for maneuver less.

All three of these investigations make use of industry-level employment data at the 4-digit level. More specifically an individual's industry is matched to industry-level employment data using the classes and subclasses of the UK Standard Industrial Classification of Economic Activities (SIC-92), meaning that individuals are classified into about 500 different 4-digit industries.

The prime source of employment information for most purposes is now the Annual Business Inquiry (ABI). Data from the ABI is available from 1998 onwards, with a survey date of December of each year. The ABI replaced the Annual Employment Survey (AES) (which was conducted in September of each year). Unfortunately, the AES methodology is now regarded as flawed and the AES data as giving a serious undercount of employment. For 1998 the AES estimate of total employee jobs was around 900,000 lower than the ABI estimate (Partington, 2001). This was due both to under-recording of employee numbers by some of the participating firms and to flaws in the way the AES processing system estimated employment numbers for businesses not covered by the survey. The ABI methods were shown to be better in both respects and the ABI estimates were also found to be more closely aligned with Labour Force Survey figures. Despite these drawbacks with the AES employment numbers, ONS believe that *growth rates* in the AES were much less affected.

The analyses here use data from the ABI for the years 1998-2000 inclusive, 2000 being the latest available, and from the AES (re-scaled) for the years 1995-1997 inclusive, 1995 being the earliest available on a consistent basis. (The revised version of the data as at September 2002 is used.)

4.4.1 Comparison of pre-minimum wage growth rates

An implicit assumption in the difference-in-differences approach is that the underlying employment growth rates in the sectors employing those in the "treatment" and "comparison" groups are the same in the absence of a minimum wage. This is examined in Table 10. For both adults and youths the medians are the same for the "treatment" and "comparison" groups and the means are close, with t-tests of their equality easily accepting the null hypothesis: t-statistics of 1.24 (p-value = 21%) for adults and 0.62 (p-value = 54%) for youths. The data therefore supports this underlying assumption of the difference-in-differences estimator.

4.4.2 Time- and sector-specific growth rate as additional control

The next aspect examined is the use of industry-specific employment growth rates at time t as an additional control variable to capture the stage of the economic cycle and state of the labour market in the particular industry in which an individual is employed. More specifically this constructs a variable for the 12-month employment growth rate at time t in the 4-digit industry in which the individual is employed at that time. Observations in the first half of the year are allocated the appropriate growth rate associated with the previous year, since the employment surveys take place towards the end of the year.

Note that this variable is time-varying. In part it attempts to control for the fact that the individuals observed in "before" and "after" time periods may face different economic conditions. There will be differences in the stage of the economic cycle and state of the labour market in the particular industry in which an individual is employed.

However adding this variable as an additional control to the various models for evaluating the impact of the introduction of the minimum wage has no effect on the results. The variable is never significant and both the difference-in-differences and "wage gap" estimates are unchanged from those presented above.

4.4.3 High- versus low-growth sectors

The third aspect of the analysis of this section examines the hypothesis that the minimum wage will bite more in sectors where growth is low (or negative). In fast growing sectors there may be more scope to accommodate the wage increases required than in slow-growth or declining sectors. The data is partitioned at the median employment growth rate (calculated at the individual-level, i.e. weighted by the LFS sample numbers), which is about 2%, and the models estimated separately for those in "high growth" and "low growth" industries.

A summary of the results is presented in Table 11. Both difference-in-differences and wage gap estimates are presented. The hypothesis is not supported by the data. Splitting the sample in this way only provides adequate cell sizes for the two adult groups. So results are only presented for them.

For men, the estimates are generally positive (with one minor exception) in both the "high growth" and "low growth" sectors, but insignificantly different from zero in all cases. They are also fairly similar in the "high growth" and "low growth" sectors.

For women, the estimates are negative but insignificant in all cases (i.e. for each estimator and each wage variable) for those in the high growth industries, while they are positive in all cases for those in the low growth industries. In most cases the latter are also insignificant, but when using the wage variable based on actual hours and a wage gap estimator the estimate for adult women in low growth industries is significantly greater than zero.

Overall there is no evidence in support of the hypothesis of a greater minimum wage "bite" in the low growth sectors.

5. Results using the New Earnings Survey

5.1 The new NES data file: NESPD-2001

The New Earnings Survey results presented in this report are based on the latest NESPD data file provided by the Office for National Statistics (the file NESPD-2001). The data in this file show some potentially important differences in construction for the years up to 1999 compared with the previous data file (NESPD-99), which was the file used for my previous report to the Low Pay Commission (Stewart, 2001) and for Stewart (2002). These changes in ONS processing procedures are catalogued in this section, together with indications of the number of cases involved.

1. "Late returns" have been added to the file for the years 1996-99. These were not included in the previously used file. ("Late returns" are also included in the new file for the years 2000 and 2001). This addition of "late returns" increases the number of cases in the file for each of these years as follows:

Year	Increase in # of cases
1996	542
1997	123
1998	3,725
1999	2,119

2. Those with temporary National Insurance numbers (more specifically NI numbers beginning with the letters PP) have been removed from the panel in the new file. This results in the removal of the following numbers of cases for the years being used in this report:

Year	# cases removed
1994	627
1995	294
1996	353
1997	233
1998	227
1999	310
2000	1,742
2001	2,227

3. Duplicate National Insurance numbers are treated differently over the years in the file. For the years 2000 and 2001 cases with duplicate National Insurance numbers (at the 6-digit level but not at the 9-digit level) have been removed by ONS where they matched to different names on the sample files. For 1999 all duplicate National Insurance numbers were removed, since it was not possible to match to the sample file. For years prior to 1999 it was not possible to identify duplicates since National Insurance numbers are not held to the 9-digit level for these years. The numbers of cases removed due to a non-unique 6-digit National Insurance number:

Year	# of cases removed
1999	748
2000	380
2001	316

The net effects of these changes at different stages of the sample selection procedure used for this report are shown in Table 12. The sample selection stages shown in the table are as follows (and are sequential).

(1) The full sample.

(2) Restricted to those aged 18 & over, but strictly less than 60.

(3) Excluding those with double jobs indicated, those with loss of pay indicated, those who worked less than one hour in the reference week, those for whom average hourly earnings excluding overtime (the variable hexo) is 0, missing, or does not match its components, and those with real basic wage (hexo deflated by retail price index) less than 50p/hour.

(4) Excluding part-timers.

The net effects show small reductions for 1994, 1995 and 1997, a small increase for 1996, and slightly larger increases for 1999 and particularly 1998.

In terms of replicating the difference-in-differences results from Stewart (2002), the overall sample sizes for the "raw" estimates for men and women are shown in the first row of the table below. Both show a slight increase: 1,284 (0.4%) for men and 552 (0.3%) for women. Once missing values for the required control variables have been excluded however, the picture is slightly different. The sample sizes of the estimation samples (old and new) are given in the second row of the table. These, in contrast, show a slight fall: 1,191 (0.4%) for men and 867 (0.4%) for women.

	Men		Women	
	NESPD-99	NESPD-2001	NESPD-99	NESPD-2001
"Raw" estimates	328,657	329,941	196,387	196,939
Logit model estimates	328,064	326,873	196,003	195,136

One particular change, not documented by ONS, affects the sample sizes used for the difference-in-differences estimator on the model with control variables included. This concerns the variable for whether or not an employee has been in the same job for more than 12 months (the variable j12m). The "raw" sample based on NESPD-99 contained no missing values on this variable. That based on NESPD-2001 contains missing values on j12m as follows:

Year	Full NESPD-2001 sample	"raw" diff-in-diffs sample
1994	0	0
1995	0	0
1996	644	324
1997	377	79
1998	5,073	2,745
1999	2,751	1,768
2000	237	11
2001	0	-

This seems very uneven and suggests that different coding rules may have been applied in different years. However given the data available, there is no choice but to exclude these cases if this variable is used.

5.2 Re-examination of the impact of the minimum wage introduction

The combined effect of these changes on employment rates for adults is as follows. The employment rate for 1999 by real wage group in 1998 shows a fall (of 0.7%) for the affected group (those below the minimum) and a rise (of 0.6%) for the comparison group. (The rate also rises for the top wage group.) Thus the employment rate gap widens and the "raw" difference-in-differences estimates become slightly more negative than before.

Difference-in-difference estimates based on the logit model with control variables, using data up to 1999 only, are given in the top half of Table 13 for both the old data (NESPD-99) and the new data (NESPD-2001). The estimates become more negative for both adult groups and for young men and less positive for young women when the new data is used. However all remain insignificantly different from zero. In particular that for adult women increases in absolute terms by about half: from -.013 to -.020. The absolute t-ratio also increases, to 1.29, but is still not close to significance.

These estimates are based on samples that exclude those with missing values on the variable indicating whether they have been in their jobs at least 12 months (the variable j12m). As indicated in the previous section, there are doubts about the consistency of this missing value coding over time. However, an alternative procedure of including these cases and adding a separate dummy variable for them produces very similar results.

The difference-in-differences estimates show more change as a result of the ONS revised processing procedures than one would have hoped to see, but the general conclusions from the estimates remain unchanged.

Estimates based on using the "wage gap" variable below the minimum and a linear spline above are given in the lower half of Table 13. For adult women the estimated effect is very similar with both the old data and the new, implying an elasticity of -.007 with the old data and -.016 with the new. The absolute t-ratio is reduced in both cases (to 0.39 with the old data and 0.91 with the new).

The absolute t-ratio on the coefficient for adult men on the other hand rises with both datasets and is now on the margins of significance when the new data is used (1.74, implying a p-value of 8.1%), although not when the old is used (absolute t-ratio of

1.32 in this case). Using the new data and using the "wage gap" specification both have the effect of making the estimated effect more negative and the absolute t-ratio greater. The two modifications in combination are enough to bring the estimated coefficient to the margins of significance. The considerable increase in absolute t-ratio for the "wage gap" estimate compared to the difference-in-differences estimate (particularly with the new data) is somewhat surprising. The implied elasticity for adult men is -.029 with the new data and -.023 with the old data.

The estimated effects and t-ratios also increase for young men when the "wage gap" specification is used, both with the old data and with the new. However the effect is never close to significance.

To sum up, for both adult groups there is more evidence of a negative effect of the introduction of the minimum wage when the new NES data file (NESPD-2001) is used than when the old one (NESPD-99) is used. The differences made by the data revisions are worrying and requires further investigation. Unfortunately because of the random allocation of personal identification numbers on the NESPD (to protect confidentiality) and because the allocation is different on the two datasets, it is not possible to examine the data changes in as much detail as I would have liked – or even to identify which observations have been changed. It is therefore hard to know how much confidence to put in the estimates based on the new revised dataset.

5.3 The impact of the 2000 minimum wage uprating

This section uses the NES data to estimate the effect of the 2000 uprating of the minimum, which took place in October for adults and in June for youths. Accordingly real wages are scaled to October 2000 for adults and June 2000 for youths. Both difference-in-differences estimates and "wage gap" estimates are presented. Two inter-temporal comparisons are used. The first compares the uprating period with the pre-introduction years. The second compares it with the "flat" year in between the introduction and the uprating.

The difference-in-difference estimates for the logit model with control variables are given in the top half of Table 14. Looking at the estimator based on the comparison with the pre-introduction years first, the estimated effect is positive but insignificant for all four demographic groups. Indeed it is never even close to significance. Turning to the estimator based on the comparison with the period between the minimum wage's introduction and the 2000 uprating, the picture is similar for adult women and both youth groups. However for adult men, the estimated effect is significantly positive, with a t-ratio of 2.28, implying a p-value of 2.2%.

The "wage gap" estimates paint a slightly different picture in this case. The estimated effect is insignificantly different from zero for all four demographic groups when the second inter-temporal comparison is used. When the comparison is made with the pre-introduction years, the estimated effect is negative for both male groups. For adult men there is an absolute t-ratio of 1.86 (implying a p-value of 6.3%), while for young men it is 2.12 (a p-value of 3.4%). Thus there are differences in the findings according to whether a difference-in-differences estimator or a "wage gap" estimator is used and according to which inter-temporal comparison is used.

As with the analysis of the LFS data, there is a problem with the unadjusted estimates due to the rather different underlying employment conditions in the two time periods being compared. As was done with the LFS data, this can be addressed using "growth adjusted" versions of the estimators as described in Section 4.2 above. Estimates using this approach are presented in Table 15. They are insignificantly different from zero in all cases: for both estimators and for all four demographic groups. Those based on the wage gap estimator are all negative, but their absolute t-ratios never rise above 1.08. Overall the evidence of an adverse employment effect of the 2000 uprating is very weak using the NES data, as it was using the LFS data.

6. Disaggregation by sector

In this section particular sectors are examined on an individual basis using the NES data. This disaggregation is only possible for certain sectors where there are sufficient low paid workers to give adequate cell sizes for both the "treatment" and "comparison" groups of the difference-in-differences estimator (and equivalents parts of the "wage gap" and spline definitions). It is also only possible for the two adult demographic groups. Both industry- and occupation-based definitions of the sectors are used.

6.1 Catering

The first sector examined is catering, one the archetypal sectors with high incidence of low paying jobs that is often held out as likely to be adversely affected by the introduction of the minimum wage. Estimates of the impact for catering are presented in Table 16. The top half of the table presents estimates for the hotels and restaurants sector using an industry definition. Specifically the estimates are for those in Section H (code 55) on the SIC-92 coding frame for 1996 onwards and Class 66 on SIC-80 for pre-1996. (Industry on the NES is coded using the 1980 Standard Industrial Classification up to and including 1995 and using the 1992 Standard Industrial Classification from 1996.)

Using this industry definition, the difference-in-differences estimate is significantly positive for adult men, with a t-ratio of 2.18 and a p-value of 2.9%. For adult women it is also positive, but insignificantly different from zero. However when the "wage gap" estimate is used it is insignificant for both groups.

An alternative way to identify the catering jobs to be focused on is to use an occupational definition. This enables one to focus particularly on low paying jobs. The definition used here identifies those in the following 3-digit groups of the Standard Occupational Classification: 621 (waiters and waitresses), 622 (bar staff), 952 (kitchen porters and hands) and 953 (counterhands and catering assistants).

If this occupational definition is used, the difference-in-differences estimate is again positive for adult men, but with a t-ratio reduced to 1.57 (implying a p-value of 11.7%). For adult women the difference-in-differences estimate is negative and on the margins of significance (an absolute t-ratio of 1.75, implying a p-value of 8%). However both of these are reduced to clear insignificance when the "wage gap" estimate is considered.

Overall, no very clear picture therefore emerges for the catering sector. There is some evidence of an adverse effect for women, but only if the occupation-based definition is used, not if the industry one is, and only if a difference-in-differences estimator is used, not if a wage gap estimator is used.

6.2 Retail

Another sector with a high incidence of low paying jobs is the retail sector. Table 17 gives equivalent estimates for the retail trade sector. Again the top half of the table uses the industry-based definition of the sector. This defines it as containing those in SIC-92 codes 64 and 65 for the years 1996 onwards and SIC-80 code 52 for 1995 and before. For this definition of the sector, the "wage gap" estimate is significantly greater than zero for adult women, but the difference-in-differences estimate is insignificantly different from zero and negative. So again there is some conflict between the two estimators. For men, both estimates are clearly insignificant, but here again they have opposite signs.

The bottom half of Table 17 uses an occupation-based definition to focus particularly on low-paying jobs. Specifically results are presented for sales assistants and checkout staff (occupation codes 720 and 721). In this case both the difference-in-differences and the wage gap estimates are insignificantly different from zero for both demographic groups.

Overall there is very little evidence in these data to support the hypothesis that the employment of those in low paid retail jobs has been adversely affected by the introduction of the minimum wage.

6.3 Cleaners

The next low paying sector examined is cleaners. A purely occupational definition is used in this case: those in Standard Occupational Classification 958. Estimated effects of the impact of the introduction of the minimum wage on employment for cleaners are presented in Table 18. The difference-in-differences estimate for women is positive and on the margins of significance, but significance is lost when the "wage gap" estimator is used. For men both estimates are insignificantly different from zero. Thus for cleaners too there is very little evidence an adverse effect on employment.

6.4 Child care and care assistants

The final low paying sector examined is child care and care assistants. This too is defined solely in occupational terms: those in Standard Occupational Classifications 644 and 659. Table 19 presents estimates for this group. Adequate cell sizes are only available for women for this group of workers. For them both estimates are negative but insignificantly different from zero.

7. Conclusions

The extended LFS dataset shows slightly less evidence of any adverse effect of the introduction of the minimum wage on employment than was the case in the previous report. In addition, the various "wage gap" estimates provided confirm the findings with the difference-in-differences estimator.

In the evaluation of the impact of the 2000 uprating it is difficult to find a suitable and adequate time period for the inter-temporal comparison of the difference-in-differences estimator. There are found to be important differences between the difference-in-differences and "wage gap" estimator results, although more in terms of t-ratios and significance than in terms of point estimates. There is some evidence of a negative effect, using the simple unadjusted (for differential growth) estimators, mainly for adult women, but it is sensitive to the choice of wage variable and estimation method. It is only significant for one of the four variable and estimator combinations. Evidence is also presented that the differential impact of the overall slowdown in employment growth biases these estimates. More credible "growth adjusted" versions of the estimators showed no significant negative employment effects.

In the case of the 2001 uprating, there is some weak evidence of an adverse effect for women, using the simple unadjusted estimators, but it is sensitive to the estimator and wage variable used: it is only significant for the "wage gap" estimator and only for the wage variable based on usual hours, and even then the evidence is not strong. The more credible "growth adjusted" versions of the estimators showed no sign of significant negative employment effects.

The NES data and estimates show more change from the previous version of the dataset (used in the previous report) than one would have hoped and I still have reservations about the changes in the data. Unfortunately it is not possible to check the validity of these. There is some evidence of a negative effect of the introduction of the minimum wage for adult men, but only with NESPD-2001, not with NESPD-99 (even though only the same years are used) and only for the "wage gap" estimator, not the difference-in-differences estimator.

There is some evidence of an adverse employment effect of the 2000 uprating using the simple unadjusted (for differential growth) estimators for adult and young men in the NES, but only when the wage gap estimator is used. There is no such evidence from the difference-in-differences estimator. The more credible "growth adjusted" versions of the estimators also gave no significant employment effects.

When specific sectors are examined (to the extent that sample sizes of low paid employees permit), no compelling evidence of a negative effect on employment is found. There is some evidence of a negative effect for female catering staff, but only if an occupation-based definition of the sector is used, not if an industry-based one is used, and only if a difference-in-differences estimator is used, not if a "wage gap" estimator is used. No evidence of a negative effect is found for the retail sector, for cleaners, or for child care workers and care assistants.

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Appendix A: The use of spline functions

Standard econometric models impose a particular functional form on the relationship between dependent and explanatory variables, for example a linear relationship. These functional forms can be made more flexible by using what are known as *spline functions*. These are made up of segments of straight lines or curves which are spliced together to form a single continuous function. The most commonly used types are *linear splines* and *cubic splines*. To simplify exposition they will be explained in the context of a simple linear regression model.

A *linear spline* is simply a continuous piecewise-linear function, i.e. a series of connected linear segments. A simple illustration is given in Figure A1.

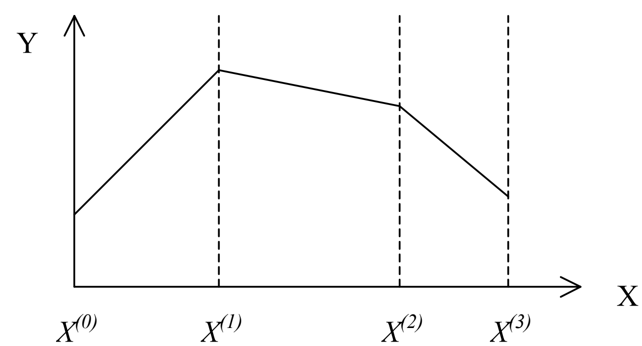

Figure A1

In this example, the range of X is divided into three intervals with interval boundaries $X^{(0)}$, $X^{(1)}$, $X^{(2)}$ and $X^{(3)}$. These boundary points are known in the spline literature as *nodes* or *knots*. The X-profile shown in Figure A1 consists of a set of three straight-line segments constrained so that consecutive segments meet at the boundary. The line segments can be defined by:

$$Y_i = [\alpha_1 + \beta_1(X_i - X^{(0)})]D_{1i} + [\alpha_2 + \beta_2(X_i - X^{(1)})]D_{2i} + [\alpha_3 + \beta_3(X_i - X^{(2)})]D_{3i} + u_i$$

where

$$D_{ki} = \begin{cases} 1 \text{ if } X^{(\text{k-1})} \le X_i \le X^{(k)} \\ \quad 0 \text{ otherwise} \end{cases}$$

This function must then be constrained to ensure continuity, i.e. the meeting of consecutive segments at the boundary. At $X^{(1)}$ the condition for continuity is:

$$\alpha_1 + \beta_1(X^{(1)} - X^{(0)}) = \alpha_2$$

and at $X^{(2)}$ the condition is:

$$\alpha_2 + \beta_2(X^{(2)} - X^{(1)}) = \alpha_3$$

To estimate the equation subject to these linear restrictions one can simply substitute the restrictions into the equation and then estimate as usual. Substitution here gives:

$$\begin{aligned} Y_i = {} & [\alpha_1 + \beta_1(X_i - X^{(0)})]D_{1i} + [\alpha_1 + \beta_1(X^{(1)} - X^{(0)}) + \beta_2(X_i - X^{(1)})]D_{2i} + \\ & [\alpha_1 + \beta_1(X^{(1)} - X^{(0)}) + \beta_2(X^{(2)} - X^{(1)}) + \beta_3(X_i - X^{(2)})]D_{3i} + u_i \end{aligned}$$

Collecting terms in each parameter gives:

$$\begin{aligned} Y_i = {} & \alpha_1 + \beta_1[(X_i - X^{(0)})D_{1i} + (X^{(1)} - X^{(0)})(D_{2i} + D_{3i})] + \beta_2[(X_i - X^{(1)})D_{2i} + \\ & (X^{(2)} - X^{(1)})D_{3i}] + \beta_3[(X_i - X^{(2)})D_{3i}] + u_i \end{aligned}$$

which we can write as:

$$Y_i = \alpha_1 + \beta_1 Z_{1i} + \beta_2 Z_{2i} + \beta_3 Z_{3i} + u_i$$

where

$$Z_{1i} = \begin{cases} X_i - X^{(0)} \text{ if } X^{(0)} \le X_i < X^{(1)} \\ \quad X^{(1)} - X^{(0)} \text{ if } X^{(1)} \le X_i \end{cases}$$

$$Z_{2i} = \begin{cases} 0 \text{ if } X_i < X^{(1)} \\ X_i - X^{(1)} \text{ if } X^{(1)} \le X_i < X^{(2)} \\ \quad X^{(2)} - X^{(1)} \text{ if } X^{(2)} \le X_i \end{cases}$$

$$Z_{3i} = \begin{cases} 0 \text{ if } X_i < X^{(2)} \\ X_i - X^{(2)} \text{ if } X^{(2)} \le X_i \end{cases}$$

which we can write in general as:

$$Z_{ki} = \begin{cases} 0 \text{ if } X_i < X^{(k-1)} \\ X_i - X^{(k-1)} \text{ if } X^{(k-1)} \le X_i < X^{(k)} \\ X^{(k)} - X^{(k-1)} \text{ if } X^{(k)} \le X_i \end{cases}$$

for $k = 1, 2, 3$. Thus to estimate the parameters of the linear spline we simply construct the variables Z_1, Z_2, Z_3 and estimate this equation. Generalization to include other explanatory variables in the equation and/or to more segments are both straightforward. The general definition of Z_k still holds in both cases.

Table 1

Difference-in-differences estimates of effect of the minimum wage introduction

Matched LFS data

	Adult men	Young men	Adult women	Young women
Raw linear difference-in-differences estimates				
Wage based on usual hours				
Sample as in 1st report: t up to Nov 98	.015	-.008	-.019	.022
	(0.43)	(0.09)	(0.99)	(0.25)
Extended sample: t up to March 99	.020	.012	-.006	.067
	(0.68)	(0.15)	(0.32)	(0.79)
Wage based on actual hours				
Sample as in 1st report: t up to Nov 98	.007	.053	.013	.030
	(0.19)	(0.56)	(0.62)	(0.31)
Extended sample: t up to March 99	.015	.059	.011	.079
	(0.49)	(0.72)	(0.60)	(0.86)
Logit difference-in-differences estimates with controls				
Wage based on usual hours				
Sample as in 1st report: t up to Nov 98	.010	.054	-.019	.063
	(0.66)	(0.57)	(1.49)	(0.60)
Extended sample: t up to March 99	.014	.073	-.010	.119
	(0.93)	(0.88)	(0.93)	(1.15)
Wage based on actual hours				
Sample as in 1st report: t up to Nov 98	.005	.141	.006	.023
	(0.34)	(1.17)	(0.50)	(0.26)
Extended sample: t up to March 99	.011	.155	.006	.065
	(0.74)	(1.36)	(0.51)	(0.78)

dd_intro.log, mdd_intro.log

Table 2

Wage gap estimates of effect of the minimum wage introduction

Matched LFS data

	Adult men	Young men	Adult women	Young women
Wage gap below minimum, dummies above				
Wage based on usual hours				
Estimated wage gap effects				
Raw linear equation estimates	.010	.070	-.005	.059
	(0.56)	(1.01)	(0.36)	(1.07)
Logit model with controls	.006	.058	-.005	.045
	(0.77)	(1.22)	(0.67)	(1.00)
Implied elasticities				
Raw linear equation estimates	.009	.057	-.004	.051
Logit model with controls	.005	.047	-.003	.038
Wage based on actual hours				
Estimated wage gap effects				
Raw linear equation estimates	.009	.153	.014	.009
	(0.53)	(2.25)	(0.97)	(0.20)
Logit model with controls	.007	.138	.008	-.005
	(0.83)	(2.70)	(1.02)	(0.11)
Implied elasticities				
Raw linear equation estimates	.009	.119	.010	.008
Logit model with controls	.006	.108	.006	-.004
Wage gap below minimum, linear spline above				
Wage based on usual hours				
Estimated wage gap effects				
Raw linear equation estimates	.003	.046	-.011	-.002
	(0.13)	(0.71)	(0.51)	(0.04)
Logit model with controls	-.001	.021	-.004	-.001
	(0.06)	(0.49)	(0.42)	(0.03)
Implied elasticities				
Raw linear equation estimates	.003	.038	-.008	-.002
Logit model with controls	-.001	.017	-.003	-.001
Wage based on actual hours				
Estimated wage gap effects				
Raw linear equation estimates	.037	.127	.003	-.026
	(1.50)	(1.98)	(0.13)	(0.62)
Logit model with controls	.015	.098	.006	-.021
	(1.45)	(2.14)	(0.50)	(0.49)
Implied elasticities				
Raw linear equation estimates	.035	.100	.002	-.023
Logit model with controls	.014	.077	.005	-.019

gap_intro.log, mgap_intro.log, spline_intro.log, mspline_intro.log

Table 3

Alternative spline specifications of effect of the minimum wage introduction

Matched LFS data

	Adult men	Young men	Adult women	Young women
Linear spline with two terms above minimum				
Estimated wage gap effects				
Raw linear equation estimates	.003	.046	-.011	-.002
	(0.13)	(0.71)	(0.51)	(0.04)
Logit model with controls	-.001	.021	-.004	-.001
	(0.06)	(0.49)	(0.42)	(0.03)
Implied elasticities				
Raw linear equation estimates	.003	.038	-.008	-.002
Logit model with controls	-.001	.017	-.003	-.001
Linear spline with extra terms				
Estimated wage gap effects				
Raw linear equation estimates	.002	.046	-.011	-.001
	(0.09)	(0.71)	(0.50)	(0.01)
Logit model with controls	-.001	.021	-.005	-.0001
	(0.09)	(0.48)	(0.44)	(0.00)
Implied elasticities				
Raw linear equation estimates	.002	.037	-.008	-.001
Logit model with controls	-.001	.017	-.003	-.0001
Cubic spline				
Estimated wage gap effects				
Raw linear equation estimates	.003	.047	-.003	.017
	(0.18)	(0.82)	(0.23)	(0.36)
Logit model with controls	.002	.032	-.002	.004
	(0.27)	(0.83)	(0.25)	(0.09)
Implied elasticities				
Raw linear equation estimates	.002	.037	-.002	.015
Logit model with controls	.001	.026	-.001	.003

Ref: spline_intro.log, sp2_intro.log

Table 4

Phases for 12-month comparisons

Phase	Months for t	Months for t+1	No. of months	m_{t+1}
Adult rates:				
Pre-introduction	Mar 1997 – Mar 1998	Mar 1998 – Mar 1999	13	-
Introduction	Apr 1998 – Mar 1999	Apr 1999 – Mar 2000	12	£3.60
No change	Apr 1999 – Sept 1999	Apr 2000 – Sept 2000	6	£3.60
Uprating 1	Oct 1999 – Sept 2000	Oct 2000 – Sept 2001	12	£3.70
Uprating 2	Oct 2000 – Feb 2001	Oct 2001 – Feb 2002	5	£4.10
Youth rates:				
Pre-introduction	Mar 1997 – Mar 1998	Mar 1998 – Mar 1999	13	-
Introduction	Apr 1998 – Mar 1999	Apr 1999 – Mar 2000	12	£3.00
No change	Apr 1999 – May 1999	Apr 2000 – May 2000	2	£3.00
Uprating 1	June 1999 – May 2000	June 2000 – May 2001	12	£3.20
No change	June 2000 – Sept 2000	June 2001 – Sept 2001	4	£3.20
Uprating 2	Oct 2000 – Feb 2001	Oct 2001 – Feb 2002	5	£3.50

Table 5

Alternative estimates of the effect of the 2000 uprating

Matched LFS data

	Adult men	Adult women	Youths
Comparison to pre-introduction phase only			
Wage based on usual hours			
Difference-in-differences estimate	.007	-.017	-.026
(t-ratio on coefficient)	(0.40)	(1.43)	(0.45)
Wage gap below minimum, dummies above	.007	-.018	-.011
(t-ratio on coefficient)	(1.07)	(2.80)	(0.37)
Implied elasticity	.007	-.014	-.009
Wage gap below minimum, linear spline above	.008	-.019	-.009
(t-ratio on coefficient)	(1.15)	(2.64)	(0.26)
Implied elasticity	.008	-.014	-.007
Wage based on actual hours			
Difference-in-differences estimate	.013	-.008	.021
(t-ratio on coefficient)	(0.82)	(0.68)	(0.38)
Wage gap below minimum, dummies above	.007	.001	.022
(t-ratio on coefficient)	(0.95)	(0.08)	(0.60)
Implied elasticity	.008	.001	.018
Wage gap below minimum, linear spline above	.008	.006	.029
(t-ratio on coefficient)	(0.89)	(0.64)	(0.73)
Implied elasticity	.008	.004	.025
Comparison to both pre-introduction & no-change phases			
Wage based on usual hours			
Difference-in-differences estimate	-.009	-.018	-.049
(t-ratio on coefficient)	(0.64)	(1.62)	(0.90)
Wage gap below minimum, dummies above	.003	-.018	-.027
(t-ratio on coefficient)	(0.55)	(2.85)	(0.85)
Implied elasticity	.003	-.013	-.022
Wage gap below minimum, linear spline above	.005	-.018	-.023
(t-ratio on coefficient)	(0.72)	(2.69)	(0.65)
Implied elasticity	.005	-.014	-.019
Wage based on actual hours			
Difference-in-differences estimate	.005	-.006	-.023
(t-ratio on coefficient)	(0.36)	(0.54)	(0.43)
Wage gap below minimum, dummies above	.003	.000	.003
(t-ratio on coefficient)	(0.44)	(0.04)	(0.07)
Implied elasticity	.003	.000	.002
Wage gap below minimum, linear spline above	.003	.004	.014
(t-ratio on coefficient)	(0.39)	(0.45)	(0.33)
Implied elasticity	.003	.003	.012

up2000u_01.log

Table 5 (continued)

Alternative estimates of the effect of the 2000 uprating

Matched LFS data

	Adult men	Adult women	Youths
Comparison to all three previous phases			
Wage based on usual hours			
Difference-in-differences estimate	-.014	-.016	-.067
(t-ratio on coefficient)	(1.07)	(1.52)	(1.31)
Wage gap below minimum, dummies above	.002	-.017	-.044
(t-ratio on coefficient)	(0.40)	(2.85)	(1.49)
Implied elasticity	.002	-.012	-.037
Wage gap below minimum, linear spline above	.004	-.017	-.042
(t-ratio on coefficient)	(0.61)	(2.67)	(1.27)
Implied elasticity	.004	-.013	-.036
Wage based on actual hours			
Difference-in-differences estimate	.001	-.009	-.013
(t-ratio on coefficient)	(0.10)	(0.89)	(0.27)
Wage gap below minimum, dummies above	.002	-.003	-.016
(t-ratio on coefficient)	(0.30)	(0.36)	(0.45)
Implied elasticity	.002	-.002	-.014
Wage gap below minimum, linear spline above	.002	.001	-.012
(t-ratio on coefficient)	(0.27)	(0.16)	(0.29)
Implied elasticity	.002	.001	-.010
Comparison to no-change phase only			
Wage based on usual hours			
Difference-in-differences estimate	-.034	-.016	NA
(t-ratio on coefficient)	(1.98)	(1.20)	
Wage gap below minimum, dummies above	-.008	-.014	NA
(t-ratio on coefficient)	(0.75)	(1.48)	
Implied elasticity	-.008	-.010	
Wage gap below minimum, linear spline above	-.007	-.015	NA
(t-ratio on coefficient)	(0.55)	(1.43)	
Implied elasticity	-.007	-.010	
Wage based on actual hours			
Difference-in-differences estimate	-.012	.001	NA
(t-ratio on coefficient)	(0.70)	(0.08)	
Wage gap below minimum, dummies above	-.026	-.000	NA
(t-ratio on coefficient)	(1.62)	(0.03)	
Implied elasticity	-.026	-.000	
Wage gap below minimum, linear spline above	-.034	-.001	NA
(t-ratio on coefficient)	(1.59)	(0.08)	
Implied elasticity	-.034	-.001	

up2000u_01.log

Table 5 (continued)

Alternative estimates of the effect of the 2000 uprating

Matched LFS data

	Adult men	Adult women	Youths
Comparison to introduction & no-change phases			
Hourly wage rate variable			
Difference-in-differences estimate	-.018	.015	NA
(t-ratio on coefficient)	(0.50)	(0.59)	
Wage gap below minimum, dummies above	.032	-.028	NA
(t-ratio on coefficient)	(0.74)	(0.58)	
Implied elasticity	.008	-.005	
Wage gap below minimum, linear spline above	.084	-.030	NA
(t-ratio on coefficient)	(1.17)	(0.61)	
Implied elasticity	.022	-.005	

up2000u_01.log

Table 6

Estimates of effect of the 2000 uprating

Matched LFS data, Logit models with control variables

	Adult men	Adult women
Comparison of group 20-30% above the minimum with group 30-40% above		
Difference-in-differences estimator, usual hours	.004	-.007
	(0.25)	(0.42)
Difference-in-differences estimator, actual hours	.006	.009
	(0.33)	(0.57)
Wage gap estimator, usual hours: elasticity	-.004	-.012
	(0.29)	(0.96)
Wage gap estimator, actual hours: elasticity	.005	-.001
	(0.37)	(0.04)
"Growth adjusted" estimates for "affected" (below minimum) group		
Difference-in-differences estimator, usual hours	.003	-.010
	(0.12)	(0.48)
Difference-in-differences estimator, actual hours	.007	-.017
	(0.30)	(0.84)
Wage gap estimator, usual hours: elasticity	-.017	0.64
	(0.28)	(1.46)
Wage gap estimator, actual hours: elasticity	-.045	-.010
	(0.61)	(0.23)

otherwgps1-4.log

Table 7

Difference-in-differences estimates of effect of the 2001 uprating

Matched LFS data

	Adult men	Adult women	Youths
Raw linear difference-in-differences estimates			
Wage based on usual hours			
Comparison to pre-introduction phase only	.029	-.005	NA
	(0.78)	(0.21)	
Comparison to both pre-introduction and	.031	.003	
no-change phases	(0.86)	(0.15)	
Wage based on actual hours			
Comparison to pre-introduction phase only	.022	.004	NA
	(0.66)	(0.18)	
Comparison to both pre-introduction and	.010	.014	
no-change phases	(0.29)	(0.58)	
Logit difference-in-differences estimates with controls			
Wage based on usual hours			
Comparison to pre-introduction phase only	.022	-.006	NA
	(1.10)	(0.37)	
Comparison to both pre-introduction and	.023	.0002	
no-change phases	(1.19)	(0.01)	
Wage based on actual hours			
Comparison to pre-introduction phase only	.018	.001	NA
	(0.76)	(0.04)	
Comparison to both pre-introduction and	.011	.007	
no-change phases	(0.48)	(0.44)	

up2comb_01.log, up2comb_03.log

Table 8

Wage gap estimates of effect of the 2001 uprating

Matched LFS data, Logit models with control variables

	Adult men	Adult women	Youths
Wage gap below minimum, dummies above			
Wage based on usual hours			
Probability unit effect	.012	-.015	NA
(t-ratio on coefficient)	(1.14)	(1.81)	
Implied elasticity	.012	-.012	
Wage based on actual hours			
Probability unit effect	.018	-.006	NA
(t-ratio on coefficient)	(1.52)	(0.59)	
Implied elasticity	.017	-.005	
Wage gap below minimum, linear spline above			
Wage based on usual hours			
Probability unit effect	.014	-.016	NA
(t-ratio on coefficient)	(1.22)	(1.82)	
Implied elasticity	.013	-.014	
Wage based on actual hours			
Probability unit effect	.016	-.007	NA
(t-ratio on coefficient)	(1.41)	(0.64)	
Implied elasticity	.016	-.006	

gap_updat2.log, mgap_updat2.log

Table 9

"Growth adjusted" estimates of effect of the 2001 uprating

Matched LFS data, Logit models with control variables

	Adult men	Adult women	Youths
Difference-in-differences estimator, usual hours	.023	-.007	NA
(t-ratio on coefficient)	(0.84)	(0.24)	
Difference-in-differences estimator, actual hours	.017	.013	NA
(t-ratio on coefficient)	(0.51)	(0.43)	
Wage gap estimator, usual hours, elasticity	-.015	.007	NA
(t-ratio on coefficient)	(0.25)	(0.11)	
Wage gap estimator, actual hours, elasticity	-.050	.080	NA
(t-ratio on coefficient)	(0.64)	(1.46)	

Table 10

Employment growth rates 1995-97 for the industries of individuals in difference-in-differences "treatment" and "comparison" groups

LFS data matched to AES (re-scaled) employment data

Employment growth, 1995-97 (%)	"Treatment" group	"Comparison" group
Adults		
Median	4.5	4.5
Mean	7.1	9.2
Lower quartile	-1.5	-1.3
Upper quartile	10.5	10.1
t-test of equal means	1.24 (p = 0.21)	
Youths		
Median	4.5	4.5
Mean	9.0	15.7
Lower quartile	-2.6	-1.9
Upper quartile	11.4	11.0
t-test of equal means	0.62 (p = 0.54)	

growthtestv2.log

Table 11

Estimates of effect of the minimum wage introduction in high- and low-growth sectors

Matched LFS data

	High growth sectors		Low growth sectors	
	Adult men	Adult women	Adult men	Adult women
Difference-in-differences estimate:				
Wage based on usual hours				
Probability unit effect	.007	-.029	.022	.006
(t-ratio on coefficient)	(0.33)	(1.78)	(0.93)	(0.36)
Implied elasticity	.006	-.021	.021	.004
Wage based on actual hours				
Probability unit effect	.006	-.016	.012	.025
(t-ratio on coefficient)	(0.33)	(0.98)	(0.51)	(1.59)
Implied elasticity	.006	-.012	.011	.018
Wage gap below minimum, dummies above:				
Wage based on usual hours				
Probability unit effect	.003	-.013	.011	.009
(t-ratio on coefficient)	(0.30)	(1.26)	(0.79)	(0.95)
Implied elasticity	.003	-.010	.010	.007
Wage based on actual hours				
Probability unit effect	.003	-.007	.018	.033
(t-ratio on coefficient)	(0.25)	(0.65)	(0.95)	(2.62)
Implied elasticity	.002	-.005	.018	.024
Wage gap below minimum, linear spline above:				
Wage based on usual hours				
Probability unit effect	.001	-.019	-.001	.020
(t-ratio on coefficient)	(0.13)	(1.23)	(0.07)	(1.29)
Implied elasticity	.001	-.014	-.001	.015
Wage based on actual hours				
Probability unit effect	.014	-.006	.028	.040
(t-ratio on coefficient)	(1.25)	(0.36)	(0.83)	(1.98)
Implied elasticity	.013	-.005	.027	.029

grow_int.log, mgrow_int.log

Table 12

New Earnings Survey sample sizes at different stages of the sample selection procedure

Old (NESPD-99) and new (NESPD-2001) data files

	NESPD-99				NESPD-2001			
year	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
1994	162,641	153,502	123,566	102,683	162,014	152,890	123,102	102,361
1995	160,074	151,072	133,260	108,202	159,780	150,784	132,905	108,034
1996	160,351	151,046	131,988	107,462	160,540	151,199	131,979	107,596
1997	151,817	143,103	126,881	102,531	151,644	142,916	126,694	102,424
1998	155,073	145,784	129,134	104,200	158,571	149,074	131,261	106,465
1999	157,468	147,837	129,967	105,026	158,529	148,805	131,215	106,024
2000					153,120	143,862	127,290	102,101
2001					155,337	145,660	131,453	105,027

(1) The full sample.
(2) Restricted to those aged 18 & over, but strictly less than 60.
(3) Excluding those with double jobs indicated, those with loss of pay indicated, those who worked less than one hour in the reference week, those for whom average hourly earnings excluding overtime (the variable hexo) is 0, missing, or does not match its components, and those with real basic wage (deflated hexo) < 50p/hour.
(4) Excluding part-timers.

Table 13

New Earnings Survey estimates of the impact of the introduction of the minimum wage

Old (NESPD-99) and new (NESPD-2001) data files

Logit model estimates with control variables

	Adult men	Young men	Adult women	Young women
Difference-in-differences estimates				
Old data file (NESPD-99)	-.008	-.036	-.013	.052
	(0.45)	(0.57)	(0.84)	(1.36)
New data file (NESPD-2001)	-.015	-.029	-.020	.034
	(0.91)	(0.50)	(1.29)	(0.56)
Wage gap below minimum, spline above				
New data file (NESPD-99)				
Probability unit effect	-.029	-.068	-.008	.047
(t-ratio on coefficient)	(1.32)	(1.08)	(0.39)	(0.78)
Implied elasticity	-.023	-.062	-.007	.040
New data file (NESPD-2001)				
Probability unit effect	-.038	-.073	-.019	.061
(t-ratio on coefficient)	(1.74)	(1.17)	(0.91)	(1.05)
Implied elasticity	-.029	-.066	-.016	.053

Nalt_01.log, baserun1.log, intro_spl.log, intro_spl_old.log

Table 14

New Earnings Survey estimates of the impact of the 2000 uprating

Logit model estimates with control variables

	Adult men	Young men	Adult women	Young women
Difference-in-differences estimates				
2000 uprating vs. pre-intro years	.014	.033	.021	.049
	(0.64)	(0.50)	(1.05)	(0.66)
2000 uprating vs. flat year in between	.071	-.027	.014	.092
	(2.28)	(0.28)	(0.49)	(0.86)
Wage gap below minimum, spline above				
2000 uprating vs. pre-intro years				
Probability unit effect	-.102	-.179	.001	.022
(t-ratio on coefficient)	(1.86)	(2.12)	(0.02)	(0.29)
Implied elasticity	-.080	-.165	.001	.019
2000 uprating vs. flat year in between				
Probability unit effect	.020	-.183	.092	-.041
(t-ratio on coefficient)	(0.31)	(1.32)	(1.27)	(0.33)
Implied elasticity	.014	-.182	.078	-.036

uprat_spla.log, uprat_splb.log

Note:
For the 2000 updating comparisons, real wages are scaled to Oct 2000 for adults and to June 2000 for youths.

Table 15

"Growth adjusted" NES estimates of the impact of the 2000 uprating

Logit model estimates with control variables

	Adult men	Young men	Adult women	Young women
Difference-in-differences estimates	.025	.049	.003	-.077
	(1.00)	(0.75)	(0.11)	(1.07)
Wage gap estimates	-.019	-.119	-.066	-.016
	(0.34)	(1.08)	(1.01)	(0.15)

Table 16

New Earnings Survey estimates of the impact of the introduction of the minimum wage in catering

Logit model estimates with control variables

	Adult men	Adult women
Hotel and restaurants (industry definition)		
Difference-in-differences estimates	.163	.017
	(2.18)	(0.26)
Wage gap below minimum, spline above		
Probability unit effect	.065	-.022
(t-ratio on coefficient)	(0.95)	(0.35)
Implied elasticity	.053	-.019
Bar, waiting staff, etc. (occupation definition)		
Difference-in-differences estimates	.145	-.109
	(1.57)	(1.75)
Wage gap below minimum, spline above		
Probability unit effect	.034	-.047
(t-ratio on coefficient)	(0.35)	(0.72)
Implied elasticity	.029	-.043

sector_1.log, sector_2.log

Sample sizes:
Hotel and restaurants (industry definition): 5,537 men, 5,141 women.
Bar, waiting staff, etc. (occupation definition): 2,179 men, 3,599 women.

Table 17

New Earnings Survey estimates of the impact of the introduction of the minimum wage in retail sector

Logit model estimates with control variables

	Adult men	Adult women
Retail trade (industry definition)		
Difference-in-differences estimates	-.064	-.051
	(1.04)	(1.22)
Wage gap below minimum, spline above		
Probability unit effect	.054	.092
(t-ratio on coefficient)	(0.91)	(1.97)
Implied elasticity	.046	.081
Sales assistants & checkout staff (occupation definition)		
Difference-in-differences estimates	-.009	-.026
	(0.11)	(0.59)
Wage gap below minimum, spline above		
Probability unit effect	.120	.041
(t-ratio on coefficient)	(1.29)	(0.85)
Implied elasticity	.102	.036

sector_3.log, sector_4.log

Sample sizes:
Retail trade (industry definition): 16,640 men, 14,892 women.
Sales assistants & checkout staff (occupation definition): 4,037 men, 7,234 women.

Table 18

New Earnings Survey estimates of the impact of the introduction of the minimum wage for cleaners

Logit model estimates with control variables

	Adult men	Adult women
Cleaners (occupation definition)		
Difference-in-differences estimates	-.043	.117
	(0.46)	(1.68)
Wage gap below minimum, spline above		
Probability unit effect	-.011	-.007
(t-ratio on coefficient)	(1.07)	(0.14)
Implied elasticity	-.009	-.006

sector_5.log

Sample sizes: 2,315 men, 3,052 women.

Table 19

New Earnings Survey estimates of the impact of the introduction of the minimum wage for child care and care assistants

Logit model estimates with control variables

	Adult men	Adult women
Child care & care assistants (occupation definition)		
Difference-in-differences estimates	NA	-.025
		(0.42)
Wage gap below minimum, spline above		
Probability unit effect	NA	-.046
(t-ratio on coefficient)		(0.84)
Implied elasticity		-.041

sector_6.log

Sample size: 4,415 women.