The other margin: do minimum wages cause working hours adjustments for low-wage workers?*

Mark B. Stewart
University of Warwick

&

Joanna K. Swaffield
University of York

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Abstract

This paper estimates the impact of the introduction of the UK minimum wage on the working hours of low-wage employees using difference-in-differences estimators, with and without propensity score matching. The estimates using the employer-based New Earnings Surveys indicate that the introduction of the minimum wage reduced the basic hours of low-wage workers by between 1 and 2 hours per week. The effects on total paid hours are similar (indicating negligible effects on paid overtime) and lagged effects dominate the much smaller and less significant initial effects within this. Estimates using the employee-based Labour Force Surveys are typically less significant.

Keywords: Minimum wages, working hours, labour demand, difference-in-differences estimator, propensity score matching.

JEL classifications: J23, J38

1. Introduction

The effects of minimum wages have long been debated and are the subject of fierce disagreement. Most heavily investigated has been the impact on employment, testing the hypothesis that a minimum wage reduces the demand for low-wage workers. Labour demand adjustments may also take place at the hours margin and there is rather less evidence on this impact. Potential hours adjustments are the focus of this paper.

In April 1999 the UK, following a change of government, and based on the recommendations of a report from the independent Low Pay Commission (LPC, 1998), introduced a National Minimum Wage. This followed a period without wage floors, following the abolition in 1993 of the Wages Councils that had covered certain vulnerable sectors. The minimum was introduced at an adult rate of £3.60 for all employees aged 22 and over and a youth rate of £3.00 for those aged 18 to 21. Compliance has been high. The available evidence suggests an almost complete truncation of the wage distribution at £3.60 for adult workers after April 1999 (LPC, 2001, Stewart and Swaffield, 2002, Dickens and Manning, 2004b) and little in the way of wage spillovers (Dickens and Manning, 2004a).

Existing empirical work on the impact of the introduction of the minimum wage suggests that while it may have had an adverse effect on employment in particular sectors (Machin et al, 2003, for care home workers), the overall effect on employment has been broadly neutral (Stewart 2002, 2004a, 2004b). However potential labour demand shifts caused by the minimum wage introduction may also manifest themselves as a change in employees' hours of work. Firms may reduce employment at either or both of the extensive and intensive margins: the number of workers and/or the number of hours per worker. A full analysis of any labour demand shift resulting from the minimum wage requires an analysis of both these potential responses.

The Low Pay Commission found in the evidence that it received that "a frequently reported response to the minimum wage was a unilateral reduction of workers' hours by the employer" (LPC, 2000, page 105). Similarly the Low Pay Unit observed that "reducing hours appears to be the most common tactic adopted by employers wishing to avoid paying the minimum wage" (New Review, Low Pay Unit, 1999). Although research in this area is relatively limited, the time-series literature suggests "hours per week fall when minimum wages increase, so the effect on hours worked is more pronounced that the effect on bodies employed" (Brown, 1999, page 2156).

The potential impact on hours is also important to the wider minimum wage debate. Michl (2000) suggests that one possible explanation for the differences between the results of Card and Krueger (1994, 2000) on the one hand and Neumark and Wascher (2000) on the other in their analyses of the impact of the New Jersey 1992 minimum wage increase on the fast-food industry may lie in their different treatment of hours worked per employee. Neumark and Wascher use total payroll hours as their dependent variable, and so would capture any hours adjustments. Card and Krueger use the number of workers as their dependent variable and so would not. Although the debate between the two sides focused largely on the quality of the data used, Card and Krueger (2000) note in their conclusions, despite their reservations about the data used by Neumark and Wascher, that "an alternative interpretation … is that the New Jersey minimum-wage increase did not reduce total employment, but it did slightly reduce the average number of hours worked per employee" (page 1419).

Other US evidence points to the importance of hours in considering the full impact of minimum wages on low-wage employees: Linneman (1982), Neumark et al (2001). Whether motivated primarily by a focus on labour demand shifts or on overall income effects for low-wage employees, there is a clear need to evaluate the impact of the minimum wage introduction on the working hours of low-wage employees.

This paper tests whether the April 1999 introduction of the UK minimum wage had a significant impact on the working hours of low-wage employees, using individual-level longitudinal data from two national datasets: the New Earnings Survey (NES) and the Labour Force Survey (LFS). The paper uses difference-in-differences estimators, with and without propensity score matching, to estimate a model of an individual employee's change in paid working hours (within the same job) as a function of the individual's initial position in the wage distribution.

The next section outlines arguments for why we might expect hours adjustments for low-wage workers directly affected by the introduction of the minimum wage. Section 3 details the estimation strategy used in the paper. Section 4 provides a brief description of the two datasets and the construction of the key variables required for the analysis. Section 5 presents basic difference-in-differences estimation results and also difference-in-differences estimates based on propensity score matching. It also tests the fundamental identifying assumption underlying the difference-in-differences estimator. Section 6 presents a summary of the main findings.

2. Theoretical predictions on the effect on working hours

In the standard textbook analysis of the impact of a minimum wage, it is seen as raising the wage above its market clearing level, leading to a reduction in the demand for labour. This is usually interpreted as a reduction in employment. However, the adjustment, as well as possibly taking place at the *extensive margin*, i.e. a reduction in the number of workers, can also take place at the *intensive margin*, i.e. a reduction in the number of (paid) hours per worker. In the long run a firm's choice of workers-hours mix depends on the extent of fixed costs of employment, the technology and productivity-hours schedule, the labour supply schedule faced, etc. However in the short run, as Hamermesh (1993) observes, "employers are quicker to alter hours in response to shocks than they are to change levels of employment" (page 294).

If effort per hour worked can also be varied, workers may be made to work harder per hour to raise productivity in line with the wage, thereby reducing the overall hours required for a particular task. For current purposes, whether this increased productivity and reduction in hours arises through higher productivity levels during paid hours of work or employers requiring workers to complete designated tasks in their own (unpaid) time if not completed during paid hours is less important than the prediction of working hours reduction - a prediction in line with the competitive model when the market wage rate is raised above the equilibrium level.

However the prediction for hours worked is more complex, even in the context of the competitive model. It is a well-established stylized fact that full-time employees are paid more than otherwise similar part-time employees, which suggests that full-timers produce more per hour. If this were the case, firms would be expected to lengthen workweeks in response to a minimum wage increase rather than reduce them (Brown, 1999, page 2117). Central to the substitutability or separability of workers and hours in the firm's labour demand function are issues such as the fixed and variable costs associated with each worker, full-time/part-time productivity differences, the presence and effectiveness of a union at the workplace, and the ease with which firms can lay off workers.

If there are high fixed costs per worker and/or full-time workers are more productive than part-time workers, we could expect, as above, the number of minimum wage workers to decline within a firm after the introduction of the minimum wage and the hours worked per minimum wage employee to increase. If the firm, or industry, is highly unionized and demand relatively inelastic, we might expect collectively bargained employment and hours to remain relatively constant and instead the wage bill increase to be passed on to the consumer or absorbed within their non-zero profit margin. If the second derivative of productivity (or effort) per hour with respect to total working hours is negative this could well suggest working hours reductions per worker rather than reductions in numbers of workers when the wage rate rises, particularly so if there is limited capital usage within the firm and limited labour-capital substitution within the production function.

A number of stylized facts concerning minimum-wage workers in the UK are also relevant. These workers tend to be relatively low skilled prior to employment and receive relatively limited training whilst in post. They also tend to have higher than average turnover rates. Both these points suggest relatively low fixed costs associated with the employment of this type of worker. Minimum wage employment also tends to be within the service sector, where the intensity of capital usage and potential substitutability of capital for labour is relatively low and the incidence of part-time employment is high. In addition, minimum wage workers tend to be from groups (for example based on race or gender) that are disadvantaged in the labour market and have low levels of union representation. Finally, UK employment law may make labour demand adjustments at the intensive margin, at least in the short to medium term, easier than at the extensive margin.

Thus even within the narrow confines of the textbook labour demand curve model, the theoretical prediction of the impact of the introduction of a minimum wage on working hours is ambiguous. This ambiguity is deepened in a dynamic monopsony model, where the imposition of a minimum wage can raise total employment input for some firms (Dickens et al., 1999, Manning, 2003). Manning (2003) observes that "the impact of minimum wages on employment should primarily be an empirical issue". The same is true for the impact on hours of work.

3. Estimation strategy

This paper estimates the effect of the introduction of the minimum wage on the hours of work of those employees affected using a difference-in-differences approach. The basic difference-in-differences estimator is explained first. The amalgamation of this estimator with propensity score matching is then described and the advantages that this might offer. The central feature is to compare the experience of those individuals affected by the minimum wage with the experience of a similar group who were not directly affected. The counterfactual questions to be addressed are what would have been the change in the working hours of employees directly affected by the minimum wage if the minimum wage had not been introduced and are observed changes in hours for this group significantly different from this? The econometric model estimated is a generalised form of the models in the literature (for example Lineman (1982), Zavodny (2000), Neumark et al (2001)).

The introduction of the minimum wage can be viewed as a "quasi-experiment" and a difference-in-differences estimator is therefore a natural method to estimate its effect. The affected group are those individuals earning below the minimum at the time of its introduction, whose wage would be required to rise to comply with the new minimum.

Ideally we would wish to compare this affected group with itself in an alternative state of the world when the minimum wage had not been introduced, which is of course not possible. The approach used here is to construct a second group of employees who act as the control or comparison group. If these individuals are similar enough to those affected by the minimum wage introduction, the effect of the minimum wage can be estimated by comparing the experiences of these two groups before and after the minimum wage introduction. The importance of the definition of the control group in this approach is clear. This group needs to be constructed to be similar enough to the sub-minimum-wage group directly affected by the minimum wage introduction to generate equivalent behavioural responses, but not be themselves affected by the introduction.

Suppose that the 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 change in the hours of work in the absence of a minimum wage only prior to t* and direct information on the change in hours of work in the presence of a minimum wage only from t* onwards. The objective is to estimate the counterfactual, i.e. what the change in hours of work between t and t+1 in group g would have been if the minimum wage had not been introduced. This is done by making comparisons across g.

Suppose that in the absence of a minimum wage the change in hours can be decomposed into two components, with the first component fixed over time and the second component common across groups. This assumes that in the absence of a minimum wage the difference in the average change in the hours of work between groups would be the same in each time period, or equivalently that the time paths of hours growth are the same for each group. This assumption will be tested in Section 5 below.

Suppose that the minimum wage has a constant effect, θ, on the change in hours of work for those in group g=1 and no effect on those in group g=2. Consider two time periods, the first, starting at t_1, where there was no minimum wage in place at either t or t+1, and a second, starting at t_2, where the minimum wage was introduced between t and t+1. Differencing across the two groups and across these two time periods (starting t_1 and t_2) gives θ. Thus the raw difference-in-differences estimate is given by double differencing these sample means.

Define h_{it} to be the hours of work of individual i at time t. The focus here is on the change in hours. Define $y_{it} = \Delta h_{it} = h_{it+1} - h_{it}$. Then the difference-in-differences estimator is given by:

$$\hat{\theta} = (\overline{y}_{TA} - \overline{y}_{TB}) - (\overline{y}_{CA} - \overline{y}_{CB}) \tag{1}$$

where T denotes treated individuals, C denotes comparison individuals, B denotes before the minimum wage introduction and A denotes after the minimum wage introduction.

The difference-in-differences estimator can also be generated by a linear regression. Under the assumptions above, the change in working hours of individuals in all groups and all time periods can be written as:

$$y_{it} = \alpha_i^g + \gamma_t + \theta D_{it} + \varepsilon_{it} \tag{2}$$

where $D_{it} = 1$ if individual i is affected by the minimum wage, i.e. individual i is in group g = 1 (defined at time period t) and the time period [t, t+1] (over which Δh is constructed) straddles the introduction of the minimum wage i.e. 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 a dummy variable for all time periods with the minimum wage in place. (If there are more than two wage groups additional interaction terms of this type for group g > 2 will also be required.)

This paper defines the groups indexed by g in terms of segments of the real wage distribution at time t. The first group (g = 1) contains those directly affected i.e. those with a real wage below the appropriate (age-specific) minimum. The second group (g = 2) is the "control" or "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.[1]

The simple differences-in-differences estimator can be extended to produce a "regression adjusted" differences-in-differences estimator by adding a vector of individual characteristics thought to affect the change in working hours as:

$$y_{it} = x_{it}'\beta + \alpha_i^g + \gamma_t + \theta D_{it} + \varepsilon_{it} \qquad (3)$$

In adding these control variables the aim is to deal with any differences between the "affected" or "treatment" group (g = 1) and the "comparison" or "control" group (g = 2) not controlled for with the additive group and time effect dummies.

There are two key identifying assumptions underlying this modelling process. The first is that the interaction effects are zero in the absence of the minimum wage. In the case of equation (3) above, this is after controlling for differences in observable characteristics. The issue of concern is that even in the absence of the minimum wage introduction, change in working hours may occur differently in different wage groups. The validity of this assumption is tested below. The second key identifying assumption is that the introduction of the minimum wage does not influence the working hours of employees in the control group (g = 2). Changes may occur to the control group due to wage spillovers or substitutions effects between different groups of workers. The evidence for the UK suggests that this has not been the case.

A key issue for the estimation strategy used here is the construction of the control group. It is constructed from those not directly affected by the introduction of the minimum wage, but needs to be as similar as possible to those who were directly affected. In the basic difference-in-differences estimator used in this paper the control group is defined as all those employees with real wages at time t between the (age and time) appropriate minimum wage and 10% above the minimum. The sensitivity of the estimates to this choice of upper limit is investigated by also widening the control group. There is a trade off here. Widening the control group improves cell sizes, but may lose some of the similarity between the affected and control groups.

One approach to improving the similarity between the affected and control groups is to use propensity score matching. Estimates based on a propensity score matching difference-in-differences estimator are also presented below. The propensity score matching improves the similarity between the affected and control groups by selecting, or giving greater weight to, observations that are a better "match" for affected individuals. It seeks to make the comparison group for the difference-in-differences estimation as similar as possible to the treatment group in terms of observed characteristics.

This soon runs up against a dimensionality problem. Even in a reasonably large sample it will be difficult to match each individual with a pre-introduction wage below the minimum with an individual paid above the threshold, but with exactly the same education, experience, tenure, region, sector, marital status, ethnic group, health and other characteristics. Rosenbaum and Rubin (1983) showed that it is sufficient to use the propensity score – the probability of receiving "treatment", i.e. being directly affected by the introduction of the minimum, conditional on covariates – to conduct the matching. This dramatically reduces the dimensionality of the matching problem, since conditioning is now on the basis of a scalar variable. (See Dehejia and Wahba (1999, 2002), Heckman et al. (1997, 1998) and Smith and Todd (2003) for discussion of the approach.) The combination of difference-in-differences estimation with propensity score matching is important. Standard matching methods control only for selection on observables. Difference-in-differences matching can be viewed as an attempt to also control for any selection on unobservables, under certain assumptions about this selection.

[1] The estimator is easily extended to include additional groups above the comparison group, but this makes little difference in practice.

A number of methods can be used to match "treated" individuals, i.e. those directly affected by the introduction of the minimum, with suitable comparisons using the propensity score. One-to-one (nearest neighbour) matching or simple smoothed matching estimators (such as radius or n-nearest neighbour matching) are simple to use but have been found to be sensitive to definition. The method used in this paper is a weighted smoothed matching estimator based on kernel density estimation, which is more efficient and less sensitive to the definition of a "close" match than the other methods referred to above (Heckman et al. (1997, 1998)). The propensity score matching can be undertaken either within a panel or using repeated cross-sections.

For the basic difference-in-differences estimator defined in (3), the individuals within the treatment (comparison) groups before and after the minimum wage introduction need not be the same individuals. For the panel matching estimator, the first step matches the treated individuals before and after - and also the comparison individuals before and after. Compared to equation (1), the sample of individuals in the "treatment before" (TB) group are restricted to being the same individuals as those in the "treatment after" (TA) group, and equivalently for the comparison groups. The second step uses propensity score matching based on the kernel-smoothed matching estimator.[2] The estimator is therefore given by:

$$\hat{\theta}_{PM} = \frac{1}{N_T} \sum_{i \in T} [(y_{iTA} - y_{iTB}) - \sum_{j \in C} K_{ij} (y_{jCA} - y_{jCB})] \tag{4}$$

where K_{ij} is the kernel weight on individual j when compared to individual i and N_T is the total number of treated individuals. This panel matching estimator needs at least three observed periods for each individual. Note also that the use of the panel matched estimator results in a further selection restriction on the sample, which is restricted to individuals in employment with the same employer at three consecutive time points.

[2] See Blundell and Costa Dias (2000) for a detailed discussion of propensity score matching within panel and repeated cross-sections contexts.

The propensity score matching estimator for the repeated cross-section case requires a slightly difference approach. Here the propensity score matching is undertaken three times, once between the treated after and treated before group, once between the treated after and comparison after group and once between the treated after and comparison before group. The estimator is given by:

$$\hat{\theta}_{RCM} = \frac{1}{N_{TA}} \sum_{i \in TA} [(y_{iTA} - \sum_{j \in TB} K_{ij}^{TB} y_{jTB}) - (\sum_{j \in CA} K_{ij}^{CA} y_{jCA} - \sum_{j \in CB} K_{ij}^{CB} y_{jCB})] \qquad (5)$$

where K_{ij}^{TB} is the kernel weight of individual j in the "treatment before" group, K_{ij}^{CA} is the kernel weight of individual j in the "comparison after" group and K_{ij}^{CB} is the kernel weight of individual j in the "comparison before" group, where in each of the three cases they are compared to individual i in the "treatment after" group. Estimates based on both panel matching and repeated cross-section matching are presented in Section 5 below.

4. Data

The two data sets used in this paper are the New Earnings Survey (NES) and the Labour Force Survey (LFS). Data from the NES are utilised from April 1994 (after the abolition of the Wages Councils) up to April 2000. Data from the LFS are used from 1997 quarter 1 up to quarter 3 2000. The LFS can only be used from 1997 quarter 1 onwards, when earnings questions were added to the wave 1 questionnaire. Prior to this earnings questions were only asked of the outgoing, wave 5, respondents. The LFS provides 15 quarterly waves of data, and therefore 11 matched (t to t+1) time periods a year apart. The NES provides 7 annual waves of data, and therefore 6 matched (t to t+1) time periods. The paper presents estimates for adult employees, defined as being aged between 22 and 59 (inclusive) at time t. An important sample restriction placed on both data sets it that employees are with the same employer (or in the same job) at times t and t+1.

The NES and LFS, both important national data sets, have differing strengthens and weaknesses. The advantages of the NES over the LFS are first, the likelihood of greater accuracy in reported hours and wage rates due to employers reporting this information (most often) directly from payroll records, and second, the very large sample size, which allows, in some cases, the only reasonable cell sizes for the different effects to be analysed.

The NES also has a major drawback, which is particularly pertinent when investigating the impact of the minimum wage on low-wage employees. Most employees earning below the PAYE deduction threshold are excluded from the NES sample. This is important as many of those who are low paid are female and the rate of part-time working is greater for women. An employee earning below the minimum at time t, who has their wage increased at time t+1 to comply with the minimum wage, could potentially fall out of the sample if working hours were reduced to the extent that weekly earnings were then less than the PAYE deduction threshold.

An advantage of the LFS is that there is not the same potential for under-sampling of those on low-wages as there is with the NES. The LFS however has a different problem, with some responses provided by proxy rather than by the employee in question. This increases the possibility of measurement error. Although also an issue with direct measures of wages and hours, this is potentially greater when the wage and hour questions are not answered directly by the employee.

For both the NES and LFS data "initial" and "lagged" effects of the introduction of the minimum wage are estimated. The initial effects are defined over time periods where the minimum wage is introduced at some point strictly after t but before (or at) t+1. The lagged effects are defined over time periods where the minimum wage is introduced at (or before) t and therefore by definition in place at t+1. The argument for estimating these lagged effects is that if employers are constrained in their labour demand responses to the introduction of the minimum wage, either through contractual obligations or the slower implementation of the minimum wage rates, labour demand responses may not be observed immediately. Instead a lagged effect of the minimum wage introduction on the change in working hours might be expected

For the NES, the initial effect is defined in terms of the difference in working hours between April 1998 (t) when no minimum wage was in place and April 1999 (t+1) when the minimum wage was in place and compares this with differences entirely pre-NMW. NES lagged effects compare differences over April 1999 (t) to April 2000 (t+1) with the same pre-NMW periods.

For the LFS data the initial effect is defined for working hour differences between t and t+1 that lie within the March 1998 to May 2000 timeframe and compares the differences with the pre-NMW differences. In comparison the LFS lagged effects compare the differences defined over April 1999 (so the minimum wage is in place at all time periods t) to September 2000 with the same pre-NMW periods. For both the LFS and NES data sets the t+1 time periods used in the lagged effects are prior to the first adult uprating in October 2000.

Two hours measures are analysed in both datasets: basic paid working hours and total paid working hours. Basic working hours are defined as the hours worked by employees as part of their standard employment contract before overtime. In comparison total paid working hours is the sum of the basic working hours and paid overtime hours. (Unpaid overtime hours are excluded.) The distinction between these two variables is potentially interesting as employers may well be more constrained in adjusting basic working hours in the short-term (which may well be contractually set) than overtime hours. The NES definition of paid basic working hours is basic hours for the employee in a normal week, excluding meal breaks and overtime. For the LFS total paid working hours are defined as total usual hours including paid overtime and excluding any unpaid overtime. Basic hours are total usual hours that are worked in the main job excluding all overtime.

For the NES the gross hourly wage variable is a “basic hourly wage rate”. It is constructed as gross weekly earnings excluding overtime, divided by normal basic hours. It is restricted to employees whose pay for the survey period was not affected by absence. For the LFS the gross hourly wage variable is an “average hourly wage rate”. It is constructed as gross pay in the most recent pay period, converted to a weekly basis and then divided by usual hours per week. For both data sets the gross hourly wage rate is deflated by the all-items RPI.

From the spring quarter of 1999 a new question was added to the LFS questionnaire that asked directly about the hourly rate that respondents are paid (but only for those who are hourly paid). Due to the timing of the introduction of this additional question, the information cannot be used to investigate the "initial" impact of the introduction of minimum wage. Its use to measure "lagged" effects is limited by the fact that it is collected only for hourly paid employees. Two approaches to this would be possible: either restricting the analysis to those who were hourly paid, which would seriously reduce already small cell sizes, or using a method of imputation for those employees not paid by the hour. This second approach would be problematic in conjunction with the estimator used. In addition the method of imputation is the subject of debate (see Stuttard and Jenkins (2001, 2002) and Dickens and Manning (2004b)). This approach has not been adopted.

5. Results

5.1 Evidence on the fundamental identifying assumption

The fundamental identifying assumption underlying the difference-in-differences approach is that there are no significant interactions in the pre-minimum wage period between the wage group dummies and the time effects. To formally examine whether this assumption holds for the data used in this paper we follow the procedure in Stewart (2004a) by restricting attention to the pre-minimum wage period and testing the significance of interactions between the wage group and time dummies for the period when no minimum was in place.

For the pre-minimum wage period in the LFS data the overall test statistic p-values with and without control variables for total paid hours were 0.25 and 0.39 for the male sample and 0.44 and 0.59 for the female sample. In each case the tests provides evidence that the underlying assumption of no interactions is well supported by the data. Test statistic p-values for basic hours are even larger and provide even stronger evidence in favour of the underlying assumption of no interactions in the absence of the minimum wage.

For the male NES pre-minimum wage period the situation is less supportive of the underlying assumption. When all available waves (1994-95, 1995-96, 1996-97 and 1997-98) are included in the pre-period the test statistics reject the hypothesis of no significant interactions for total and basic working hours. The p-values based on total (basic) paid hours were 0.002 (0.05) both with and without controls. The significant interaction driving this result is for the first period, 1994-95. If this time period is excluded from the pre-minimum wage period, the hypothesis of no significant interactions is strongly supported. As a result throughout the paper the NES data period 1994-95 was excluded from the pre-minimum wage period for men.

For the NES female pre-minimum wage samples the results are more supportive of the assumption. For total paid hours, test statistic p-values of 0.43 (0.23) without (with) controls provide strong evidence of the underlying assumption being supported by the data. For basic paid hours there is some evidence of a marginally significant interaction for the NES 1997-98 time period. However to allow equivalent comparisons between total and basic paid hours in the reported estimates the NES 1997-98 data was not excluded from the female pre-minimum wage period. If this wave is excluded, the baseline estimates presented below change little.[3]

5.2 Basic difference-in-differences estimates

Basic difference-in-differences estimates of initial and lagged effects, for the NES for adult male and female employees are given in Table 1.[4] For men, the raw difference-

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[3] Note that the NES 1994-95 data was only excluded from the male samples. Although this does reduce potential comparability between the male and female NES samples it was felt that this was outweighed by the greater efficiency of keeping the 1994-95 data in the female NES pre-minimum wage sample.

[4] For the NES the control vector includes year dummies, regional dummies, age (and square), hourly earnings (linear, square and cube) and part-time control. For the LFS the year and month dummies, regional dummies, hourly earnings (linear, square and cube), dummies for part-time, married, ethnic group, public sector, permanent, health and highest educational qualifications, labour market experience (linear, square and cube), length of tenure with current employer (and square) and

in-differences estimate of the initial effect is insignificant for both the change in total paid hours and the change in basic hours. In comparison the lagged effect, capturing the impact after the initial period, is negative and significant for both hours measures.

The estimates imply a lagged effect of a reduction in total paid hours of about 1.8 hours per week as a result of the introduction of the minimum wage and a total effect (initial plus lagged) of 1.5 hours. Almost all of this effect comes through a reduction in basic hours. There is a negligible effect on paid overtime hours. The inclusion of a control vector of explanatory variables has relatively little effect on the estimates. Both initial effects remain insignificant. The lagged effects decline slightly and become slightly less significant.

As discussed above the construction of the comparison group is potentially crucial to the difference-in-differences estimator. In the third row of Table 1 the estimates are presented for a comparison group constructed to include those up to 15% above the April 1999 minimum (with a control vector included). The widening of the control group reduces the magnitude and significance of the lagged effect further. The combined effect (initial + lagged) however changes relatively little.

In the final row of the top panel of Table 1 the sensitivity of the difference-in-differences estimate to the choice of real wage deflator is shown. The real wage deflator used throughout the previous rows of the table is the Office for National Statistics all-items RPI, while in the final row the RPI excluding mortgage payments is used. The results are fairly robust to this change, although indicating a slight fall in overall magnitude and significance for the lagged effects.

The bottom panel of Table 1 gives the corresponding estimates for women. All are negative. For the initial effects the estimates for total paid hours are all significantly negative, while those for basic hours are all insignificant. The lagged effects are negative and significant for both total and basic hours. For women the overall (initial + lagged) effect on total paid hours is fairly similar to that for men at about 2 hours.

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educational leaving age. The real wage cubic is included in each control vector to try and deal with the potential correlation between wages and hours in the absence of the minimum wage.

Overall the female NES estimates appear relatively robust to the inclusion of the control vector, widening of the comparison group and an alternative real wage deflator.

The results based on the LFS are given in Table 2. The estimated initial effect is insignificant for both total and basic hours and for both men and women. This finding is robust to the inclusion of a control vector, widening of the control group and the choice of an alternative real wage deflator.

The lagged effect estimates for total paid hours are insignificant for both men and women, as are those on basic hours for women. The lagged effect on basic hours for men, however, is significantly negative and relatively large in magnitude, indicating a change in basic hours for the affected group of 2.5 hours less than that of the comparison group, after the minimum wage introduction. The significance of this estimate is similar to that of the estimates in Table 1 for men on the NES, although the magnitude of the LFS estimate is slightly larger. The difference between the estimates for basic and total hours suggests that changes in the former may have been offset by changes in overtime hours in this case.

In the final two rows of each panel of Table 2 the sensitivity of the LFS estimates to two further issues are shown. As mentioned above the LFS suffers from a potential problem of additional measurement error caused by proxy responses. In the second to last row of each panel the observations with a proxy response at either t or t+1 are excluded from the sample. This obviously affects cell sizes and this has to be borne in mind when any conclusions are drawn. The findings are however not greatly affected. For men both lagged effects become more negative, but the standard errors also rise markedly.

In the final row of each block of Table 2 the sensitivity of the estimates to the construction of the hourly wage rate used to construct groups based on the real wage at time t are shown. In the final rows, instead of dividing usual weekly pay by usual weekly hours to construct an average hourly wage, the actual hours of work for a specified week are used. All four effects for women and both initial effects and the lagged effect on total paid hours for men remain insignificant. However now the lagged effect on basic hours for men also becomes insignificant and the magnitude is much reduced. Of all the sensitivity issues investigated for the LFS the one with the greatest impact is the choice of hours measure, usual or actual, that is used to construct the hourly wage.

5.3 An alternative estimator using the "wage gap"

The difference-in-differences estimator uses a binary treatment indicator (for whether the real wage at t was below the minimum wage). This approach restricts the impact of the introduction of the minimum to be the same regardless of whether the individual's wage would have needed to have been raised by a relatively small or large amount to comply with the introduction of the minimum wage. An alternative estimator can be used which relaxes this assumption and instead takes into account the amount by which the individual's wage at t would have had to be raised. This alternative approach uses a "wage gap" variable that is the difference between the wage at time t and the minimum wage in place at time t+1 (Currie and Fallick, 1996). This is equivalent to replacing D_{it} in equation (3) with its product with this new "wage gap" variable.

In Table 3 the wage gap estimates are presented in the "wage gap: dummies above" row. The rows below go one step further by modelling the range of the wage distribution above £3.60 with linear spline terms as well as that below £3.60. Both rows provide similar results to each other. Comparisons with the original NES "binary indicator" estimates show the "wage gap" specification to increase the significance of the estimates for both the initial and lagged effects for men, but only for the initial effects for women. For the LFS all "wage gap" estimates were found to be insignificant. This included the previously significant lagged impact on basic hours for men.

To compare the magnitudes of the "binary indicator" and "wage gap" estimates it is most informative to evaluate the "wage gap" impact at the average distance of the wage at time period t from the minimum wage rate at t+1. For men in the NES the average lagged impact of the minimum wage is estimated at approximately -1.6 (the product of the average wage gap for wage group 1 (£0.54) and the coefficient estimate (-3.02)) for total paid hours and -1.5 for basic hours. For women in the NES the initial effect was -0.69 for total paid hours and -0.56 for basic hours. In both these cases the magnitude of the average effect based on the "wage gap" specification was similar to the original "binary treatment" estimate. In contrast, the male initial "wage gap" effects are oppositely signed to the original effects and the female lagged average "wage gap" estimates are less than half the "binary treatment" estimated effects.[5]

While it has certain advantages as stated above, a problem with the "wage gap" estimator is that it is adversely affected by unduly low values of the wage variable. Experimentation, not reported here, suggests that the "wage gap" estimator is, as a result, more vulnerable to measurement error in the wage than the "binary indicator" estimator.

5.4 Measurement error in hours

As with all survey data, hours variables are likely to suffer from a degree of measurement error. How this potential measurement error affects the estimates here depends on the nature of the measurement error, for example whether it satisfies the assumptions of the classical model or not. Unfortunately there are no formal validation studies, for example comparing the responses of employees with those of their employers, for the hours variables on either the NES or LFS. However, evidence from the US on measurement error in reported hours data may provide some general pointers. Bound et al (2001) suggest that individuals tend to over-report hours of work (men more than women), that changes in working hours constructed from the collection of working hours at two separate and consecutive panel points provides a better estimate of the change than collecting retrospective data and current data at one particular time period, and that there seems to be some social desirability to over

[5] The average value of the "wage gap" variable for the NES initial (lagged) estimates was £0.62 (£0.54) for male employees and £0.45 (£0.42) for female employees.

reporting hours.[6] Mellow and Sider (1983) find that professional and managerial workers report more hours worked than their employers. In addition they find that more educated and non-white workers are more prone to report more hours than their employers, while women report comparatively fewer hours. Duncan and Hill (1985) found some evidence that changes in working hours were overstated in interviews compared to company records.

The concern with regard to this paper is that observed changes in working hours might in some cases be due to measurement error. A number of methods have been employed to attempt to reduce any potential measurement error bias within the presented estimates. Of course we do not know which observations are measured with error. The approach used is to remove those that have a higher probability of measurement error and see how the estimates are affected.

Tables 4 and 5 present difference-in-differences estimates for samples where observations with a higher potential for measurement error are excluded from or truncated within the sample. These exclusions or truncations are based on the reported changes in hours between t and t+1 and/or the underlying levels at t and t+1. Results for both the NES and LFS samples appear relatively robust to whether these restrictions/truncations are imposed on the changes in hours or the underlying levels. Similarly robust results were also found when we excluded observations based on the magnitude of the absolute residuals instead. Encouragingly, it would appear that the estimates presented in this paper, and the implied impact of the introduction of the minimum wage on the working hours of low-wage employees, are relatively robust to excluding observations that are potentially more likely to suffer from measurement error or are outliers.

[6] Although this might be less of a problem for the low-paid than for the professional/managerial groups.

5.5. Difference-in-differences estimation with propensity score matching

Difference-in-differences estimates using propensity score panel matching for the NES panel are presented in Table 6.[7] A probit model is used for the probability of "treatment" to produce the propensity score.[8] An Epanechnicov kernel, with bandwidth 0.06, is used for the kernel weights. The first panel of the table shows the male estimates, the second the female estimates. In the top panel the first three rows show the basic difference-in-differences estimates (without propensity score matching) using three definitions of the comparison group, minimum wage plus 10%, 20% and 30%. The widening of the comparison group allows there to be at least as many observations in the control groups as there were in the treated groups.

The estimates in the first three rows show clearly insignificant initial effects of the minimum wage, but some evidence of a significant negative lagged effect. The magnitude of the effect is diluted as the control group is widened. In the next two rows estimates using panel matching show a reduction in the magnitude and significance of the lagged effect. (This result is reinforced when additional propensity score matching is added to the panel matched treatment and comparison groups.) The initial effects are now uniformly negative, with increased significance, although in most cases still insignificant at conventional levels. It is also noteworthy that the total effects (initial + lagged) are similar in magnitude to the original difference-in-differences estimates.

For female employees in the NES the significant negative lagged effect on both total and basic hours remains with the panel matching and also with the additional propensity score matches. The previously insignificant initial effect on total paid

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[7] The pre-NMW period is restricted to April 1997-98 so the restriction imposed between t and t+1 that the individual is with the same employer is also imposed for the t-1 period also i.e. the employee is with the same employer at all three time periods required to construct the panel changes.

[8] The set of observable characteristics used to selected comparison individuals were regional dummies, age (and square) and a part-time control for the NES and regional dummies, dummies for part-time, married, ethnic group, public sector, permanent, health and highest educational qualifications, labour market experience (linear, square and cube), length of tenure with current employer (and square) and educational leaving age for the LFS all defined at time period t. For the NES panel matching with additional propensity score matching the observable characteristics were as defined at the first observed of the three time periods (t-1).

working hours becomes significant when panel matching is used and remains so with additional propensity score matching.

The difference-in-differences estimates with propensity score matching for the NES and LFS repeated cross-section samples are presented in Table 7. As for the panel matching, the significance of the male NES lagged effects are removed when propensity score matching is used with the repeated cross-sections. The female NES lagged effects in Table 7 however do not retain their significance when propensity score matching is used with the repeated cross-sections. Table 7 also presents estimates based on the LFS data. With the LFS, all propensity score matched difference-in-differences estimates – male and female, total and basic hours, initial and lagged – are insignificant.

To give an overview, the propensity score matching estimates of the total effects (initial + lagged) present a fairly consistent picture in terms of the magnitudes of the effects, but rather less so in terms of their significance. The estimated total effect is negative for both basic and total hours, for both men and women, on both datasets and for all the matching estimators examined. Where there are both panel and repeated cross-section propensity score estimates (i.e. for the NES), the estimated total effects (i.e. initial + lagged) are always smaller (in absolute value) for the latter. The NES total effect estimates indicate a reduction of between 1 and 2 hours per week in basic hours for both men and women. The LFS estimated total effects on basic hours are greater for men and lower for women, but in both cases with reduced precision. In all cases the implied total effect on paid overtime hours is smaller than that on basic hours. The sign of the effect is mixed and in most cases the effect is close to zero.

6. Conclusions

This paper examines the impact of the introduction of the minimum wage on the working hours of low-wage workers. The results presented broadly indicate a negative effect on hours, although the evidence is not unanimous and there is some variation both in terms of the magnitude and significance of the estimated effects.

The majority of the effect on total paid hours is found to be through the effect on basic hours. Typically the effects on these two are very similar and the effect on paid overtime hours is minimal and insignificant.

Lagged effects are usually found to dominate the initial effects. On the basis of the NES, the lagged effect on basic hours is estimated to be a reduction of between 1.0 and 1.5 hours per week. The lagged effect on total paid hours is very similar. The initial effect on basic hours is also estimated to be negative, but smaller and insignificant. The LFS results are typically weaker than the corresponding NES ones in terms of significance.

The picture is clearest on the estimates of the total effect (i.e. initial + lagged effects). The estimates are generally found to be negative for both basic and total hours, and for both men and women. The NES total effect estimates indicate a reduction of between 1 and 2 hours per week in basic hours for both men and women, and similar for total paid hours. The LFS estimated total effects on basic hours are greater for men and lower for women, but in both cases with reduced precision. The implied total effect on paid overtime hours is typically close to zero.

Although the results are not found to be completely robust across data sets, specifications and estimators, the evidence presented in this paper suggests strongly that the introduction of the minimum wage led to a reduction in the paid working hours of both male and female low-wage workers.

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Table 1

Difference-in-differences estimates of the effect of the minimum wage introduction on hours: NES

	Total paid hours	Basic paid hours	Total paid hours	Basic paid hours
	Initial effect		*Lagged effect*	
Males				
Raw difference-in-differences	0.326 (0.53)	0.156 (0.30)	-1.792 (2.23)	-1.867 (2.53)
With control vector	0.312 (0.52)	0.120 (0.24)	-1.452 (1.85)	-1.532 (2.13)
Comparison group to MW + 15%	0.186 (0.35)	0.026 (0.06)	-1.306 (1.76)	-1.224 (1.78)
Alternative real wage deflator	0.535 (0.87)	0.278 (0.54)	-1.415 (1.80)	-1.501 (2.09)
Females				
Raw difference-in-differences	-0.615 (2.16)	-0.312 (1.29)	-1.496 (3.54)	-1.368 (3.43)
With control vector	-0.634 (2.28)	-0.328 (1.39)	-1.494 (3.65)	-1.360 (3.55)
Comparison group MW plus 15%	-0.575 (2.23)	-0.225 (1.02)	-1.292 (3.31)	-1.081 (2.92)
Alternative real wage deflator	-0.630 (2.19)	-0.418 (1.72)	-1.483 (3.63)	-1.354 (3.53)

Notes:
1. "Initial effect" refers to the period with t = April 1998-April 1999. "Lagged effect" refers to the period with t = April 1999-April 2000.
2. "Prior to NMW introduction" refers to the pooled period with t = April 1995-96, 1996-97 and 1997-98 for men. That for women also includes April 1994-95.
3. Sample includes adult (22-64) employees with the same employer at t+1 as at t.
4. Sample sizes for men: 1st row: 191,446 for initial effect, 190,863 for lagged effect; other rows: 190,747 for initial effect, 190,174 for lagged effect. Sample sizes for women: 1st row: 188,066 for initial effect, 187,829 for lagged effect; other rows: 187,442 for initial effect, 187,203 for lagged effect
5. Dependent variable is the change in paid working hours (either total or basic).
6. Robust absolute t-ratios are in the parentheses.
7. Control vector defined at time period t includes year dummies, regional dummies, age (and square), hourly earnings (linear, square and cube) and part-time control.
8. The alternative real wage deflator is the all-items RPI excluding mortgage repayments (CHMK), which replaces the baseline deflator which is the all-items RPI (CHAW).

Table 2

Difference-in-differences estimates of the effect of the minimum wage introduction on hours: LFS

	Total paid hours	Basic paid hours	Total paid hours	Basic paid hours
	Initial effect		***Lagged effect***	
Males				
Raw difference-in-differences	-0.966 (0.81)	-1.078 (1.13)	-0.686 (0.46)	-2.545 (2.22)
With control vector	-0.741 (0.62)	-0.891 (0.93)	-0.859 (0.56)	-2.867 (2.44)
Comparison group NMW plus 15%	-0.840 (0.82)	-0.870 (1.00)	-0.887 (0.68)	-2.299 (1.99)
Alternative real wage deflator	-0.691 (0.58)	-0.843 (0.87)	-0.973 (0.64)	-2.989 (2.53)
Proxy responses at either t or t+1 excluded	0.131 (0.07)	-1.280 (0.92)	-2.771 (1.05)	-3.630 (1.95)
Actual weekly hours used in wage construction	0.152 (0.13)	0.449 (0.43)	0.266 (0.20)	-1.135 (0.98)
Females				
Raw difference-in-differences	0.112 (0.26)	0.227 (0.58)	-0.385 (0.70)	-0.384 (0.78)
With control vector	0.028 (0.06)	0.244 (0.63)	-0.416 (0.77)	-0.383 (0.79)
Comparison group NMW plus 15%	-0.149 (0.39)	-0.007 (0.02)	-0.661 (1.34)	-0.786 (1.77)
Alternative real wage deflator	-0.079 (0.19)	0.054 (0.14)	-0.360 (0.66)	-0.381 (0.78)
Proxy responses at either t or t+1 excluded	-0.236 (0.47)	-0.114 (0.25)	-0.202 (0.33)	-0.102 (0.18)
Actual weekly hours used in wage construction	-0.179 (0.39)	0.015 (0.04)	-0.960 (1.59)	-0.843 (1.51)

Notes:
1. "Initial effect" refers to the 2 period panels where t and t+1 lie between March 1998 and May 2000.
2. " Lagged effect" refers to the 2 period panels where t and t+1 lie between April 1999 and September 2000.
3. Pre-minimum period is the pooled 2 period panels where t and t+1 lie between March 1997 and March 1999.
4. Sample includes adult (22-64) male employees with the same employer at t+1 as at t.
5. Sample sizes for 1st row: for men: 21,483 for initial effect, 15,928 for lagged effect: for women: 21,575 for initial effect, 16,150 for lagged effect
6. Dependent variable is the change in paid working hours (either total or basic).
7. Robust absolute t-ratios are in the parentheses.
8. Control vector (defined at time t) includes year and month dummies, regional dummies, hourly earnings (linear, square and cube), dummies for part-time, married, ethnic group, public sector, permanent, health and highest educational qualifications, labour market experience (linear, square and cube), length of tenure with current employer (and square) and educational leaving age.
9. The alternative real wage deflator denotes the ONS data series of all-items RPI excluding mortgage repayments (CHMK) rather than the baseline real wage deflator using the ONS data series of all-items RPI (CHAW).
10. The average hourly wage construction uses actual paid hours in the denominator rather than usual paid hours. Please note that fewer observations are available when actual hours are used to construct the wage variable, predominantly due to those away from their jobs (who are asked for their usual hours but not their actual hours).

Table 3

"Wage gap" estimates of the effect of the minimum wage introduction on hours: NES and LFS

	Total paid hours	Basic paid hours	Total paid hours	Basic paid hours
	Initial effect		***Lagged effect***	
NES: men				
Difference-in-differences	0.312 (0.52)	0.120 (0.24)	-1.452 (1.85)	-1.532 (2.13)
Wage gap: dummies above	-0.992 (1.56)	-0.819 (1.42)	-3.024 (2.81)	-2.764 (2.62)
Spline, three segments	-1.207 (1.71)	-0.884 (1.38)	-3.148 (2.80)	-2.749 (2.50)
NES: women				
Difference-in-differences	-0.634 (2.28)	-0.328 (1.39)	-1.494 (3.65)	-1.360 (3.55)
Wage gap: dummies above	-1.527 (3.47)	-1.250 (3.02)	-1.674 (1.86)	-1.415 (1.57)
Spline, three segments	-1.351 (2.83)	-1.253 (2.78)	-1.921 (2.22)	-1.543 (1.79)
LFS: men				
Difference-in-differences	-0.741 (0.62)	-0.891 (0.93)	-0.859 (0.56)	-2.867 (2.44)
Wage gap: dummies above	0.172 (0.25)	0.121 (0.19)	-0.102 (0.13)	-0.255 (0.38)
Spline, three segments	0.244 (0.32)	0.182 (0.26)	0.036 (0.05)	0.178 (0.25)
LFS: women				
Difference-in-differences	0.028 (0.06)	0.244 (0.63)	-0.416 (0.77)	-0.383 (0.79)
Wage gap: dummies above	-0.564 (1.26)	-0.076 (0.17)	-1.010 (1.21)	-1.083 (1.30)
Spline, three segments	-0.384 (1.41)	-0.145 (0.31)	-0.893 (1.00)	-1.032 (1.15)

Notes:
See Tables 1 and 2.

Table 4
Difference-in-differences estimates with restrictions on working hour changes and levels: NES

	Total paid hours	Basic paid hours	Total paid hours	Basic paid hours
	Initial effect		***Lagged effect***	
Men				
Difference-in-differences	0.312 (0.52)	0.120 (0.24)	-1.452 (1.85)	-1.532 (2.13)
(a) Restrict regression sample to *changes* lying within:				
Δht≥-30 and 30 ≥Δht	0.025 (0.05)	-0.311 (0.72)	-1.522 (2.31)	-1.483 (2.44)
Δht≥-20 and 20 ≥Δht	-0.346 (0.85)	-0.422 (1.22)	-1.077 (2.06)	-0.847 (1.74)
Δht≥-10 and 10 ≥Δht	-0.056 (0.21)	-0.035 (0.18)	-0.031 (0.09)	-0.136 (0.46)
(b) Truncate sample by replacing maximum and minimum changes with				
Min Δht= -30 and Max Δht=30	0.110 (0.20)	-0.030 (0.06)	-1.674 (2.30)	-1.635 (2.43)
Min Δht= -20 and Max Δht=20	-0.007 (0.02)	-0.173 (0.42)	-1.495 (2.40)	-1.423 (2.46)
Min Δht= -10 and Max Δht=10	-0.046 (0.13)	-0.123 (0.44)	-0.866 (2.06)	-0.835 (2.16)
(c) Restrict regression sample to total hours *levels* at time t and t+1 within:				
10 and 70	-0.235 (0.47)	-0.483 (1.14)	-1.574 (2.39)	-1.346 (2.24)
10 and 60	-0.229 (0.47)	-0.458 (1.11)	-1.645 (2.71)	-1.647 (2.85)
10 and 50	-0.325 (0.71)	-0.271 (0.65)	-1.309 (2.30)	-1.350 (2.41)
(d) Restrict regression samples based on levels (10 and 70) and changes (-20 to +20):				
	-0.447 (1.09)	-0.461 (1.34)	-0.878 (1.77)	-0.695 (1.48)
(e) Truncate sample by replacing maximum and minimum levels at t and t+1 and then construct changes				
Levels at 10 and 70	-0.014 (0.03)	-0.014 (0.03)	-1.834 (2.58)	-1.649 (2.46)
Levels at 10 and 60	-0.034 (0.07)	-0.041 (0.09)	-1.855 (2.78)	-1.762 (2.75)
(f) Largest residuals excluded				
Largest 1% excluded	-0.124 (0.26)	-0.250 (0.87)	-1.726 (2.73)	-0.456 (1.12)
Largest 5% excluded	-0.131 (0.38)	-0.115 (0.94)	-0.505 (1.24)	-0.627 (3.20)
Largest 10% excluded	-0.039 (0.15)	0.143 (1.29)	-0.607 (1.84)	-2.647 (11.02)
Women				
Difference-in-differences	-0.634 (2.28)	-0.328 (1.39)	-1.494 (3.65)	-1.360 (3.55)
(a) Restrict regression sample to *changes* lying within:				
Δht≥-30 and 30 ≥Δht	-0.705 (2.84)	-0.470 (2.15)	-1.389 (3.86)	-1.300 (3.84)
Δht≥-20 and 20 ≥Δht	-0.597 (2.79)	-0.423 (2.24)	-0.982 (3.28)	-0.994 (3.47)
Δht≥-10 and 10 ≥Δht	-0.340 (2.41)	-0.123 (0.99)	-0.272 (1.38)	-0.338 (1.85)
(b) Truncate sample by replacing maximum and minimum changes with:				
Min Δht= -30 and Max Δht=30	-0.667 (2.52)	-0.389 (1.70)	-1.495 (3.86)	-1.376 (3.77)
Min Δht= -20 and Max Δht=20	-0.670 (2.75)	-0.409 (1.92)	-1.368 (3.94)	-1.292 (3.92)
Min Δht= -10 and Max Δht=10	-0.544 (3.03)	-0.322 (2.04)	-0.857 (3.47)	-0.836 (3.60)
(c) Restrict regression sample to total hours *levels* at time t and t+1 within:				
10 and 70	-0.746 (2.74)	-0.458 (1.99)	-1.305 (3.31)	-1.185 (3.22)
10 and 60	-0.687 (2.59)	-0.492 (2.15)	-1.303 (3.43)	-1.187 (3.35)
10 and 50	-0.759 (2.98)	-0.574 (2.53)	-1.351 (3.73)	-1.233 (3.56)
(d) Restrict regression samples based on levels (10 and 70) and changes (-20 to +20):				
	-0.649 (2.86)	-0.490 (2.45)	-1.076 (3.42)	-1.045 (3.47)
(e) Truncate sample by replacing maximum and minimum levels at t and t+1 and then construct changes				

Levels at 10 and 70	-0.594 (2.25)	-0.329 (1.47)	-1.481 (3.79)	-1.371 (3.74)
Levels at 10 and 60	-0.607 (2.34)	-0.347 (1.57)	-1.513 (3.92)	-1.400 (3.87)
(f) Largest residuals excluded				
Largest 1% excluded	-0.723 (3.25)	-0.407 (2.21)	-1.230 (3.85)	-1.173 (4.04)
Largest 5% excluded	-0.563 (3.61)	-0.093 (0.84)	-0.689 (3.14)	-0.568 (3.46)
Largest 10% excluded	-0.390 (3.46)	-0.248 (3.39)	-0.532 (3.26)	-0.533 (4.78)

Notes:

1. As in table 1.

Table 5

Difference-in-differences estimates with restrictions on working hour changes and levels: LFS

	Total paid hours	Basic paid hours	Total paid hours	Basic paid hours
	Initial effect		***Lagged effect***	
Men				
Difference-in-differences	-0.741 (0.62)	-0.891 (0.93)	-0.859 (0.56)	-2.867 (2.44)
(a) Restrict regression sample to *changes* lying within:				
Δht≥-30 and 30 ≥Δht	-0.215 (0.22)	0.279 (0.35)	-0.700 (0.49)	-1.741 (1.80)
Δht≥-20 and 20 ≥Δht	0.361 (0.44)	0.424 (0.65)	-0.307 (0.26)	-1.930 (2.15)
Δht≥-10 and 10 ≥Δht	0.038 (0.07)	-0.563 (1.32)	-0.310 (0.39)	-1.104 (2.08)
(b) Truncate sample by replacing maximum and minimum changes with:				
Min Δht= -30 and Max Δht=30	-0.650 (0.59)	-0.552 (0.64)	-0.972 (0.65)	-2.548 (2.40)
Min Δht= -20 and Max Δht=20	-0.326 (0.34)	-0.248 (0.32)	-0.629 (0.47)	-2.266 (2.32)
Min Δht= -10 and Max Δht=10	-0.195 (0.29)	-0.270 (0.49)	-0.538 (0.58)	-1.709 (2.48)
(c) Restrict regression sample to total hours *levels* at time t and t+1 within:				
10 and 70	0.081 (0.08)	0.378 (0.49)	-0.661 (0.48)	-1.283 (1.38)
10 and 60	0.310 (0.35)	0.213 (0.29)	-0.391 (0.34)	-1.259 (1.33)
10 and 50	-0.129 (0.16)	-0.111 (0.15)	-1.304 (1.36)	-2.035 (2.62)
(d) Restrict regression samples based on levels (10 and 70) and changes (-20 to +20):				
	0.220 (0.27)	0.607 (0.90)	-0.464 (0.41)	-1.217 (1.36)
(e) Truncate sample by replacing maximum and minimum levels at t and t+1 and then construct changes				
Levels at 10 and 70	-0.136 (0.13)	0.018 (0.04)	-0.526 (0.40)	-1.971 (2.05)
Levels at 10 and 60	-0.017 (0.02)	0.002 (0.00)	-0.115 (0.10)	-1.498 (1.75)
(f) Largest residuals excluded				
Largest 1% excluded	-0.397 (0.43)	0.075 (0.10)	-0.660 (0.50)	-2.105 (2.28)
Largest 5% excluded	0.200 (0.28)	-0.354 (0.72)	-1.397 (1.45)	-1.739 (2.81)
Largest 10% excluded	-0.383 (0.63)	-0.680 (1.84)	-2.054 (2.55)	-1.589 (3.41)
Women				
Difference-in-differences	0.028 (0.06)	0.244 (0.63)	-0.416 (0.77)	-0.383 (0.79)
(a) Restrict regression sample to *changes* lying within:				
Δht≥-30 and 30 ≥Δht	-0.218 (0.55)	0.107 (0.30)	-0.512 (1.09)	-0.225 (0.51)
Δht≥-20 and 20 ≥Δht	-0.132 (0.37)	0.061 (0.20)	-0.117 (0.28)	-0.194 (0.50)
Δht≥-10 and 10 ≥Δht	-0.315 (1.21)	-0.367 (1.70)	-0.443 (1.36)	-0.239 (0.88)
(b) Truncate sample by replacing maximum and minimum changes with:				
Min Δht= -30 and Max Δht=30	-0.053 (0.13)	0.188 (0.51)	-0.444 (0.86)	-0.373 (0.80)
Min Δht= -20 and Max Δht=20	-0.075 (0.19)	0.160 (0.46)	-0.390 (0.83)	-0.312 (0.72)
Min Δht= -10 and Max Δht=10	-0.195 (0.63)	-0.064 (0.24)	-0.420 (1.12)	-0.319 (0.96)
(c) Restrict regression sample to total hours *levels* at time t and t+1 within:				
10 and 70	0.134 (0.32)	0.384 (1.04)	-0.447 (0.86)	-0.296 (0.62)
10 and 60	-0.021 (0.05)	0.311 (0.86)	-0.415 (0.81)	-0.212 (0.45)
10 and 50	0.001 (0.00)	0.229 (0.65)	-0.207 (0.42)	0.001 (0.00)
(d) Restrict regression samples based on levels (10 and 70) and changes (-20 to +20):				
	-0.169 (0.45)	0.088 (0.27)	-0.066 (0.15)	0.066 (0.16)
(e) Truncate sample by replacing maximum and minimum levels at t and t+1 and then construct changes				

Levels at 10 and 70	-0.185 (0.23)	0.232 (0.64)	-0.393 (0.77)	-0.360 (0.79)
Levels at 10 and 60	-0.038 (0.05)	0.252 (0.71)	-0.417 (0.84)	-0.333 (0.74)
(f) Largest residuals excluded				
Largest 1% excluded	-0.187 (0.50)	0.061 (0.19)	-0.274 (0.62)	-0.265 (0.67)
Largest 5% excluded	-0.345 (1.16)	-0.369 (1.61)	-0.370 (1.04)	-0.330 (1.13)
Largest 10% excluded	-0.264 (1.14)	-0.231 (1.36)	-0.629 (2.18)	-0.279 (1.36)

Notes:
1. As in table 2.

Table 6
Difference-in-differences estimates with panel propensity score matching of the effect of the minimum wage introduction on hours: NES

	Total paid hours	Basic paid hours	Total paid hours	Basic paid hours
	Initial effect		Lagged effect	
Male NES employees				
DID with original control group	0.034 (0.04)	0.037 (0.05)	-2.084 (2.17)	-1.986 (2.28)
DID with control group NMW+20%	0.133 (0.20)	-0.036 (0.06)	-1.599 (1.88)	-1.422 (1.77)
DID with control group NMW+30%	0.069 (0.11)	-0.111 (0.19)	-1.434 (1.76)	-1.328 (1.69)
Panel matches between treated after & before and also control after & before				
DID with control group NMW+20%	-1.262 (1.29)	-0.927 (1.08)	-1.445 (1.12)	-0.984 (0.82)
DID with control group NMW+30%	-1.893 (2.12)	-1.270 (1.54)	-0.678 (0.57)	-0.807 (0.71)
Additional propensity matching between treated and control (kernel)				
DID with control group NMW+20%	-1.253 [1.20]	-0.944 [1.05]	-1.752 [1.27]	-1.355 [1.06]
DID with control group NMW+30%	-1.729 [1.84]	-1.157 [1.35]	-0.613 [0.51]	-0.811 [0.73]
Female NES employees				
DID with original control group	-0.413 (1.12)	0.098 (0.31)	-1.294 (2.68)	-0.958 (2.13)
DID with control group NMW+20%	-0.264 (0.83)	0.210 (0.73)	-0.928 (2.09)	-0.615 (1.46)
DID with control group NMW+30%	-0.482 (1.56)	0.123 (0.44)	-0.886 (2.04)	-0.594 (1.44)
Panel matches between treated after & before and also control after & before				
DID with control group NMW+20%	-1.080 (2.36)	-0.458 (1.30)	-2.004 (2.40)	-1.455 (2.04)
DID with control group NMW+30%	-1.238 (2.81)	-0.483 (1.42)	-2.040 (2.50)	-1.445 (2.07)
Additional propensity matching between treated and control (kernel)				
DID with control group NMW+20%	-1.150 [2.52]	-0.474 [1.38]	-2.065 [2.35]	-1.482 [2.03]
DID with control group NMW+30%	-1.224 [2.80]	-0.469 [1.46]	-2.049 [2.52]	-1.450 [1.96]

Notes:
1. Robust absolute t-ratios in the round parentheses.
2. Ratio of coefficient to bootstrapped standard error in square parentheses (based on 300 replications)
3. The matched control group from which matches are made is defined either from £3.60 to £4.32 per hour (NMW+20%) or from £3.60 to £4.68 per hour (NMW+30%).
4. The pre-NMW period is restricted to April 1997-98 only

Table 7
Difference-in-differences estimates with propensity score matching with repeated cross-sections of the effect of the minimum wage introduction on hours: NES and LFS

	Total paid hours	Basic paid hours	Total paid hours	Basic paid hours
	Initial effect		*Lagged effect*	
Male NES employees				
DID with original control group	0.326 (0.53)	0.156 (0.30)	-1.792 (2.23)	-1.867 (2.53)
DID with control group NMW+20%	0.237 (0.47)	0.028 (0.06)	-1.495 (2.05)	-1.357 (1.97)
DID with control group NMW+30%	0.197 (0.42)	-0.103 (0.24)	-1.306 (1.85)	-1.319 (1.97)
Matched control group				
DID with control group NMW+20%	0.367 [0.49]	0.124 [0.19]	-1.414 [0.73]	-1.315 [0.94]
DID with control group NMW+30%	0.369 [0.50]	0.072 [0.11]	-1.290 [0.96]	-1.300 [0.97]
Female NES employees				
DID with original control group	-0.615 (2.16)	-0.312 (1.29)	-1.496 (3.54)	-1.368 (3.43)
DID with control group NMW+20%	-0.533 (2.10)	-0.214 (0.97)	-1.192 (3.00)	-1.040 (2.74)
DID with control group NMW+30%	-0.725 (2.97)	-0.292 (1.36)	-1.128 (2.89)	-1.008 (2.70)
Matched control group				
DID with control group NMW+20%	-0.572 [1.09]	-0.255 [0.68]	-1.205 [1.25]	-1.049 [1.64]
DID with control group NMW+30%	-0.763 [1.28]	-0.328 [0.81]	-1.134 [1.26]	-1.013 [1.22]
Male LFS employees				
DID with original control group	-0.966 (0.81)	-1.078 (1.13)	-0.686 (0.46)	-2.545 (2.22)
DID with control group NMW+20%	-0.701 (0.76)	-0.794 (1.01)	-1.004 (0.83)	-1.957 (1.80)
DID with control group NMW+30%	-0.802 (0.98)	-0.675 (0.96)	-0.695 (0.61)	-1.748 (1.66)
Matched control group				
DID with control group NMW+20%	-1.131 [0.77]	-1.365 [1.00]	-0.684 [0.31]	-1.416 [0.60]
DID with control group NMW+30%	-1.037 [0.80]	-0.970 [0.83]	-0.850 [0.43]	-1.779 [0.74]
Female LFS employees				
DID with original control group	0.112 (0.26)	0.227 (0.58)	-0.385 (0.70)	-0.384 (0.78)
DID with control group NMW+20%	-0.184 (0.50)	-0.081 (0.25)	-0.909 (1.89)	-0.803 (1.86)
DID with control group NMW+30%	-0.234 (0.68)	-0.203 (0.65)	-0.885 (1.92)	-0.697 (1.68)
Matched control group				
DID with control group NMW+20%	-0.365 [0.70]	-0.178 [0.40]	-0.886 [1.05]	-0.737 [0.96]
DID with control group NMW+30%	-0.395 [0.79]	-0.237 [0.57]	-0.961 [1.16]	-0.744 [1.02]

Notes:
1. Robust absolute t-ratios in the round parentheses.
2. Ratio of coefficient to bootstrapped standard error in square parentheses (based on 300 replications).
3. All matched estimates have separate probit models for each of the three matches.
4. The matched control group from which matches are made is defined either from £3.60 to £4.32 per hour (NMW+20%) or from £3.60 to £4.68 per hour (NMW+30%).