Work-related Training

and the New National Minimum Wage in Britain*

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Abstract

In this paper we use important new training and wage data from the British Household Panel Survey to estimate the impact of the national minimum wage (introduced in April 1999) on the work-related training of low-wage workers. We use two different 'treatment groups' for estimating the impact of the new minimum wage – those workers who explicitly stated they were affected by the new minimum and those workers whose derived 1998 wages were below the minimum. Using difference-in-differences techniques for the period 1998 to 2000, we find no evidence that the introduction of the minimum wage reduced the training of affected workers, and some evidence that it increased it. In particular we find a significant positive effect of about 8 to 11% for affected workers. Consequently we interpret our findings as being consistent with the results from the new theories based on imperfectly competitive labour markets. Our estimates also suggest that two of the goals of the UK government – improving wages of the low paid and developing their skills – have been compatible, at least for the introductory rates of the national minimum wage.

Keywords: minimum wages, human capital, work-related training, difference-in-differences estimation.

JEL Classification: J24, J31, J41

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1. Introduction

Compared to the voluminous body of work on the employment effects of minimum wages, the literature dealing with the effects on training is relatively slight. The proposition that a minimum wage would restrict training opportunities open to workers was initially suggested by Rosen (1972) and followed from developments in human capital theory. With competitive labour markets, human capital theory predicts that the introduction of a minimum wage will reduce investment in training by covered workers who can no longer contribute to training costs through lower wages.

In the absence of good data on the amount of training actually received by individuals, most of the early studies tended to use wage growth as a proxy for human capital formation and found minimum wages lower wage growth (Leighton and Mincer (1981); Hashimoto (1982)). The finding that minimum wage laws lead to slower wage growth does not directly tell us about the impact on training since, by definition, a minimum wage would reduce the wage growth of low paid workers by increasing their wages without necessarily affecting training.

With the availability of better training data in micro datasets, the handful of more recent studies – all using US data - have been able to perform more direct tests. If the labour market for the low paid is competitive and workers are not credit constrained, a minimum wage will reduce training. In the absence of workers posting training bonds with the firm - or binding training contracts - a minimum wage raises the floor below which the wage cannot fall to allow workers to finance general training or to facilitate worker-firm sharing in specific training investments. If, instead, the labour market for the low paid is imperfectly competitive and workers are credit constrained, then a minimum wage can increase investment in the general component of training. The basic rationale is provided by several models which predict that under imperfect competition, firms may pay for general training (Stevens, 1994; Chang and Wang, 1996; Acemoglu and Pischke, 1999; Booth and Zoega, 1999). Intuitively, the monopsonistic character of the labour market introduces a 'wedge' between the wage and the marginal product. If this wedge increases with general training, so that wages are compressed, then the firm can keep some of the surplus generated by general training. In this context, since the introduction of a minimum wage acts to compress wages, it can actually increase general training over a range of human capital and induce employers to train their unskilled workers (Acemoglu and Pischke, 2003).

It is an empirical question as to which, if any, of these effects dominates training incidence and volumes in the real world. The answer will depend on the general-specific training mix, the existence of credit constraints on low paid workers, and the degree of imperfect competition in the labour market. With this in mind, our purpose is to evaluate the effect of the new UK National Minimum Wage on continuing work-related training. Whilst this type of training may include both specific and general components, we argue that our measure is mainly capturing general training, and so may be particularly susceptible to the monopsony effects describe above.

US findings about the training effects of minimum wages have been mixed. Schiller (1994) reported evidence that workers subject to a minimum wage received less training, as did Neumark and Wascher (2001), whilst Grossberg and Sicilian (1999) and Acemoglu and Pischke (2003) found no clear evidence that minimum wages affected training either way.[1] However Grossberg and Sicilian (1999) did find that minimum

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[1] All the studies of minimum wages and training employ US data to exploit the variation in state (real) minimum wages in their effort to identify the effects on training. Leighton and Mincer (1981) use Panel Study of Income Dynamics (PSID) data 1973-5 and the National Longitudinal Survey of Youth (NLSY) 1967-9. Grossberg and Sicilian (1999) use the Employment Opportunities Pilot Project (EOPP) data while Neumark and Wascher use the supplements to the January 1983 and 1991 Current Population (CPS) data. Acemoglu and Pischke (2003) utilise NLSY data for 1987-1992 supplemented by CPS data.

wages significantly reduced men's wage growth, highlighting the disadvantage of using wage growth as a proxy for training.

In April 1999 a National Minimum Wage (NMW) was introduced in the United Kingdom for the first time. It followed a period of 6 years, from the abolition of the Wages Councils, during which there was no statutory wage floor in any sector of the economy except for agriculture. According to the UK Government, the NMW is an "important cornerstone of Government strategy aimed at providing employees with decent minimum standards and fairness in the workplace" [http://www.dti.gov.uk/er/nmw/index.htm]. At the same time the Government states as a priority the development of workforce skills – "particularly the basic skills of some adults" [http://www.dfes.gov.uk/research/]. The unique opportunity offered by the introduction of the new national minimum wage, and a large individual level data set collected over the period of this major change, enable us to address the important policy question of whether or not the two goals are compatible, and thereby contribute to the debate on the effects of the NMW on training.

Against this background, our study presents a number of novel features. First, it not only provides the first investigation of the training effects of minimum wages in Britain, but does so in the wake of a major policy intervention.[2] The second novel feature of our study is that it makes use of very recent data on a labour market that was subject to extensive deregulation in the 1980s and 1990s. Our basic strategy is to compare the evolution of training across various groups of individuals over the period 1998-2000, which saw the introduction of the NMW.[3] The data set that we use - the British

[2] As noted by Stewart (2003), this intervention can be viewed as a "quasi-experimental situation", since individual assignment to the treatment group - coverage by the NMW - is random (individuals do not choose when to be covered). Stewart looked at the employment effects of this quasi-experiment.

[3] We use the data from 1998 onwards because major changes to the training questions were introduced in 1998. See Booth and Bryan (2002) for discussion about the differences in the questionnaires and

Household Panel Survey (BHPS) - provides detailed information on training. Whilst previous studies have generally looked at the effect of a minimum wage on training incidence, our data are sufficiently rich to enable us to look at the effect on training intensity as well.[4] This is important since, to the extent that training is a continuous variable, the NMW might have an effect on the amount of training received instead of just on the incidence.

The third novel feature of our study is that we are able to utilise an important new question asked in Wave 9 of the BHPS (conducted in 1999 immediately after the NMW was introduced) asking individuals whether or not their wage was increased to comply with the NMW. The estimation technique used in the analysis requires the identification of the groups that have been 'affected' and those 'not affected' by the introduction of the NMW. The information gathered from this question enables us to identify the 'affected' and 'unaffected' groups and thereby compare the results to those derived using an alternative and more conventional definition based on the hourly wage. The methodology followed in this paper is in the spirit of Stewart (2003), who used the BHPS to analyse the effect of the NMW on employment probabilities of low-wage workers.[5]

We find, using difference-in-differences techniques for the period 1998 to 2000, no evidence that the introduction of the minimum wage reduced the training of affected workers. Instead, we find that the introduction of the minimum wage increased both training incidence and training intensity. Consequently our findings can be interpreted as

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training responses before and after this change. Bryan (2002) analysed training changes over 1995-1997, when no minimum wage was in place, finding no significant differential effect for workers who would have been covered by the minimum wage. The government which introduced the NMW came to power in May 1997, so one might have expected to see any 'announcement effect' reflected in reported training in 1997.

4 Whilst intensity was available to Acemoglu and Pischke (2003), they report that there were too many missing values to make it useable. Grossberg and Sicilian (1999) used intensity.

5 Linneman (1982), Currie and Fallick (1996) and Abowd *et. al.* (2000) represent examples of earlier studies estimating the effect of minimum wages on employment probabilities and using as controls groups of individuals further up the wage distribution.

providing no evidence in support of the orthodox human capital model as it applies to work-related training, and some evidence in support of the new theories based on imperfectly competitive labour markets. Our estimates also suggest that two of the goals of the UK government – improving wages of the low paid and developing workers' skills – have been compatible, at least for the introductory rates of the national minimum wage.

The next section presents the empirical framework. In section 3 we describe the data and how 'affected' and 'unaffected' groups are defined, as this is crucial to the estimation strategy followed in the identification of the effect of NMW. We also discuss issues of measurement error in calculated hourly wages. Section 4 presents our main results. In Section 5 we examine the robustness of our findings to alternative definitions of the 'affected' and 'unaffected' groups and estimating samples, and we report some model extensions. Section 6 concludes.

2. Empirical Framework

The parameter of interest in our empirical work is the mean impact of the NMW on training for those who were affected by this policy intervention. This is often referred to as the mean effect of 'treatment on the treated' (Heckman, LaLonde and Smith, 1999). The estimation of this parameter requires two pieces of information: first, the identification of the group of individuals affected by this policy intervention ('treatment' group); and second, information on what would have happened to the training experiences of these individuals in the absence of NMW. Even if we are able to identify the group of individuals who have been affected, the second requirement will never be satisfied as this situation (known as the 'counterfactual') is never observed. The estimation strategy then proceeds by trying to identify a group of individuals, known as the 'control' or 'unaffected' group, who would provide information regarding the missing counterfactual. Under the assumption that the mean change in the training experiences of those affected and unaffected groups are the same in the absence of NMW, we are able to identify the parameter of interest.

More formally, let T_{it} denote the outcome variable – the training experience (to be defined more precisely later) – in period t for individual i. Define the dummy variables (i) D_t =1 in post NMW period and zero otherwise; (ii) M_i =1 if individual i is in the treatment group and zero otherwise. Assuming a linear specification, the regression-adjusted outcome equation, which incorporates the influences of various observed and unobserved characteristics, can be represented by:

$$T_{it} = \boldsymbol{X}_{it}'\boldsymbol{\beta} + \alpha D_t + \gamma D_t M_i + \mu_i + \varepsilon_{it} \qquad (1)$$

where $\boldsymbol{X}_{it}$ is a vector of individual and job characteristics that influence the outcome variable; the associated parameter vector is $\boldsymbol{\beta}$; μ_i is an unobserved individual specific effect; and ε_{it} is a random error term. The parameter of interest is γ.

It is important to note the following in the above specification. First, differences in training experiences that are common across individuals due to, say business cycle effects, are captured by the time dummy D_t. Second, the parameter of interest, γ, is identified by the assumption that the only change in training, for the 'affected' group of individuals, comes via the introduction of the NMW, *ceteris paribus*. Third, conditional on the $\boldsymbol{X}$s, the control group should be chosen such that the differences in the training during this period would have been the same for both groups of individuals in the absence of NMW. Fourth, unlike the standard cross-sectional specification in which a group dummy is included in the specification, the above specification is more general as it allows for an unrestricted individual-specific term $\mu_{i.}$ Fifth, additional groups can be incorporated into the above specification by including appropriate interactions to account for any possible "spill-over" effects that might occur if individuals in other parts of the wage distribution receive a pay increase to maintain relative pay differentials and which might have affected their training.

It is likely that the unobservable effect μ_i is correlated with some of the regressors, in particular the group dummy M_i which identifies the affected group. For example, inherently productive individuals are likely to earn higher wages and be particularly able to benefit from training. Insofar as this (time-invariant) individual heterogeneity is not captured by the observable characteristics in the $\boldsymbol{X}_{it}$ vector, then μ_i will be correlated with treatment group dummy M_i. We therefore difference the equation prior to the application of OLS (also known as the difference-in-difference estimation), to estimate the parameter of interest.[6] The equation thus estimated is

$$\Delta T_{it} = \Delta \boldsymbol{X}_{it}'\boldsymbol{\beta} + \alpha + \gamma M_i + \Delta\varepsilon_{it} \qquad (2)$$

3. The Data and Sample

Our data come from waves 8 to 10 of the BHPS, covering the period 1998-2000. The BHPS is a nationally representative random-sample panel survey of private households in Britain. Every year the survey seeks to interview all adults (defined as individuals aged over 16 years) from the original sample, as well as all other adult members of their current households. The panel is therefore replenished in each wave by original sample members who reach the age of 16, and by adults who join the survey due to the changing composition of original sample members' households.[7] Our data span the introduction of the NMW on 1st April 1999. Thus the pre-NMW period data are from Wave 8 (conducted in 1998) and post-NMW period data are from Wave 10 (conducted in 2000).

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[6] The $\Delta\varepsilon$ is allowed to have an arbitrary heteroskedastic covariance matrix, in the estimations reported in the paper.

[7] Individuals who move in with original sample members, as well as adults in new households formed or joined by original sample members, become sample members themselves.

A new question format was introduced from wave 8 to elicit more detailed information on training than had previously been available. The new questions cover up to three training events experienced by the respondent since September of the previous sample year, and deal with aspects such as where the training took place, how it was financed, its duration and whether or not it led to qualifications (see Appendix A). Consistent with the theory, we focus on training intended to increase or improve skills in the current job. Our sample of individuals was interviewed in wave 8 between August 1998 and March 1999 about training received since 1st September 1997. In wave 10 they were interviewed between September 2000 and May 2001 about training experienced since 1st September 1999.[8] The training reported therefore falls unambiguously before and after the introduction of the NMW. We do not use training data from wave 9 since we cannot determine whether training events occurred before or after the NMW was introduced.

The NMW was introduced at three levels: a main rate of £3.60 per hour, a youth rate of £3.00 for 18-21 year olds and a special development rate of £3.20 for workers over 21 years old undertaking specific types of approved training. Use of the development rate was the only way employers could lower the wage in return for providing training. Although we are not able explicitly to identify individuals covered by this provision in the data, we do have a directly reported hourly wage measure for a sub-sample of individuals. Since we did not find anybody in this sub-sample who was paid the development rate, so it seems this provision was not widely used. We discuss the reported hourly wage variable in Section 3.4.[9]

8 For both waves, 98% of interviews were completed by the end of January.
9 One individual who was under 21 reported a wage of £3.20.

The sample consists of employees aged between 18 and 60 years in wave 8 who are not in the army or in farming or fisheries and have valid training information.[10] Individuals reporting more than 100 working hours per week (hours are used to derive an hourly wage measure) were dropped. Where there were substantial numbers of missing observations on control variables, these missing cases were set to zero and dummy variables created to indicate their status, in order to maintain reasonable sample sizes. Individuals must satisfy the selection criteria in at least waves 8 and 10. In addition, for one of the treatment-control group definitions, which we discuss below, they must be present in wave 9.

3.1 Outcome Variable - Training

Our interest is in the estimation of the effect of NMW on training experiences defined as training undertaken to increase or improve skills in the current job. The survey questions are designed to elicit detailed information on the type of training and how it was financed (see Appendix A) From the responses we find that most recipients (about 80 %) of training for skills in the current job view it as general, in which case, according to general human capital theory, it should be paid for by the employee. However, we also find that the direct costs of this training are typically paid almost exclusively by employers, in spite of its general nature.[11] This finding, while apparently at odds with the predictions of human capital theory, is also reported by Loewenstein and Spletzer (1998) for the US.[12]

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[10] We restrict our analysis to cases where the training decision is economically meaningful and where the minimum wage is a consideration. These criteria determine our age limits. The NMW does not apply to individuals under 18. Consistent with human capital theory, the raw data show a sharp drop in the incidence of training for older workers: for example, whilst training incidence is 31.2% amongst workers under 30 years old in wave 8, it is 15.1% for 56-60 year olds and 7.1 % for 61-65 year olds.

[11] Lower paid workers are slightly less likely to have their training financed by the employer. For the two groups of workers whom we identify below as being directly affected by the NMW, 80% of individual training was paid for by employers, compared to 91% for the full sample.

[12] For an extensive analysis for Britain, see Booth and Bryan (2002). Loewenstein and Spletzer (1998) use NLSY data.

We first define, T_{it}=1 if the individual received any training during the past 12 months and 0 otherwise. It is possible that the introduction of the NMW not only affected the incidence of training but also had an effect on the intensity of training received (where intensity is defined as days spent in training). To allow for this, we also extend the definition of our outcome variable by incorporating information on the intensity transitions. The definition of the dependent variables used in the analyses is presented below:

Table 1

Definition of dependent variables

Intensity		Incidence transition ΔT_{it}	Intensity transition ΔT_{it}^{*}
Wave 8 – Pre NMW	Wave 10 – Post NMW		
Zero	Zero	0	0
Zero	Positive	+1	+1
Positive	Zero	–1	–1
Positive	Positive	0	+1 if intensity(10) > intensity(8) 0 if intensity(10) = intensity(8) –1 if intensity(10) < intensity(8)

The new variable ΔT_{it}^{*} used in the estimation of (2) is identical to ΔT_{it} unless training incidence is positive in both periods; then ΔT_{it}^{*} = 1 if intensity increases, ΔT_{it}^{*} = -1 if intensity decreases and ΔT_{it}^{*} = 0 if intensity remains the same. Equation (2) therefore represents a Linear Probability Model (LPM) in differences.

3.2 Definition of Treatment and Control groups

As Stewart (2003) notes, the introduction of the national minimum wage brought about a change in wages at the bottom end of the wage distribution. We would therefore expect that the training of those workers whose wages were increased to comply with the new minimum would be more affected than those higher up the distribution who were not directly affected. These anticipated effects might be positive or negative, as explained in the Introduction, depending on the general-specific training mix, the existence of credit constraints, and the degree of imperfect competition in the labour market. In order to capture possible spill-over effects we use an additional control group as explained below.

The identification of the effect of NMW hinges crucially on the correct identification of the treatment and the control groups. The differing results found in the US literature emphasise the importance of defining appropriately the split between the treatment and control groups. Studies using US data have exploited the variation in state (real) minimum wages, which existed in the late 1980s, as well as changes over time.[13] In the simplest specification, all individuals in states and years where the minimum wage binds relatively tightly were used as the treatment group and the control group comprised individuals in other states and years.[14] State and year dummies were used to capture state- and time-specific effects. But note, in this simple case the treatment group would have included many high wage individuals whose training decision perhaps had not depended on the minimum wage. In order to alleviate this problem to some extent, estimation was restricted either to a subgroup likely to have been more affected by the minimum wage (such as 16-24 year olds as in Neumark and Wascher (2001); or to the low-waged as in Acemoglu and Pischke (2003)). Others have also allowed the effects to vary for such a subgroup relative to a control group assumed unaffected (35-54 year olds in Neumark and Wascher (2001)). Nevertheless the dilution problem remains.[15] However, one advantage with this is that these broadly defined treatment groups

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[13] The US federal minimum wage was unchanged 1981 to 1990.

[14] Measures of how binding the minimum wage is at state level include the gap between the state and federal minimum wages (Neumark and Wascher (2001)), and the state minimum wage relative to median state wages (Acemoglu and Pischke (2003)).

[15] Furthermore, when using a minimum wage indicator, which only varies at state-year, rather than individual-level, the standard error of the estimated coefficients is likely to be underestimated and should be adjusted to account for the state-year error component structure (Moulton, 1982).

generally contain a large number of observations, so that small cell size was not a concern.

An alternative approach taken was to define the treatment group as those individuals directly affected by the minimum wage, for example those earning less than a new minimum wage level before it was introduced (Acemoglu and Pischke, 2003), or those starting new jobs at the minimum wage (Grossberg and Sicilian, 1999). Providing the wage was not measured with too much error and the control group was truly unaffected, this definition should afford a sharper test of minimum wage effects. This is the approach we take in this paper. However, it does raise the concern that selection into the treatment group might be endogenous if wages and training were jointly determined. Although our model accounts for this by the inclusion of an unobserved individual-specific effect, there is still the possibility of small cell sizes in the treatment group sample, as found in some previous studies.[16]

We define two alternative treatment and control group splits and denote them 1 and 2, as summarised in Table 2. Treatment group 1 contains individuals whose derived hourly wage was below the level of the NMW for their age in wave 8.[17] On the assumptions that their derived wages are free of measurement error and would not have increased (in real terms) between the interview date in wave 8 and the introduction of the NMW on the 1st April, 1999, these individuals should have had their wages raised to the

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[16] For example, by Acemoglu and Pischke's (2003) tightest definition of the treatment group (minimum wage increased and wage in prior year is below current minimum wage), Table 3 in their paper implies that the treatment group contains 638 individual-year observations. In the text they report that training incidence for this group is 5.2%, suggesting there are about 33 observations where training took place. Grossberg and Sicilian (1999) have about 110 men and 150 women in the minimum wage groups of their samples (the exact size of which is not very clear from the paper). However, training (formal or informal) is reported in about 95% of cases (these individuals are observed from the start of their jobs), so that about 105 men and over 140 women are observed to train.

[17] The derived wage is calculated as wage = (usual gross pay per month) / [(usual standard weekly hours) + 1.5*(usual paid overtime weekly hours)] *(12/52). All wages are deflated to 1999 levels using the CHAWRPI non-seasonally adjusted retail price index from the Office of National Statistics. There was no significant change to the estimated effects when the overtime premium was changed from 1.5 to 1.0.

NMW in April 1999. Since we are interested in comparing the training probabilities of individuals whose wage increased as a result of the minimum wage with otherwise comparable individuals in the wage group just above the new minimum wage, we define a control group of individuals earning between the NMW and 15% more than the NMW in wave 8. We investigate the sensitivity of our estimates to changes in the definitions of treatment group 1 and its control group in Section 5.

The total estimating sample comprises all individuals who satisfy the selection criteria stated above in both waves 8 and 10. To investigate possible spill-over effects of the new minimum wage, we also include the group of individuals from the rest of the wage distribution, that is, above 115% of the minimum wage in 1998. This group is termed the 'high-wage' individuals.

The definition of treatment group 2 is based on the responses to an important new question in wave 9 which sought to identify individuals who were directly affected when the NMW was introduced. Specifically, those individuals who replied positively to the question "Has your pay or hourly rate in your current job been **increased** to bring you up to the National Minimum Wage or has it remained the same?" were categorised as belonging to treatment group 2. This question was only asked of individuals who did not change jobs between 1st April 1999 and the date of interview (from August 1998 to March 1999), and hence, this definition will exclude some workers who were subject to the NMW in a new job.[18] However, the question does have the advantage of providing us with a treatment group that is arguably much less likely to be characterized by measurement error than the derived wage used for treatment group 1. Control group 2 consists of individuals who were job stayers and who answered no to the above

[18] The treatment group will therefore tend to over-represent job stayers. However, insofar as individuals remain in their jobs because of characteristics, which are constant over time, the differencing estimator will eliminate potential selection bias.

question[19]. Since the question was only asked in Wave 9, this selection requires individuals to be present in all three waves (8, 9, and 10). The sample size is therefore smaller than that used for treatment group 1, as indicated by Table 2.

We stress that all our analysis is conditional on employment. A separate question, much more extensively investigated in the literature, is whether minimum wages affect the probability of employment. Firms might, for example, lay off the least able workers and then train up those remaining. However, Stewart (2003), using the same data set, finds no statistically significant evidence of employment effects of the NMW. It should anyway be emphasised that the model we estimate implicitly accounts for selection biases coming via correlation with unobserved individual specific characteristics.

3.3 Sample descriptions

The sample means at wave 8 are shown in Table 3. The figures for treatment group 1 and control group 1 are reported in the first two columns. The mean derived hourly wage is £2.82 in the treatment group and £3.82 in the control group, so on average, affected workers were paid 26% less than the control group before the NMW was introduced. Below we discuss the wage distribution of each group in wave 9, just after the NMW came into effect. In most other respects, the treatment and control groups are quite similar: in both, mean experience was about 15 years and mean tenure 3.7 years. Education levels were quite low: only a fifth of individuals in each group had some vocational qualification and over a third of had no qualifications at all at GCSE level or above. Both groups were also mobile, with around a third changing employer since the previous wave. There are also some small differences. For example the control group individuals were slightly less likely to be female (the sample proportion is 0.69 compared

[19] Control group 2 also contains high-wage individuals because of the categorical nature of the response variable (wage affected or not affected by the NMW).

to 0.78 for the treatment group), to be part-time (0.39 compared to 0.44), and to be on temporary contracts (0.03 compared to 0.08). Training incidence was also somewhat higher amongst the control group (0.19 compared to 0.16). Their unconditional training intensity of 6.49 days is more than double that of the treatment group, but this figure is driven by very long training events (over 100 days) reported by five individuals.

Now consider the group of individuals that we defined earlier as the 'high-wage' that is, earning more than 115% of the minimum wage in 1998. As we can see from column (3) of Table 3, the means for this group, show substantial differences relative to the treatment and control groups, generally in the expected directions. These individuals were much better paid (the mean wage was £8.96). They tended to have longer tenure (a mean of 4.7 years) with only 17% changing employers since wave 8. They were better educated (17% had some form of university degree and only 16% no qualifications at GCSE level or beyond).

Some other striking features emerge from a comparison of columns (1) to (3). First, affected workers are not predominantly young; the mean age in all three groups is about 37 years. Second, the NMW disproportionately affects women and part-time workers. Third, about three quarters of affected workers are found in two one-digit industries, distribution, hotels and catering; and other services. Fourth, minimum wage workers tend on average to be in private-sector (sample proportion 0.88), non-unionised (0.73) jobs.

Columns (4) and (5) report the means for the alternative treatment and control groups 2 (defined by whether or not individuals' reported that their wages had been increased in line with the NMW). As discussed above, the columns (4) and (5) figures are likely to represent disproportionately job stayers. This is clear from the higher tenure and lower employer change figures relative to column (1). However, apart from characteristics reflecting job mobility, treatment group 2 appears to be similar to treatment group 1.[20] Training incidence in the sample is, however, lower (0.10 compared to 0.16 for treatment group 1).

3.4 Measurement error and misclassification

We now consider the possibility of measurement error in our treatment group indicators; that is, that individuals may be wrongly classified as either belonging or not belonging to the treatment group. Table 4 shows a cross tabulation of the treatment group 1 and 2 indicators for the sub-sample of cases where a valid treatment group 2 indicator is available (i.e. where the individual did not change jobs between 1st April 1999 and the wave 9 interview). In the absence of misclassification the two groups should overlap perfectly. In fact, of the 189 individuals with a derived wage less than the NMW in wave 8 (treatment group 1), and of the 99 who reported that their wage was increased up to the NMW in wave 9 (treatment group 2), only 53 are in both treatment groups.

There are two possible sources of these anomalies. First, it is likely that some individuals received a pay rise, which brought them up to the NMW without knowing the reason. So we would expect treatment group 1 to be larger. Second, other studies have noted evidence of error in wage measures that are derived from reported earnings and hours worked (see *inter alia* Stewart and Swaffield, 2001). The effect of measurement error will be that some individuals will be incorrectly classified as belonging to the treatment group, whilst others who are truly affected will be classified in the control or 'high-wage' groups.

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[20] However the mean derived hourly wage in treatment group 2 is £3.84 compared to only £2.82 in treatment group 1. This disparity is likely to be caused by measurement error in the derived wage, since inclusion in treatment group 1 is based on the observed wage falling below a threshold. So individuals with observed wages which are too high due to measurement error will tend systematically to be excluded from treatment group 1, whilst individuals whose observed wages are lower than their true value will tend to be included. Therefore the treatment group 1 mean wage will be biased downward relative to the mean in treatment group 2. We discuss the extent of measurement error in Section 3.4.

We can also get a feel for the importance of measurement error by looking at the wage distributions of the treatment and control groups in wave 9, which is just after the NMW was introduced. We plot two measures of the wage (truncated at £15 per hour for clarity): the first is the derived wage that, in wave 8, was used to define treatment group 1. The second is the wage rate directly reported by hourly paid workers. This question was asked in the BHPS from wave 9 onwards and only of hourly paid workers.[21] Note that only 76% and 79% of workers in treatment groups 1 and 2 reported an hourly rate (of those who did not, the vast majority were salaried or on some other payment system). Graphs 1(a) and 1(c) show the plots for treatment group 1. We would expect both wage measures to be clustered around £3.60.[22] The derived wage distribution 1(a) shows a clear spike (the median is 3.67) but also a good deal of dispersion, for example the 75th and 90th percentiles are £3.20 and £4.39. Graph 1(c) of the reported rate also shows a spike at 3.60, with almost no one reporting a lower rate, but a similar amount of dispersion to graph 1(a) at higher wages. The control group graphs 1(b) and 1(d) are similar, but are centred about 15% higher up the distribution.

Graphs 2(a) and 2(c) show the plots for treatment group 2, and it is noticeable that both are more tightly clustered around 3.60, especially the reported hourly rate. This does suggest that a degree of noise is introduced when the treatment group is defined by the derived wage.[23] We conclude nevertheless that the treatment groups do seem broadly to be picking out individuals affected by the NMW.

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[21] We considered the definition of a third treatment and control group pair based on the hourly rate in wave 9. However, the resulting group was very small, containing only 65 individuals. Furthermore, the results would only have been valid for hourly paid individuals, who may not be a random sample of affected workers.

[22] Affected workers under 22 years of age in wave 9 should have been paid at £3.00 per hour. However, they only make up around 9% of both treatment groups.

[23] We investigated whether the dispersion decreased if we reduced our assumed overtime premium of 1.5 to 1.0 or zero in the wage calculation. We also tried restricting our sample to hourly paid workers or those not reporting overtime. We found that none of these factors had a significant effect.

3.5 Training incidence and intensity means

How does training vary over the sample period? Table 5 reports mean training for the pre and post NMW periods. The table shows that training incidence typically increased in all the groups, with a particularly marked proportionate increase in the treatment groups. For example, incidence in treatment group 2 increased from 0.10 in wave 8 to 0.17 in wave 10; in control group 2, incidence went from 0.28 to 0.30. Despite the increases, training is much less prevalent amongst workers earning close to the minimum wage than in the higher paid groups.

The pattern of changes is less clear when we consider (unconditional) training intensity. In treatment group 1 mean intensity rose from 2.6 days in wave 8 to 4.8 days in wave 10. In control group 1, intensity fell sharply from 6.5 days to 2.8 days. A continuous increase over the period, from 2.3 days to 6.5 days is observed in treatment group 2. In the two groups of higher paid workers (‘high-wage’ group 1 and control group 2), intensity, like incidence, is quite stable at around 5 days a year. The volatility in the smaller groups is possibly caused by their sizes, or perhaps reflects that training intensity is a noisy measure. In that case, incidence change ΔT may be the preferred dependent variable but we present results for both variables ΔT and ΔT^*. In section 4.1, we discuss the statistical significance of these changes when we report the simple differenced LPM estimates.

As mentioned above, one objection to the use of groups defined by the wage is that wages and training are jointly determined. Therefore the treatment group could mainly be picking out workers with a low starting wage but with high training and wage growth. To explore this issue, we include in Table 5 the mean derived wage in each subgroup and wave, and the percentage growth of the mean relative to the previous wave. Turning first to the bottom panel of the table, we see that the mean wage of individuals in treatment group 2 grew by about 13% between waves 8 and 9 but by only about 5% the following year. For the control group of higher paid workers, mean wages grew by 4.5% from waves 8-9 and 3.1% from waves 9-10. These figures do not suggest that workers in the treatment group are on a high wage growth path, apart from a large wage increase between waves 8 and 9 that is presumably the effect of the NMW.

The top panel of the table reports the figures when the treatment, control and high-wage groups are defined by the wave 8 derived wage. Thus workers in treatment group 1 experienced very large wage gains of 40% on average between waves 8 and 9 and much smaller average increases of 12% between waves 9 and 10. A similar pattern (though with smaller increases of 17% and 8%) is evident for workers in control group 1, and workers in the high-wage group received increases of about 3% between both pairs of waves. The large estimated increases for lower paid workers are likely to be biased by measurement error in the derived wage since this measure is used to define the groups as well as calculate the growth figures.[24] Therefore it is difficult to conclude from them that the lower paid enjoy higher wage growth. This is especially so since there is no evidence of higher wage growth in the lower panel, where the group indicator should be independent of any wage measurement error.

3.6 Training transitions

In Tables 6 and 7 we summarise the changes in the training incidence and intensity variables between waves 8 and 10. Our identification strategy relies on a sufficient proportion of the treatment and control groups making transitions. In treatment group 1, Table 6 shows that 44 individuals moved from no training in wave 8 to training in wave

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[24] Intuitively, an understatement of the true wave 8 wage increases the probability of being classified as low paid and inflates the wave 8-9 growth figure, *ceteris paribus*. This is analogous to regressing wage growth on the initial level: even if there is no true relation between levels and growth, the presence of measurement error will tend to give rise to negative coefficient estimates.

10, while 23 went from training in wave 8 to no training in wave 10. 19 trained in both waves and 173 did not train in either wave. In control group 1 almost equal numbers took up and stopped training between the two waves (26 and 28 individuals). Turning to treatment group 2, we see that again more individuals started than stopped training, 12 against 5. This low number of transitions reflects the small size of the treatment group. Nevertheless, they represent over 17% of the cell size of 99.

Table 7 shows that there is more variation in the intensity variable, because it includes the intensity changes of individuals who had positive incidence in both waves. So in treatment group 1, 17 of the 19 individuals who trained in both waves experienced changes in intensity: 11 trained for longer and 6 trained for shorter periods. These individuals thus contribute to the observed transitions. Similarly in control group 1 there are 14 extra transitions. Treatment group 2 contains 5 more transitions. The low number of transitions in this group clearly implies a caveat to our estimates from this sample.[25]

4. Results

4.1 Raw difference-difference estimates

The estimates obtained from equation (2) with no additional controls are reported in Table 8, columns (1)-(4). Columns (1) and (2) show the results for changes in incidence only variable ΔT, and columns (3)-(4) the results for ΔT^* which accounts for changes in the intensity of training receipts. Confirming the pattern presented by the raw training figures of Tables 5, column (1) indicates that the probability of training receipt in treatment group 1 increased by about 9 percentage points more than it did in the control

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[25] One way to alleviate the problem of small cell sizes in the treatment group is to include the sub-sample of low-income individuals, which was added to the BHPS as part of its contribution to the European Communities Household Panel Survey (ECHP) in Wave 7. The above analysis was carried out for this combined sample (with appropriate weighting applied to make it a representative sample of the population) and the results did not differ from those reported here. See Bryan (2002) for further details.

group. Furthermore, this increase is statistically significant at the 5% level. The probability of training receipt also increased in the high wage group relative to the control group but the coefficient is not statistically significant. This suggests no spill-over effect of the NMW into this group. From column (2) where we use treatment and control groups 2, we see that, although training incidence increased more in the treatment group (by 5.0 percentage points) than in the control group, the estimate of the effect of the NMW on the probability of training receipt is not statistically significant at conventional levels. In this equation, however, the constant, capturing the trend increase in incidence, is significant at 10%. The differences between columns (1) and (2) may be because control group 1 comprises workers just above the NMW, whereas control group 2 contains higher paid workers as well.[26]

However, the results in columns (3) and (4) show that a similar result is obtained when information on changes in intensity is incorporated into the definition of training. More specifically, affected workers appear to be 10 percentage points more likely to experience an increase in incidence/intensity than workers in the control group. The increases are statistically significant. These results provide some evidence that the NMW may have resulted in increased training. We next include additional controls and estimate the regression-adjusted difference-in-difference effect.

4.2 Regression-adjusted difference-in-difference estimates

Columns (5)-(8) of Table 8 show the estimates of equation (2) when various individual and job characteristics are added. The variables that are entered in first-differences in the first differenced equation are age squared, part-time status, whether the job is fixed-term or temporary, whether the worker changed employers, marital status, whether the job was

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[26] In Arulampalam, Booth and Bryan (2004) we summarise the results of Table 8 in a special issue of The Economic Journal Conference Volume devoted to an over-view of the economic effects of the NMW.

unionised, whether it was in the public or charity sector, firm size, 1-digit industry, and the local travel-to-work area (TTWA) unemployment rate.[27] We exclude potentially endogenous variables like tenure and occupation. Insofar as the additional variables change over time, they help control for individual differences in training growth.

Turning to columns (5)-(8) of Table 8, we see that the estimates of the treatment effect are slightly reduced compared to those in columns (1)-(4). Thus for treatment group 1, the NMW is estimated to increase the probability of training by 8.0 percentage points *ceteris paribus*, significant at the 10% level. The probability of training receipt in the high-wage group is expected to increase by 4.0 percentage points, but the coefficient is not significant at conventional levels. Both figures are relative to the base case of control group 1. When the dependent variable is redefined to incorporate the information on intensity, the estimate, shown in column (7), is slightly higher at 8.8 percentage points.

In the equations comparing treatment group 2 against control group 2 (columns (6) and (8)), the estimates are again similar to those where additional control variables are omitted (columns (2) and (4)). The NMW does not appear to have a significant impact on the probability of training incidence (the coefficient is positive), but is expected to significantly increase the probability of incidence/intensity by 9.8 percentage points.

We interpret these results as providing support for the hypothesis that the NMW increased work-related training against the null hypothesis of no effect. The estimated

[27] The linear age term is subsumed in the constant of the difference equation. The coefficient estimates of the rest of the variables are provided in Bryan (2002).

effects imply the probability of training increased by about 8 to 10 percentage points. We now describe extensions of the analysis intended to test the robustness of these results. [28]

5. Model Extensions

5.1. Alternative treatment/control group thresholds

We experimented with altering the width of both treatment group 1 and its control group. We first report results from our experiments with an alternative definition of *treatment group 1* (which included only individuals earning less than the NMW in the pre NMW period). The reason we wish to try altering the treatment group relates to measurement error and to the "spill-over" argument. Neumark and Wascher (2001) contend that, from a perspective of perfect competition, workers earning substantially more than the minimum wage might be affected if their wages net of training costs would fall below the minimum wage floor.[29] We therefore re-estimated our basic model, using an alternative definition of the treatment group as individuals earning up to NMW+8%, with the control group being individuals earning between NMW+24%, and the high-wage group comprising individuals earning more than NMW+24%. We chose to expand the treatment group by 8% because of the following calculation. For our sample, annual mean training intensity conditional on undertaking training is approximately 20 days (4 working weeks), representing an opportunity cost of 4/48=8% of a working year. If we assume wages are changed once a year (usual in the UK) and workers pay for training

[28] Further model extensions are reported in Bryan (2002). They are (i) the use of data from the 1995-1997 waves, when no minimum wage was in place, as additional control observations, (ii) restriction of the definition of training to that financed by the employer only, and (iii) the use of a fixed effect logit model as an alternative to the difference-in-difference estimator. These alternative models produced similar results to those reported here, and our conclusions are not changed.

[29] This is because, even though their wages might be, say, 5% above the minimum wage, if they have to pay for training through a wage reduction of 10%, then they hit the wage floor and training will thereby be affected.

out of that year's salary, an average worker receiving training would have to take an 8% pay cut to pay for it.

The results of this experiment are reported in columns (1) and (2) of Table 9, and should be compared to columns (5) and (7) of Table 8, which contain estimates derived using the original thresholds. None of the estimates of the treatment effect is significant at the 5% level, but all are positive. However, the magnitudes are substantially down relative to those of Table 8.

We now report results from our experiments with widening the *control group* for treatment group 1. The advantages of a wider control band than individuals between the minimum and 115% of the minimum wage is that it (i) lessens the proportion of cases who are there due to misclassification arising from measurement error, and (ii) increases the number of observations making the estimates more precise. (These same arguments do, of course, also apply to a widening of the treatment group 1.) The advantage of a narrower control band is that the individuals therein are more likely to be similar to the treatment group. The results of this experiment of widening the control group band are reported in columns (3) and (4) of Table 9, and should be compared to columns (5) and (7) of Table 8. Interestingly, the estimates of the treatment effects are very similar to the earlier ones.

5.2. Controlling for Imperfect Competition in the Labour Market

The new training theory, as summarised in Acemoglu and Pischke (1999 and 2003), suggests that minimum wages might increase investment in general training when labour markets are imperfectly competitive. We experimented with incorporating two separate measures for this to see if this hypothesis is supported by our data. Our first measure of imperfect competition is a dummy variable taking the value of one if the labour force in an individual's travel-to-work area (TTWA) is more than 500,000 people, and zero otherwise.[30] This measure is included to proxy the degree of employers' labour market power. The larger the labour market, the lower any employer's oligopsony power, since there are many alternative firms at which an individual could work (see for example, Stevens, 1996; Booth and Zoega, 1999; and Bhaskar, Manning and To, 2002). These results are reported in columns (1) to (4) of Table 10. The treatment group estimates remain at around 10 percent in three of the specifications. This implies that, in general for covered individuals in *smaller TTWA labour markets,* the introduction of the new minimum wage increased the probability of training by 10 percentage points. However, the impact for covered individuals in *the larger TTWA labour markets* is negative, although not statistically significant. To the degree that the size of the TTWA labour force is negatively correlated with employer oligopsony power, these estimates provide some support for the hypothesis that the impact of the minimum wage on training is greater where firms have more labour market power.

Our second measure to control for imperfect competition follows the approach of Acemoglu and Pischke (2003), who use the industry wage differential as a proxy for rents in an industry. Their presumption is that industrial groupings with lower industry wage differentials are more competitive.[31] Using US data, Acemoglu and Pischke (2003) find a positive coefficient to the interaction of their treatment group with industry rents. They interpret this as lending some support to the new training literature.

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[30] This indicator picks out London plus other very large urban centres. Approximately 13% of individuals fall into this category, including 12% of treatment group 1 and 8% of treatment group 2. Changing the cut-off to 300,000 or 400,000 did not make any qualitative differences to the estimated parameters of interest.

[31] Rents to a firm can arise from either imperfect competition in the product market (oligopoly) or in the labour market (oligopsony). Labour market rents will not be shared with workers since they arise by definition from exploiting workers through lower wages. Consequently they are unlikely to be observed in inter-industry wage premia, which will reflect only the share of product market rents accruing to workers. Thus a low industry wage premium can reflect the following: (i) workers have no labour market power to extract a share of the product market surplus; and/or (ii) there is no surplus. Acemoglu and Pischke seem to use the first interpretation.

Our results following this procedure are reported in columns 5 to 8 of Table 10.[32] Note that the mean rents for treatment group 1 and 2 are 0.15 and 0.14 log points respectively, as compared to the mean of the whole sample of 0.26 log points. Unlike Acemoglu and Pischke, we find that the term interacting the treatment group with industry rents has a negative coefficient, although this is not statistically significant. As an illustration we evaluate the marginal effects implied by the estimates from column (5), at a premium value of 0.16 log points (close to the means of both the treatment and control groups). For individuals in the affected group and in a "typical" industry, the expected probability of training incidence increased by about 8 percentage points (=0.1521-0.4328*0.16) with respect to control individuals in the same industry. However, an affected individual in the base industry (SIC64) could expect an increase of 15 percentage points (the uninteracted treatment effect) with respect to a control individual also in the base industry. The figures show that the treatment effect is quite sharply attenuated in the higher-paying industries. Only if there is a negative correlation between employers' market power in the product and labour markets, would our estimated coefficient to the interaction term lend support to the new training theory as summarised in Acemoglu and Pischke. While we believe this is likely to be the case, as shown in Stewart (1990) for example, the estimated coefficient to our interaction term is statistically insignificant and so we do not wish to push this interpretation.

[32] The industry premia were calculated as the estimated coefficient on the 2 digit industry dummies relative to 'Retail Distribution' (2 digit industry 64) in the regression of log real wage on age, age squared, tenure, tenure squared, Travel-to-Work-Area (TTWA) unemployment rate, and binary indicators for TU coverage, highest education, gender, marital status, occupation, charity status, firm size, fixed term contract, temporary contract, year, region. The model was estimated using pooled data comprising 26,286 individual-years from waves 5-10. The mean number of observations per 2-digit cell in this equation was 1156. The mean premium (relative to retail distribution) in this pooled sample was 0.28 log points. Individuals in the estimating sample of Table 10 were given a premium based on their industry affiliation in wave 8. This premium and its interactions were entered as levels in the differenced LPM.

5.3. Gender differences in the effect of NMW

Given that the treatment group was dominated by women, we extend the basic model to allow for differential effects by gender. The estimated results are presented in columns (1) and (2) of Table 11. Because of small cell sizes we were only able to estimate this extension in the treatment/control group 1 variant. As seen in columns (1) and (2) of Table 11, we do not find any significant (at conventional levels) gender differences in the effect of the NMW.

5.4. Disaggregating Treatment Group 1 into Movers and Stayers

Since treatment group 2 was defined on a sub-sample of stayers, we next report results of disaggregating our treatment group 1 into movers and stayers, so that we can then compare the estimates of the stayers across the two treatment groups. The results, presented in columns (3) and (4) of Table 11, do not indicate that treatment group 1 movers behaved significantly differently to stayers. The estimated treatment effects should be compared to those in columns (6) and (8) of Table 8. They are similar in magnitude, though statistically insignificant at conventional levels.

6. Conclusions

In this paper we used important new training and wage data from the British Household Panel Survey to estimate the impact of the new national minimum wage on the work-related training of low-wage workers. We used two 'treatment groups' for estimating the impact of the new minimum wage – those workers who explicitly stated they were affected by the new minimum and those workers whose derived 1998 wages were below the minimum. Using difference-in-differences techniques for the period 1998 to 2000, and information on training incidence and intensity, we found no evidence that the introduction of the minimum wage reduced the training of affected workers, and some evidence that it increased it. In particular we found that the probability of training incidence/intensity increased by about 8 to 11 percentage points for the affected workers. Consequently our findings can be interpreted as providing no evidence in support of the orthodox human capital model as it applies to work-related training, and weak evidence of the new theories based on imperfectly competitive labour markets. Finally, our estimates suggest that two of the goals of the UK government – improving wages of the low paid and developing their skills – have been compatible, at least for the introductory rates of the national minimum wage.

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Table 2- Summary of treatment and control group definitions

Definition	Selection variable	Treatment Group	N	Control Group	N	'High-wage' Group	N	Total N
1	Derived wage in wave 8, calculated as wage = (12/52) * (PAYGU/[JBHRS + 1.5*PDOT]), where PAYGU is usual gross pay per month, JBHRS is usual standard weekly hours and PDOT is usual paid overtime weekly hours.	Wage < NMW	259	NMW ≤ Wage < 1.15*NMW	221	Wage ≥ 1.15*NMW	2777	3257
2	"Has your pay or hourly rate in your current job been **increased** to bring you up to the National Minimum Wage or has it remained the same? Variable INMWPACH (wave 9 only, asked if respondent did not change jobs btw 1/4/99 and interview)	Answer: yes	99	Answer: no	2405	-	-	2504

Notes: (i) N is number of individuals in each group; (ii) Since the second definition of what constitutes a treatment group and what a control group is **not** based on information on wages, we do not define a 'high-wage' group here.

Table 3 - Sample means in wave 8 - Pre NMW period

	Treatment group 1 (1)	Control group 1 (2)	High-wage group 1 (3)	Treatment group 2 (4)	Control group 2 (5)
Sample Size	259	221	2777	99	2405
Derived hourly wage[b] (£)	2.82	3.82	8.96	3.84	8.45
Total weekly hours	31.98	34.86	40.58	29.55	39.91
Training incidence	0.16	0.19	0.29	0.10	0.28
Training intensity (days)	2.59	6.49	5.24	2.26	4.99
Age (years)	37.53	36.92	37.43	39.30	38.34
Experience (years)	15.31	15.13	16.65	17.28	17.25
Tenure (years)	3.78	3.67	4.67	4.77	4.91
Female	0.78	0.69	0.46	0.84	0.48
Part time	0.44	0.39	0.12	0.46	0.15
O level	0.26	0.26	0.21	0.27	0.21
A level	0.13	0.15	0.15	0.13	0.14
Vocational qualification	0.18	0.21	0.31	0.19	0.31
University degree	0.03	0.03	0.17	0.01	0.16
Fixed term contract	0.03	0.03	0.03	0.00	0.02
Temporary contract	0.08	0.03	0.02	0.04	0.01
Manager	0.02	0.06	0.17	0.04	0.16
Professional	0.02	0.02	0.13	0.01	0.12
Non manual	0.37	0.36	0.39	0.38	0.39
Skilled manual	0.39	0.42	0.27	0.32	0.28
Changed employers	0.34	0.31	0.17	0.21	0.17
Married/cohabiting	0.70	0.73	0.75	0.63	0.77
TU covered	0.27	0.32	0.54	0.21	0.53
Public sector	0.12	0.14	0.29	0.12	0.28
Charity sector	0.04	0.02	0.03	0.03	0.03
Small firm size (1-49)	0.72	0.61	0.41	0.80	0.43
Medium firm size (50-499)	0.20	0.29	0.39	0.12	0.38
Large firm size (>500)	0.07	0.09	0.19	0.05	0.19
Energy and water	0.00	0.00	0.02	0.00	0.01
Extraction, chemicals	0.00	0.02	0.04	0.02	0.04
Metal goods	0.03	0.06	0.11	0.02	0.10
Other manufacturing	0.09	0.10	0.09	0.10	0.09
Construction	0.02	0.03	0.04	0.02	0.04
Dist, hotels, catering	0.47	0.42	0.14	0.52	0.16
Transports	0.05	0.06	0.07	0.02	0.07
Banking & finance	0.07	0.05	0.16	0.03	0.14
Other services	0.26	0.26	0.34	0.27	0.34

Table 3 - Continued

	Treatment group 1 (1)	Control group 1 (2)	High-wage group 1 (3)	Treatment group 2 (4)	Control group 2 (5)
TTWA unemployment rate	0.04	0.04	0.04	0.05	0.04
TTWA Labour force>500k	0.08	0.06	0.14	0.12	0.12
2-digit industry wage premia	0.15	0.17	0.28	0.14	0.27

Notes:

(a) See Table 2 for definitions of treatment/control groups.

(b) The derived wage is calculated as wage = (usual gross pay per month) / [(usual standard weekly hours) + 1.5*(usual paid overtime weekly hours)] *(12/52).

(c) All wages are in 1999 prices (deflated by RPI).

(d) The educational qualification variables are as follows: 'O-level' denotes highest qualification is one or more 'Ordinary'-level qualifications (later replaced by GCSE), usually taken at the end of compulsory schooling at age 16. 'A-level' denotes highest qualification is one or more 'Advanced'-level qualifications, representing university entrance-level qualification typically taken at age 18. Vocational denotes HND, HNC, teaching, other higher qualification, nursing. 'University degree' denotes first or higher level university degree.

Table 4 - Comparison of treatment groups 1 and 2 for individuals with valid treatment group 2 indicator

		Treatment group 1 (derived wage in wave 8 less than the NMW)	
		Yes	No
Treatment group 2 (individual reported that the wage had increased because of NMW)	Yes	53	46
	No	136	2269

Note: individuals have a valid treatment group 2 indicator if they did not change jobs between 1st April and their interview in wave 9; otherwise they were not asked the question.

Table 5 - Means of training incidence, training intensity and the derived wage

Treatment / control group definition	Wave	Treatment group					Control group					High-wage group				
		Training incidence	Training intensity	Derived wage	Annual Wage growth (%)	N	Training incidence	Training intensity	Derived wage	Annual Wage growth (%)	N	Training incidence	Training intensity	Derived wage	Wage growth (%)	N
1 (based on derived wage in wave 8)	8	0.162	2.591	2.819		259	0.190	6.495	3.817		221	0.291	5.242	8.959		2777
	9			3.933	0.395				4.453	0.167				9.294	0.037	
	10	0.243	4.833	4.402	0.119	259	0.181	2.836	4.812	0.081	221	0.317	5.335	9.576	0.030	2777
2 (based on whether wage increased to NMW)	8	0.101	2.257	3.837		99	0.282	4.993	8.470		2405					
	9			4.353	0.134				8.843	0.044						
	10	0.172	6.508	4.583	0.053	99	0.303	4.808	9.127	0.032	2405					

Notes: (i) Wave 8 refers to the pre-NMW period and Wave 10 to the post-NMW period.

Table 6 - Summary of changes in the training incidence between Waves 8 & 10.

Treatment/control definition		0 to 0	0 to 1	1 to 0	1 to 1	Total
1 (based on derived wage in wave 8)	Treatment	173 (66.8)	44 (17.0)	23 (8.9)	19 (7.3)	259 (100.0)
	Control	153 (69.2)	26 (11.8)	28 (12.7)	14 (6.3)	221 (100.0)
	High-wage	1497 (53.9)	471 (17.0)	401 (14.4)	408 (14.7)	2777 (100.0)
2 (based on whether wage increased to NMW)	Treatment	77 (77.8)	12 (12.1)	5 (5.1)	5 (5.1)	99 (100.0)
	Control	1342 (55.8)	384 (16.0)	335 (13.9)	344 (14.3)	2405 (100.0)

Table 7 - Summary of changes in the training intensity between Waves 8 & 10.

Treatment/control definition		No change (zero intensity)	Increase	Decrease	No change (+ve intensity)	Total
1 (based on derived wage in wave 8)	Treatment	173 (66.8)	55 (21.2)	29 (11.2)	2 (0.8)	259 (100.0)
	Control	153 (69.2)	34 (15.4)	34 (15.4)	0 (0.0)	221 (100.0)
	High-wage	1497 (53.9)	652 (23.5)	600 (21.6)	28 (1.0)	2777 (100.0)
2 (based on whether wage increased to NMW)	Treatment	77 (77.8)	17 (17.2)	5 (5.1)	0 (0.0)	99 (100.0)
	Control	1342 (55.8)	539 (22.4)	499 (20.8)	25 (1.0)	2405 (100.0)

Table 8 - The effect of the NMW on training

	Raw difference-in-difference				Regression adjusted			
	ΔT		ΔT^*		ΔT		ΔT^*	
	Treatment/control group		Treatment/control group		Treatment/control group		Treatment/control group	
	1	2	1	2	1	2	1	2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment group	0.0901**	0.0503	0.1004**	0.1046**	0.0785*	0.0422	0.0876*	0.0984**
	(1.98)	(1.18)	(1.97)	(2.19)	(1.72)	(1.00)	(1.71)	(2.02)
High-wage group	0.0343		0.0187		0.0392		0.0242	
	(0.98)		(0.47)		(1.12)		(0.61)	
Intercept	-0.0090	0.0204*	-0.0000$^+$	0.0166	-0.0706	-0.0418	-0.0790	-0.0663
	(0.27)	(1.83)	(0.00)	(1.24)	(1.41)	(0.94)	(1.36)	(1.26)
Observations	3257	2504	3257	2504	3257	2504	3257	2504
R-squared	0.00	0.00	0.00	0.00	0.01	0.01	0.01	0.01

Notes:
(a) Absolute robust t statistics in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%
(b) Regression adjusted estimates are from model that has the following additional controls in first differences: for age squared, part-time status, whether the job is fixed-term or temporary, whether the worker changed employers, marital status, whether the job was unionised, whether it was in the public or charity sector, firm size, 1-digit industry, local unemployment rate, and dummies for missing values in any of these variables.
(c) See Table 1 for the definition of the dependent variables ΔT and ΔT^*.
($^+$) The estimated standard error for this coefficient is 0.04.

Table 9 - The effect of the NMW on training: alternative treatment/control group thresholds

	Wider Treatment Group[b]		Wider Control Group[c]	
	ΔT (1)	ΔT^* (2)	ΔT (3)	ΔT^* (4)
Treatment group	0.0133	0.0228	0.0790*	0.0818*
	(0.33)	(0.50)	(1.89)	(1.73)
High-wage group	-0.0030	-0.0223	0.0410	0.0184
	(0.09)	(0.61)	(1.37)	(0.53)
Constant	-0.0309	-0.0370	-0.0713	-0.0733
	(0.64)	(0.66)	(1.51)	(1.33)
Observations	3257	3257	3257	3257
R-squared	0.01	0.01	0.01	0.01

Notes:

(a) Also see Notes (a),(b), and (c) to Table 8.

(b) Individuals are classified into the groups on the basis of their wave 8 derived wage as follows: treatment group - wage less than the NMW+8%; control group - wage between NMW+8% and NMW+24%; high-wage group – wage more than NMW+24%.

(c) Individuals are classified into the groups on the basis of their wave 8 derived wage as follows: treatment group - wage less than the NMW; Control group - wage between NMW and NMW+20%; high-wage group – wage more than NMW+20%.

Table 10 – Estimates from Models that Control for Imperfect Competition in the Labour/Product Market

	Regression adjusted				Regression adjusted			
	ΔT		ΔT^{*}		ΔT		ΔT^{*}	
	Treatment/control group		Treatment/control group		Treatment/control group		Treatment/control group	
	1	2	1	2	1	2	1	2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment group	0.0931*	0.0545	0.0984*	0.1007**	0.1521**	0.0980	0.1487**	0.1186*
	(1.95)	(1.24)	(1.83)	(1.99)	(2.13)	(1.54)	(2.00)	(1.70)
High-wage group	0.0324		0.0222		0.1176**		0.1350**	
	(0.89)		(0.54)		(1.97)		(2.13)	
Binary indicator for TTWA Lab force > 500k	0.0816	0.0902**	0.0663	0.0500				
	(0.61)	(2.51)	(0.49)	(1.18)				
Lab force>500k * treatment group	-0.1875	-0.0909	-0.1428	-0.0130				
	(1.15)	(0.61)	(0.83)	(0.08)				
Lab force>500k * high-wage group	0.0083		-0.0208					
	(0.06)		(0.15)					
2-digit Industry Premium+					0.5030**	0.1280	0.5892**	0.1183
					(2.04)	(1.42)	(2.12)	(1.12)
Premium*treatment group					-0.4328	-0.2830	-0.3366	-0.0386
					(1.23)	(0.88)	(0.85)	(0.10)
Premium*high-wage group					-0.4813*		-0.6309**	
					(1.85)		(2.14)	
Intercept	-0.0703	-0.0446	-0.0801	-0.0680	-0.1562**	-0.0774	-0.1793**	-0.0992*
	(1.39)	(1.01)	(1.36)	(1.29)	(2.39)	(1.49)	(2.53)	(1.65)
Observations	3257	2504	3257	2504	3257	2504	3257	2504
R-squared	0.02	0.02	0.01	0.01	0.01	0.01	0.01	0.01

Notes:
Also see Notes (a), (b), and (c) to Table 8.

+ Industry Premia are calculated as the estimated coefficient on the industry dummies relative to 'Retail Distribution' in the regression of log real wage on TU coverage, age (sq), tenure (sq), education, gender, marital status, occupation, charity status, firm size, fixed term, temp contract, TTWA unemployment rate, year dummies, region, 2 digit industry dummies. The sample was 26286 individual-years from waves 5-10. The mean number of observations per 2 digit cell in this equation was 1156. The lowest paying industry was retail distribution (SIC64). After the normalisation the mean premium was 0.28 log points. Individuals are given a premium based on their industry affiliation in wave 8.

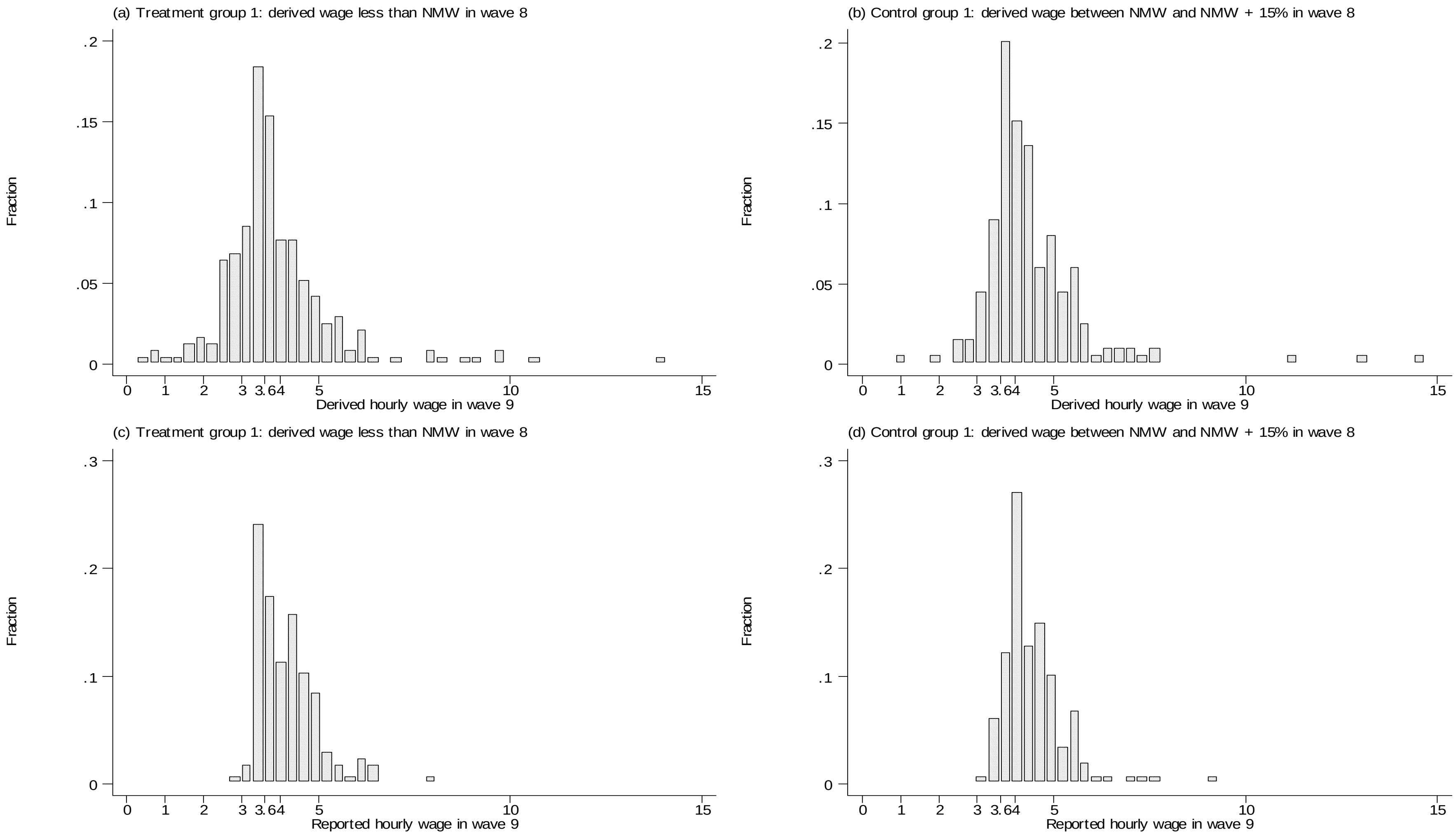
Table 11 - The effect of the NMW on training: Gender and Job Mover/Stayer Effects

	Regression adjusted		Regression adjusted	
	ΔT Treatment/Control Group 1	ΔT^* Treatment/Control Group 1	ΔT Treatment/Control Group 1	ΔT^* Treatment/Control Group 1
	(1)	(2)	(3)	(4)
Treatment group	0.1064** (2.22)	0.1111** (2.05)	0.0529 (1.01)	0.0807 (1.36)
High-wage group	0.0594 (1.61)	0.0458 (1.08)	0.0571 (1.38)	0.0516 (1.12)
Male*treatment group	-0.1278* (1.67)	-0.1073 (1.24)		
Male*high-wage group	-0.0375* (1.76)	-0.0402 (1.56)		
Mover			0.0693 (0.94)	0.0973 (1.15)
Mover*treatment group			0.1035 (1.00)	0.0364 (0.31)
Mover*high-wage group			-0.0583 (0.75)	-0.0919 (1.02)
Constant	-0.0660 (1.32)	-0.0748 (1.28)	-0.1006* (1.80)	-0.1156* (1.80)
Observations	3257	3257	3257	3257
R-squared	0.01	0.01	0.01	0.01

Notes:
(a) Also see Notes (a),(b), and (c) to Table 8.
(b) Mover is a binary indicator which =1 if the individual changed job between 1/4/99 and the wave 9 interview and zero otherwise. This dummy identifies the subgroup excluded from the analysis based on group definition 2 (753 individuals).

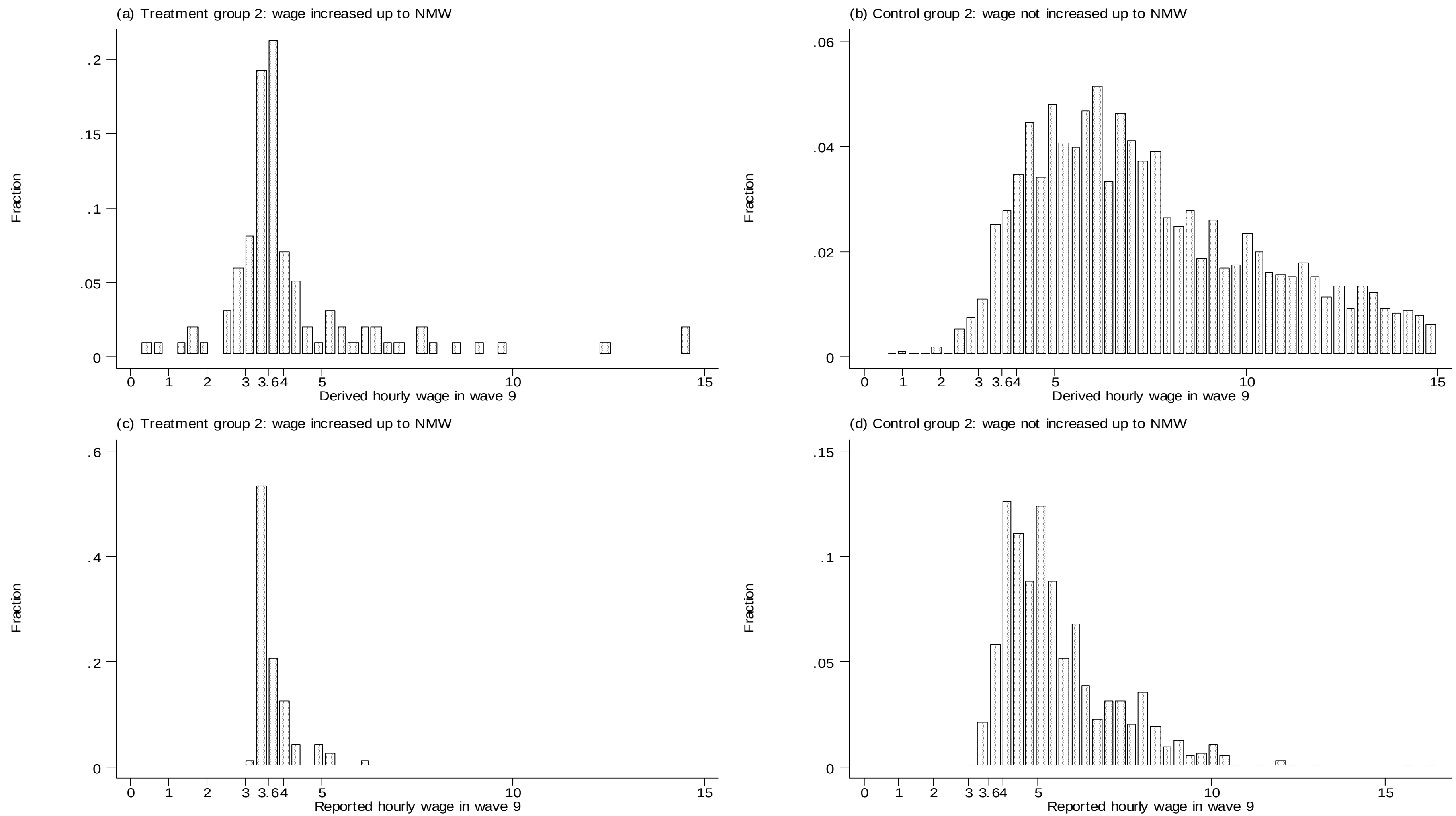
Graph 1

Derived and reported hourly wage distributions in wave 9 for treatment and control groups 1



Graph 2

Derived and reported hourly wage distributions in wave 9 for treatment and control groups 2



Appendix A.: Form of Relevant Training Questions in the BHPS, Waves 8-10

(<u>Apart from the full-time education you have already told me about:</u>) Have you taken part in any other training schemes or courses at all since September 1st last year or completed a course of training, which led to a qualification? Please include part-time college or university courses, evening classes, training provided by an employer either on or off the job, government training schemes, Open University courses, correspondence courses and work experience schemes.
EXCLUDE LEISURE COURSES
INCLUDE CONTINUING COURSES STARTED BEFORE SEPTEMBER 1st 1997

D69. How many training schemes or courses have you done since September 1st 1997, including any that are not finished yet?
EXCLUDE FULL-TIME COURSES ALREADY MENTIONED
WRITE IN NUMBER

I would like to ask some details about all of the training schemes or courses you have been on since September 1st last year, (other than those you have already told me about), starting with the most recent course or period of training even if that is not finished yet.

Where was the main place that this course or training took place? (Write in place.)

Was this course or training. . .
To **help** you get started in your current job?.......
To **increase** your skills in your current job for example by learning new technology?.............
To **improve** your skills in your current job?........
To **prepare** you for a job or jobs you might do in the future?............
To **develop** your skills generally?.......................

Since September 1st last year how much time have you spent on this course or training in total?

Hours....................	1
Days......................	2
Weeks...................	3
Months..................	4
Other **(SPECIFY)**..	5

Which statement or statements on this card describe how any fees were paid, either for the course or for examinations?

No fees.........................	01
Self/family...................	02
Employer/future emp...	03
New Deal scheme.........	05
Training for work, Youth/Emp training/ TEC...	06
Other arrangement **(SPECIFY)**	

Was there a course or qualification designed to lead directly to a qualification, part of a qualification, or no qualification at all?

Did you actually get any qualification from this course or training since September 1st last year?