The Australian National University
Centre for Economic Policy Research
DISCUSSION PAPER

Training, minimum wages and the earnings distribution

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DISCUSSION PAPER NO. 537
October 2006

ISSN: 1442-8636
ISBN: 1 921262 08 7

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Acknowledgements
This paper was presented at a workshop in honour of Tony Atkinson at Nuffield College Oxford on 15 September 2006. A version of the paper is forthcoming in *Inequality and Poverty Re-examined*, S.P. Jenkins and J. Micklewright (eds), Oxford University Press, Oxford, 2007.

ABSTRACT

In this paper we highlight the relevance of work-related training to the minimum wage debate. We initially situate training incidence within the broader picture of the earnings distribution in Britain and demonstrate that lower-paid workers are less likely than workers towards the top of the hourly wage distribution to receive work-related training. We then show that work-related training is potentially important from a distributional standpoint, since it significantly increases individuals' longer-term earning prospects. Next we report empirical results indicating that the introduction in 1999 of a national minimum wage in Britain had a small but statistically significant positive effect on subsequent training incidence for affected workers. In conclusion, we note that the available empirical evidence for Britain shows that minimum wages (i) are associated with a small increase in work-related training for the low paid and (ii) have not adversely affected the employment of British workers. We therefore suggest that the minimum wage has the potential to reduce wages inequality in the longer-term provided that it continues to be set at a level that does not threaten employment. This potential arises not just because of the direct and obvious effect of a minimum wage in increasing wages at the bottom of the distribution, but also through its more indirect effect on work-related training.

JEL Classification: J24, J31

Keywords: work-related training, national minimum wage, earnings distribution

> "The Government believes that work is the best route out of poverty and is committed to making work pay by improving incentives to participate and progress in the labour market. Through the Working Tax Credit and the National Minimum Wage, the Government has boosted in-work incomes, improving financial incentives to work and tackling poverty among working people." (HM Treasury 2006: 92)

Work is widely seen as representing the "best route out of poverty", as the quotation above makes clear. In the UK in 2004-5, working-age adults had a 48 per cent chance of being poor if they belonged to a workless household, compared to only a 23 per cent chance if they lived in a working household (DWP 2006: 71).[1] Nevertheless, having a job may not be sufficient to lift low skilled workers and their families out of poverty; in fact most of the working-age poor (57 per cent) are in households that contain one or more working adults (DWP 2006: 68). This suggests that, as well as getting people into work, an effective anti-poverty strategy will need to increase the earnings of low-skilled individuals when they do work. This in turn will further increase the incentives to participate.

In recent years in the UK, policy to increase low earnings has been based on the introduction of a national minimum wage and the expansion of in-work tax credits (see Brewer and Shephard 2004 and Sutherland 2001). These measures provide a direct boost to earnings and thereby help reduce poverty and inequality. But the longer term prospects of low-paid workers are also likely to depend on improving their skills.[2]

Our aim in this chapter is look at work-related training in relation to the overall wages distribution and in particular to focus on the interaction between the national minimum wage and the training of low-paid workers. 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. If the labour market for the low paid is instead imperfectly competitive and workers are credit constrained, then a minimum wage can increase investment in the general component of training (Stevens 1994; Chang and Wang 1996; Acemoglu and Pischke 1999; Booth and Zoega 2004).[3] This arises because 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. Since the introduction of a minimum wage acts to compress wages, it can induce employers to train their unskilled workers (Acemoglu and Pischke, 2003).

Our chapter is set out as follows. In Section 12.1, we demonstrate that work-related training is disproportionately received by workers who are towards the middle and top of the wages distribution. In Section 12.2, we address the issue of whether or not training affects the longer-term job prospects of British workers. We find, using British panel data, that training has statistically significant positive effects that do not seem to decline over our seven-year estimating period. In Section 12.3, we summarize the results of our earlier work (Arulampalam *et al.* 2004a; Bryan 2005) that uses difference-in-difference methods to estimate whether or not the 1999 introduction of the national minimum wage in Britain had an adverse effect on the training of low-paid workers. Overall, there is no evidence that the minimum wage introduction reduced the training of affected workers, and some evidence that it increased it by around 8 to 11 percentage points. The findings do not support models of training investment based on competitive labour markets, but are consistent with more recent theories involving imperfect labour market competition. In the final section we argue that the minimum wage therefore has the potential to reduce wages inequality in the longer-term *ceteris paribus*, since available empirical evidence for Britain shows that minimum wages (i) are associated with a small increase in work-related training for the low paid and (ii) have not adversely affected the employment of British workers.[4]

12.1 How training incidence varies across the hourly wages distribution

It is well-known that more highly educated workers have a higher probability of receiving work-related training (see *inter alia* Arulampalam *et al*. 2004b, Bassanini *et al.* 2005 and references therein). It is also well-documented that work-related training has a positive effect on wages and year-on-year wages growth (see the survey by Blundell *et al.* 1999). Although to our knowledge no research has documented variation in the *incidence* of work-related training across the *hourly wages* distribution, we did report in Arulampalam *et al*. (2003b: Appendix Table A.2) summary statistics using ten countries from the European Community Household Panel (ECHP).[5] The figures suggested that, in most of the European countries analysed, low-paid workers received substantially less training than their higher-paid counterparts. For Britain, the ECHP data revealed that training participation for men in the bottom fifth of the wages distribution was 43% of training participation of men in the top fifth of the wages distribution, while for women the analogous figure was 47%. This compares with the mean across all countries of 30% for men and 19% for women. The country with the least unequal distribution of training incidence was the Netherlands, being 131% for men and 117% for women, while the most unequal for men was Ireland at 15% and Italy for women at 15%. The highest training incidence countries in our ECHP sample were Britain, Denmark and Finland.

To explore in more detail the variation in training incidence across the British wages distribution, we next report information on training and wages from Waves 8–10 of the British Household Panel Survey (BHPS), conducted over the period 1998 to 2000. This window was chosen to be comparable with the analysis reported in the Section 12.3.[6] The BHPS is a nationally representative survey of private households in Britain. We pool observations from three waves, where our sample includes all employed (private sector and public sector) men and women aged between 18 and 60 years. We then position these men and women within the hourly wage distribution. The hourly wages data for each year are obtained from the annual survey points.[7] Wages were deflated to 1999 levels for comparison later in this chapter with the initial level of the national minimum wage.

We construct, from responses to the training questions, a training incidence variable. This is an indicator variable measuring any training schemes or courses intended to increase or improve skills in the current job, whether employer-provided or not, received by individuals since September 1st in the previous year.[8] It excludes spells of full-time education and leisure courses.

Figure 12.1. Training Incidence for Men and Women across the Hourly Wages Distribution

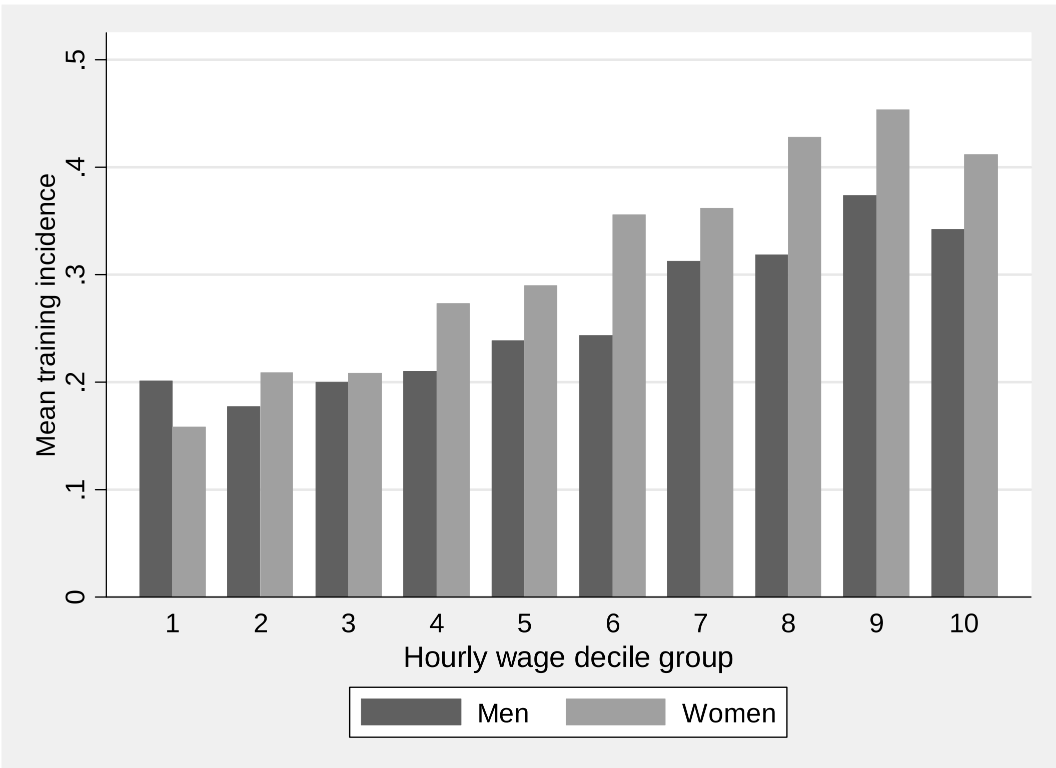

Figure 12.1 shows the proportion of men and women within each hourly wage decile group who receive training to increase or improve their skills in the current job. The data are also reported in Table 12.1, together with the hourly wage in each decile group. Inspection of the first column of Table 12.1 reveals that, in the bottom decile group of the hourly wage distribution, training incidence is 17 per cent for all workers. It then increases to around 20 per cent at the 2nd and 3rd deciles, and continues to increase monotonically across deciles up to a peak of just over 40 per cent at the 9th decile before declining to 36 per cent at the 10th decile. Average training incidence for the entire sample is 28.5 per cent and there are 12,531 person-year observations.

Table 12.1. Receipt of training across the Hourly Wages Distribution

Hourly wage	Training incidence in past yr (%)			Mean hourly	Percentage
decile group	All	Men	Women	wage (£)	of women
1	17.1	20.1	15.9	2.99	71.6
2	19.8	17.7	20.9	4.08	66.1
3	20.4	20.0	20.8	4.82	55.6
4	24.3	21.0	27.3	5.55	52.9
5	26.6	23.9	29.0	6.34	52.1
6	30.0	24.3	35.6	7.25	50.2
7	33.5	31.3	36.2	8.42	46.6
8	36.6	31.8	42.8	10.05	43.4
9	40.4	37.4	45.3	12.45	38.5
10	36.3	34.2	41.2	20.32	29.9
All	28.5	27.5	29.4	8.22	50.7

Notes: Unweighted data, waves 8–10 pooled; 12531 person-year observations.

Figure 12.1 and Table 12.1 show that the distribution for women is even more skewed. Only 16 per cent of women in the bottom decile group receive training, compared to 45 per cent in the ninth decile group. For men the gradient is less steep, with 20 per cent receiving training in the bottom decile group compared to 37 per cent at the 9th decile. The low incidence of training at the bottom of distribution affects women disproportionately because they are concentrated in this part of the distribution. The table shows that over two thirds of workers in the bottom two decile groups are women and in fact they account for over a quarter of all women (not reported in the table). Although, overall, women have slightly higher training incidence than men (29.4 per cent compared to 27.5 per cent), this reflects the fact that the minority of women in the higher part of the distribution get substantially more training than their male counterparts.

How is this information relevant to minimum wages? Workers covered by the national minimum wage are in the lowest decile group. Figure 12.1 and Table 12.1 indicate that overall they receive less work-related training than any other decile group. In particular, those above the 8th decile are more than twice as likely to receive work-related training as workers in the bottom decile group. And for women (who make up two thirds of UK minimum wage workers, LPC 2005) the gradient is steeper. This lack of human capital acquisition may impede the wage growth of these low paid workers. Whether or not the national minimum wage affects the training incidence of low paid workers is therefore of considerable interest, since it could further advantage or disadvantage them through affecting their wage position.

In section 12.3 we investigate whether or not the introduction of the national minimum wage did affect the training receipt of low-paid workers. If the minimum wage affects training incidence, then in the longer term workers might be indirectly affected by the minimum wage through its training effects, as well as through the more direct employment effects much discussed in the literature. If the introduction of the minimum wage reduces the amount of training of workers at the bottom of the wages distribution, it is likely to worsen the longer-term wage prospects of the low paid. On the other hand, if its introduction increases training incidence of low paid workers, then it has the potential to improve their longer-term wage prospects.

As background to our analysis of the minimum wage effects, the next section provides an illustration, using BHPS data, of how training can affect long-term wage growth.

12.2. A longer-term perspective

In this section we demonstrate the potential longer-term effects of work-related training by estimating its impact on individuals' long-term wage prospects. To do this, we clearly need more waves of data than the three utilized in the analysis in Section 12.1. We therefore use wages data from Waves 7–14 of the BHPS, spanning the period 1997 to 2004. The training data are from Waves 8 to 14.[9] Because we wish to estimate the impact of *accumulated* training events on wages growth over the period 1997 to 2004, our main estimating sub-sample is a balanced panel comprising men and women who were aged 18 to 53 years in 1997 and who were employed at all waves.

Of course, by estimating on this balanced panel, we are of necessity dropping individuals with discontinuous labour market histories over that period. These may be lower-paid individuals, and potentially include some workers who lost their jobs following the introduction of the minimum wage (though as we note below, there is little evidence that the minimum wage has caused job losses). We are therefore estimating on a rather selected sample. To check the sensitivity of our results to this criticism, we also re-estimated our wages growth specifications on a larger sample of individuals including those who moved in and out of work over the period 1997 to 2004. We report on those estimates towards the end on this sub-section.

The preceding analysis in Section 12.1 showed that higher-paid workers get more training, and therefore it would not be surprising to find that workers who do well in the long-term also receive more training over the years. The aim of our analysis is to measure the causal effect of training on wages, whilst netting out the correlation between the two that would exist even if training had no effect. To do this, it is necessary to control for factors which raise wages but are also associated with higher training. Some of these factors, such as education, are observed in the BHPS data but others, like career orientation, are not. Our estimation method is therefore based on *changes* in wages between 1997 and 2003. This technique eliminates unobserved individual-specific effects that are time-invariant, since these are differenced out. The set of characteristics used in the estimation includes age, education, and marital status, occupation, industry, part-time and temporary contract status, trade union coverage and firm-size. In our basic wage equation, the growth of wages is modelled as a function of changes in these characteristics. We also estimate an extended specification which also allows wage growth to depend on the levels of observed characteristics in 1997. The detailed list of controls is given in the notes under Table 12.3.

Accumulated training is measured as the total number of times training was experienced over waves 8–14. We constructed two measures: first, the number of waves in which training was received (accumulated incidence) and, second, the total number of events reported (up to 3 per wave). There are several reasons for using these aggregate measures rather than year-by-year indicators. First, Booth and Bryan (2005) showed, using the BHPS, that most of the wage increase following training is realized when workers subsequently change jobs. We argued that this finding is consistent with imperfections in labour or credit markets allowing training firms to receive some of the return to general training. Alternatively, if it is costly to evaluate general human capital, workers may only receive the full returns to training when they are interviewed for new jobs (Hart and Ritchie 2002). Training may also raise wages indirectly through future promotions (Melero 2004), and some training may not be used immediately or may serve as a basis for future skills acquisition. The way that wages respond to training is likely to differ across course types and jobs, and so the cumulated training measure will reflect the average of these effects over the seven year observation window.

A second consideration is that workers who receive training in one year are much more likely get training again the next year. In our estimating sample, 52 per cent of workers trained in one year also received training the following year, while only 18 per cent of workers who were not trained in the first year got training the next year. Thus training is highly serially correlated and in practice it can be difficult to separate the effects arising from different years (the multicollinearity problem). Finally, if there is measurement error in the training variables (for example, if respondents do not recall some training events), then a cumulative measure may be preferred because it will 'average out' measurement error.

Table 12.2. Receipt of training over waves 8–14

Sample	Accumulated training measure [1]	Mean training (unconditional) [2]	Percentage receiving training [3]	Mean training (conditional) [4]	Minimum training (conditional) [5]	Maximum training (conditional) [6]
Employed all waves 7–14	Incidence	2.00	70.6	2.83	1	7
	Counts	3.56	70.6	5.04	1	20
Employed at least in waves 7 and 14	Incidence	1.95	70.8	2.76	1	7
	Counts	3.45	70.8	4.87	1	20

Notes: Unconditional mean in column [2] calculated over all observations. Conditional mean in column [4] calculated over trainees (individuals receiving one or more training event).

Column [1] of Table 12.3(a) reports the basic estimates of accumulated training incidence for the sample employed at all waves, controlling for changes in their labour market characteristics between waves 7 and 14. Training incidence takes a value of one if an individual has experienced at least one training course between two consecutive waves.

Summing across waves, the maximum value for accumulated training incidence that an individual can accumulate is 7, while the minimum – for someone who has received no training over the entire period – is zero. The estimated coefficient to accumulated training incidence is 0.015 and this is statistically significant at the 5 per cent level. Thus an individual who has, for example, experienced at least one training event each year across all waves will experience wages growth that is over 10 per cent higher than an otherwise identical individual who has experienced no training [i.e. (0.015).(7)=0.105]. Table 12.2 shows that 71 per cent of the sample received training at some point over the period and that mean accumulated training incidence for those receiving any training was 2.8. Thus the average trainee (receiving training in nearly 3 waves) is expected to experience wage growth of over 4 per cent compared to workers who get no training.

Table 12.3. The effect of training on wage growth from wave 7 to wave 14

(a) Sample employed at all waves 7–14

	Excluding base year characteristics [1]	Including base year characteristics [2]	Excluding base year characteristics [3]	Including base year characteristics [4]
Accumulated training incidence	0.0146***	0.0125**		
	(2.95)	(2.24)		
Accumulated training counts			0.0064***	0.0053**
			(2.82)	(2.11)
R-squared	0.128	0.157	0.127	0.157
N	1518	1518	1518	1518

Notes: (a) The dependent variable is the change in the log hourly wage between waves 7 and 14. Controls also included in all specifications are: changes in age, age squared, highest qualification (6 levels), marital status (with gender interaction), whether covered by a trade union, firm size (3 categories), one-digit occupation and industry, public sector affiliation, temporary or fixed-term contract and part-time status. The specifications in columns [2] and [4] also include the levels of these variables at wave 7 and a dummy variable for female gender. (b) Absolute robust t statistics in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

(b) Sample employed at least in waves 7 and 14

	Excluding base year characteristics [1]	Including base year characteristics [2]	Excluding base year characteristics [3]	Including base year characteristics [4]
Accumulated training incidence	0.0132***	0.0127**		
	(2.88)	(2.46)		
Accumulated training counts			0.0061***	0.0058**
			(2.87)	(2.43)
R-squared	0.121	0.140	0.122	0.141
N	2275	2275	2275	2275

Notes: (a) The dependent variable is the change in the log hourly wage between waves 7 and 14. Controls are those of Table 12.3(a) as well as the number of waves 8–13 in which the respondent was missing from the survey, in self-employment, in full-time study, on a government scheme or not in employment (unemployed or not participating). (b) Absolute robust t statistics in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

Column [2] of Table 12.3(a) shows the result when also controlling for the levels of workers' characteristics in wave 7, thus allowing wage growth profiles to differ across, for example, education groups and industries. As shown in the list of controls under Table 12.3(a), this specification also allows for the possibility that women experience different wage growth to men (although this coefficient was not significant). The estimated training coefficient in this extended specification is a little smaller, at 0.013, but still significant at the 5 per cent level.

Of course the value of a training course may drop over time if the embodied skills become obsolete. For this reason we experimented with including accumulated incidence in quadratic form (not reported in the tables). The quadratic term is negative, suggesting diminishing returns, but is not statistically significant.

The specifications reported in Columns [1] and [2] of Table 12.3(a) do not use all the available training information available from the questionnaire. This is because the incidence measure effectively treats someone with one training event between waves the same way as someone with at least three training events (the maximum reported from one wave to the next). In the specifications reported in Columns [3] and [4] we therefore use a richer training measure – accumulated counts. This ranges between 0 and 21. The upper bound of 21 is for individuals who have experienced 3 or more training events across each of the waves, while the lower bound is for individuals who have received no training over the entire sample period. As shown in Table 12.2, individuals experienced 3.45 events on average, with a maximum in the sample of 20 events. The estimated coefficient to accumulated counts, reported in Column [3] of Table 12.3(a), is 0.006. This is statistically significant at the 5 per cent level. For example, someone who has experienced at least three training events each year across all waves will experience wages growth that is over 12 per cent higher than an otherwise identical individual with no training. [i.e. (0.006).(21)]. The expected wage growth of the average trainee (experiencing 5 training events, as shown in Table 12.2) is somewhat lower, at 3 per cent, than for a worker with no training. Column [4] shows that the results are very similar using the extended specification which includes the levels of the explanatory variables in wave 7 (the base year).[10]

As for the equations using accumulated training incidence, we also experimented with including accumulated training counts in quadratic form. Again, the quadratic terms were negative, suggesting diminishing returns, but they were not statistically significant.

Finally, we checked the sensitivity of the results to the inclusion of workers with lower labour market attachment. We re-estimated all specifications using a larger sample which included those additional people who were in employment in both waves 7 and 14, but who did not respond to the survey or who were not in employment in some of the intervening years. To control for these intermittent histories, we added to the estimating equations the number of waves for which individuals did not respond, were self-employed, in full-time study, on a government scheme, or out of employment (non-participating or unemployed). Only being out of employment had a negative effect on wage growth (of 5.5 per cent in absolute terms for each wave of missing employment), and the training effects are almost identical to the previous results, as shown in Table 12.3(b).

In summary, we find, using waves 7 to 14 of the BHPS, that work-related training has a long term effect on wages. Thus training can potentially affect the longer term upward wage mobility of British workers.

12.3. Training and the national minimum wage

12.3.1 Background

The national minimum wage was introduced in the UK on 1st April 1999. Its introduction followed a period of 6 years, from the abolition of the Wages Councils, without any statutory wage-floor except for agriculture. It constituted a major policy intervention aimed at increasing the earnings of low-skilled employees and helping low-income households out of poverty. It has also provided an excellent opportunity for evaluation of the effects of a minimum wage.

Research, most recently Bryan and Taylor (2004), has confirmed that the minimum wage is well targeted at the bottom of the income distribution of working households.[11] In this section we focus on whether the minimum wage also has a more indirect effect on workers' longer-term prospects by affecting their training and therefore potentially their wages. We begin with the theoretical background and then present evidence from the BHPS.

As noted at the start of this chapter, there are good theoretical reasons why minimum wages are expected to affect training. In a perfectly competitive labour market, a binding minimum wage will prevent wages from being lowered to pay for the costs of training. The introduction of a minimum wage is therefore likely to restrict training, in particular general training financed by low-paid workers themselves. However, if the labour market is *imperfectly* competitive, a minimum wage can have the opposite effect by compressing wages at the bottom of wages distribution. Imperfect competition means that the gains in productivity due to training accrue to employers and not to minimum wage workers, and so firms can find it profitable to pay for training. Depending on other factors, such as whether or not workers are credit constrained, the existence of training contracts and the mix of general and specific training, the overall effect of a minimum wage can be to increase training (Acemoglu and Pischke 2003). These differing predictions about training parallel the familiar predictions about employment. In a perfectly competitive labour market a minimum wage is likely to destroy jobs, while in an imperfectly competitive labour market employment may increase.

How do the arguments about training relate to the UK labour market? The possibility that workers share in the cost of major training events – and that a minimum wage might be a disincentive to some forms of training – was recognized in the design of the national minimum wage. The national minimum wage does not apply to young apprentices in their first year, and a lower 'development' rate applies to workers receiving some other forms of accredited training (lasting at least 26 days) in new jobs. The Low Pay Commission (LPC) monitors the workings of the national minimum wage and concluded that, without the apprenticeship exemption, initial training in some sectors would drop (LPC Report 2005: 150). But the LPC has also found little evidence that employers use the trainees' development rate and has recommended that it be abolished (LPC report 2005: chapter 5). At the same time, the Commission recognizes that the national minimum wage might actually spur employers to increase training in order to raise their workers' productivity (see for example, LPC 2001: 60).

So far, there has been very little formal empirical work using representative data to evaluate the overall impact of the national minimum wage on training receipt. Indeed to our knowledge the only empirical work in this area prior to our own was undertaken for the US (see Schiller 1994, Neumark and Wascher 2001, Grossberg and Sicilian 1999, Acemoglu and Pischke 2003, and for a summary see Arulamapalam *et al.* 2003a).

In the remainder of this section, we discuss the results of our earlier work using representative British survey data to examine the effects of the introduction of the national minimum wage on training.

12.3.2 The effect of the UK national minimum wage training receipt

Our data description using the decile approach, reported in Section 12.1, revealed that low paid workers do not receive much training. It is therefore interesting to see what the introduction of the national minimum wage did to them, especially in view of the various theories predicting diverse effects. The studies by Arulampalam *et al*. (2004a) and Bryan (2005) provided the first investigation of the training effects of the UK national minimum wage. The research utilized important new data from the BHPS – on both training and whether or not individuals' wages were increased to comply with the national minimum wage – facilitating a comparison of training evolution across 'affected' and 'unaffected' groups.

The data used were from Waves 8 to 10 (1998–2000) of the BHPS, which spanned the introduction of the national minimum wage and thereby allowed a comparison of training before and after its implementation.[12] As in the above analysis, training was defined to cover courses intended to increase or improve skills in the current job. The outcome variables were the change in training incidence and the change in training intensity. Training intensity was defined as the total duration of the reported training events (up to 3) in each wave.[13]

The Wave 8 interviews of the BHPS took place between August 1998 and March 1999 and covered training received since 1st September 1997. The Wave 10 interviews were conducted between September 2000 and May 2001 and covered training experienced since 1st September 1999. The training reported in these two waves therefore fell unambiguously before and after the introduction of the national minimum wage. Training data were also collected in Wave 9 but could not be used in the analysis, since it was not known whether the reported events fell before or after the introduction of the national minimum wage. The analysis covered employees aged under 60 who were potentially covered by the national minimum wage, that is those aged 18 or over and who were not in the army or agriculture.

The national minimum wage was introduced at a main rate of £3.60, with a youth rate of £3.00 for 18–21 year olds and a development rate of £3.20 for some older trainees (although as already mentioned, there is scant evidence – including in the BHPS data used in the analysis – that the development rate has been widely used). The national minimum wage has been uprated annually since its introduction and the upratings have often exceeded the growth of average earnings. In principle it would be possible to use the upratings as additional tests of minimum wage effects, but in practice they have much less ‘bite’ than the original introduction of the national minimum wage into a labour market without any wage floor. The frequency of the upratings also makes it more difficult isolate ‘before’ and ‘after’ observations corresponding to each national minimum wage increase and not affected by other increases. The analysis presented here is therefore restricted to the introduction of the national minimum wage and does not cover the upratings.

The methodology in Arulampalam *et al.* (2004a) was similar to that of Stewart (2004). Stewart used the BHPS and two other datasets to analyse the employment effects for low-wage workers of the national minimum wage and found no significant evidence of employment effects. The idea in these studies is to compare the training of workers ‘affected’ by the national minimum wage (a treatment group) with a similar group of ‘unaffected’ workers (a control group).

To control for differences between the two groups which might affect the level of training received even in the absence of the national minimum wage, the analysis in Arulampalam *et al.* (2004a) compares the change in training – rather than the level – for each group over the period of the introduction of the national minimum wage (1998–2000). The *difference-in-differences* estimate of the minimum wage effect is then the difference (between the affected and unaffected groups) of the difference in training over time for each group.

It is important to define appropriate treatment and control groups. The treatment group needs to compare workers whose wages were increased by the national minimum wage, while the control group ideally needs to contain workers who are very similar to the treatment group but whose wages were unaffected by the minimum wage. To check the sensitivity of the results to the choice of groups, Arulampalam *et al.* used two alternative treatment/control group definitions. The first (treatment/control group 1), was based on the hourly wage observed just prior to the national minimum wage in Wave 8, comparing workers whose wages would need to increase to comply with the national minimum wage with workers in a wage band just above this level. Treatment and control group 2 were defined using the answers to a question in wave 9 in which respondents were specifically asked whether or not their wages had been raised up to the minimum wage.

Table 12.4. The effect of the national minimum wage on training

	Raw difference-in-difference				Regression adjusted			
	Change in training incidence Treatment/control group		Change in training intensity Treatment/control group		Change in training incidence Treatment/control group		Change in training intensity 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

† The estimated standard error for this coefficient is 0.04.

Notes: (a) Absolute robust t statistics in parentheses; * significant at 10%; ** significant at 5%. (b) Regression-adjusted estimates have the following first-differences controls: age-squared, part-time status, whether the job is fixed-term or temporary, whether the worker changed employers, marital status, union-coverage, sector, firm size, 1-digit industry, local unemployment rate, and dummies for missing values.

The main results of the Arulampalam *et al.* (2004a) analysis are reproduced in Table 12.4. They show difference-in-difference estimates from a linear probability model (LPM) using the two different outcome variables: changes in training incidence, denoted by $\Delta TB_{it,}$ and changes in training intensity, denoted by ΔT_{it}^{*}. The training intensity variable ΔT_{it}^{*} 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. Thus the estimated equation represents a LPM in differences.

The left hand panel of Table 12.4 presents the raw difference-in-difference estimates (without any added control variables), while the right-hand panel also controls for observable personal and job and characteristics. Considering first the raw difference-in-difference estimates, column [1] indicates that the training probability in treatment group 1 (based on the wage in Wave 8) increased by about 9 percentage points more than it did in control group 1, and the increase is statistically significant at the 5 per cent level. Using the treatment and control group 2, column [2] shows that training incidence also increased more in the treatment group (by 5.0 percentage points) than in the control group, although the estimate is not statistically significant at conventional levels. However looking at changes in training intensity, columns [3] and [4] report positive and significant estimates for both treatment/control group definitions. Affected workers appear to be 10 percentage points more likely to experience an increase in training intensity than workers in the control group. The increases are statistically significant.[14]

A similar pattern of results is evident in the right hand panel of Table 12.4 (the regression-adjusted difference-in-difference estimates, controlling for individual and job characteristics), showing that the previous estimates were not caused by (observable) differences between the workers in the sample.[15] Overall, Arulampalam *et al.* (2004a) concluded that there was no evidence that the minimum wage introduction reduced the training of affected workers, and some evidence that it increased it by around 8 to 11 percentage points. These findings provided little evidence supporting the perfectly competitive human capital model as it applies to training, and weak evidence of new theories based on imperfectly-competitive labour markets.

12.4. Conclusions

In this chapter we highlighted the relevance of work-related training to the minimum wage debate. We demonstrated that lower-paid workers are less likely than workers towards the top of the hourly wage distribution to receive work-related training. We then showed that work-related training is potentially important from a distributional standpoint, since it significantly increases individuals' longer-term earning prospects. We summarized our earlier empirical results indicating that the introduction of a national minimum wage in Britain had a small positive effect on subsequent training incidence for affected workers, and we argued that this provided some evidence in favour of new theories based on imperfectly-competitive labour markets.

A number of separate empirical studies have using British data to estimate the *employment* effects of the national minimum wage. These find that the introduction of the minimum wage has had no adverse effects on employment overall (see Stewart 2004), although small employment losses have been detected in one heavily affected sector, care homes (Machin, Manning and Rahman 2003). Moreover, while Draca, Machin and Van Reenen (2006) find some evidence that firms' *profits* may have declined, they find no evidence that firms closed down because of this.

In summary, the available empirical evidence for Britain shows that minimum wages (i) are associated with a small increase in work-related training for the low paid and (ii) have not adversely affected the employment of British workers. Based on the UK's short experience of the national minimum wage it appears that, rather than shedding workers, firms have responded by trying to get more out of existing workers. Interesting questions for future research that have not yet been addressed are: will the effects of the minimum wage continue to be so benign and will the minimum wage reduce wages inequality in the longer-term? The answers will depend partly on future increases in the minimum wage. We would suggest that the minimum wage will reduce wages inequality in the longer-term provided that it continues to be set at a level that does not threaten employment. This potential arises not just because of the direct and obvious effect of a minimum wage in increasing wages at the bottom of the distribution, but also through its more indirect effect on work-related training.

Ultimately, assessing the long-term effects of the minimum wage will require evidence about the progress of low-skilled workers over many years. There is a need for data which contain sufficient numbers of low-skilled individuals and which allow their trajectories – both in and out of the labour market – to be followed over substantial parts of the lifecycle. We hope that continuing developments in panel data resources will allow further investigation of these issues both for the UK and for a wider set of countries than those studied so far.

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[1] Poverty is defined here as a net equivalized disposable household income before housing costs of less than 60% of the median. A working household is a household containing at least one adult in work.

[2] There have been various initiatives to encourage basic training amongst low-paid workers. One of the latest measures is a Train to Gain programme, which from 2006 will offer subsidized training to low-skilled workers in small firms.

[3] General training refers to skills that are useful to other firms as well as to the firm providing the training.

[4] For evidence on the employment effects of the introduction of the national minimum wage in Britain, see *inter alia* Stewart (2002 and 2004) and Machin, Manning and Rahman (2003).

[5] See Arulampalam *et al* (2003b) for full details of how these figures were calculated.

[6] We are constrained to start our analysis of the minimum wage and training at wave 8 of the BHPS since that was when the training questions in the BHPS were altered to elicit more detailed information about specific training events. As discussed in Booth and Bryan (2004) the responses are not directly comparable to those from previous waves. Second, waves 8 and 10 bracket the introduction of the minimum wage in 1999. Stopping our analysis at wave 10 allows us to examine the impact of the introduction of the National Minimum Wage rather than estimating the impact of subsequent increases in the minimum wage, as we explain in greater detail in Section 12.3.

[7] Gross hourly wages were calculated as hourly wages = (usual gross pay per month * 12/52) / [(usual standard weekly hours)+ 1.5*(usual weekly paid overtime hours)].

[8] The survey asks for details of up to three training events received since 1st September last year. The precise question is: “Was this course or training: (i) To help you get started in your current job? (ii) To increase your skills in your current job? (iii) To improve your skills in the

current job? (iv) To prepare you for a job or jobs you might do in the future? (v) To develop your skills generally?" The categories are not mutually exclusive. We combined categories (ii) and (iii) to create variables that measured training intended to increase or improve skills in the current job. Some 85% of reported training events are covered by this definition (see Booth and Bryan, 2004, for a detailed analysis of the training data).

[9] Since the training data are retrospective over the 12 months since the previous survey, it is appropriate to use wages data from wave 7 onwards and training data from wave 8 onward.

[10] In all specifications we also tested whether the returns to training were different for women, by interacting the training variable with a dummy variable for women. The interaction coefficients were statistically insignificant, suggesting no differential returns.

[11] It is less well targeted at the bottom of the income distribution of all households, because the poorest households typically do not contain wage earners.

[12] We used the data from 1998 onwards because major changes to the training questions were introduced in 1998. See Booth and Bryan (2004) for discussion about the differences in the questionnaires and training responses before and after this change. Bryan (2005) 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.

[13] The duration question is: "Since September 1st [of previous fieldwork year] how much time have you spent on this course or training in total?"

[14] We modelled the sign of changes in training intensity rather than the magnitude, since this relates directly to relevant theory. Modelling the exact change in training intensity would require us to address the issue that a change from 8 to 10 days is not necessarily the same as a change from 4 to 2 days or even a change from 2 to 0 days. Such analysis was beyond the scope of the current study.

[15] Separate effects were not estimated for men and women because of sample size limitations. In extensions to the main analysis, Arulampalam *et al.* (2003a) and Bryan (2005) found no evidence for differential effects by gender. These studies also investigated the sensitivity of the estimates to changes in the definitions of treatment group 1 and its control group. The results were similar to those reported here.