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Abstract

Using data for the years 1991–96 from the British Household Panel Survey, the authors investigate how union coverage affected work-related training and how the union-training link affected wages and wage growth for a sample of fulltime men. Relative to non-covered workers, union-covered workers were more likely to receive training and also received more days of training. Among workers who received training, those with union coverage enjoyed greater returns to training and higher wage growth than did those without. While some of these results have been found in previous studies, others are new. The wage results, in particular, suggest a need for rethinking the conventional view that union wage formation in Britain reduces the incentives to acquire work-related training.

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UNIONS, WORK-RELATED TRAINING, AND WAGES: EVIDENCE FOR BRITISH MEN

ALISON L. BOOTH, MARCO FRANCESCONI, and GYLFI ZOEGA*

Using data for the years 1991–96 from the British Household Panel Survey, the authors investigate how union coverage affected work-related training and how the union-training link affected wages and wage growth for a sample of full-time men. Relative to non-covered workers, union-covered workers were more likely to receive training and also received more days of training. Among workers who received training, those with union coverage enjoyed greater returns to training and higher wage growth than did those without. While some of these results have been found in previous studies, others are new. The wage results, in particular, suggest a need for rethinking the conventional view that union wage formation in Britain reduces the incentives to acquire work-related training.

Investments in human capital are central to economic performance and growth. The new growth theory has put them center stage (Lucas 1988; Romer 1990). When tastes and technologies are changing rapidly, such investments (including work-related training) are crucial for keeping labor employed and maintaining high levels of competitiveness. Without a work force that is continually acquiring new skills, it is difficult to reap all the returns from technological progress.

Against this background, we investigate how union coverage affects work-related training and how the union-training link affects wage levels and wage growth for a sample of full-time men in Britain. Our analysis is motivated by recent theoretical developments suggesting—contrary to the

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A data appendix with additional results, and copies of the computer programs used to generate the results presented in the paper, are available from Marco Francesconi at the Institute for Social and Economic Research, University of Essex, Wivenhoe Park, CO4 3SQ, United Kingdom.

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predictions of standard human capital theory—that union-covered workers and firms might face greater incentives to invest in work-related training than do non-covered workers and firms, and that the returns to any such training investment might differ by union coverage status. While limitations of the data used in the empirical analysis prevent us from carrying out a clear test of such theories, we are able to see whether or not the standard human-capital-theory predictions are borne out by our data. We therefore aim to establish a number of new stylized facts about the relationship between union recognition and training and their joint impact on wage levels and wage growth.

That project is important for policy reasons, because if training and union coverage have both separate and combined effects on workers' performance, policies aimed at stimulating skill formation ought to account for the independent effects of the presence of collective bargaining agreements. Similarly, policies that directly affect employment relations and collective bargaining might also need to take into account the wage impact of training programs. Many countries recognize the link between work-related training and performance by subsidizing company training or offering support for a training "market" through loan provision and dissemination of information about good practice. Moreover, trade unions are known to affect labor market outcomes, such as productivity, investments, profitability, and employment. However, the effects of any interplay of unionization and training on wages and wage growth are not yet fully understood by social researchers or embodied, even partially, within labor market policies.[1]

[1]For example, the 1999 Employment Relations Act in Britain requires employers to provide recognized unions with information and access to discussion on training policies and practices. However, it does not emphasize the relationship of these two institutions and their potential effects on outcomes such as profitability, workers' morale, and job mobility.

Previous related studies have focused on the union-training link, on the impact of training on wages, or on the impact of unions on wages. But no study has yet examined the relationship between union coverage and work-related training and how coverage and training jointly affect wage formation and wage dynamics. Such an analysis is obviously complicated by the joint non-random selection of training participants and union workers. In our study, however, the potential endogeneity problem induced by union status is likely to be mitigated by the fact that we are concerned with union coverage rather than membership. The element of individual choice related to coverage is clearly limited in the British context, whereas the decision to become a member of a trade union is possibly determined by constraints, preferences, and unobservable factors, which are themselves influenced by membership status. In our empirical analysis, nonetheless, we address the issue of the joint endogeneity of training and coverage when estimating wages, using various econometric techniques and partitioning our sample into subgroups that, in principle, are less likely to suffer from such endogeneity problems.

Our data are from the British Household Panel Survey (BHPS) for the period 1991–96. Besides a host of worker and job characteristics, the BHPS provides the only contemporary, longitudinal information on work-related training, union status, and individual wages for a representative sample of workers in Britain. Longitudinal data such as these are essential for disentangling at least part of the genuine impact of coverage and training on wages from other effects shared by these three processes.

Conceptual Framework and Related Literature

The implications of unionism for training and pay depend, *inter alia,* on the degree of competition in the labor market and on whether the union effect on training is indirect (through the wage struc-

ture) or direct (through the negotiation of training). Thus the channels through which union collective bargaining can affect training and pay are potentially quite complex, and it is not immediately obvious that unionism will be associated with positive or negative returns to training. In this section we discuss some of the channels through which unions can affect training and training returns.

Otherwise Competitive Labor Markets

First, we consider the degree of competition in the labor market. We define as "otherwise competitive" the situation in which the labor market is perfectly competitive except for union presence. The benchmark case is a perfectly competitive labor market without any trade union presence. According to standard human capital theory, in perfectly competitive labor markets, workers will pay for general work-related training by receiving low training wages, and reap the returns to this investment by receiving higher wages afterward (Becker 1964). Specific human capital theory, on the other hand, predicts that the requirements of efficient turnover for skilled workers will induce the firm and the worker to agree to share both the costs of and returns to the training investment (Hashimoto 1981).

A necessary condition for efficient training investments in competitive labor markets is that wages are set to facilitate such investments. In an otherwise competitive labor market, union reduction of wage dispersion means that wages cannot be lowered during training and increased after training to allow workers to bear the costs and benefits of general training.

Some studies therefore argue that where wages are set collectively by trade unions in an otherwise competitive labor market, wage dispersion is reduced and incentives to invest in general training at the workplace are distorted (for example, see Mincer 1983). In particular, workers and firms will not efficiently invest in such training, and there will be a negative correlation between union presence and work-related training (Duncan and Stafford 1980; Barron, Fuess, and Loewenstein 1987). Furthermore, the pay returns to training will be lower for union-covered workers than for non-covered workers. These predictions are summarized in the first row of Table 1.

Imperfectly Competitive Labor Markets

Next, we define an imperfectly competitive labor market as one characterized by some degree of oligopsony, which may arise through search frictions, workers' stochastic preferences for different firms, and the like. In oligopsonistic labor markets, workers receive wages below their marginal product, and thus their incentives to invest optimally in general training will be lowered. Some of the returns to training will accrue to the training firms, whose incentives to invest are increased.[2] Stevens (1996), Acemoglu and Pischke (1999b), and Booth, Francesconi, and Zoega (2002) identified conditions under which the wage "compression" associated with imperfectly competitive labor markets will increase the incentive for firms to finance general or transferable training.[3] In this context, wage compression implies that post-training productivity is increasing in training intensity at a faster rate than wages. Hence, the gap between productivity and wages is increasing in training intensity and, by definition, a firm's profits over some range. But the amount of training provided in equilibrium will be sub-optimal from society's viewpoint.

In imperfectly competitive labor markets, unionism will have ambiguous effects

[2]A number of studies (Ryan 1980; Jones 1986; Acemoglu and Pischke 1999a; Leuven and Oosterbeek 1999) have shown that firms do incur significant financial costs in providing general training.

[3]This model is directed at sectors of the labor market characterized by a production technology requiring skilled labor. Thus oligopsonistic wage compression will be associated with a greater rate of firm-provided training and lower wage growth for skilled workers relative to unskilled workers in the alternative sector whose technology does not require skilled labor.

on the pay returns to training. For example, Acemoglu and Pischke (1999b) argued that unions cause wage compression in imperfectly competitive labor markets. In their model, unions set wages and the firm determines training. The model predicts that unionism will be associated with increased firm-financed transferable training. However, the pay returns to union-covered workers from such training may be lower if the direct (adverse) effect of unions on wages is stronger than the indirect effect through more training.

In contrast, Booth, Francesconi, and Zoega (2002) modeled the source of wage compression as workers' stochastic preferences for different firms or heterogeneous mobility costs. In this framework, industry-wide unions bargaining directly over training and wages can extract a share of the surplus and give it to workers in the form of more training and higher wages. Consequently, industry-wide unionism will be associated with more transferable training and with higher pay returns from such training. This is because the union is effectively internalizing the friction.[4] These various predictions are summarized in the second row of Table 1.

Union Concern over the Wage-Employment Package

Suppose union utility is increasing in the wages and job security or employment of its members, as is assumed in most models of union behavior. Unions may ensure that covered workers receive higher wages and greater job security by directly intervening in training provision, for example by making sure that workers' skills are deepened or kept up-to-date through more training. Thus training is an instrument through which the union goals of increasing employment and job security are attained. Consequently, powerful trade unions might be willing to negotiate training opportunities for their covered workers that are not ordinarily available to workers in non-covered firms, especially in non-competitive product markets in which the available surplus is larger.[5] Testable predictions from this hypothesis, summarized in row (3) of Table 1, are that union-covered firms will provide more training and higher returns for such training, relative to non-covered firms.

Labor Turnover

It has long been recognized that unions may be instrumental in improving worker morale and organization at the workplace. This means that in establishments where unions are recognized, labor turnover may be reduced (Blau and Kahn 1983; Freeman and Medoff 1984). Consequently, union-covered firms may have greater incentives than non-covered firms to provide training, because they are less likely to lose highly productive trained workers (Booth, Francesconi, and Zoega 2002). Through this mechanism, unionism may be associated with increased training and productivity, and consequently higher wages.[6] The testable predictions of this hypothesis are that union-covered firms train a greater proportion of their workers and give each worker more training, because covered workers are characterized by lower turnover, *ceteris paribus*. Thus the training returns for covered workers will be higher than for non-covered workers because of

[4]In another context, Booth and Chatterji (1998) showed that union-firm wage bargaining can prevent ex-post monopsonistic wage-setting by firms and can thereby reduce inefficient quits.

[5]A referee has raised the possibility that a union might comprise heterogeneous membership that is differentiated by skill level. This might have different implications for the model outlined in the text depending on the skill level of the median voter. See Ryan (1994) for further discussion of this approach.

[6]See Booth (1995) for a review of the empirical evidence on unions and productivity for Britain and the United States. Furthermore, analyzing a panel of British industries between 1983 and 1996, Dearden, Reed, and Van Reenen (2000) found that higher training is systematically associated with higher productivity.

their greater training intensity (holding tenure and all else constant). These predictions are summarized in the fourth row of Table 1.

Unions' Use of Training to Control Labor Supply

It may be the case that union-covered workers, in contrast to their non-covered counterparts, hold jobs characterized by an apprenticeship structure, whereby individuals accept lower starting wages in return for the opportunity to be trained and receive higher post-training wages. Indeed, union organization in Britain originally developed on a craft (or occupational) basis, and only later along industrial lines. A traditional strategy of British craft unions was to influence access to training (typically youth access to apprenticeship) as a means for determining labor supply, as well as for monitoring the quality of training provided (Webb and Webb 1898; Ryan 1994). To the extent that some elements of this strategy have persisted into the present, we may still observe this channel of influence for specific groups of workers, such as apprentices or young and inexperienced employees. However, the implications for training and training returns are ambiguous. For example, union control over the *number* of trainees might result in a negative association between unions and training receipt. Trainee numbers might be restricted to increase labor scarcity, thereby lowering incidence but increasing training returns. Conversely, union control over the *quality* of training might result in a positive association between unions and training incidence and intensity (more and better training per worker to sustain occupational standards) and also higher wage growth. These various predictions are summarized in the fifth row of Table 1.

Selectivity and Other Issues

It is often argued that in firms that become unionized, management responds to higher union wages by more carefully vetting new hires in order to have a better-quality work force. This implies the selection, by union-covered firms, of higher-ability workers and perhaps also the boosting of their skills. In addition, from the labor supply side, an implication of some of the theories above is that better-quality workers, or more motivated workers, might self-select into union jobs if the training opportunities and returns are higher in the union-covered sector. For example, if unions bargain directly over training as well as wages, only workers who are able to benefit from such training (that is, those for whom the costs of training are lower) will wish to queue for union jobs, or will be offered such jobs.

These predictions are summarized in the last row of Table 1. Notice that they suggest that any observed link between unions, training, and training returns may simply be spurious. This underscores the need for our empirical analysis to control not only for potential self-selection into training, but also for potential self-selection into union coverage. We shall return to this issue below. We would, however, point out that in Britain union coverage is attached to the job and not the individual, and thus the issue of selection is somewhat mitigated.

Several of the above hypotheses regarding the impact of unionism on training and training returns are observationally equivalent given the data used in our empirical analysis, as inspection of the last column of Table 1 makes clear. Thus the hypotheses summarized in rows (2.ii), (3), (4), and (5.ii) all predict more training and higher training returns for union-covered workers than for non-covered workers. The "otherwise competitive" model (row 1) stands out as the only hypothesis considered that predicts (a) a negative correlation between union presence and training and (b) lower training returns for union-covered workers than for non-union-covered workers, *ceteris paribus*.

Furthermore, the hypotheses are not mutually exclusive. For instance, higher job retention among union-covered workers than among non-covered workers may

Table 1. Testable Predictions of Various Hypotheses.

Model	*Description*	*Empirical Predictions for Individual-Level Data*
[1] Otherwise competitive	Unions flatten wage profiles, reducing wage dispersion and distorting workers' incentives to invest in training.	Negative correlation between union presence and training. Training returns lower for union-covered than non–union-covered workers.
[2] Oligopsonistic labor market	[i] Wage compression associated with unions means that firms are more likely to finance training.	Union-covered workers receive sub-optimal levels of training. Ambiguous predictions as to the training returns of union-covered relative to non–union-covered workers.
	[ii] Unions bargain at industry-level directly over wages and training.	Union-covered workers receive more training and higher training returns than do non-covered workers.
[3] Union concern over the wage and employment package	Unions directly negotiate better training opportunities for covered workers, especially in non-competitive product markets where the available surplus is larger.	Union-covered workers receive more training and higher training returns than do non-covered workers.
[4] Turnover	Because unions reduce turnover, they have an indirect effect: union firms train more workers and each worker gets more training.	Union-covered workers receive more training than do non-covered workers, and also higher training returns, owing to their greater training intensity.
[5] Union control over supply of labor	[i] Control over the number of trainees reduces the supply of trained workers, lowering incidence but increasing returns.	Negative correlation between unions and training incidence. Training returns for union-covered workers are greater than for non-union workers.
	[ii] Control over the quality of trainees may lead to more and better training per worker to sustain occupational standards.	A positive association between unions and training intensity and also higher wage growth.
[6] Selection models	Union firms more carefully vet new hires, who are thus on average of better quality.	More training and greater pay returns for union-covered workers, but this reflects their higher unobserved ability/quality. Controlling for unobserved ability should eliminate this effect.

be at work in imperfectly competitive labor markets. Similarly, apprenticeship types of training may go hand in hand with oligopsonistic labor markets. The implications of these hypotheses for the incidence, intensity, and pay effects of training may also not be as clear-cut as discussed above. In general, they depend on labor market structure and whether the union effect is direct (through the wage structure) or indirect (through negotiation of training packages). For example, if unions improve worker morale and job organization and thereby reduce labor turnover, but operate in an imperfectly competitive labor market in which we observe increasing wage compression (à la Acemoglu-Pischke), then the wage returns to training become lower for covered than non-covered workers. In addition, in this case, the effect of unions on training may be negative if the cost to the firm of training is greater than the benefit

of retaining the trained workers, and the rents earned from trained workers fall.

Therefore, testing each of the proposed explanations against the others is beyond the scope of this paper and left for future research with richer data. But we can ascertain whether the implications of standard human capital theory in otherwise competitive labor markets are borne out by our data. In addition, to try to better understand the link between training and union coverage and, especially, the reasons for any training difference between covered and non-covered workers that may have existed in Britain during the 1990s, we undertake at least a partial exploration of some of the alternative explanations.

A number of previous studies using British data have found a positive correlation between work-related training incidence and measures of union presence, such as union coverage for collective bargaining, union recognition, and union density (Booth 1991; Greenhalgh and Mavrotas 1992; Arulampalam and Booth 1998; Green, Machin, and Wilkinson 1999). The evidence for the United States draws from a larger body of empirical research and is mixed. Some of the early U.S. studies found a negative impact of unions on training (Duncan and Stafford 1980; Mincer 1983). More recent studies, however, have found that the probability of receiving on-the-job training and the amount of work-related training received are higher for unionized workers than for non-unionized workers (Lynch 1992; Veum 1995; Osterman 1995; Frazis, Herz, and Horrigan 1995). An exception is Lynch and Black (1998), which, using data from a 1994 representative survey of U.S. establishments, reported no statistically significant impact of unionization on either the provision of formal training or the proportion of workers receiving it.

To our knowledge, no empirical studies have investigated whether the effects of training on wages differ depending on union coverage. The various hypotheses outlined above do have implications for wage levels and wage growth of covered and non-covered trained workers, and our principal aim is to establish some stylized facts in this regard. We shall return to these hypotheses in interpreting our results later in the paper.

The Data

The data are from the first six waves of the British Household Panel Survey (BHPS), covering the period 1991 to 1996. The BHPS is a nationally representative random sample survey of private households in Britain. Wave 1 interviews were conducted during the autumn of 1991, and annually thereafter. Our analysis is based on the subsample of men born after 1936 who provided complete information at each of the six interview dates, were in full-time employment at the time of the survey, and were not self-employed, in the armed forces, or farmers.[7] These restrictions yield a balanced panel of 950 men and 5,700 person-year observations.[8]

Our measure of training incidence takes the value of one if, since September 1 of the year preceding the interview, individuals received training to increase or improve skills in the current job, and zero otherwise. Our measure of training intensity (or duration) is days spent in training to increase or improve skills over the previous 12 months in the current job. The Appendix provides the relevant questions and details of the construction of our measures of training incidence, intensity, and union coverage.

Since the training question explicitly asks for details on "training schemes or courses" that form part of the respondent's present employment, our measure of training is

[7]Our analysis is complicated by the potential endogeneity of both coverage and training status to wages. To avoid the additional complication of self-selection into paid employment, we exclude women from the current study. We plan in future work to investigate these same issues for women.

[8]We also performed our analyses on an unbalanced panel and found the same broad results as those we reported for the balanced panel. These are available from the authors on request.

Table 2. Training in the Current Job and Wages by Union Coverage, 1991–96.

Characteristic [1]	*All Men* [2]	*Covered Men* [3]	*Non-Covered Men* [4]	*Significant Difference*
Training:				
Incidence (%)	38.56	42.46	32.72	yes [0.0000]
Intensity (days)	4.41	5.25	3.16	yes [0.0000]
Hourly Wages (£)	8.83	8.96	8.63	yes [0.0253]
Hourly Wages if Received Training (£)	9.75			
Hourly Wages if Received No Training (£)	8.25			yes [0.0000]
Hourly Wages for Trained Workers:				
Pre-Training Wages (£)	7.94	7.50	8.57	yes [0.0088]
Post-Training Wages (£)	9.00	9.05	8.93	no [0.7279]
Training Pay Gap (%)	13.3 [0.0001]	20.7 [0.0000]	4.2 [0.4307]	
N	5,700	3,417	2,283	

Note: Wages (in UK sterling) are deflated with the Retail Price Index and expressed in constant (1996) prices. The computation of hourly wages is explained in the text. "Training pay gap" (second row from bottom) is defined as the difference between hourly wages in any wave of data t and the hourly wages of the wave $t-1$, divided by the hourly wages for trained workers in wave $t-1$ (expressed in %). Column [4] ("Significant Difference") reports whether the difference between the figures in columns [2] and [3] is statistically significant; p-values of the t-tests for such differences are in square brackets. N denotes the number of person-wave observations for our sample of full-time male employees.

likely to exclude the more *informal* types of training that occur on the job.[9] Therefore, in interpreting our results, it should be borne in mind that such types of training are not picked up by the BHPS questions, and our estimates will identify the impact of the more formal aspects of work-related training. To the extent that union and non-union workers are equally likely to receive informal training, our estimates will not be affected by this measurement problem. But if, say, non-union employees receive systematically more informal training, we may underestimate the effect of training for these workers. While this is an interesting issue, we cannot investigate it with our data.

The responses to the training questions are given in Table 2, for all person-year observations, disaggregated by trade union coverage. The union coverage variable takes the value of unity for workers covered

[9]A potential problem with training data relates to respondents' interpretation of the question. Barron, Berger, and Black (1997) used U.S. data from a matched survey to compare the employer's response about training with the responses of the worker who received the training. They found that there was substantial measurement error in the training variables, and that firms tend to report more training than workers do. Campanelli et al. (1994), in a study of both linguistic and survey data, noted that the interpretation of the term "training" varies across groups in the population, in particular employers, employees, and training researchers. They emphasized that individuals in the general population typically interpret training as referring to "that which happens in formal courses" (p. 92). This—rather than less formal training, which is harder to measure—is our focus of interest in the present study. The possibility that informal training is included by some respondents cannot, however, be ruled out.

by a union, zero otherwise. This variable was constructed from responses to the question about whether there is a recognized trade union or staff association for negotiation of pay or work conditions. The potential endogeneity problem associated with the inclusion of union status in a wages equation is likely to be mitigated by the fact that we are concerned in this paper with union coverage rather than membership.[10] Approximately 60% of the sample is covered by union collective bargaining arrangements.

From column (1) of Table 2 we see that 38.6% of the sample received training to increase or improve their skills in the current job. Conditional on training receipt, the average number of training days was 4.4.[11] Columns (2) and (3) give the means for the union-covered and non-covered subsamples, respectively. Training incidence was about 10 percentage points higher for union-covered men than for non-covered men. On average, union-covered men who received training spent over two days more each year in training than did their non-union counterparts. Notice from column (4) that the difference in training means between the union-covered and non-covered subsamples is statistically significant at the 1% level.[12] There is evidence of some "recidivism" in training receipt over time. About 60% of workers who received on-the-job training in any given year t (t = 1991, … ,1994) would receive training again in the following year. Conversely, however, notice that those who did *not* receive any training in year t had a 78% probability of again receiving no training one year later.

Hourly wages are given in the third row of Table 2.[13] Over the entire sample, workers who received some training earned, on average, £1.50 per hour more than workers who received no training. This difference is significant at standard statistical levels. Furthermore, hourly wage rates are higher for union than non-union men, and this difference is statistically significant. Consider the pre- and post-training hourly wages for all workers: on average over all men, those who received training have a statisti-

[10]The distinction between membership and coverage is important in Britain. This is because closed shops are illegal, and it is therefore possible for workers to be covered by a union for pay (and other job-related aspects of their contract) while "free riding" on membership, thereby avoiding paying the union dues or incurring any costs associated with membership (Booth 1995).

[11]The proportion of workers receiving on-the-job training was fairly stable over 1992–96, ranging between 36% and 40% in each wave. The only exception is the first wave of the BHPS in 1991, when 45% of the respondents in our sample reported having received skill-enhancing training. Similarly, the highest number of training days was reported in the 1991 survey (5.9 days on average), the second highest in 1994 (5.4 days), and the lowest in 1996 (3.2 days).

[12]Using data from the 1993 Quarterly Labor Force Survey (QLFS), Green, Machin, and Wilkinson (1999) found that approximately 7% of manual workers and 17% of non-manual workers in Britain received formal training in the four weeks preceding the interview. Using the same data source between 1983 and 1996, Dearden, Reed, and Van Reenen (2000) found that the proportion of workers receiving training during the previous four weeks grew from about 5% to 15%, while the incidence of on-the-job training remained constant over their sample period. It is hard to see how these figures differ from ours, because the definition of training receipt in the QLFS is different (for example, in Green, Machin, and Wilkinson [1999], training receipt is equal to unity if, in the previous four weeks, the worker had taken part in any education *or* any training connected with the job, *or* a job that he or she might be able to do in the future), and the time frame over which training is measured is also different (the last four weeks rather than the last year or so). The incidence of work-related training appears to have been lower in Germany between 1986 and 1989, at about 31% for men aged 16–64 (Pischke 2001), and even lower in the United States, where Lynch (1992) found that 22% of white men had some training between 1980 and 1983.

[13]The hourly wage rate for worker i in year t is given as $\omega_{it} = \text{PAYGU}_{it}/[(30/7)(\text{HS}_{it} + \kappa\text{HOT}_{it})]$, where PAYGU is the usual gross pay per month in the current job (deflated by the 1996 Retail Price Index), HS is standard weekly hours, HOT is paid overtime hours per week, and κ is the overtime premium. We set κ at 1.5, the standard overtime rate, but all our results below are robust with respect to alternative values of κ ranging between 1 and 2. Our wage figures are expressed in U.K. sterling throughout the paper. The average U.S. dollar equivalent (that is, the exchange rate) of £1 was $1.77 in 1991 and $1.56 in 1996 (see Office for National Statistics 2000, Table 22.10, p. 398).

cally significant training wage premium of 13.3%. We disaggregate this wage differential to see how it varies across the union and non-union sectors (columns 2–3). As shown in the last row of Table 2, the pre- and post-training wage differential for union-covered workers is a statistically significant 20.7%, as compared with a statistically insignificant differential of just 4.2% for non–union-covered workers. This is an interesting finding, suggesting that the wage profile for men receiving training is steeper for those who are union-covered than for those who are not. Notice also that the pre-training hourly wages of non-union men are £1 higher than those of union men, a statistically significant difference. However, the post-training wage difference between union and non-union men is not statistically significant, as a result of the large wage gain consequent upon training for union workers.

To investigate why this training premium exists for union-covered men, we stratified the sample by number of years of work experience and number of years of service in the firm. If union-covered jobs were more likely to be characterized by an apprenticeship type of structure (whereby individuals accept a lower starting wage for the opportunity to be trained and receive higher post-training wages), then among individuals with either modest experience or short firm tenure, wages of those with union coverage may be significantly lower than wages of those without. On the other hand, if unionization were related to higher job retention, then we would expect to observe greater training receipt and higher wages for union-covered workers than for their non-union counterparts.

Among the pool of men receiving training in any of the survey years, there is a statistically significant premium of £0.70–0.80 in the hourly wage for those who are in union-covered jobs, if their work experience ranges between 2 and 12 years. For the same group of men, we also observe a statistically significant wage premium of £0.70–1.00 if they have been with the same firm between one and eight years. We cannot detect any premium or penalty for union-covered workers with higher or lower experience and tenure levels, nor can we find any union/non-union pay differentials among untrained workers. These results, therefore, do not support the hypothesis that the training measured in our data is the type of training that union-covered workers are more likely to receive at the beginning of their careers or when they start working for a firm. But the existence of wage premia for union-covered workers who are more experienced and have longer firm tenure is in line with the possibility that unionization is correlated with higher job retention.

In summary, not only are union-covered male workers more likely than non-covered male workers to experience work-related training, but they also receive larger wage gains consequent upon training. These larger gains are reaped by union-covered workers with relatively long firm tenure and some (but below average) work experience, suggesting that the higher job retention of young experienced union-covered workers may be one reason union-covered workers have greater returns to training. The raw data therefore provide some evidence that is not consistent with the hypothesis based on otherwise competitive labor markets. In the next section, we shall see if this evidence persists after we control for a large set of explanatory variables. These variables—along with the training, union, and wage measures—are defined in Table A1, which also reports their sample means.

Results

In this section, we report estimates from a number of models that explore the relationship between union coverage, training, and wages. First, using various econometric techniques, we estimate the effect of union coverage on training. Second, we investigate the degree to which the impact of training on wages and wage growth differs across union-covered and non-union-covered men. We also present and discuss results from different econometric models that address the issue of training-coverage

Table 3. The Union-Training Link.

	Training Incidence		*Training Intensity*	
	Cross-Sectional Probit Model	*Fixed-Effects Logit Model*	*Cross-Sectional Tobit Model*	*CLAD Model*
Union Coverage	0.092***	0.052**	4.348***	2.987***
	(4.288)	(1.987)	(5.031)	(4.010)

Note: CLAD = censored least absolute deviations. Figures are marginal effects computed at mean values. The figure in the column labeled CLAD represents the partial effect on intensity conditional on participating in training. All regressions include also the variables listed in Table A1 (in the fixed-effects logit model, the time-invariant variables are not included). These estimates are not reported, but can be obtained from the authors. Absolute *t*-statistics are in parentheses. In the cross-sectional probit regressions, the *t*-statistics are obtained from Huber-White standard errors. The number of person-wave observations is 5,700. The mean of the dependent variable in the training incidence regressions is 0.386. The mean of the dependent variable in the training intensity regressions is 4.412.

**Statistically significant at the .05 level, ***at the .01 level.

endogeneity and the alternative explanations summarized in Table 1 as to why differences in training between covered and non-covered workers may exist.

Do Union-Covered Men Get More Training?

We address this question using cross-sectional and panel models of the determinants of training incidence and training intensity for the entire sample of men. Table 3 reports the marginal effects of union coverage, which are calculated as the derivative of the conditional expectation of the observed dependent variable evaluated at the sample means. For brevity, we do not report the marginal effects of other variables used in these regressions.

The cross-sectional probit estimates reveal that men who were covered by a union were significantly more likely to receive work-related training in the current job than were non–union-covered men (t-ratio = 4.29). The impact is quite large, with union coverage increasing the training probability by more than 9 percentage points. After we control for individual-specific unobserved permanent components (for example, motivation and ability), the fixed-effects logit estimates reveal that the probability of receiving job-related training was 5 percentage points higher for union-covered workers than for non-union-covered workers. The tobit estimates show that the presence of labor unions also increased the intensity of work-related training: indeed, men who were covered by a union received approximately four more days of training than their non–union-covered counterparts. This result is unchanged when we account for unobserved random effects (estimates not shown).

In addition, Table 3 reports the results obtained from a censored regression equation, which is estimated using Powell's (1984) censored least absolute deviations (CLAD) estimator. Because the distribution of the intensity variable is highly skewed, this estimator will reflect the central tendency of the data possibly better than the tobit model does (Chay and Powell 2001). The results are to be interpreted as the partial effects on the number of days of training conditional on participating in training. The coefficients are qualitatively similar to, though slightly smaller than, those obtained from the corresponding specification of the tobit models. The point estimate implies that, each year, union-covered men who get trained receive about three more days of training than do non-covered employees.[14]

[14]These estimates and the estimates of the other individual and job-related characteristics are broadly consistent with those found in existing studies for

Hence, contrary to the predictions of standard human capital theory, union-covered men were statistically significantly more likely to be trained and received more days of training than their non-union counterparts, a result we obtain even after we account for a large set of relevant controls and unobserved heterogeneity. With just this evidence, however, it is hard to see which of the hypotheses discussed above is most appropriate for explaining why this effect emerges. For this purpose, we now investigate whether the impact of training on wages differed across union and non-union workers, as suggested by the raw data.

Does the Training Effect on Wages Vary with Union Coverage?

To answer this question, we estimate wage-level and wage-growth equations in order to measure the differential training impact on wages for union and non-union workers. In particular, an individual's hourly wage at time t, w_{it}, can be expressed as

$$(1) \qquad \ln(w_{it}) = \alpha U_{it} + \beta T_{it} + \gamma(T_{it} \times U_{it}) + \delta Z_{it} + f_i + \varepsilon_{it},$$

where U_{it} is a 0–1 indicator of whether individual i at time t is in a job covered by a recognized union, T_{it} is the relevant measure of work-related training (either a dummy variable of training receipt or the number of training days), Z_{it} is a vector of variables affecting wages that may vary for each individual t over time, the f_i's are unobserved characteristics that are individual-specific but time-invariant, and ε_{it} is an i.i.d. random shock. The f_i's may be correlated with whether workers undergo training or with the likelihood of working in a unionized firm. Thus, fitting equation (1) while omitting f_i will lead to biased estimates of α, β, γ, and δ.

Differencing individuals' wages between year t and year $t-1$, however, causes all time-invariant effects (both observed and unobserved) to drop out, and allows the parameters of interest to be estimated without bias. In addition, this first-difference estimation allows us to identify how specific changes in union status are associated with the earnings profile, both on their own and in conjunction with changes in training incidence or training intensity. That is, we separately estimate the effects of entry into, exit from, and retention in union-covered jobs on wage growth as well as changes in training (receipt or intensity). This is given by

$$(2) \qquad \Delta\ln(w_{it}) = \sum_j \alpha_j \Delta U_{it}^j + \sum_j \beta_j \Delta T_{it}^j + \delta \Delta Z_{it} + \varepsilon_{it},$$

where $\Delta y_{it} = y_{it} - y_{it-1}$ for any variable y in equation (2), and j = entry (e), exit (x), stay (s) in a job covered by collective bargaining agreement or offering training, and stay (0) non-covered or untrained. Hence, ΔU_{it}^e denotes entry into a union-covered job. In the case of the training *intensity* measure, model (2) will have six possible changes from one interview to the next: being trained the same number of days (β_s); moving from untrained to trained (β_e); moving from trained to untrained (β_x); increasing the number of days of training (β_+); decreasing the number of days of training, but still receiving training (β_-); and remaining untrained (β_0).

Notice that in moving from equation (1) to equation (2), we deliberately leave out all the interactions between coverage and training. We do so to reduce the computational burden, and also in recognition of the fact that training and union coverage are likely to be jointly endogenous. To address the problem of potential joint endogeneity, we then introduce four interaction categories in each year t—covered-trained (s_{1t}), covered-untrained (s_{2t}), non-covered-trained (s_{3t}), and non-covered-untrained (s_{4t}). Changes in these categories

Britain (for example, Arulampalam and Booth 1998; Green, Machin, and Wilkinson 1999) and the United States (for example, Veum 1995). A discussion of these other estimates can be found in Booth, Francesconi, and Zoega (2002).

are then represented by movements across each particular status between years. This leads to 16 different change categories in the case of training receipt and 32 categories in the case of training intensity. The reason the latter number is higher is that four changes, not merely two, are possible for workers who received some positive number of days of training in a given year: they may, in the following year, receive no training, receive increased (more days of) training, receive the same amount of training, or receive decreased training. For ease of exposition and to increase sample sizes, however, we combine three of the four possible training intensity transitions—(a) receiving no training the next year, (b) receiving the same number of days of training the next year, and (c) receiving a reduced number of days of training the next year—into one category and present evidence on 16 transitions only. In this case, the estimated wage growth equation will take the form

$$\Delta \ln(w_{it}) = \sum_{j,k} \gamma_{jk} (\Delta s_{j,it} \times \Delta s_{k,it}) + \delta \Delta Z_{it} + \Delta \varepsilon_{it}, \quad (3)$$

where j,k=1, … ,4, and the parameters α and β in (1) are not estimated given our definitions of s_{jt}.

Training, Union Coverage, and Wage Levels

In Table 4 we present pooled ordinary least squares (OLS) and fixed-effects (FE) estimates of the natural logarithm of real (1996 pounds) hourly wage levels.[15] Table 4 shows the estimates of two different specifications, one that does not include a training/union coverage interaction term (specification i) and another that includes it (specification ii). Panel A contains the estimates obtained with our measure of training incidence, and panel B those obtained with training intensity. Even if the OLS estimates fail to account for the presence of the individual-specific effects, f_i in (1), they represent a useful benchmark for comparison purposes, while the FE estimates are consistent.[16]

Workers who were covered by a union received nearly 4% higher wages than their non–union-covered counterparts (specification i in both training incidence and training intensity FE regressions). We find that this union premium works in conjunction with higher training incidence or intensity (specification ii): union-covered workers who received training earned almost 6% more than workers who received training but were not union-covered (that is, 0.027 + 0.031 from the FE estimates, Panel A). Although a higher number of training days is associated with a statistically significant increase in covered workers' wages (FE estimates only), this effect is small, with 10 more days of training per year leading to only a 1% wage increase (Panel B, specification i). The wage effect of training intensity, however, disappears in specification ii, where it is the union-training interaction that becomes statistically significant: 10 additional days of training a year leads union-covered workers to earn 5% higher wages.

With the goal of distinguishing between some of the explanations outlined earlier, we performed OLS and FE regressions by experience and tenure groups, and found substantial differences across groups. A simpler way to capture this relationship is to introduce an interaction term between training receipt, union coverage, and an experience dummy variable (taking a value of one if experience is between 2 and 12

[15]The fixed-effects estimates in this table are obtained using a differences-from-means approach. As such, they differ from the other fixed-effects estimates presented below, which have been obtained by estimating equations (2) and (3).

[16]Hausman's specification tests strongly reject the hypothesis that the individual effects are uncorrelated with the right-hand side of equation (1), suggesting that random-effects models may be problematic. The estimates for the other explanatory variables listed in the note of Table 4 are omitted because of space limitations, but are available from the authors on request.

Table 4. Log Hourly Wage Estimates.

	Ordinary Least Squares		*Fixed-Effects*	
Variable	*(i)*	*(ii)*	*(i)*	*(ii)*
A. Training Incidence				
Training	0.033**	–0.006	0.010	–0.008
	(2.231)	(0.260)	(1.428)	(1.329)
Union Coverage	0.057***	0.034**	0.039***	0.027**
	(2.674)	(2.123)	(3.124)	(2.069)
Training × Union		0.064**		0.031***
		(2.168)		(2.633)
ρ			0.753	0.753
R^2	0.468	0.469	0.241	0.241
B. Training Intensity				
Training	0.0001	–0.001	0.001***	0.0001
	(0.222)	(1.007)	(3.768)	(0.140)
Union Coverage	0.060***	0.055**	0.038***	0.039***
	(2.789)	(2.513)	(3.152)	(3.127)
Training × Union		0.001*		0.001**
		(1.672)		(2.064)
ρ			0.753	0.753
R^2	0.467	0.467	0.241	0.241

Notes: Absolute *t*-statistics are in parentheses (Huber-White standard errors for OLS estimates). Other time-varying controls included in all regressions are highest educational qualifications (4 dummies), experience, experience squared, tenure, tenure squared, married/cohabiting, living in London, firm size dummies (6), sector (public and charity dummies), occupational dummies (4), whether the worker changed employer in the last 12 months, local unemployment rate, and industry quit rate. The OLS regressions also include time-invariant controls for occupation of origin (4 dummies) and cohort of entry in the labor market (2). The term GKrho is the fraction of variance accounted for by the unobserved heterogeneity component. The number of person-year observations is 5,700.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

years) and another interaction term between training receipt, union coverage, and a tenure dummy variable (taking a value of one if tenure is between 1 and 8 years). The FE estimates show that such interaction terms are positive, with an estimated statistically significant wage premium of between 1% and 4%; this suggests that trained union-covered workers in those experience/tenure brackets did enjoy an extra wage premium. This finding provides some support for the job retention hypothesis.

Notice, however, that for workers with lower levels of firm tenure (less than one year) and experience (less than two years), there was no statistically significant difference between the pay returns of trained-covered workers and untrained-non-covered workers. This does not seem to be consistent with the hypothesis that training in unionized firms is more likely to follow an apprenticeship type of structure. But it may still be true that (some) unions use training as a means for controlling the quality of young workers, since our data do not contain any information on training quality.

Selectivity Checks

Before turning to the wage growth estimates, we discuss three further exercises to investigate the possibility that our results are affected by selectivity problems. To save space, the results from these three exercises are not reported, but they can be

obtained from the authors upon request.

First, since participation in training and union coverage are potentially jointly endogenous, we used a two-step procedure to reduce the resulting potential bias. In this instance, the selectivity correction term is obtained by first estimating a bivariate probit model, in which training receipt and union coverage are the dependent variables and the explanatory variables are those used to generate the estimates in Table 3. The estimated coefficients from this model are then used to construct a selectivity correction term using the method developed by Poirier (1980). The estimates for the training receipt and the union coverage variables are very similar to the OLS estimates reported in Table 4, with the estimated selection term being always statistically insignificant at conventional levels. This suggests that training and coverage are not necessarily jointly endogenous, but it may also mean that the correction term is not suitably identified.[17] However, Sargan's and other tests (suggested by Bound, Jaeger, and Baker 1995) cannot reject the validity of our instruments at conventional statistical levels.

A second check on the robustness of our results is a test of one of the assumptions of the FE model. Consistency of the FE estimates relies on the assumption that individuals who change coverage-status and training-status are representative of the whole work force. This assumption is at odds with the idea that nonrandom selection of covered and trained workers is important, which motivated the use of the FE procedure in the first place. Furthermore, if either heteroskedasticity or serial correlation is present in the data, it may be possible to improve on the FE estimates.

For these reasons, we also estimated a generalized method of moments (GMM) wage model of training and union coverage with additional moment conditions being available from the strict exogeneity assumption (Wooldridge 2001, Chapters 10 and 11). This model was estimated on four different samples (all workers, stayers, quitters, and involuntary job changers) using two specifications (one based on first moments only, the other based on both first-order and second-order conditional moments of wages).

The GMM estimates for the entire sample of workers reveal that men who were covered by a collective bargaining agreement received about 4.5% higher wages than their non-covered counterparts. As we found earlier, this coverage premium works in association with higher training incidence, with trained covered workers earning 7% more than trained non-covered workers. A similar picture emerges for the other groups of workers, although the group of "stayers" faced the highest and the group of "quitters" the lowest joint gain from coverage and training. The over-identification test statistics for each sample and specification are smaller than their critical values at the 5% level, indicating that the over-identification restrictions are not inconsistent with the data. However, this is not true in the case of "quitters" when the GMM estimates are based on both first and second moments. This suggests that changes in coverage and training status are exogenous for stayers and involuntary changers but probably not for quitters.

As noted earlier, the issue of selection into union coverage is weakened by the fact that coverage is associated with a job rather than with a worker. But workers with high f_i's may have enjoyed careers that were characterized by more training, while better job matches are, *ceteris paribus,* likely to arise for workers with greater ability and a higher propensity to shop for a job (higher search effort). Also, training and union coverage statuses are possibly correlated with work-

[17]Identification has been achieved in our models by including the ratio of unemployment to job vacancies in the local labor market, the industry quit rate, and the interaction between unemployment rate and industry quit rate in the bivariate probit only, and by including tenure and tenure squared in the wage regressions only.

[18]One way to provide some correction for this problem in the FE regressions presented above was to add controls for changes in union coverage status as

ers' match quality components.[18] We exploit the job-coverage link in our data to use the instrumental variable methodology proposed by Altonji and Shakotko (1987) as a third check on the robustness of our results. In addition to individual fixed effects as specified in (1), this methodology allows us to introduce a more complex error structure, which consists of job-match and sector-specific (coverage/non-coverage) effects.[19]

The results from these regressions uphold the findings obtained with the GMM procedure. In particular, the estimates for the subsample of stayers reveal that trained covered workers earned just under 7% more than trained non-covered workers, while among quitters this premium was only about 4%. Because coverage changes can occur only if unions gain or lose recognition in a firm, these changes are more likely to be exogenous to job-match and individual-specific fixed effects in the case of stayers than in the case of quitters. Thus the estimated returns to training (for covered and non-covered workers) are arguably consistent for the subsample of stayers. Interestingly, the fraction of the residual variance that is attributable to match-specific unobservables is quite large (particularly for quitters, for whom it is about 50% of the total variance). This indeed may help explain the differences between stayers and quitters in the returns to training and coverage.

The results of Table 4 are therefore confirmed by GMM, selectivity-corrected, and error-component models, at least for all workers but quitters. Although both instrument selection and exogeneity assumptions—which characterize the estimates presented so far—are a matter of contention, our results appear to be quite robust and are thus likely to be of general interest.

Training, Union Coverage, and Wage Growth

In our wage growth analysis, the dependent variable is the change in log wages between two consecutive years, and the independent variables are the year-on-year changes in the workers' characteristics. As discussed above, the differencing procedure eliminates the effect of any omitted-variables bias due to selection into training and coverage, if it is assumed that the selection process varies only across individuals and over time for the same individual (Lynch 1992; Veum 1995).[20]

Table 5 reports the results for the wage growth equations (2) and (3), which differ by whether we use training receipt or training intensity and by the way training and coverage changes are modeled. Notice that, with few exceptions, the estimates by equation are remarkably similar regardless of whether we employ training incidence or training intensity.

The results from equation (2) reveal that, compared to workers who remained non-covered in each successive year, those who got a job covered by collective bargaining agreements experienced a 6% higher wage growth (α_e), those who left a covered job faced a 4% reduction in wage growth (α_x), and those who stayed covered experienced no statistically significant wage change (α_s). The same estimates show also that joining a training scheme led to almost 7% higher wage growth (β_e), while leaving training was associated with about 8% higher growth ($\beta_{e'}$). Workers who either increased or decreased the number of hours of training also experienced substantial wage growth

suggested in Loewenstein and Spletzer (1998). We shall follow this procedure below while estimating wage growth regressions.

[19]In this analysis, we treat training, coverage, experience, and job tenure as endogenous. All the other variables, which are also used to obtain the estimates reported in Table 4, are treated as exogenous. The instrumental variables used in estimation are given by (a) the deviations from within-job means of both exogenous and endogenous time-varying variables and (b) the within-job means of all exogenous variables. For similar applications, see Light and McGarry (1998) and Parent (2000).

[20]With this procedure we cannot identify the effects of the time-invariant characteristics.

Table 5. Annual Growth of Hourly Wages—Equations (2) and (3).

		Training Incidence		*Training Intensity*	
Variable	*Parameter*	*Eq. (2)*	*Eq. (3)*	*Eq. (2)*	*Eq. (3)*
Union:[a]					
Entry	α_e	0.063** (2.221)		0.062** (2.215)	
Exit	α_x	–0.042** (1.984)		–0.042** (1.973)	
Stay	α_s	–0.004 (0.379)		–0.005 (0.457)	
Training:					
Entry	β_e	0.068*** (3.352)		0.068*** (3.358)	
Exit	β_x	0.078*** (3.668)		0.079*** (3.685)	
Stay Trained	β_s	0.067*** (3.210)		0.042 (1.338)	
Stay Untrained	β_0	0.007 (0.843)		0.005 (0.502)	
Increase Training	β_+			0.062*** (2.858)	
Decrease Training	β_-			0.078*** (3.411)	
Covered-Trained to:					
Covered-Trained	γ_{11}		0.057*** (3.509)		0.051** (2.422)
Covered-Untrained	γ_{12}		0.059*** (2.843)		0.066*** (3.248)
Non-Covered-Trained	γ_{13}		–0.007 (0.473)		–0.024 (0.331)
Non-Covered-Untrained	γ_{14}		–0.008 (1.193)		–0.030 (0.571)
Covered-Untrained to:					
Covered-Trained	γ_{21}		0.024* (1.763)		0.058*** (2.718)

Continued

(β_+ and β_-). The only workers who did not face any real wage change were those who did not receive any training in two successive years (β_0). It is therefore apparent that currently receiving training or having received training sometime in the last year had a positive impact on wage growth.

The estimates from equation (3) allow us to assess whether or not union recognition mediated higher returns to training, while dealing with potential endogeneity biases more adequately, as they account for all the possible (annual) transitions in coverage and training status.[21] The estimates in Table

[21]We also estimated OLS and FE models of hourly wages similar to (1), in which union coverage and training status are not entered as separate variables but are included as covered-trained (s_{1t}), covered-untrained (s_{2t}), non-covered-trained (s_{3t}), and non-covered-untrained (s_{4t}, base category). The FE estimates show that, relative to the workers in the base category, those who are covered and trained in any year t (s_{1t}) receive on average 5–9% higher hourly

Table 5. Continued.

		Training Incidence		*Training Intensity*	
Variable	*Parameter*	*Eq. (2)*	*Eq. (3)*	*Eq. (2)*	*Eq. (3)*
Covered-Untrained	γ_{22}		0.052*** (3.822)		0.057*** (3.269)
Non-Covered-Trained	γ_{23}		–0.040 (1.153)		–0.032** (1.973)
Non-Covered-Untrained	γ_{24}		–0.045** (2.154)		–0.037 (1.528)
Non-Covered-Trained to:					
Covered-Trained	γ_{31}		0.029** (2.477)		0.020** (1.967)
Covered-Untrained	γ_{32}		0.033** (2.462)		0.031** (1.984)
Non-Covered-Trained	γ_{33}		0.040 (1.546)		0.038** (2.178)
Non-Covered-Untrained	γ_{34}		0.038* (1.865)		0.041 (1.497)
Non-Covered-Untrained to:					
Covered-Trained	γ_{41}		0.040* (1.780)		0.054*** (2.801)
Covered-Untrained	γ_{42}		0.002 (0.053)		0.028 (0.720)
Non-Covered-Trained	γ_{43}		–0.031 (1.388)		–0.026 (1.277)
Non-Covered-Untrained	γ_{44}		–0.039*** (2.784)		–0.043*** (3.256)
R^2		0.063	0.064	0.063	0.067

Note: Absolute *t*-statistics in parentheses (Huber-White standard errors). All regressions are performed on 4,750 transitions. Other controls included in all regressions but not reported in the table are yearly changes in highest educational qualification, tenure and tenure squared, marital status, residential location, firm size, current occupation, sector, employer, two-digit industry quit rate, and local unemployment rate.

[a]Stay non-covered is the base category.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

5 clearly document that gaining coverage was typically associated with large wage growth (except when workers did not receive training in any two successive years, γ_{42}). For example, non-covered-trained workers who became union-covered enjoyed 2–3% higher wage growth (γ_{31} and γ_{32}). On the other hand, moving from untrained to trained was not associated with steeper wage profiles, unless this change was also accompanied by the acquisition of union coverage or by being always covered (γ_{41} and γ_{21}, respectively). In this specific instance, however, the training-coverage impact could be quite substantial, resulting in up to 6% higher wage growth.

The table also shows that workers who remained covered by a union agreement across any given pair of years between 1991 and 1996 experienced the highest wage

wages in that year; those who are covered and untrained (s_{2t}) earn 1–3% higher wages (but this premium is never statistically significant); and those who are trained and non-covered (s_{3t}) receive hourly wages that are 3–4% *lower* (a statistically significant difference).

growth, regardless of the measure of training used. This is particularly true in the case of union-covered workers who left training, who enjoyed approximately 6–7% higher hourly wage growth per year than did non-covered workers who left training (γ_{12}), but it is also true for workers who remained in training both years (γ_{11}) and for those who engaged in no training either year (γ_{22}), among whom those with union coverage had 5–6% higher growth than those without. Staying non-covered in any two successive years was associated with lower wage growth, although men who received training in both years may have experienced a positive effect. Leaving coverage was associated with flatter or declining wage profiles, especially if workers also left training or were untrained for two consecutive years (γ_{23} and γ_{24}). Thus, as in the estimation of equation (1), where we found that the positive union coverage effect operates together with greater training receipt or duration, the estimates of equation (3) confirm that the combination of training and coverage had a substantial impact also on wage growth.

To explore the possibility that unions directed their negotiated training toward younger (in the firm) or less experienced workers, we again partition our sample by work experience and firm tenure, and re-estimate equation (3) by experience-tenure subgroups. The results (not reported) show that workers with some experience in the labor market (4–12 years) and workers with longer service in the firm (2–7 years) achieved the highest wage growth—ranging between 6% and 10%—when they became/stayed covered and received/continued to receive training. In contrast, workers with less labor market experience or shorter firm tenure experienced relatively small wage growth effects if they acquired coverage or joined a training scheme. Therefore, the wage profiles of experienced (or long-tenured) covered workers appear to have been steeper. This finding casts further doubt on the hypothesis that unionized firms tend to organize training in an apprenticeship type of structure (although, again, the possibility that unions use training to control the labor supply and quality of young workers cannot be ruled out with BHPS data). It does, however, support the hypotheses based on job retention, higher job security, and imperfectly competitive labor markets, which cannot be further tested in our data.

Finally, we separately re-estimated equation (3) for three different groups of workers: stayers, quitters, and workers who involuntarily moved to another firm.[22] This exercise reveals that both stayers and dismissed workers had only a weak positive growth effect if their current/new firm acquired union recognition, but suffered a strong negative effect on wage growth if they lost union coverage, regardless of training status. The opposite results emerge for quitters. On the other hand, training status changes produced wage growth effects similar to those presented earlier for all three groups of workers as long as they were union-covered, with the effects for quitters being only slightly higher. Thus, although moving to another firm (either voluntarily or involuntarily) is likely to be correlated with wage growth, the main results of Table 5 do not appear to be affected by the potential endogeneity bias induced by the workers' decision to change firm.

Conclusions

We have used data from the British Household Panel Survey for the period 1991–96 to estimate the impact of trade unions on work-related training and wage formation for a sample of full-time male

[22]We performed this analysis to try to limit the selection problem caused by the fact that the wage growth effect of acquiring coverage may not be entirely separated from the effect of changing firm (Machin 2000). To increase sample sizes, in this exercise, workers who voluntarily or involuntarily left their firm in any given year between 1991 and 1996 are classified as "quitters" or "dismissed workers" over the entire sample period. This implies that these regressions are performed on 2,000, 1,560, and 1,190 observations in the subsamples of stayers, quitters, and dismissed workers, respectively.

employees. Our main findings are that union-covered workers were more likely to receive training, and received more days of training, than their non-covered counterparts, and also experienced greater returns to training and higher wage growth than did trained-non-covered men.

These findings are at odds with the predictions of standard human capital theory that there will be a negative correlation between union presence and training, and that the pay returns to training will be lower for union-covered than for non–union-covered workers. Instead, our results are consistent with some of the alternative hypotheses that we outlined earlier in the paper and summarized in rows (2.ii), (3), (4), and (5.ii) of Table 1. With our data, such explanations are largely observationally equivalent, so it is not possible to discriminate further between them. Nevertheless, several robustness checks and ancillary econometric evidence seem to provide little support for the hypothesis based on union workers holding apprenticeship-type jobs or for the hypothesis based on selectivity. Conversely, the three remaining explanations—one based on imperfectly competitive labor markets wherein unions are able to extract some of the surplus associated with the labor market frictions (through higher wages and training), a second based on unions' ability to reduce labor turnover, and a third based on unions' objective of increasing job security—appear to be consistent with the results. However, our data do not allow us to assess these hypotheses further, and we would require more sophisticated information to be able to test one against the other more formally. In addition, our data cannot adequately identify informal workplace-based training, which may affect non-union workers differently from union workers. Indeed, this could be one reason why, contrary to the predictions of economic theory, we find that the wage returns to training are negligible for non-covered male employees.

While it is well known that in Britain union presence is associated with more work-related formal training, the positive impact of unions on the wages of trained men has not been noted before. Whether this is a peculiarity of Britain, or even of Britain in the 1990s, remains to be seen.

APPENDIX

The Data

Training Incidence

The precise form of the BHPS training incidence question, asked of all individuals currently in work, is as follows: "Since September 1st last year, have you taken part in any education or training schemes or courses, as part of your present employment?" If yes, the respondent was then asked: "Was any of this training (a) training to help you get started in your current job? (b) to increase your skills in your current job, for example by learning new technology? (c) to improve your skills in the current job? (d) to prepare you for a job or jobs you might do in the future? (e) to develop your skills generally?"

Our focus of interest was work-related training to improve or increase skills in the current job, rather than induction training or training for future work or for skills generally. For this reason, we used the responses to (b) and (c) of this question to construct our training incidence measure. In addition to the results reported in this paper, however, we also performed the entire analysis with a measure of training incidence defined over the five types of training (a)–(e) listed above. We do not present the results obtained from this alternative measure because they were virtually identical to those reported in this study.

Total Time Spent in Training

The questions on training incidence were followed by a question on total time spent in all forms of training, as follows: "Since September last year, how long have you spent on this training? Please tell me approximately how much time you have spent on training in total." The units of time requested varied across earlier waves of the BHPS. At Wave 1, individuals were asked to report how many days were spent in training; at Wave 2, how many hours per week and the number of weeks; at Waves 3, 4, 5, and 6, respondents were free to choose the unit of time spent in training. For all waves, we converted responses to this question into days spent in training of type (b) and/or (c) over the past 12 months in the current job. This was hours, days, weeks, or other at Waves 3 and 4, and hours, days, weeks, months, or other at Waves 5 and 6. The cases for which a measure of training intensity could not be consistently computed were dropped.

Union Status

The precise form of the question about union status is as follows: "Is there a trade union, or a similar body such as a staff association, recognized by your management for negotiating pay or conditions for the people doing your sort of job in your workplace?" While Waves 1, 5, and 6 of the BHPS asked both job-movers and job-stayers for information on union status, the Waves 2–4 questionnaires only requested this if individuals changed employer. In our empirical analysis, we assume that Wave 1 union coverage remains constant across Waves 2, 3, and 4 for people who did not change employer, which is reasonable given that there is evidence that coverage did not alter for people in work over the period.

Table A1
Definition and Means of Variables

Variable	*Definition*	*Mean*
Union and Training:		
Union Coverage	Recognized trade union or similar organization for negotiating pay and other similar conditions in the workplace	0.599
Training Incidence	Any training meant to increase or improve skills in the current job over the previous 12 months	0.386
Training Intensity (days)	Number of days spent in skill-enhancing training in the past 12 months in the current job	4.412
Individual and Labor Market Characteristics:		
Age	Age in years	38.093
Partner Present	Married or cohabiting at interview date	0.787
Experience	Experience (years) in employment since labor market entry	19.971
Tenure	Firm tenure (years) in current job	6.657
Disabled	Registered as disabled either with social services or a green card	0.009
Changed Job	Changed employer in the previous 12 months (either though a quit or after a layoff)	0.126
London	Resident in Greater London	0.093
No Qualification (base)	No educational qualification	0.209
O-Level	Highest qualification is one or more "Ordinary"-level qualifications (later replaced by GCSE), usually taken at the end of compulsory schooling at age 16	0.323
A-Level	Highest qualification is one or more "Advanced"-level qualifications, representing university entrance-level qualification typically taken at age 18	0.234
Vocational	HND, HNC, Teaching, other higher qualification, Nursing	0.086
Degree Qualification	University degree or above	0.149
Professional[a]	Professional occupations	0.116
Managerial[a]	Managerial occupation	0.195
Non-Manual[a]	Skilled non-manual occupation	0.199
Skilled Manual[a]	Skilled manual occupation	0.291
Other Manual (base)[a]	Semi-skilled and unskilled manual occupations	0.199
Date of Labor Market Entry:		
Cohort 1 (base)	Entered the labor market before 1961	0.098
Cohort 2	Entered the labor market 1961–1970	0.268
Cohort 3	Entered the labor market 1971–1980	0.385
Cohort 4	Entered the labor market 1981–1990	0.248
Firm Size:		
Size25 (base)	Fewer than 25 employees at the establishment	0.236
Size50	25–49 employees at the establishment	0.123
Size100	50–99 employees at the establishment	0.141
Size200	100–199 employees at the establishment	0.125
Size500	200–499 employees at the establishment	0.164
Size1000	500–999 employees at the establishment	0.100
Size1000+	1000+ employees at the establishment	0.111
Public Sector	Works in public sector	0.274
Charity	Works in non-profit-making organization (charities, co-operatives, and so on)	0.019
Other Variables:		
Unemployment Rate	Local unemployment rate. The geographic unit is 306 matched job centers. Obtained from the National On-Line Manpower Information Service.	0.083
Industry Quit Rate	Average quit rate for the two-digit Standard Industrial Classification (SIC)	0.074

[a]Occupational categories are constructed from the current three-digit Standard Occupation Classification (SOC). Occupation-of-origin categories (not shown here) are the same as for current occupation. Occupations of origin are identified by the first full-time job after leaving full-time education using the retrospective work history information collected in the third wave (1993) of the BHPS.

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