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Unions, Training, and Firm Performance

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ABSTRACT

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The present paper uses a combination of workplace and linked employee-workplace data from the 1998 Workplace Employee Relations Survey and the 2004 Workplace Employment Relations Survey to examine the impact of unions on training incidence, training intensity/coverage, and training duration. It also examines the impact of unions and training on earnings and a measure of establishment labour productivity. In addition, the implications of training for the firm's bottom line are evaluated. Union effects on training emerge as fairly subtle, and are more positive when using individual rather than plant-wide training data. A positive impact of training on earnings is detected in both the individual and plant-wide wage data, albeit only for the earlier survey. Consistent with other recent findings, the effects of union recognition on earnings are today rather muted, while union-training interaction effects vary greatly. Instrumenting training provides positive results for the labour productivity outcome and, in the case of the earlier survey, for the financial performance indicator as well. However, some negative effects of unions are now also detected.

JEL Classification: J24, J33, J51

Keywords: union recognition, bargaining structure, employer-provided training, training Incidence, training intensity/coverage, training duration, earnings, labour productivity, financial performance

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I. Introduction

The issues of the productivity of workplace training and union impact on firm performance are two of the more controversial issues in applied labour economics. The training question is complicated by the diversity of training forms, measurement difficulties, and the likelihood that training participants and training firms will differ materially from their counterparts without (or with less) training. The empirical evidence from individual and firm data is mixed, especially as regards the productivity of on-the-job or internal training.

For its part, the union literature is in one sense more settled but lacks definition in the sense that the manner in which unions may influence workforce performance has been a black box. Implicitly, the major exception to this statement is training. Thus, in their early study of productivity using state-by-industry aggregates, Brown and Medoff (1978) attempted to go behind their finding of a positive union productivity differential of between 22 and 30 per cent by including a quit rate variable in the union-augmented production function. The effect was to reduce the union coefficient estimate by around one-fifth. In the years since this pioneering study, however, interest in the mechanisms through which unions might raise productivity has waned in line with much reduced estimates of the magnitude of that differential at lower levels of aggregation (the U.S. literature is reviewed in Hirsch, 2004, and the British literature in Addison and Belfield, 2004, and Metcalf, 2003). But the potential for greater training in union regimes (facilitated in part by reduced voluntary turnover) and the issue of its productivity are no less valid topics of empirical inquiry today than heretofore. Indeed, in Britain at least, there is evidence of heightened interest in these issues partly because of an apparent sea change in union impact on firm performance allied in part to bargaining structure, and partly because of research into the impact of high performance work practices (e.g. Wood and de Menezes 1998; Metcalf, 2003).

In the present study, we seek to examine these links using private-sector establishment data for Britain. Specifically, we consider the determinants of training to include unionism and then examine the impact of unions and training on earnings, labour productivity, and financial performance. Although we have information on just one type of training – namely, employer-provided off-the-job training – we shall consider its influence along three dimensions: *incidence*, *intensity* (strictly, coverage) and *duration*. We also deploy both individual and plant-level measures of training. Our earnings analysis likewise uses linked employer-employee data as well as plant-level average earnings. In investigating the impact of the two key variables on labour productivity and financial performance, however, we shall use plant-level information alone because of the very limited sampling of workers. In our analysis of plant performance, we will allow for the endogeneity of training.

To motivate the present study, we first outline the circumstances in which unions might influence training investments by the firm, and the manner in which the productivity of such investments has been addressed in the existing literature. We next describe the datasets used in this inquiry. There follows a detailed presentation of our findings. A brief summary concludes.

II. Theoretical Conjectures and the Existing Literature

At the level of theory, unions might be associated with either more or less training. A negative union effect might be expected if the union premium impairs the ability of employees to finance training. Assuming that labour markets are perfect, general training investments should be paid for by the worker since the skills learned are transferable and are thus fully appropriable by workers. Just as with a binding minimum wage, the payment of union wages prevents workers from taking a wage cut and financing their general training investments. More generally, seniority rules and wage compression may also reduce the worker's incentive to undertake or invest in training.[1]

Furthermore, the scale of such negative effects of unions on training might be expected to vary directly with union strength or bargaining power.

The literature has focused on the situations in which unions can stimulate training. Thus, the expression of union voice, underwritten by the wage premium, should cut down on labor turnover and increase the incentive of the employer to invest in *firm-specific* training because of the longer payback period (Freeman and Medoff, 1984). A more thorough-going application of the collective voice model would of course encompass workplace governance. A number of aspects of a governance apparatus may be expected to facilitate training by foreclosing the opportunities for workers to behave opportunistically. But a governance apparatus *that includes unions* may counteract the tendency of the employer to misrepresent his private information. More concretely, there may be a hold-up problem on the part of the employer: firms might hold-up the sunk investments of workers in training, leading to an under-investment in human capital. Here the union could act to prevent the hold-up problem by making the firm honour its commitments (Menezes-Filho and Van Reenen, 2003, p. 299).

The unions-as-a-commitment-device or agent of contract enforcement – first discussed by Malcomson (1983) – can be extended to cover the firm-financing of general training investments in circumstances where long-term wage contracts are otherwise infeasible. For example, Dustmann and Schönberg (2004) argue that (German) firms will pay for apprentice training because of the union-imposed wage floors that lead to wage compression. Wage compression allows productivity to rise faster than wages and the union wage level act as a guarantee against subsequent employer opportunism. Related models suggesting that unions may move general training closer to the social optimum either emphasize asymmetric information with respect to incumbent and outside forms, in

conjunction with wage compression (Acemoglu and Pischke, 1998) or complementarities between general training and specific training (Acemoglu and Pischke, 1999).

Past British (if not U.S.) work on the determinants of training has generally reported a positive union effect. That is, not only older research using union density (e.g. Greenhalgh and Mavrotas, 1994; Arulampalam, Booth, and Elias, 1995) but also more recent research using the preferred metric of union recognition point to a statistically significant direct association between unionism and training. Thus, for example, using individual data from the 1993 Quarterly Labor Force Survey and establishment-level data from the 1991 Employers' Manpower and Skills Practices Survey, Green, Machin, and Wilkinson (1999) report that the incidence of training is positively related to union recognition. Union recognition is also reported to lead to an increased duration of that training in hours/days. Reflecting recent British preoccupations, the authors also examine the role of bargaining structure. The argument is that the union wage will be higher where there are multiple unions that bargain separately at the workplace, with higher wages discouraging employers from paying for training courses (Green, Machin, and Wilkinson, 1999, p. 181). They report that neither the incidence nor the duration of training (from the employer survey) is affected by multiple unionism at the workplace.[2] Interestingly, there is some indication in this study that the positive impact of union recognition on training may be increased in the presence of employee involvement mechanisms.

Similarly, in an exercise that matches employee to establishment data from the WERS98, Böheim and Booth (2004) report a positive correlation between union recognition and employer-provided training in the private sector for three out of the four worker groups identified, namely, manual and nonmanual males and nonmanual females. In an expanded model that takes account of bargaining structure, the main change is that for male manual workers training incidence is only

higher under union recognition where there is multiple unionism with joint bargaining. Otherwise, bargaining structure has no separate effect on training incidence.

The incidence (and extent) of workplace training is one thing, its productive impact quite another. The traditional approach in Britain to measuring the productivity of training has been via earnings functions (some limitations of which are noted below).[3] Although there is an extensive literature on the impact of educational investments that dealing explicitly with work-related training is less developed. Nevertheless, most studies point to statistically significant positive returns to such training (see the survey by Cohn and Addison, 1998) even if few address the interaction between unionism and training.[4]

Two recent studies by Forth and Millward (2004) and Booth, Francesconi, and Zoega (2003) merit attention. The principal focus of the former study is on the role of high performance workplace practices, including employee involvement. Using matched employee-employer data from the WERS98 for the private sector – the wage, training, and human capital/demographic variables are taken from the employee component of the survey and the establishment data from the employer component of the survey (see the data section below) – Forth and Millward report that (log) earnings are strongly positively related to certain training durations (viz. 1-2 days and 2-5 days) relative to no training. Some effect of high performance work practices is also found, seemingly underwritten by job security guarantees. As far as unionism is concerned, only its direct effect on earnings is estimated. Although union recognition is associated with a wage premium of around 10 percent, this arises only in circumstances of multiunionism (under both single-table and separate bargaining). That is, there is no such wage differential when there is only a single recognized union at the workplace. Forth and Millward note the results of interacting high performance work practices with unionism, arguing that the premium associated with the former is

augmented under multiple unionism. But, to repeat, they do not interact unionism with the training argument.

This omission is tackled by Booth, Francesconi, and Zoega (2003) in a study using information from the British Household Panel Survey Data for a balanced panel of full-time males, 1991-96. The analysis considers both the duration of employer-provided training as in Forth and Millward (but now as a continuous rather than a categorical variable) and also its incidence. The authors find that union-covered workers are significantly more likely to receive training (between 5 and 9 percentage points) and to receive longer training (between 3 and 4 days) than their non-covered counterparts. (The lower estimates are for the panel estimates.) Moreover, the effects of training on wages are found to vary positively with union coverage, even as the positive effect of training on wages found using OLS is not replicated in fixed effects estimates. Taking the authors' fixed effects estimates for incidence, for example, it is found that union workers who receive any training earn roughly 6 percent more than their uncovered counterparts, half of which is the simple union premium. The effect of training duration is small, although the interaction of intensity and union recognition is again positive and statistically significant. Finally, in a separate analysis of wage growth that allows for changes in collective bargaining status and training incidence/duration, the main result is that gaining coverage is much more important for earnings growth than receiving training or obtaining longer training without gaining union coverage. In short, there is little in these data to suggest that unionism is associated with lower returns to training or lower wage growth.

But if unions do not reduce the incentives to acquire work-related training it is too early to conclude from one study that the productivity of training in union plants is higher than in nonunion establishments. Moreover, as Dearden, Reed, and Van Reenen (2000, p. 6) caution, earnings "only tell half the story." The course of earnings reflects not only value marginal product development

but also the share principle covering training investments (both firm-specific and also general training in the light of recent theoretical developments stressing labour market imperfections), effort-motivating career wage profiles, product market imperfections, and indeed other types of training investments (principally informal types of training on which the standard data sets are silent). It is therefore necessary to supplement the earnings function approach with a more direct measure of productivity than the wage. Indeed, testing modern theories of wage compression and training require such data to test the implication that productivity increases faster than earnings.

There is a small but growing training-in-the-production-function literature. A summary of the main studies is consigned to Appendix Table 1.[5] As can be seen, just one study is for Britain, namely, Dearden, Reed, and Van Reenen's (2000, 2006) analysis of a panel of *industries* between 1983 and 1996, and where the training information is derived from the Labour Force Survey. The study is notable for its use of an extended panel with information on training and productivity for each year of the sample period. This enables the authors to deal with problems of unobserved heterogeneity (some industries may have lower rates of technological change and therefore offer less scope for training) and endogeneity (training may be undertaken when its opportunity costs is lower as when firms experience transitory demand shocks) using GMM system methods. The upshot of this approach is that weak training effects on productivity detected in OLS are considerably strengthened using a within group estimator and further strengthened using a GMM estimator with endogenous training. In robustness tests, the authors report that collective voice effects do not seem to underpin the stronger training results: the point estimate of training is little affected by the inclusion of a union measure (viz. density) (only available from 1989 onward) while the coefficient estimate for unionism is negative albeit statistically insignificant. (We note

parenthetically that the union variable when considered alongside training in the production function literature is often statistically insignificant.)

Finally, Dearden, Reed, and Van Reenen offer a parallel earnings function analysis. Familiarly, the strong association between training and wages is considerably reduced with controls for skills**,** but is stronger in the within group estimates and more so using the GMM estimator. But the wage gradient of training is half the productivity gradient, leading the authors to conclude that the earnings function approach "ignores the benefits the firm may capture through higher profits" (Dearden, Reed, and Van Reenen, 2000, p. 53).

One problem with this important study apart from its neglect of the union-training nexus – the production function studies in Appendix Table 1 typically ignore union impact[6] and none considers the productivity of training in union and nonunion regimes – is possible aggregation bias. That said, this level of analysis may capture externalities from training (knowledge spillovers) that perforce escape identification at the firm level.

The bottom line is that some progress has been made in charting the impact of unions on workplace training and of training on earnings and output. However, only limited progress has been made in determining how unionism might mediate the impact of training on earnings, and there has been no examination of this interaction for performance measures other than earnings. The present exercise seeks in part to redress this imbalance. It does so by exploring the determinants of training in a framework that investigates the impact of training and union recognition on two subjective measures of firm performance, namely, relative labour productivity and financial performance in addition to earnings.

III. Data

Our data are taken from the 1998 Workplace Employee Relations Survey (WERS98) and the 2004 Workplace Employment Relations Survey (WERS04). Each survey follows closely the format of the earlier Workplace Industrial Relations Surveys/WIRS (for 1980, 1984, and 1990), albeit with some differences (see Cully et al., 1999). WERS98 is a national survey of 2,191 British establishments in the public and private sectors with at least 10 workers; respectively, the WERS04 has data on 2,295 establishments with at least 5 workers. Otherwise, the surveys are almost identical in terms of questions, sampling frames, and representation of workplaces across the British economy. The main focus of the surveys is a *management questionnaire* that provides detailed information on the composition of the workforce, management of the personnel function, representation at work, consultation and communication, payment systems and pay determination, workplace flexibility, and (largely qualitative) information on workplace performance. In addition, 25 employees at each workplace – or all employees at smaller establishments – are randomly selected for an *employee questionnaire*.[7] The individual worker questionnaire inquires of the employee respondent the nature of the job held, the training received, attitudes towards the organization and management, representation at work, as well as educational level and earnings. Unlike earlier WIRS, therefore, WERS98 and WERS04 include information on both individual and workplace characteristics. Both surveys also have high response rates. Using both 1998 and 2004 data illuminates changes in workplaces over time as well as providing a very recent description of British workplaces. (More details are provided in Kersley et al., 2005).

For that part of our analysis dealing with the determinants of training and the effect of training on wages, we will match the employee and workplace (i.e. management survey) components of WERS98/WERS04. We shall also present parallel results using workplace data on training and earnings information from the management survey. Earnings from the *employee*

questionnaire are in the form of gross weekly earnings that are reported in twelve earnings bands.[8] Using the relevant midpoint value in conjunction with reported hours of work, we derive a measure of gross hourly wages. Earnings from the *management survey* are in the form of gross annual wages in six earnings bands. Using midpoint values – this time in association with the number of workers populating each band – we are able to construct a measure of plant 'median' earnings.

The critical training variable also differs as between the two components of WERS98/WERS04, even if in each case it refers to formal off-the-job training. For the employee survey the training question asks**:** "During the last 12 months, how much training have you had, either paid for or organized by your employer?"[9] The employee is asked is required to tick one of six 6 boxes: 'none,' 'less than 1 day,' '1 to less than 2 days,' '2 to less than 5 days,' '5 to less than 10 days,' and '10 days or more'). From these responses, we construct two training measures: first, we define *training incidence* to take the value of 1 if the respondent received any such training, zero otherwise; second, we define *training duration* as either 0 or the midpoint of the reported bands.

The (main) training question in the management survey asks**:** "What proportion of experienced employees in the largest occupational group have had formal off the job training over the past 12 months?"[10] There are seven possible responses, comprising upper and lower limits of 'all' and 'none' and five intermediate bands. We define *training incidence* to be 1 where the employer responds that more than 0 percent received training. We also use the question to define *training intensity* (or *coverage*), assigning the plant to one of the seven intervals, using the midpoints of the bands as appropriate. We also employ responses to a second training question in the employer survey to derive a measure of *training duration* analogous to that contained in the employee survey. This second question asks the employer to identify which of six intervals best

describes “on average, about how much time did these… employees … spend in formal off-the-job training sessions over the past 12 months?” We weight these hourly values by the corresponding intensity or coverage values to produce an estimate of plant-level training duration.

In addition to the conventional use of earnings as a performance indicator (see the literature review), we also use two other outcome indicators, namely, labour productivity and financial performance, taken from the management survey. Given the partial sampling of employees in the employee survey, we do not link these data to the employee survey. Thus, when we form an instrument for training for inclusion in the performance equations (see below) this is based on plant-level data alone.

We next briefly describe the labour productivity and financial performance dependent variables. Each is subjective, the manager respondent being asked to “assess your workplace’s labour productivity/financial performance” vis-à-vis the average of “other establishments in the same industry.” Responses in each case are coded ‘a lot better/better than average,’ ‘above average,’ ‘about average,’ and ‘a lot below/below average.’ For both indicators, we define *above average performance* as 1 (combining the first three responses), zero otherwise.[11]

Our measure(s) of unionism is the same across all estimations and is a plant-level measure. *Union recognition* is set equal to 1 if the employer recognizes any trade union at the place of work for the purpose of negotiating pay and conditions for any sections of the workforce. We also use an alternative measure of unionism based on *bargaining structure*. Vis-à-vis no recognition (the omitted category), we identify circumstances in which the employer bargained with a single union or with multiple unions either jointly (i.e. single-table bargaining) or severally.

The remaining variables are more easily described because they have been widely used in previous empirical work. Thus, the equations using as dependent variables individual-level

earnings and training data from the employee questionnaire contain standard human capital arguments (such as educational attainment, occupational controls, and age and tenure) and demographic controls (such as marital status, gender, and ethnicity). They also include the same workplace covariates as are used in all equations based on management survey data alone – other than those used to identify the plant-level training equations fitted to those data. In addition to various high performance working practices (described below), the workplace-level arguments include labour force composition (proportion of female, part-time, and manual workers), plant and wider organization characteristics (establishment/organizational size, status as a single operating establishment or otherwise, and capital intensity), product market competition, firm ownership, and (eight) industry dummies.

As for the high performance working practices, these comprise *team working* (at least 60 percent of employees work in the largest occupational group work in formally designated teams), *quality circles* (presence of workplace groups that solve specific problems or discuss aspects of performance or quality), *briefing groups* (a system of briefing nonmanagerial employees at the workplace that occurs at least monthly and where at least 10 percent of time is dedicated to questions/contributions from employees), *information disclosure*, (management regularly provides workers with information on the plant's financial situation and external investment plans), and *financial participation* (based on eligibility to participate in an ESOP arrangement or profit- or performance-related pay). We do not allow for the bundling such practices or for their frequency and intensity (on which, see Forth and Millward, 2004) as our main interest lies elsewhere.

One practice that is often considered to belong to the group of innovative work practices is *job security guarantees*. We would anticipate that job security guarantees either call for a more flexible (i.e. more highly trained) workforce or stimulate training in the downturn. Two other rather

more obvious training-related arguments proxy the scope for training and the priority accorded training. As an indicator of the scope for training, we deploy a dummy variable set equal to 1 if *normally takes at least one month before new employees in the largest occupational group are able to do their job*, and 0 if less than that. And, as an indicator of the priority accorded training, we use another dichotomous variable that assumes the value of 1 if the establishment *sets targets for workplace training*, 0 otherwise. All three variables are used to help identify the training equation.

In addition, these three variables are used as instruments for training in our labour productivity and financial performance equations. Recognizing that outcomes may be endogenously determined with the decision to provide training, we perform an instrumental variables regression. Predicted values for each measure of training – along with their interactions with unionism – replace the endogenous regressor (the direct measures of training).

Finally, we restrict our analysis to the private sector and use the sampling weights given in the two surveys. For the WERS98, our cross sections overall cover 17,092 individuals with complete data on 1,100 establishments. The corresponding values for the WERS04 are 13,643 individuals and 1,449 establishments.

Descriptive statistics for the matched employee-employer sample and for the workplace sample are reported in Appendix Tables 2 and 3, respectively. For the former sample, the incidence of training across the two surveys is quite similar, at 51 percent and 59 percent. Most establishments offer some form of training, with the rate higher in 2004. For training intensity, more firms offer either all or few workers training in 2004 whereas the spread was more even in 1998. The durations of training are also similar: the mean duration of training is 2.86 days for the WERS 1998 and 2.71 for the WERS 2004. However, union recognition is lower in 2004 – at 43 percent compared with 51 percent in 1998. The difference is mainly explained by the lower rate of

multiple unionism in 2004. For the latter workplace samples, the measures for financial performance and labour productivity are very close; just over one-half of firms are identified as having financial performance "better than average" and almost exactly half report "better than average" labour productivity. The workplace-level data do, however, reveal a much sharper difference in union recognition: with 42 percent of workplaces being recognized in 1998 versus just 15 percent in the 2004 sample.

IV. Findings

Our starting point is the determinants of training and earnings. In each case, we will provide results using both individual and plant-level measures of training and earnings. As noted above, the former information (as well as the human capital and demographic measures) are derived from the employee questionnaire of each WERS and are also matched to establishment-level information from the management questionnaires. This part of our analysis corresponds most closely with the existing British literature. Our separate results using plant-level training information are based on workplace-level data from the management surveys alone and are organically linked to the subsequent analyses of plant productivity and financial performance.

(Table 1 near here)

Table 1 provides summary probit and tobit regression results for individual-level training incidence and duration, respectively, using two measures of unionism in each case. Panel (a) provides results from the WERS98, and panel (b) from the WERS04. As can be seen, whether or not a worker receives training appears uninfluenced by the union recognition status of the plant or by the structure of collective bargaining. Results for the full set of regressors – not provided in the table but available from the authors upon request – reveal that comparatively few of the variables

taken from the employee surveys are statistically significant.[12] Rather, plant-level variables tend to dominate. Thus, the likelihood that the worker receives training frequency is higher the larger the plant and where the organization deploys high performance work practices, and it is lower the larger the share of part-timers and manual workers in the workforce and in single-plant firms.

For its part, training duration also appears unaffected by union recognition per se in either the WERS98 or the WERS04. But for the former dataset at least differences emerge by bargaining structure: compared with plants without union recognition, workers in establishments that bargain with multiple (single) unions offer longer (shorter) training spells over the course of a year. For the WERS04 although the coefficient estimates for all forms of collective bargaining are positive, none approaches statistical significance at conventional levels. Most of the plant-level variables found to influence training incidence operate in the same manner with respect to training duration. Interestingly, in the case of the WERS98, the role of the human capital and demographic variables taken from the employee survey assume much more importance for training duration than they did for training incidence. (Notable across both datasets is a decline in the duration of training with age and tenure.) Also of interest is the finding across both datasets that U.K.-owned plants offer shorter training spells than their foreign-owned counterparts.

(Table 2 near here)

The corresponding results for training using plant-level variables alone are given in Table 2. The equations now include three plant-level measures not encountered in Table 1 that are used to identify our plant-level training equation for subsequent stages of the analysis (on which more below). The results given in the first four columns of the table refer to the incidence and coverage of training by union measure, while the last two relate to our imputed duration of training measure. The most notable result is the general statistical insignificance of the union variable. Union

recognition is not statistically significant in any model. For the WERS98 the different collective bargaining structures show either insignificant or inconclusive impacts: for incidence, single-table bargaining is positive; for duration, multiple bargaining is negative. For the WERS04 no union variable attains statistical significance.

Interestingly, two out of the three variables we consider as identifiers – picking up the scope for training (time taken for new workers to become proficient) and the priority accorded training (targets set for workplace training) – are both positive and well determined across all six regressions for the WERS98. Although the variable proxying the need for a more flexible workforce and/or suggestive of more training in the downturn plays no role for the WERS98, the opposite is true for the WERS04 where the coefficient estimate for job security guarantees is statistically significant in four out of six specifications. Training is higher in the WERS04, as in the WERS98, when targets are set for workplace training but there is no indication that the time taken for new workers to become proficient in a job is a determinant of either the intensity or duration of training in the later dataset.

As far as the other covariates are concerned, across both data sets the results for training frequency (incidence and coverage) in the first four columns are roughly coincident. Thus, training frequency is declining in the share of part-timers and manual workers and increasing in establishment size and with three out of four high performance work practices. For training duration, although much the same arguments are statistically significant for the WERS98 the results are weaker for the WERS04. Interestingly, older establishments seem to train workers longer in both surveys.

The impact of training and unions on (log) hourly pay using matched employee-establishment data is shown in Table 3A. As before, two characterizations of unionism are

provided. Also as before, we consider both training incidence and duration and all specifications include interactions between training and unionism. Table 3B supplements this analysis with results for the categorical measure of training duration, using the actual bands identified in the employee questionnaire, although in this case only the union recognition argument is deployed for reasons of expositional convenience.

(Tables 3A and 3B near here)

There are few points of agreement as regards the estimates in panels (a) and (b) of Table 3A. Beginning with the WERS98, then, it seems to be the case that training incidence, if not duration, is positively associated with earnings but union recognition per se is not, while the interaction between training incidence and union recognition is positive and marginally significant. Once we amend the union measure to reflect bargaining structure, it can be seen that (direct) union effects on earnings seem to be confined to multiple unionism, while the interaction between unionism and training incidence is positive and significant for one type of multiple unionism, namely, where there is single-table bargaining. Where training duration is expressed as a continuous variable neither it nor union recognition is statistically significant, and the same is true of their interaction. Nevertheless, the union argument alone is well determined in the case of multiunionism. Once training duration is entered in categorical form, it can be seen from Table 3B that some intermediate levels of training have well determined positive effects. While the union recognition coefficient estimate in this specification is now strongly significant, those of the interaction terms are negative – the one exception is the interaction with training of ten days duration or more where training itself is not positively associated with wages.

Turning to the WERS04 in Table 3A, one notable result is the virtual absence of any union effect on earnings. This outcome is not unexpected and seems to mark a further reduction in what

we might generically term the disadvantages of unionism (Addison and Belfield, 2004). No less notable is the general absence of a positive effect of training on wages. In the case of training incidence, training only achieves statistical significance when interacted with one of the (multiunionism) bargaining structure arguments. Exactly the same result applies for training duration in continuous form. Only when we consider training duration in categorical form (Table 3B) are both union and training effects more evident, and more in line with the WERS98. Unions have a small but well determined positive effect on wages, and all training intervals – with the exception of the shortest and longest spells – are associated with higher earnings. That said, there is less suggestion of material interactions in either direction between union recognition and training duration.[13]

By way of summary, our findings up to this point differ from those reported in the literature in a number of important respects. One is the absence of any simple effect of unions on training incidence, duration, or coverage. And while our findings on the union wage premium are consistent with the literature – in denying a simple association between unionism and pay and noting that the premium where observed (for the WERS98) hinges on bargaining structure – the near absence of significantly positive interaction effects between unionism and training conflicts with the optimistic findings of the one British study to have investigated this issue.[14]

These observations and the limitations of wages as a measure of productivity led us to consider whether stronger effects of training (and unions) might be discernible using the labour productivity question in each workplace survey, as well as the longer-term financial performance indicator. It will be recalled that both indicators are dummy variables where *above average performance* equals 1, zero otherwise. Initially, when we regressed our labour productivity measure on the reported values of various training and union arguments, the results were unspectacular. In

the case of the WERS98, for example, just one association was statistically significant (a negative coefficient estimate for single-table bargaining under multiple unionism). For WERS04, the union effects were more clearly negative, and the training results weakly positive.

Given possible endogeneity bias, both performance equations were duly estimated with predicted rather than actual training values. We instrumented our three plant-level training variables using the specifications in Table 2. The validity of the instrumental variables estimation depends on finding variables correlated with the endogenous regressor (training), but uncorrelated with the performance measures. As shown in Table 2, two of our three instrumental variables are strongly correlated with training incidence, intensity, and duration. When these variables were included in the labour productivity and financial performance equations they evinced very weak explanatory power (the strongest effect being the experience requirement, although this too was inconsistent). Next, we ran all the equations (six per outcome indicator) using the reported training values. A comparison of the coefficient estimates for actual and predicted training – a direct Hausman test was not performed because the equations are not OLS – revealed that in most cases (all cases for the WERS04) the latter exceeded the former, while preserving levels of statistical significance. Moreover, the union status variables were unaffected. Finally, we performed an augmented regression test, as proposed by Davidson and MacKinnon (1993, p. 236). Specifically, we included both the actual and predicted measures of training together in each of the performance equations: circumstances in which the coefficient on the predicted training measure is statistically significant can be interpreted as indicating that that the instrumental variables approach is consistent. For the WERS98, in seven of the twelve equations, the predicted measure of training was statistically significant ($p<0.05$) and the union coefficients were unaffected. For the WERS04,

on the other hand, the coefficients were mostly insignificant, reflecting the generally weaker results we review below.

(Table 4 near here)

Turning therefore to the performance equations, panels (a) and (b) of Table 4 present the labour productivity results for WERS98 and WERS04, respectively, using instrumented plant-level training variables. For the earlier survey, the coefficient estimates for predicted training – incidence, intensity, and duration – are each positive and statistically significant. For its part, the simple union variable – union recognition – is with one exception poorly determined, and the interaction term between union recognition and the training measure is never statistically significant. For equations taking explicit account of the bargaining structure there are few statistically significant coefficients, except for single-table bargaining which shows a negative relationship to labour productivity. The results for the WERS04 are similar in several respects. Thus, the coefficient estimate for the predicted training argument is again positive and statistically significant throughout – albeit now only marginally so for training duration – and the simple union recognition variable is never statistically significant when interacted with predicted training. That said, single-table bargaining is now associated with higher labour productivity and conversely for separate bargaining with a number of unions. If these two latter results are regarded as more consistent with one's priors, note however that the directional effects of each variable are reversed in interactions with predicted training.

(Table 5 near here)

Finally, Table 5 charts the association between training, unions, and financial performance. Comparison of panels (a) and (b) of the table now indicates sharp differences between the two

workplace surveys. First, for the WERS98 predicted training is positively associated with above average financial performance, whereas for the WERS04 no such association is evident. Second, in results reminiscent of a much earlier literature, the effects of union recognition on financial performance are adverse in two out of three specifications for the WERS04, while no such negative effects are discerned for the WERS98. Third, single-table bargaining is associated with below average financial performance in the WERS98, whereas for the WERS04 it is separate bargaining with several unions that is associated with this outcome. The latter results pertaining to bargaining structure mirror those reported earlier in Table 4 for labour productivity (as do the respective interactions with predicted training).

Given the subjective nature of the productivity and financial performance variables, however, we should be wary of placing too much emphasis on the specifics of the above. Moreover, these results are non-commensurate with the earnings findings and it will be interesting to see if they will be corroborated using objective measures that can be fashioned out of the Financial Performance Questionnaire of WERS04. That said, there is a measure of consistency in the labour productivity results. Moreover, the type of training considered here does seem to be pro-productive and unionism does not seem to prejudice its payoff in any consistent manner.

V. Conclusions

This study is the first using WERS data to examine the impact of training at the workplace on earnings, labour productivity, and financial performance. It does so in a framework that accords equal emphasis to labour unions. Contrary to the predictions of the simple competitive model at least, union recognition does not appear to reduce the frequency of employer-provided off-the-job training. Indeed, individual worker data suggest that training duration *may* be longer in certain

union bargaining regimes. Plant-level training data also contain no suggestion of any reduction in training incidence or coverage in union regimes, but do hint that training duration may be reduced in situations where multiple unions bargain separately.

What of the productivity of training? Although there are some differences between the results obtained from individual and plant-level earnings data, the productivity of training is at least weakly confirmed in the earnings data. The suggestion that single-table bargaining may actually be associated with higher returns to training (incidence in the matched employee-employer regressions and to duration in the workplace-level estimates) has also to be considered alongside some other less positive results (specifically, negative interactions between union recognition and some training durations in the matched data).

If we were to stop here, we might conclude that the balance of our evidence is more favourable to the new view of unionism than to the standard competitive model. But we are not speaking of a ringing endorsement, which recognition led us to experiment with a more direct measure of output than earnings. Our investigation of a subjective measure of labour productivity taken from the management survey cast some doubt on the new view of unionism while considerably strengthening the pro-productivity effects of training. In the former case, we are speaking of the appearance of some negative direct union effects rather than a reduced payoff to training in union regimes. Our separate analysis of financial performance produced results that were broadly consistent with the same-survey labour productivity results although only for the WERS98 was there the clear suggestion that training benefits the bottom line.

Finally, we should note the difficulties of measuring training and identifying its impacts across entire establishments. Many individual workers may engage in and benefit from training, even in firms where little training is undertaken; these effects will be hard to capture if there is

significant within-firm variation in training compared to between-firm variation. Here we are constrained by the data, which ruled out the use of a fixed effects estimator. However, we were able to use both workplace and individual data, with multiple measures of wages, training, and unionization. Moreover, our inquiry further looked to impacts along the dimensions of overall labour productivity and financial performance.

Endnotes

*The authors thank the sponsors of WERS (DTI, ACAS, ERSC, and PSI) and the ESRC Data Archive.

1. The classic reference is Mincer (1983); see also Lindbeck et al. (1993).

2. The authors' actual variable is the presence of multiple unions at the workplace (interacted with union recognition), thus conflating separate and joint bargaining on the part of multiple unions.

3. For a wide-ranging German study of the effect of training on earnings by type of employee and form of training, see Kuckulenz and Zwick (2003).

4. A negative correlation between wages and training might be anticipated: during the training period, workers are not fully productive and will only become more productive after the training is concluded (Bartel, 1995). (We appreciate the contribution of a reviewer in pointing this out.) However, our training measure is retrospective, extending over an entire year. It therefore covers periods before, during, and after training. Nevertheless, we recognize that we may not have fully captured the wage effects of training.

5. Additional studies not cited in the table would include Ballot et al. (2001) for France and Sweden, and Bellmann and Bücherl (2001) for Germany.

6. An exception is Zwick (2005) who includes a codetermination variable (i.e. works council presence) as a regressor in his lagged productivity estimates of training intensity and alternative forms of training. Works council presence is associated with higher productivity in his OLS estimates. In his two-step panel estimates, however, codetermination is no longer statistically significant whether or not selection into training is accommodated. He obtains stronger results for the works council variable in a subsequent paper examining training intensity alone (albeit over a

longer sample period) that also controls for time invariant unobserved heterogeneity between firms and the endogeneity of training (Zwick, 2006). Neither study tests whether the productivity effects of training are contingent on other establishment characteristics or the establishment's environment, but in a separate treatment examining the effect of works councils on the productivity impact of *direct employee participation*, Zwick (2003) reports that the payoff to such practices is restricted to works council regimes. This study also looks at training, which is found to have a bigger payoff in works council regimes. Unlike employee participation, however, training is now assumed to be exogenous.

7. It follows that the matched employee-employer aspect of the survey is considerably more limited than in other datasets, and, in particular, the German LIAB. Also, because the two datasets are not longitudinal but harmonized cross-sectional surveys we cannot apply the techniques detailed by Abowd et al. (1999).

8. For the highest and lowest bands, we compute a hypothetical midpoint assuming that the range matches that of the next and the previous earnings band, respectively. The selfsame procedure is used in the case of all other variables with open intervals. Prior studies using WERS98 data have found that results are not sensitive to the method of banding.

9. The employee is asked to "include only training away from your normal place of work, but it could be on or off the premises." More generally, we do not of course know how much training – more or less – the worker received in any preceding interval. The same limitation attaches to the management questionnaire.

10. The survey explicitly includes the *prompt*: "off the job training is training away from the normal place of work, but either on or off the premises."

11. We note here that these are subjective measures of performance and productivity (financial data is not available for the WERS98); and as such we place more reliance on the results for earnings. Nevertheless, these measures have been used extensively in other empirical investigations into the determinants of firm performance (see Addison and Belfield, 2004).

12. That said, for the WERS04 the coefficient estimates for gender and tenure as well as those for some of the education categories (and almost all of the occupational arguments) are well determined.

13. We also fitted a (log median annual) wage equation to data from the management survey, using plant-level covariates alone. This estimation serves as a check for plant-level wage effects from 'high-training' firms. For the WERS98 some statistically significant associations between training and wages were found for training intensity and training duration if not its incidence. There were no systematic union effects on earnings, and where significant the effects were inconclusive. Further, just two out of a total of twelve union-training interaction terms were statistically significant. For the WERS04 very few of the coefficients are statistically significant. In re-estimating these wage equations excluding interaction terms, the coefficient estimates for each training measure were uniformly positive and statistically significant while those for unionism were always poorly determined in the case of the WERS98, and statistically insignificant throughout for the WERS04.

14. We ran a series of sensitivity checks on the training determinants and wage equations using alternative estimation techniques. These techniques were applied to see if the coefficients were driven by unobservable firm-specific characteristics. (Full results are available from the authors upon request.) The first check was to re-estimate the wage equations to adjust for intra-firm correlations (using the post-estimation cluster command, see Moulton, 1986). The second

alternative was to apply a random effects estimator across firms. Overall, the changes were minor and again do not point to a consistent pattern of union influence on training or a more emphatic impact of unions and training on wages. For the WERS98, unions still do not increase training incidence whatever the configuration of unionism; although they do now elevate training duration, with single unions now no longer having a negative impact. The WERS04 coefficient estimates remain statistically insignificant at the .05 level. For wages, the impact of training incidence and its interactions is now weaker but the effects remain as strong for the duration measures in the WERS98. Again, no clear effects from unions or training emerge when using the WERS04.

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Table 1: Determinants of Training in Matched Employee-Employer Sample

	Training Incidence		Training Duration	
	(1)	**(2)**	**(3)**	**(4)**
(a) WERS 1998:				
Union	-0.0011		-0.0667	
	(0.0318)		(0.1267)	
Single-table bargaining		0.0145		0.6972***
		(0.0485)		(0.1835)
Joint bargaining		0.0321		0.6860***
		(0.0562)		(0.2023)
Single union		-0.0117		-0.4665***
		(0.0348)		(0.1386)
n	*17092*	*17092*	*17092*	*17092*
(b) WERS 2004:				
Union	0.0442		0.1406	
	(0.0344)		(0.1500)	
Single-table bargaining		0.0284		0.2498
		(0.0498)		(0.2291)
Joint bargaining		0.0643		0.3964
		(0.0604)		(0.2664)
Single union		0.0458		0.0411
		(0.0384)		(0.1687)
n	*13643*	*13643*	*13732*	*13732*

Notes: Robust standard errors in parentheses. Probit estimation of equations (1) and (2); Tobit estimation of equations (3) and (4). The equations include the following individual covariates: gender; tenure; tenure squared; marital status; minority status; education (3 categories); children; disability; age (5); and occupation (8). The equations include the following workplace covariates: establishment age; U.K.-owned; single firm; % female workers; % part-time workers; % manual; shift-workers; capital–labour ratio (3); employment size; market competitiveness (2); organization size (2); team work; quality circles; briefing groups; information disclosure; financial participation; industry (8); and a constant term and 8 industry-sector dummies.
*, **, *** denote significance at 0.10, 0.05, and 0.01 levels, respectively.

Table 2: Determinants of Training at Workplace Level

	Training Incidence		Training Intensity		Training Duration	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
(a) WERS 1998:						
Union	0.0861		2.4125		-0.2540	
	(0.1328)		(2.8368)		(0.2281)	
Single-table barg.		0.8585**		5.4552		-0.4880
		(0.3905)		(4.3461)		(0.3475)
Joint barg.		-0.1687		-4.4933		-0.8369**
		(0.2305)		(4.4728)		(0.3610)
Single union		0.0451		3.2029		-0.0378
		(0.1437)		(3.1496)		(0.2534)
Experience reqd. to	0.3726***	0.3939***	13.1535***	13.8603***	1.1682***	1.2037***
do job >1 month	(0.1108)	(0.1132)	(2.5290)	(2.5438)	(0.2050)	(0.2060)
Job security	-0.0790	-0.0637	-1.3122	0.4570	-0.0708	0.0636
guarantees	(0.4399)	(0.4484)	(6.5732)	(6.6077)	(0.5247)	(0.5278)
Targets set for	0.2207*	0.2302*	15.3616***	15.6911***	0.7102***	0.7451***
training	(0.1198)	(0.1211)	(2.4336)	(2.4352)	(0.1960)	(0.1964)
n	*1100*	*1100*	*1100*	*1100*	*1100*	*1100*
(b) WERS 2004:						
Union	0.1043		5.6650		0.5066	
	(0.2391)		(3.7086)		(0.3244)	
Single-table barg.		0.1908		7.2810		0.1289
		(0.4002)		(6.8967)		(0.5822)
Joint barg.		0.2816		-9.5415		0.1424
		(0.5082)		(11.1538)		(0.9169)
Single union		0.0824		6.4949		0.6272
		(0.2619)		(3.9892)		(0.3491)
Experience reqd. to	0.0539	0.0541	0.0775	0.0488	-0.3061	-0.3032
do job >1 month	(0.1464)	(0.1465)	(2.4417)	(2.4398)	(0.2333)	(0.2332)
Job security	0.7448	0.7427	25.1900**	24.3222**	2.2711**	2.3316**
guarantees	(0.4568)	(0.4600)	(8.3617)	(8.4349)	(0.7447)	(0.7524)
Targets set for	0.2708	0.2711	16.9754***	16.8804**	1.7193**	1.7156**
training	(0.1670)	(0.1671)	(2.5222)	(2.5207)	(0.2232)	(0.2232)
n	*1449*	*1449*	*1471*	*1471*	*1228*	*1228*

Notes: Probit estimation of equations (1) and (2); Tobit estimation of equations (3) through (6). Standard errors in parentheses. Equations include the following workplace covariates: establishment age; U.K.-owned; single firm; % female workers; % part-time workers; % manual; shift-workers; capital–labour ratio (3 categories); employment size; market competitiveness (2); organization size (2); team work; quality circles; briefing groups; information disclosure; financial participation; industry (8); and a constant term and 8 industry-sector dummies.

*, **, *** denote significance at 0.10, 0.05, and 0.01 levels, respectively.

Table 3A: Determinants of Log Hourly Pay with Training/Union Interactions

	Training Measure			
	Incidence	**Incidence**	**Duration**	**Duration**
(a) WERS 1998:				
Union	0.0086		0.0163	
	(0.0120)		(0.0101)	
Training	0.0266**	0.0312***	-0.0016	-0.0013
	(0.0107)	(0.0107)	(0.0013)	(0.0013)
Training * union	0.0311*		0.0026	
	(0.0164)		(0.0019)	
Single-table barg.		0.0369**		0.0722***
		(0.0172)		(0.0182)
Joint barg.		0.0750***		0.0666***
		(0.0199)		(0.0182)
Single union		-0.0012		-0.0039
		(0.0135)		(0.0113)
Training * Sing.-table barg.		0.0740***		0.0024
		(0.0265)		(0.0033)
Training * Joint barg.		-0.0001		0.0027
		(0.0275)		(0.0030)
Training * Single union		-0.0025		0.0003
		(0.0183)		(0.0022)
R^2	0.56	0.57	0.56	0.57
n	*17092*	*17092*	*17092*	*17092*
(b) WERS 2004:				
Union	0.0164		0.0118	
	(0.0165)		(0.0130)	
Training	-0.0022	-0.0023	0.0008	0.0008
	(0.0016)	(0.0016)	(0.0015)	(0.0015)
Training * union	0.0020		0.0020	
	(0.0023)		(0.0022)	
Single-table barg.		0.0323		0.0201
		(0.0263)		(0.0199)
Joint barg.		0.0145		0.0207
		(0.0239)		(0.0201)
Single union		0.0128		0.0089
		(0.0191)		(0.0148)
Training * Sing.-table barg.		-0.0035		0.0002
		(0.0037)		(0.0029)
Training * Joint barg.		0.0102**		0.0109**
		(0.0041)		(0.0054)
Training * Single union		0.0018		0.0001
		(0.0028)		(0.0025)
R2	0.44	0.44	0.44	0.44
N	*11628*	*11628*	*13289*	*13289*

Notes: Robust standard errors in parentheses. OLS estimation. Equations include the following individual covariates: gender; tenure; tenure squared; marital status; minority status; education (3 categories); children; disability; age (5); and occupation (8). Equations include the following workplace covariates: establishment age; U.K.-owned; single firm; % female workers; % part-time workers; % manual; shift-workers; capital-labour ratio (3); employment size; market competitiveness (2); organization size

(2); team work; quality circles; briefing groups; information disclosure; financial participation; industry (8); and a constant term and 8 industry-sector dummies.
*, **, *** denote significance at 0.10, 0.05, and 0.01 levels, respectively.

Table 3B: Determinants of Log Hourly Pay with Training/Union Interactions

	Survey	
	WERS 1998	**WERS 2004**
Union	0.0379***	0.0319*
	(0.0125)	(0.0168)
Training (<1 day)	0.0314	-0.0166
	(0.0193)	(0.0215)
Training (1-2 days)	0.0656***	0.0577***
	(0.0162)	(0.0207)
Training (2-4 days)	0.0758***	0.0394**
	(0.0166)	(0.0160)
Training (5-9 days)	0.0703***	0.0504**
	(0.0206)	(0.0233)
Training (>=10 days)	-0.0477**	-0.0046
	(0.0212)	(0.0235)
Training (<1 day)*union	-0.0340	-0.0101
	(0.0276)	(0.0289)
Training (1-2 days)*union	-0.0294	-0.0854***
	(0.0240)	(0.0291)
Training (2-4 days)*union	-0.0501**	-0.0226
	(0.0226)	(0.0238)
Training (5-9 days)*union	-0.0528*	0.0043
	(0.0309)	(0.0368)
Training (>=10 days)*union	0.0670**	0.0237
	(0.0310)	(0.0349)
R^2	0.57	0.44
n	*17,092*	*13,289*

Notes: Robust standard errors in parentheses. OLS estimation. Equations include the following individual covariates: gender; tenure; tenure squared; marital status; minority status; education (3 categories); children; disability; age (5); and occupation (8). Equations include the following workplace covariates: establishment age; U.K.-owned; single firm; % female workers; % part-time workers; % manual; shift-workers; capital-labour ratio (3); employment size; market competitiveness (2); organization size (2); team work; quality circles; briefing groups; information disclosure; financial participation; industry (8); and a constant term and 8 industry-sector dummies.

*, **, *** denote significance at 0.10, 0.05, and 0.01 levels, respectively.

Table 4: Determinants of Labour Productivity with Full Interactions

	Effect on Labour Productivity By Training Measure					
	Incidence	**Incidence**	**Intensity**	**Intensity**	**Duration**	**Duration**
(a) WERS 1998:						
Predicted training ($\check{T}$)	0.9935***	0.9315***	0.0169***	0.0154***	0.2737***	0.2545***
	(0.2569)	(0.2008)	(0.0047)	(0.0046)	(0.0695)	(0.0681)
Union	-0.3220		-0.3325*		-0.1849	
	(0.2043)		(0.1766)		(0.1252)	
$\check{T}$ * Union	0.0590		0.0035		0.0884	
	(0.1385)		(0.0041)		(0.0634)	
Single-table barg.		-1.0266*		-0.9707**		-0.2946
		(0.5273)		(0.4290)		(0.2635)
Joint barg.		-0.4318		-0.3779		-0.2049
		(0.2845)		(0.2922)		(0.1935)
Single union		-0.3153		-0.2788		-0.1946
		(0.1958)		(0.2037)		(0.1453)
$\check{T}$ * Single-table barg.		-0.0312		0.0107		0.0579
		(0.1870)		(0.0082)		(0.1270)
$\check{T}$ * Joint barg.		0.1869		0.0045		0.1254
		(0.1538)		(0.0069)		(0.0992)
$\check{T}$ * Single union		0.1865		0.0046		0.1209
		(0.1444)		(0.0049)		(0.0780)
n	*1100*	*1100*	*1100*	*1100*	*1100*	*1100*
(b) WERS 2004:						
Predicted training ($\check{T}$)	1.0484***	1.0413***	0.0162**	0.0164**	0.1177*	0.1231*
	(0.3767)	(0.3767)	(0.0072)	(0.0073)	(0.0710)	(0.0705)
Union	-0.3706		-0.2531		-0.6969*	
	(0.3425)		(0.4248)		(0.3861)	
$\check{T}$ * Union	-0.0819		-0.0036		0.0683	
	(0.1506)		(0.0066)		(0.0968)	
Single-table barg.		1.3940*		1.6178*		0.6114
		(0.7150)		(0.8545)		(0.6773)
Joint barg.		-1.7050**		-1.6966*		-1.2869*
		(0.8497)		(0.9018)		(0.6786)
Single union		-0.5962		-0.4260		-0.8883*
		(0.3930)		(0.4856)		(0.4586)
$\check{T}$ * Single-table barg.		-0.6267**		-0.0212*		-0.0910
		(0.2497)		(0.0121)		(0.1570)
$\check{T}$ * Joint barg.		0.3197		0.0247		0.2557
		(0.2852)		(0.0158)		(0.2132)
$\check{T}$ * Single union		-0.0348		-0.0036		0.0707
		(0.1801)		(0.0074)		(0.1128)
n	*1297*	*1297*	*1329*	*1329*	*1329*	*1329*

Notes: Probit estimation. Robust standard errors in parentheses. Predicted training is based on training incidence (columns (1) and (2)), training intensity (columns (3) and (4)), and training duration (columns (5) and (6)). Equations include the following workplace covariates: establishment age; U.K.-owned; single firm; % female workers; % part-time workers; % manual; shift-workers; capital-labour ratio (3 categories); employment size; market competitiveness (2); organization size (2); team work; quality circles; briefing groups; information disclosure; financial participation; industry (8); and a constant term and 8 industry-sector dummies.
*, **, *** denote significance at 0.10, 0.05, and 0.01 levels, respectively.

Table 5: Determinants of Financial Performance with Full Interactions

	Effect on Financial Performance By Training Measure					
	Incidence	**Incidence**	**Intensity**	**Intensity**	**Duration**	**Duration**
(a) WERS 1998:						
Predicted training ($\check{T}$)	1.0439***	0.9548***	0.0182***	0.0156***	0.3370***	0.3014***
	(0.2653)	(0.2048)	(0.0048)	(0.0048)	(0.0709)	(0.0697)
Union	-0.0441		-0.1130		0.0535	
	(0.1979)		(0.1773)		(0.1261)	
$\check{T}$ * Union	-0.0922		0.0000		-0.0367	
	(0.1360)		(0.0042)		(0.0646)	
Single-table barg.		-0.6672		-1.5644***		-0.4822*
		(0.5451)		(0.4687)		(0.2739)
Joint barg.		0.0319		0.1630		0.3401*
		(0.3007)		(0.3041)		(0.2003)
Single union		-0.0267		-0.0303		0.0373
		(0.1926)		(0.2032)		(0.1442)
$\check{T}$ * Single-table barg.		-0.2106		0.0217**		0.1316
		(0.1949)		(0.0092)		(0.1336)
$\check{T}$ * Joint barg.		0.0957		-0.0005		-0.0463
		(0.1746)		(0.0074)		(0.1079)
$\check{T}$ * Single union		-0.0275		-0.0006		-0.0257
		(0.1446)		(0.0050)		(0.0777)
n	*1069*	*1069*	*1069*	*1069*	*1069*	*1069*
(b) WERS 2004:						
Predicted training (Ť)	0.4813	0.4444	0.0027	0.0022	-0.0392	-0.0384
	(0.3743)	(0.3720)	(0.0071)	(0.0071)	(0.0697)	(0.0696)
Union	-0.5321		-0.8308*		-1.2274***	
	(0.3644)		(0.4810)		(0.3761)	
Ť * Union	0.1376		0.0094		0.2651***	
	(0.1625)		(0.0072)		(0.0930)	
Single-table barg.		0.8823		0.6273		-0.2698
		(0.7331)		(0.9315)		(0.6799)
Joint barg.		-3.1056***		-2.8938***		-2.2325***
		(0.8493)		(1.0392)		(0.7913)
Single union		-0.6922		-0.9035		-1.2862***
		(0.4346)		(0.5613)		(0.4355)
Ť * Single-table barg.		-0.3895		-0.0073		0.1073
		(0.2591)		(0.0126)		(0.1546)
Ť * Joint barg.		0.8068***		0.0374*		0.4203*
		(0.2876)		(0.0197)		(0.2545)
Ť * Single union		0.2249		0.0099		0.2673**
		(0.2026)		(0.0083)		(0.1061)
n	*1318*	*1318*	*1352*	*1352*	*1352*	*1352*

Notes: Probit estimation. Robust standard errors in parentheses. Predicted training is based on training incidence (columns (1) and (2)), training intensity (columns (3) and (4)), and training duration (columns (5) and (6)). Equations include the following workplace covariates: establishment age; U.K.-owned; single firm; % female workers; % part-time workers; % manual; shift-workers; capital-labour ratio (3 categories); employment size; market competitiveness (2); organization size (2); team work; quality circles; briefing groups; information disclosure; financial participation; industry (8); and a constant term and 8 industry-sector dummies.
*, **, *** denote significance at 0.10, 0.05, and 0.01 levels, respectively.

Appendix Table 1: Selected Production Function Estimates of the Effect of Training on Productivity

Study	Data	Methodology	Training Measure	Other Controls	Findings
1. Black and Lynch (1996)	Educational Quality of the Workforce (EQW) National Employers Survey firm data for 1993 (n=1,346).	Cobb-Douglas production function framework. Dependent variable is log sales.	Total number of workers trained in 1993; the percentage of formal training off-the-job; and dummy variables for 3 training activities (computer, teamwork, and supervisor training).	Multiple establishment enterprise; age distribution of capital stock; labour quality; tenure structure of workforce; use of TQM or benchmarking; exporter; capacity utilization; union status; R&D; grades or communication skills important in recruitment.	Numbers of workers trained currently or in 1990 has no effect on productivity in 1993. For manufacturing, the greater the proportion of time spent in formal off-the-job training, the greater productivity. For non-manufacturing, content of training is more important, especially computer training. TQM and benchmarking statistically insignificant throughout, but union effect is positive in non-manufacturing.
2. Barrett and O'Connell (2001)	Firm-level data for 1993 and 1997 (n=215)	Cobb-Douglas production function framework. Changes in sales per unit of reported labour, 1993-97, regressed on changes in labour, investment, training investments in 1993.	Employer-provided specific and general training. Three measure: trainees/total employment; training days/total employment; training expenditure/payroll.	Corporate restructuring; personnel and organizational policies; seven industry dummies; and large firm dummy.	Training incidence and training intensity but not training expenditure are positively related to labour productivity. General training but not specific training positively associated with labour productivity. Corporate and personnel innovations are unrelated to productivity.
3. Holzer, Block, Cheatham, and Knott (1993)	1990 survey of firms that applied for a training grant under the MJOB program, 1988-89 (n=395).	Training and output regressions, where the dependent variables are first differences. Output proxied by scrap rate.	Annual hours of training per employee.	Grant receipt; log of sales, employment, and wages; union membership; worker participation; incentive pay; grievance procedures; training purpose.	Training grants increase training in year of receipt. Positive effects of grants and training and changes in training on the output measure. Results highly sensitive to addition of controls.
4. Bartel (1994)	495 Compustat II business lines in 1986, matched to Census of Manufacturers data.	Cobb-Douglas production function framework. Dependent variables: log labour productivity in 1986, and percent change in labour productivity 1983-96.	Training index based on the presence of training programs for each of seven employee groups, plus percentage of employee groups that had a training program implemented after 1983 for first difference estimate.	Age of business; percent unionized; personnel policies (formal job design, formal performance appraisal, employee involvement/ quality circles); two-digit industry dummies.	Formal training is insignificant in levels equation, and is unaffected by inclusion of human resource policies. But new training is pro-productive in the first difference estimate. This is not the case for new human resource policies. Union coefficient estimate is no longer statistically significant. Indication that businesses operating below their expected productivity levels in 1983 implemented these new training programs.
5. Zwick (2005)[a]	IAB Establishment Panel. Cross sections for 1998 and 1999 and longitudinal data	Cobb-Douglas production function framework. Cross section estimates of effect of training in 1997 on labour	Training incidence (share of firms offering training); training intensity (number of trainees divided by	Share of qualified employees; exporter; works council; state-of-the-art technical equipment; investment in	In cross section, lagged training intensity (actual and predicted) are positively associated with labour productivity. The effect of formal external training courses is positive and statistically significant

	for 1997-1999.	productivity levels in 1998 and 1999. Selection into training handled through a probit regression to calculate the probability that a firm offers training and a Tobit equation to predict the intensity of training. Fixed effects panel estimation using Black and Lynch (2001) two-step procedure (see row 6, below), with and without selectivity correction	number of employees; and training type (formal external courses, formal internal courses, training on the job, seminars and talks, job rotation, self-induced learning, and quality circles).	information technology; legal form of firm; employee participation; teamwork; units with own costs and results accounting; stringent hiring rules/incentive payments.	throughout, although that of training on-the-job is negative. Selection term is negative. Panel estimates indicate that training intensity, formal external and internal courses, and self-induced learning have a positive and significant effect on productivity. Allowing for selection increases the training intensity coefficient estimate and those of formal training courses.
6. Black and Lynch (2001)	EQW National Employers Survey data matched to Longitudinal Research Database, 1987-93 (n=638)	Cobb-Douglas production function approach. Dependent variable: sales per production worker. Cross-section estimation for 1993. Panel analysis for 1988-93 involves a two-step procedure, using both the within estimator and a GMM estimator for the production function to generate in a first step the firm-specific time-invariant residual. In a second step this residual is regressed on the training measure and other workplace practices.	Log number of employees trained.	Workplace practices are the main focus of the study. They include TQM, benchmarking, number of managerial tiers, employees per supervisor, proportion of workers in self-manager teams. Training viewed as one such high-performance work system. Other arguments: technology (e.g. capital vintage); worker characteristics; employee voice (unionization and the proportion of workers meeting regularly in groups); profit sharing; and recruitment strategies.	Training measure statistically insignificant in cross section and panel estimation. Unionization insignificant other than in combination with TQM. Mixed effects of high performance work systems, other than benchmarking. Pro-productive voice restricted to workers meeting regularly in groups.
7. Dearden, Reed, and Van Reenen (2000, 2006)	Panel of 94 British Industries, 1985-96. Labour Force Survey (LFS) data aggregated by three-digit industry. Annual Census of Production data for capital, labour, and output. Main	Panel estimates of Cobb-Douglas production function using GMM techniques. Subsidiary wage regression analysis.	Training data from LFS: the proportion of workers in an industry who received training in a given 4-week period. Flow data connected into a training stock in some estimations. Also a distinction drawn between off-the-job and on-the-job training.	Hours worked: labour turnover; gender; proxies for human capital (education, occupation, age, and tenure); R&D intensity; proportion of small firms; and regional composition.	In simple OLS regressions the large positive impact of training on productivity falls dramatically with controls added. Fixed effects estimation yields highly significant (and increased) point estimates of training effect (including lagged training). GMM results for manufacturing with exogenous training are similar to fixed effects results. With allowance for endogeneity, training effect increases markedly and remains higher in dynamic specifications. Results by type of training suggest off-the-job training has bigger

	analysis focuses on production sector.		productivity effect. Stock measure yields more precise point estimates. Training effects on productivity are greater than their effects on wages.

Notes: [a]See also Zwick (2006)

Appendix Table 2: Descriptive Statistics for Matched Employee-Employer Samples

	WERS 1998		WERS 2004	
	Mean	(SD)	Mean	(SD)
Log hourly pay	1.841	(0.534)	1.973	(0.632)
During the last 12 months, how much training have you had, either paid for or organized by your employer?				
Training incidence (1=any; 0=none)	0.509		0.587	
Training duration (days of training)	2.86	(4.32)	2.71	(4.22)
Training (<1 day)	0.094		0.099	
Training (1-2 days)	0.131		0.138	
Training (2-4 days)	0.191		0.182	
Training (5-9 days)	0.090		0.083	
Training (>=10 days)	0.085		0.079	
Union recognition	0.508		0.426	
Multiple unions, single-table bargaining	0.175		0.123	
Multiple unions, joint bargaining	0.098		0.072	
Single union	0.235		0.231	
N	*17092*		*14245*	

Appendix Table 3: Descriptive Statistics for Workplace-Level Samples

	WERS 1998		WERS 2004	
	Mean	SD	Mean	SD
Financial performance ('better than average'=1, 0 otherwise)	0.59	0.49	0.53	0.50
Labour productivity ('better than average'=1, 0 otherwise)	0.51	0.50	0.49	0.50
What proportion of experienced employees have had formal off-the-job training over the past 12 months?				
Training incidence (>0%=1; 0%=0)	0.82	0.38	0.86	0.34
Training intensity (100%)	0.09		0.27	
Training intensity (80-99%)	0.23		0.11	
Training intensity (60-79%)	0.30		0.09	
Training intensity (40-59%)	0.11		0.09	
Training intensity (20-39%)	0.08		0.14	
Training intensity (1-19%)	0.05		0.17	
Training duration (proportion trained * number of days)	41.85	36.71	30.70	31.20
Union recognition	0.42	0.49	0.15	0.36
Multiple unions, single table bargaining	0.11	0.31	0.03	0.16
Multiple unions, joint bargaining	0.09	0.29	0.01	0.10
Single union	0.23	0.42	0.11	0.32
Experience required to do job >1 month	0.71	0.46	0.63	0.48
Job security guarantees	0.27	0.44	0.02	0.14
Targets set for training	0.40	0.49	0.27	0.45
N	*1100*		*1449*	