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The Impact of Training on Productivity and Wages: Evidence from British Panel Data*

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Abstract

It is standard in the literature on training to use wages as a sufficient statistic for productivity. This paper examines the effects of work-related training on direct measures of productivity. Using a new panel of British industries 1983-1996 and a variety of estimation techniques we find that work-related training is associated with significantly higher productivity. A one percentage point increase in training is associated with an increase in value added per hour of about 0.6% and an increase in hourly wages of about 0.3%. We also show evidence using individual level datasets that is suggestive of training externalities.

JEL classification numbers
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Keywords
Productivity, training, wages, panel data

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I. Introduction

It is a widely-held view that Britain needs to increase work-related training to improve long-term economic performance and address the 'skills gap'.[1] Despite the policy interest and the huge economics literature on human capital, there are hardly any papers that examine the impact of work-related training on direct measures of productivity. The primary contribution of our paper is to provide such evidence for the first time in the UK and for the first time anywhere over a long period (we have 14 years of data). Analysis of the impact of training on productivity has focused almost entirely on estimating the impact of training on wages. Most studies looking at the private return to work-related training find that training results in workers receiving higher real wages.[2]

Although these studies are informative, they only tell half the story as they ignore the impact on the employer's productivity. The relationship between wage increases and productivity gains can vary according to the structure of the labour and product markets and according to who actually pays the costs of training. In the simplest neoclassical view of the labour market where the market is perfectly competitive, wages will be equal to the value of marginal product. Thus the wage can be taken as a direct measure of productivity. This simple relationship can break down for many reasons. For example, in Becker's model of specific human capital, the employer will pay for training, so there should be no effect of completed training spells on observed wages even though there may be a large impact on productivity.[3]

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[1] See Green and Steedman (1997) or National Skills Task Force (1998). In the December 2003 Pre-Budget Report, the British Chancellor justified the extension of the Employer Training Pilots in order to help improve the skills gap and UK productivity. (http://www.hm-treasury.gov.uk/media//2E3BD/03_Meeting%20the%20Pro_EF.pdf)

[2] See Greenhalgh and Stewart (1987), Booth (1991, 1993) or Blundell *et al*. (1996) for UK evidence. US studies using panel data include Lillard and Tan (1992), Lynch (1992), Blanchflower and Lynch (1992) and Bartel and Sicherman (1999). Winkelmann (1994) uses German data and Bartel (1995) looks within a large US manufacturing company.

[3] There are many other reasons for a wedge between productivity and wage in a competitive labour market. First, employees may receive non-pecuniary benefits from training. Second, workers may implicitly pay the costs of a training scheme in the form of lower wages whilst being trained, which then rise after training is completed – so we might see a greater increase in *observed* wages than in productivity. Third, employees' wages could be lower during training because they are not contributing to firm productivity whilst actually being trained. Fourth, there may be deferred compensation packages where the employee's remuneration is 'backloaded' towards later post-training years as a means of ensuring loyalty and/or effort early in the employee's tenure (e.g. Lazear, 1979).

If the labour market is characterised by imperfect competition then the strict link between wages and productivity is usually broken. Employees can find themselves being paid less (or more) than their marginal revenue product. Nevertheless, it is still the case that conditional on a given degree of rent-sharing or monopsony power; increases in wages have to be paid out of productivity gains. Therefore we can assert the general principle that these real wage increases should provide a lower bound on the likely size of productivity increases. In practice, the productivity gains are likely to be higher than this. For instance, in a labour market with frictions and some wage compression (e.g. from a binding minimum wage), there will be productivity gains even from general training that are not passed on to the employee in terms of wages but are only reflected in direct measures of productivity.[4] Similar results can be found in some bargaining models (e.g. Booth *et al*., 1999).

There exist a small number of empirical papers that relate firm productivity to a measure of training.[5] Although a positive correlation is generally found, it is very difficult to interpret because the training measures are only measured at a single point of time and could be picking up many unobservable firm-specific factors correlated with both training and productivity. Black and Lynch (2001) used an establishment training survey at two points of time. In the cross section, they identified some effects of the type of training on productivity, but they found no significant association when they controlled for plant-specific effects. Ichniowski *et al*. (1997) investigated what factors influence productivity in a panel of US steel finishing mills. After controlling for fixed effects, they found a role for training only in combination with a large variety of complementary human resource practices. Carriou and Jeger (1997), Ballot *et al*. (1998) and Delame and Kramarz (1997) used French firm-level panel data to look at the effects of training on value added and found positive and significant effects. Although these studies are broadly

[4] See Acemoglu and Pischke (1999, 2003).

[5] Black and Lynch (1996), Bartel (1994), Barrett and O'Connell (2001), de Koning (1994), Boon and van der Eijken (1997) and Ballot and Taymaz (1998) have objective productivity measures. Bartel (1995), Holzer (1990), Barron *et al*. (1989) and Krueger and Rouse (1998) use subjective measures of productivity. Holzer *et al*. (1993) do find effects of changes in productivity on changes in one measure of quality – the scrap rate.

consistent with our own, they do not fully exploit the potential of their panel data by allowing training to be a choice variable.[6]

Our contribution in this paper is to advance the literature in at least three ways. Black and Lynch (2001) emphasise the problems of trying to identify the effects of training in a short panel (they have only two separate training observations). Although unobserved heterogeneity can be controlled for through fixed effects with only two periods, attenuation biases due to measurement error are exacerbated. To address this, we build a panel that contains up to 14 consecutive years of training data. Second, we explicitly allow training to be a choice variable by using General Method of Moments (GMM) estimators developed to deal with endogenous variables in production functions. Third, we combine estimation of the productivity effects of training with estimation of the wage effects of training. Although comparisons between the production function and the wage equation are becoming more common for other worker characteristics such as gender and human capital, this is the first time the strategy has been used for training.[7] In principle, this allows us to examine whether trained workers are paid the value of their marginal product.

We conduct our main analysis of the effects of training at the industry level (although we have also performed estimation at the firm and individual level for comparative purposes). There is simply no alternative to this strategy if one wishes to use long time series of training and productivity information. The only publicly available firm-level panel data in the UK is a sample of about 119 firms in the late 1990s with only very basic training information (a concise investigation of this is presented in Appendix B). Aggregation has pros and cons that are discussed in the paper. On the positive side, if there are important spillovers to training within an industry (e.g. through a faster rate of innovation) then a firm-level analysis will potentially miss out these linkages and underestimate the return to human capital.[8] On the negative side, there

[6] See Greenhalgh (2002) for a much more extensive review of the French and UK literature in this area.

[7] Hellerstein *et al*. (1999), Hellerstein and Neumark (1999), Hægeland and Klette (1999) and Jones (2001) examine the differential impact of human capital and gender on wages and productivity. A recent study that utilises our methodology and looks at this question using a panel of French and Swedish firms is Ballot *et al*. (2002). They find that both French and Swedish firms appropriate a high proportion of the returns to training (82% and 67% respectively).

[8] For example, O'Mahony (1998) finds that the coefficient on labour skills in a production function is more than twice that assumed by traditional growth accounting from relative wages. Other recent papers that have looked at the impact of human capital on directly measured productivity include Moretti (2004) on US data and Haskel *et al*. (2003) on UK data.

may be aggregation biases at the sectoral level that could lead to negative or positive biases on the training coefficient. We follow Grunfeld and Griliches (1960) in arguing that the pros of aggregation probably outweigh the cons.

The format of the paper is as follows. Section II describes the simple economic models of productivity and wages that we will estimate and Section III details the econometric strategy. The data are described in Section IV and the results are presented in Section V. Section VI offers some concluding comments. Appendix A contains more information on the data and some additional experiments. Our main result is that we find a statistically and economically significant effect of training on industrial productivity. A 1 percentage point increase in training is associated with about a 0.6% increase in productivity and a 0.3% increase in hourly wages. The productivity effect of training is twice as large as the wage effect, implying that existing studies have underestimated the benefits of training by focusing on wages.

II. A model of training and productivity

To see our approach, assume that we can characterise a representative plant in an industry by a Cobb–Douglas production function written in value added form[9]

$$Q = AL^{\alpha} K^{\beta} \tag{1}$$

where Q is value added, L is effective labour input allowing for quality and quantity dimensions, K is capital and A is a Hicks neutral efficiency parameter.

We consider that trained workers are more productive than untrained workers, so that effective labour input can be written as

$$L = N^{U} + \gamma N^{T} \tag{2}$$

where N^U is the number of untrained workers, N^T is the number of trained workers and γ is a parameter which, if trained workers are more productive than non-trained workers, will be greater than unity. The total number of workers, *N*, is equal to the sum of trained and untrained workers. Substituting equation (2) into equation (1) gives

$$Q = A[1 + (\gamma - 1)TRAIN]^{\alpha} N^{\alpha} K^{\beta} \tag{3}$$

[9] This should be viewed as a first-order approximation to a more complicated functional form. It is straightforward to generalise this to more complex functional forms such as translog and some experiments are included in the empirical results.

where $TRAIN = N^T/N$ is the proportion of trained workers in an industry. Taking natural logarithms, we obtain

$$\ln Q = \ln A + \alpha \ln[1 + (\gamma - 1)TRAIN] + \alpha \ln N + \beta \ln K \tag{4}$$

This could be estimated by non-linear least squares. If $(\gamma-1)$*TRAIN* is 'small', we can use the approximation $\ln(1+x) = x$ and rewrite the production function as[10]

$$\ln Q = \ln A + \alpha(\gamma - 1)TRAIN + \alpha \ln N + \beta \ln K \tag{5}$$

If the industry exhibits constant returns to scale (i.e. $\alpha+\beta=1$) then equation (5) can be rewritten in terms of labour productivity as

$$\ln(Q/N) = \ln A + (1-\beta)(\gamma - 1)TRAIN + \beta \ln(K/N) \tag{6}$$

If the trained are no more productive than the untrained (γ =1) then the coefficient on *TRAIN* will be zero.

This method can be easily extended to a larger number of different types of heterogeneous workers in the labour quality index. If we index the discrete type of labour by *k* (where until now we have discussed *k* solely in terms of the training status of workers) then equation (4) can be written

$$\ln Q = \ln A + \alpha \ln\{1 + \sum_k [(\gamma_k - 1)(N_k / N)]\} + \alpha \ln N + \beta \ln K \tag{7}$$

Empirically, we will allow for many other dimensions of labour quality such as education, occupation, age, tenure and gender.[11] There are a large number of other influences on productivity captured in *A*, so we allow for differential hours, worker turnover rates, innovation (as proxied by research and development expenditures), regional composition and the proportion of small firms. Labelling these factors as *X*, imposing constant returns and using the log approximation, the basic production function becomes

$$\ln(Q/N) = (1-\beta)\sum_k [(\gamma_k - 1)(N_k / N)] + \beta \ln(K/N) + \delta' X \tag{8}$$

[10] The results were estimated both by non-linear least squares and by least squares using the approximation. The results did not significantly differ (see Table 3), so the more convenient log linear approximation is used for the baseline results.

[11] We follow Hellerstein *et al*. (1999) by entering these variables in linear proportions. One could allow a larger number of cells for interactions of the labour quality variables (e.g. the proportion of educated men – a two-way interaction – or the proportion of educated men who are trained – a three-way interaction). We experimented with some breakdowns like this on the training variable, but Labour Force Survey cell sizes by industry were generally not large enough.

The wage equation that we estimate parallels the productivity equation in (8). We view the wage equation as more of a descriptive regression than the structurally derived production function. Under competitive spot markets for labour, relative wages should equal the relative marginal productivities of workers of different types. This is because if the relative productivity of trained workers, γ, exceeded the relative wages of trained workers then employers would only employ trained workers (Hellerstein *et al.*, 1999).

Consider the wage bill, W, for the representative plant in an industry. Again, take the simplest model where there are only two types of workers: trained workers paid average wage w^T and untrained workers paid average wage w^{NT}. Relative wages are $\lambda = w^T / w^{NT}$. By definition,

$$W = w^{NT}(N - N^T) + \lambda\, w^{NT} N^T \quad = w^{NT}[N + (\lambda - 1)\, N^T] \tag{9}$$

In logarithms, the average wage (w) is

$$\ln w = \ln(W / N) = a + \ln[1 + (\lambda - 1) TRAIN] \tag{10}$$

where $a = \ln(w^{NT})$.

Clearly, estimation of equation (10) can be used to recover the relative wage mark-up associated with training, λ, and then compared to the relative productivity effect of training, γ. Parallel to the productivity equation, we will allow for multiple types of labour quality, capital and other factors to influence wages. The empirical wage equation to be estimated is therefore[12]:

$$\ln w = a + \sum_k \left[(\lambda_k - 1)(N_k / N)\right] + \beta^w \ln K + \delta^{w\prime} X \tag{11}$$

III. Econometric modelling strategy

The basic equation we wish to estimate can be written in simplified form as

$$y_{it} = \theta x_{it} + u_{it} \tag{12}$$

where y is Q/N and x is a vector of (suspected endogenous) variables including training. Subscript i indicates the representative firm in an industry, t

[12] One could argue that firm variables such as capital intensity and R&D should be excluded from the wage equation under competitive labour markets. However, these variables are typically quite informative in wage equations, either because they are picking up some measure of unobserved labour quality (Hellerstein and Neumark, 1999) or because of departures from perfect competition. In either case, omitting such variables is likely to cause bias on the training variable and our baseline specifications will include them.

is time and θ is the parameter of interest. Assume that the stochastic error term, u_{it}, takes the form

$$\begin{aligned} u_{it} &= \eta_i + \tau_t + \omega_{it} \\ \omega_{it} &= \rho\omega_{it-1} + \upsilon_{it} \end{aligned} \tag{13}$$

The τ_t represent macroeconomic shocks captured by a series of time dummies, η_i is an individual effect and υ_{it} is a serially uncorrelated mean zero error term. The other element of the error term, ω_{it}, is allowed to have an AR(1) component (with coefficient ρ), which could be due to measurement error or slowly evolving technological change. Substituting (13) into (12) gives us the dynamic equation

$$y_{it} = \pi_1 y_{it-1} + \pi_2 x_{it} + \pi_3 x_{it-1} + \eta^*{}_i + \tau^*{}_t + \upsilon_{it} \tag{14}$$

The common factor restriction (COMFAC) is $\pi_1\pi_2 = -\pi_3$. Note that $\tau^*{}_t = \tau_t - \rho\tau_{t-1}$ and $\eta^*{}_i = (1-\rho)\eta_i$.

In our main results section, we present several econometric estimates of production functions (random effects, within groups and GMM). The most rigorous approach follows that recommended by Blundell and Bond (2000), which uses a ‘system GMM’ approach to estimate equation (14) and then imposes the COMFAC restrictions by minimum distance. We now turn to describing the GMM approach in more detail.

How should equation (14) be estimated? If training is strictly exogenous and there are no dynamics (i.e. $\rho = 0$) then the only problem with OLS estimation of (12) is the presence of the individual effects, η_i. If these individual effects are uncorrelated with x_{it} then the random-effects estimator is unbiased and efficient. If the individual effects are correlated with x_{it} but remain strictly exogenous then although the random-effects estimator is biased, the within-groups estimator will be unbiased.

If we allow training to be endogenous (i.e. allowing training decisions to react to shocks to current productivity), we will require instrumental variables. In the absence of any obvious natural experiments, we consider moment conditions that will enable us to construct a GMM estimator for equation (14). A common method would be to take first differences of (14) to sweep out the fixed effects:

$$\Delta y_{it} = \pi_1 \Delta y_{it-1} + \pi_2 \Delta x_{it} + \pi_3 \Delta x_{it-1} + \Delta \tau_t^* + \Delta \upsilon_{it} \tag{15}$$

Since υ_{it} is serially uncorrelated, the moment condition

$$E(x_{it-2} \Delta \upsilon_{it}) = 0 \tag{16}$$

ensures that instruments dated *t*–2 and earlier[13] are valid and can be used to construct a GMM estimator for equation (14) in first differences (Arellano and Bond, 1991). A problem with this estimator is that variables with a high degree of persistence over time (such as capital) will have very low correlation between their first difference (Δx_{it}) and the lagged levels being used as instruments (e.g. x_{it-2}). This problem of weak instruments can lead to substantial bias in finite samples.

Blundell and Bond (1998) point out that under a restriction on the initial conditions, another set of moment conditions are available:[14]

$$E(\Delta x_{it-1}(\eta_i + \upsilon_{it})) = 0 \tag{17}$$

This implies that lags of the first differences of the endogenous variables can be used to instrument the levels equation (14) directly. The econometric strategy is then to combine the instruments implied by the moment conditions (16) and (17). We stack the equations in differences and levels, i.e. (14) and (15). We can obtain consistent estimates of the coefficients and use these to recover the underlying structural parameters in (12).

The estimation strategy assumes the absence of serial correlation in the levels error terms (υ_{it}).[15] We report serial correlation tests in addition to the Sargan-Hansen test of the over-identifying restrictions in all the GMM results below.[16]

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[13] Additional instruments dated *t*–3, *t*–4, etc. become available as the panel progresses through time.

[14] The restrictions are that the initial change in productivity is uncorrelated with the fixed effect $E(\Delta y_{i2} \eta_i) = 0$ and that initial changes in the endogenous variables are also uncorrelated with the fixed effect $E(\Delta x_{i2} \eta_i) = 0$.

[15] If the process is MA(1) instead of MA(0) then the moment conditions in (16) and (17) no longer hold. Nevertheless $E(x_{it-3} \Delta \upsilon_{it}) = 0$ and $E(\Delta x_{it-2}(\eta_i + \upsilon_{it})) = 0$ remain valid, so earlier-dated lags could still be used as instruments. This is the situation empirically with the wage equations.

[16] These are based on the first-differenced residuals, so we expect significant first-order serial correlation but require zero second-order serial correlation for the instruments to be valid. If there is significant second-order correlation, we need to drop the instruments back a further time period (this happens to be the case for the wage equation in the results below).

This GMM 'system' estimator has been found to perform well in Monte Carlo simulations (Blundell and Bond, 1998) and in the context of the estimation of production functions (Blundell and Bond, 2000). The procedure should also be a way of controlling for transitory measurement error (the fixed effects control for permanent measurement error). Random measurement error has been found to be a problem in the returns to human capital literature, typically generating attenuation bias (see Card, 1999).

In order to assess the importance of biases associated with fixed effects and endogeneity, we will estimate random-effects, within-groups and GMM estimates in the results section.

Finally, consider two more issues which are harder to deal with: aggregation and training stocks vs. training flows. Estimation at the three-digit industry level has advantages but also disadvantages relative to micro-level estimation. The production function in equation (1) at the firm level describes the private impact of training on productivity. However, many authors, especially in the endogenous growth literature (e.g. Aghion and Howitt, 1998), have argued that there will be externalities to human capital acquisition. For example, workers with higher human capital are more likely to generate new ideas, which may spill over to other firms.[17] If spillovers are industry specific, this implies that there should be additional terms added to equation (5) representing training in other firms (e.g. the mean number of trained workers in the industry). In this case, the coefficient on training in an industry-level production function should exceed that in a firm-level production function.[18] Second, grouping by industry may smooth over some of the measurement error in the micro data and therefore reduce attenuation bias.

On the negative side, there may be aggregation biases in industry-level data. A priori it is not possible to unambiguously sign these biases. We expect that the fixed effects will control for some of the problem. For example, we are taking logs of means and not the means of logs in aggregating equation (4), but so long as the higher-order moments of the distributions are constant over time in an industry then they will be captured by a fixed effect.[19] If the

[17] Although there are many papers that examine externalities of R&D (e.g. see the survey by Griliches, 1992) and a few that look at human capital (Acemoglu and Angrist, 2000; Moretti, 2004), there are none that focus on training spillovers.
[18] For the same argument in the R&D context, see Griliches (1992).
[19] If they evolve at the same rate across industries, they will be picked up by the time dummies.

coefficients are not constant across firms in equation (4), but are actually random, this will also generate higher-order terms at the industry level. In the empirical results, we experiment with including higher-order moments and allowing the coefficients to vary across cross-sectional units.

Turning to the problem of training stocks and flows, note that the model in equation (1) assumes that we know the stocks of trained workers in an industry. What we actually have in the data is an estimate of the proportion of workers in an industry who received training in a given four-week period (the training *flow*). Since individuals are sampled randomly over time in the Labour Force Survey (LFS), this should be an unbiased estimate of the proportion of people in training in a given industry in a given year.[20] As an alternative to using the flow, we calculate a stock of training in an analogous way to using investment flows to calculate a capital stock through the perpetual inventory method (the main form of depreciation is the turnover rate). This is described in Appendix A.

IV. Data description

The database we construct combines several sources (see Appendix A for full details). The critical individual-level source is the individual-level UK Labour Force Survey, which contained about 60,000 households per year. Most importantly, the LFS has a consistent training measure since 1984 as well as detailed information on skills, demographics, hours worked, tenure and wages. We work with this information aggregated by broadly three-digit industries. The LFS only started asking questions on wages at two points of time in 1997 (and at one point of time in 1992 when the panel was set up). We present some individual-level panel wage regressions at the end of Section V for comparison.

The second major dataset we use is the Annual Census of Production (ACOP). This gives production statistics on capital, wages, labour and output, for industries in the production sector (manufacturing, mining and utilities). For the services industries, we drew on the OECD's ISDB data.

[20] If there are many multiple training spells in the month, we will underestimate the proportion of employees who are being trained. If Spring (the LFS quarter we use) is a particularly heavy training season then we will overestimate the proportion being trained in a year. These biases are likely to be small and offsetting.

There was a change in SIC classification in 1992 which forced us to aggregate some of the industries and prevented us from using some of the industries after the change. Additionally, we insisted on having at least 25 individuals in each cell in each year. After matching the aggregated individual data from LFS, we were left with 94 industry groupings over (a maximum of) 14 years.

The main LFS training question was '*over the 4 weeks ending Sunday … have you taken part in any education or training connected with your job, or a job that you might be able to do in the future?*'.[21] The average proportions of employees undertaking training grew steadily from about 8% in 1984 to 14% in 1990 where it stabilised for the next six years. Most of this growth was upgrading within industry rather than between industries.[22]

Figure 1 gives the scatterplot of labour productivity (log real value added per worker) against training propensity and Figure 2 repeats the exercise for log hourly wages. Not surprisingly, training has a strong positive correlation with both variables, but the association is somewhat weaker for wages than for productivity.

The outliers in both graphs tend to be in the service sector. Unfortunately, the published series for real value added and capital stocks are rather unreliable in the service sector. For example, in banking and financial services, measured real value added per person declined every year between 1983 and 1996. Given the poor quality of the service sector production data, we reluctantly decided to focus the econometric part of the analysis on the production side of the economy. This is still a substantial share of the economy – about 50% of private sector net output in 1986.[23] Until robust measures of service sector productivity are developed, there is simply no alternative to the empirical strategy of focusing on the production sector.

[21] Unfortunately, it is not possible to separate out 'education' from 'training'.
[22] There is also a question on the length of the training spell, but this was only asked in particular years and there were too many missing values to use it as a separate regressor. Median spell length was two weeks and the mean higher.
[23] This led to the loss of only 91 observations and the results are robust to including the service sector in the unweighted regressions. We generally weight the regressions by the number of LFS observations in order to reduce sampling variability. In the weighted regressions, including the service sector does have more substantial effects on the results because of its large employment shares. Full sets of these results are available on request from the authors.

Figure 1. Labour productivity and training in British industries

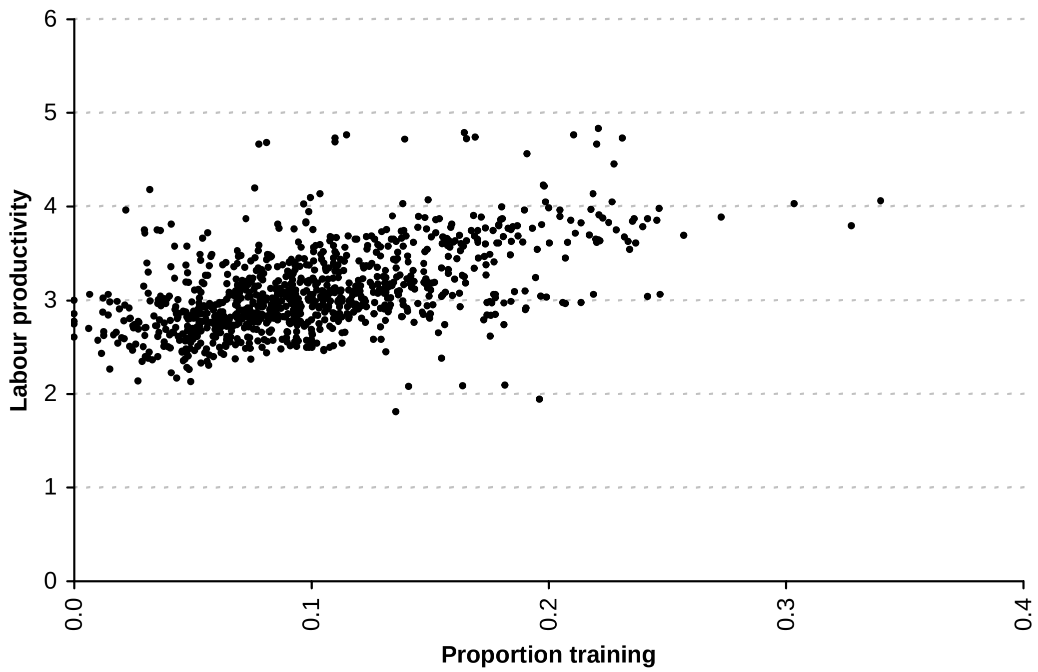
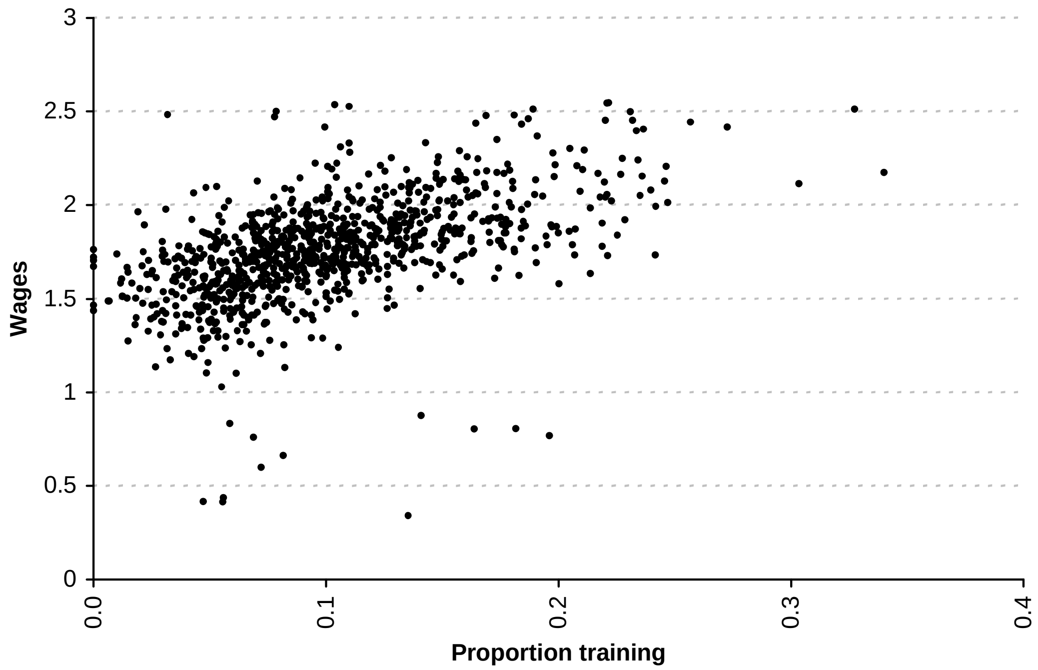


Figure 2. Wages and training in British industries



Notes to Figures 1 and 2: Each point is an industry-year observation. The OLS regression line has a slope of 4.91 for productivity and 2.95 for wages. Labour productivity is ln(value added per employee) aggregated from the ACOP and ISDB, wages are ln(hourly wages) from the ABI and ISDB (wages) and the LFS (hours); training is the proportion of workers involved in training in the last 4 weeks from the LFS.

Care must be taken in interpreting the scatterplots presented in Figures 1 and 2 as they say nothing about the causal impact of training on productivity or wages. High-training industries are characterised by higher fixed capital intensity, more professional workers, more educated workers and higher R&D (see Table A1 in Appendix A). We need to turn to an explicit econometric model to investigate whether there is a causal effect of training on productivity, and this forms the focus of the rest of the paper.

V. Results

Baseline industry results

In Table 1, we present the basic results for the industry-level regressions treating all variables as exogenous. The first three columns have productivity (log real value added per head) as the dependent variable and the last three columns have wages as the dependent variable.

TABLE 1

Training, productivity and wages

	(1)	(2)	(3)	(4)	(5)	(6)
	ln(value added per worker)				ln(wages)	
	Random effects	*Random effects*	*Within groups*	*Random effects*	*Random effects*	*Within groups*
Training	**.788**	**.700**	**.696**	**.425**	**.344**	**.365**
	(.168)	**(.169)**	**(.201)**	**(.117)**	**(.119)**	**(.157)**
ln(capital/worker)	**.252**	**.244**	**.212**	**.058**	**.051**	**.069**
	(.020)	**(.019)**	**(.053)**	**(.012)**	**(.012)**	**(.035)**
ln(hours/worker)	.184	.196	**.275**	**.274**	**.272**	**.310**
	(.181)	(.181)	**(.207)**	**(.123)**	**(.126)**	**(.116)**
Lagged R&D	**1.628**	**1.390**	**1.251**	–.212	–.356	–.942
intensity	**(.430)**	**(.428)**	**(.662)**	(.284)	(.281)	(.717)
Worker turnover	**–.632**	**–.683**	–.430	.132	.070	.163
	(.206)	**(.207)**	(.332)	(.143)	(.145)	(.202)
Occupations: base group is manual workers						
Managers		**.487**	**.282**		**.324**	**.195**
		(.123)	**(.131)**		**(.084)**	**(.099)**
Clerical		**.366**	–.076		.161	–.126
		(.174)	(.190)		(.121)	(.121)
Personal/security		–.049	–.522		**.504**	.204
		(.355)	(.371)		**(.250)**	(.223)
Sales people		.443	–.078		–.037	–.328
		(.276)	(.281)		(.191)	(.190)
No qualifications	**–.251**	–.036	.107	**–.145**	–.033	.101
	(.096)	(.109)	(.096)	**(.065)**	(.075)	(.069)
Experience: base group is age 35–44						
Age 16–24	**–.579**	**–.461**	**–.390**	**–.315**	**–.259**	–.153
	(.170)	**(.172)**	**(.175)**	**(.118)**	**(.121)**	(.119)
Age 25–34	**–.341**	–.282	–.314	–.198	–.155	–.196
	(.155)	(.158)	(.171)	(.109)	(.110)	(.111)
Age 45–54	–.058	–.042	–.104	–.139	–.148	–.150
	(.158)	(.156)	(.160)	(.110)	(.111)	(.101)
Age 55–64	.178	.244	.142	**–.263**	–.263	**–.271**
	(.190)	(.192)	(.237)	**(.133)**	(.136)	**(.138)**
Male	.037	.114	–.116	**.293**	**.364**	–.112
	(.097)	(.099)	(.128)	**(.064)**	**(.065)**	(.078)
Small firm	.068	.016	.005	–.118	–.126	–.056
	(.112)	(.113)	(.127)	(.076)	(.076)	(.074)
Observations	968	968	968	968	968	968
Estimation period	1984–96	1984–96	1984–96	1984–96	1984–96	1984–96

Notes: Standard errors (robust to heteroskedasticity) are given in parentheses under coefficients. In the first three columns the dependent variable is ln(value added per worker) and in the last three columns the dependent variable is ln(wages). Bold typeface indicates that the variable is significant at the 5% level. All regressions include a full set of regional dummies (10), time dummies (12) and tenure dummies (6). Observations are weighted by number of individuals in an LFS industry cell. Random effects are estimated by GLS. Within groups are estimated by least squares dummy variables (85 industries).

The first two columns are estimated by random effects; the only difference is that column (1) does not include the occupational controls. This omission makes some difference to the ‘no qualifications’ variable, which has a significantly negative association with productivity in column (1) but is insignificantly different from zero in column (2) – the occupational proportions (especially the professional/managerial category) do a better job at proxying for workforce skill than education.[24] The variables generally take their expected signs, although it is clear that there is some loss of precision when a full set of fixed effects is added in column (3). Capital per worker is strongly correlated with productivity, although the coefficient is lower (0.21 to 0.25) than capital's share of value added, which is about 30%. Worker turnover has a significantly negative association with productivity and R&D a significantly positive correlation. Younger workers (aged between 16 and 24) are significantly less productive than the 35- to 44-year-old group. Most importantly for our purposes, training has a statistically significant and economically important effect on productivity according to Table 1. The magnitude of the coefficient falls as we move to the more rigorous specification which controls for fixed effects, but the change is not dramatic. The estimates imply that raising the training variable by 1 percentage point (say, from the 1996 economy-wide mean of about 14% to 15%) is associated with an increase in productivity of about 0.7%. We will return to the plausibility of the magnitude of these effects in the last subsection of Section V.

The last three columns repeat the specifications but instead use ln(wages) as the dependent variable. The most interesting contrast for our purpose is the coefficient on training. As with productivity, training enters the earnings equation with a consistently positive and significant coefficient across all three columns. The magnitude of the coefficient is lower in the wage equation than in the productivity equation – about half the size. At face value, then, estimating the returns to training solely on the basis of wage equations would generate an underestimate of the importance of work-related training.[25]

Turning to the other variables in the wage equation, the signs of most of them are the same as those in the productivity equations, although there are some differences. As expected, earnings are significantly higher in more capital-intensive, hours-intensive and highly-skilled industries. The R&D

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[24] This conclusion does not change if we break down the qualifications into four groups. Machin *et al*. (2003) adopt a much finer classification of education using post 1992 LFS data where there are a larger number of observations. Exploiting the regional and industry aspects of the aggregated data, they find some role for college proportion, even in fixed-effect specifications.

[25] A test of the equality of the coefficients on training in the wage and productivity equations rejected equality at the 0.10 level for training (p-value = 0.068). We would, however, expect the coefficient on training in the wage equation (λ-1) to be lower than in the production function as the training coefficient in equation (7) is $\alpha(\gamma-1)$. Although a test of the equality between λ and γ cannot be rejected at the .05 level for the sample as a whole it *can* be rejected for the "low wage" industries – see footnote 33 below.

coefficient is surprisingly negative (although insignificantly different from zero), but this turns out to be because of mis-specified dynamics – including longer lags of R&D demonstrates there is actually a positive correlation of technology with wages[26].

An important concern with Table 1 is that we do not allow for the endogeneity of training or other suspected endogenous variables. To deal with this, we implemented the GMM approach described in Section III above. Table 2 contains a summary of the main results.[27] All the same variables are included in these regressions as in Table 1, but we report only the key coefficients to preserve space.

In column (1) we present the production function and in column (2) we present the wage equation. The GMM estimates tell a similar story to the within-groups estimates. Training has a positive and significant impact on both productivity and wages, although the training coefficient in the production function remains almost twice the size of the coefficient in the wage equation (0.60 vs. 0.35). There are some minor changes to the other coefficients – the coefficient on capital intensity has risen to 0.33 in the production function, the R&D coefficient is positively signed in the wage equation and the coefficient on hours is somewhat larger in magnitude than in Table 1.

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[26] Consistent with the findings of, *inter alia*, Bartel and Sicherman (1999).
[27] Table B2 in Appendix B has more detailed results, and even more detailed specifications are available from the authors or in Dearden *et al*. (2000).

TABLE 2

Production functions and wage equations estimated by GMM

	(1) *ln(real value added per worker)*	(2) *ln(wages)*
Training	**.602**	**.351**
	(.181)	**(.074)**
ln(capital/worker)	**.327**	**.106**
	(.016)	**(.011)**
ln(hours/worker)	**.498**	**.489**
	(.064)	**(.027)**
Lagged R&D intensity	**1.905**	**.443**
	(.262)	**(.182)**
Proportion of employees who are professionals or managers	**.306**	**.160**
	(.068)	**(.034)**
Autocorrelation coefficient	**.741**	**.797**
(ρ)	**(.015)**	**(.013)**
LM1(d.f.)	–4.892(85)	–6.053(85)
[p-value]	[0.00]	[0.00]
LM2(d.f.)	–.940(85)	–1.44(85)
[p-value]	[.347]	[.158]
Sargan(d.f.)	8.819(121)	11.83(146)
Instruments	$(TRAIN)_{t-2,t-3}$, $\ln(Q/N)_{t-2,t-3}$, $\ln(Hrs/N)_{t-2,t-3}$, $\ln(K/N)_{t-2,t-3}$ in differenced equations; $\Delta(TRAIN)_{t-1}$, $\Delta\ln(Hrs/N)_{t-1}$, $\Delta\ln(K/N)_{t-1}$ in levels equations	$(TRAIN)_{t-3,..,t-5}$, $\ln(Q/N)_{t-3,..,t-5}$, $\ln(Hrs/N)_{t-3,..,t-5}$, $\ln(K/N)_{t-3,..,t-5}$ in differenced equations; $\Delta(TRAIN)_{t-2}$, $\Delta\ln(Hrs/N)_{t-2}$, $\Delta\ln(K/N)_{t-2}$ in levels equations
Estimation period	1984–96	1985–96
Observations	898	883

Notes: Estimation by GMM-SYS in Arellano and Bond (1998) DPD-98 package written in GAUSS; one-step robust estimates reported. All regressions include the current values of all the variables in columns (3) and (6) of Table 1 (i.e. turnover, other occupations, qualifications, age, tenure, gender, region, firm size and time dummies). Capital intensity, training, hours and lagged productivity are always treated as endogenous. The other variables are assumed weakly exogenous. One-step standard errors (robust to arbitrary heteroskedasticity and autocorrelation of unknown form) are given in parentheses under coefficients (variables significant at 5% level are in bold). LM1 (LM2) is a Lagrange Multiplier test of first- (second) -order serial correlation distributed N[1,0] under the null (see Arellano and Bond, 1991). Sargan is a Chi-squared test of the over-identifying restrictions. Observations are weighted by number of individuals in an LFS industry cell. Full details in Table B2 in Appendix B.

The diagnostics reported at the base of the table are also satisfactory – there is no sign of second-order serial correlation (in the first-differences residuals) and the Sargan test of over-identifying restrictions does not reject. Note that the wage regression uses instruments dated *t*–3 and before in the differenced equation (and dated *t*–2 in the levels equation). This is because there were some signs of significant second-order serial correlation using *t*–2 dated instruments in the wage equation, which invalidates the IVs (we dropped one period in order to be able to use the longer lags in estimation).

Using the (invalid) instruments on the longer time period gave a coefficient (standard error) on training of 0.141 (0.067) in the wage equation.[28]

Robustness of the results

We conducted a large number of robustness tests on the models in Tables 1 and 2. Table 3 reports some of these. Given the similarity of the within-groups and GMM results, we performed these tests on the within-groups specifications of Table 1, column (3). The first row of Table 3 simply reports the coefficient and standard error from that column. Using the stock of trained employees instead of the flow (calculated allowing for depreciation due to inter-firm turnover) results in very similar results in row 2.[29] Keeping only industries that had 14 continuous years of data (balanced panel) in row 3 means losing 40% of the observations; the coefficient falls, but the change is not significant (cf. Nickell, 1981). The fourth row includes average wages on the right-hand side of the production function as a measure of unobserved worker quality; although wages have a positive coefficient, the training association remains robust. In row 5 we include all the service sectors, ignoring our concerns over data quality. The coefficient on training rises to 0.73 and remains significant.

[28] Using the shorter time period with longer-dated instruments in the production function gave a coefficient (standard error) on training of 1.043 (0.325). See Table B2 in Appendix B for full details.

[29] The non-fixed-effects results were significantly different, but the deviations around the fixed effect in the training stock are dominated by the flow, explaining the similarity of the results. In addition to using the empirical turnover rates, we assumed an exogenous depreciation of training at 40% per annum (see Appendix A). The coefficient was stable to reasonable changes in these parameters (e.g. increasing the depreciation rate to 50% p.a. increased the coefficient to 0.81, to 60% to 0.82; decreasing the depreciation rate to 30% reduced the coefficient to 0.70).

TABLE 3

Robustness tests of the production function

Row	*Robustness test*	*Observations*	*Training coefficient (standard error)*
1	Original training coefficient in production sector, Table 1 column (3)	968	**0.696(0.201**)
2	Using 'stock' of trained workers instead of flows	968	**0.775(0.189)**
3	Using the balanced panel only	572	0.508(0.289)
4	Conditioning on wage in productivity regression (to control for any residual unobserved worker quality)	968	Training: **0.659(0.219)** Wage coeff.: **0.099(0.130)**
5	Including service sectors	1,059	**0.727(0.206)**
6	Include union density (only available 1989–96)	547	Training: **0.604(0.266)** Union: –0.177(0.183)
7	Allow all industries to have different training coefficients	968	Mean of heterogeneous coefficients: 0.505
8	Allow non-constant returns	968	**0.725(0.201)**
9	Estimating a translog production function	968	**0.703(0.201)**
10	Estimation by non-linear least squares	968	**0.518(0.197)**
11	Estimation on 1993–2001 data (region–industry cells)	1,873	**0.436(0.188)**

Notes: These all use the specification in column (3) of Table 1 (unless otherwise specified). Estimation by within groups; robust standard errors in parentheses (except row 10). Bold typeface indicates that the variable is significant at the 5% level.

Since training is correlated with unionisation, we could be picking up 'collective voice' effects in the main results. Union membership is only available in LFS since 1989. Despite the loss in sample in row 6, the training effect is robust to inclusion of union density (density is insignificantly negatively associated with productivity). In row 7 we allow the training effect to be different in each of the 85 industries; the mean of these heterogeneous coefficients is close to the pooled results. The next two rows allow for more general functional forms, first relaxing constant returns (row 8) and then estimating a translog production function (row 9); in both cases, the training coefficient is essentially unchanged. Row 10 gives the results from a non-linear least squares estimation of equation (8) again showing no significant difference.

We also compared our results with a recent paper (Machin *et al.*, 2003) which has built up similar data to our own covering a more recent period and exploiting the larger size of the LFS post-1992 to construct industry-by-region cells. Against these advantages, their dataset has a shorter time-series component (1993–2001) and lacks some of the covariates we use. Reestimating identical specifications on their dataset gives an estimate of the training association with productivity of 0.436 with a standard error of 0.188 (see row 11 of Table 3). This is lower, but is still significant and remains well within two standard errors of our main results.[30] On our dataset, we tested whether there was a tendency for the training coefficient to fall (or rise) over time in the production function, but we found it to be stable.[31]

Does the 'wedge' between the wage and productivity effect of training arise from specific human capital or imperfect competition? Under most forms of imperfect competition, we conjectured that the wedge would be larger in those industries where workers were earning less than would be implied by their human capital (i.e. inter-industry wage premiums were low). This could be because the 'low-paying' industries were monopsonistic with large search frictions or because workers are more able to capture the quasi-rents from training in the 'high-paying' industries.

In order to identify such industries, we used estimates of inter-industry wage premiums taken from the US Current Population Survey (CPS).[32] We matched the US industries to the UK industries and split the sample at the median sectoral wage premium. Allowing an interaction between training and this industry split revealed that the wedge between the training effect on productivity and the training effect on wages was solely within the 'low-wage' industries. To be precise, including an interaction in the wage equation between training and low-wage industries gave a coefficient (standard error) of –0.664 (0.196) on the interaction and 0.531 (0.113) on the linear training effect. In the production function, the interaction was 0.332 (0.297) – positive but insignificant (the linear training term took a coefficient of 0.612 with a

[30] The specification is identical to column (3) of Table 1 except we drop the occupational proportions and R&D and include employment. On our data, this gives a coefficient (standard error) on *TRAIN* of 0.732 (0.205).

[31] For example, interacting *TRAIN* with a trend in the production function gave a coefficient of 0.003 with a standard error of 0.044.

[32] Estimating inter-industry wage premiums from UK wages would have been more problematic as these could reflect endogenous influences – US wage-setting will be driven by the structural characteristics of the industries in question. These US inter-industry wage premiums were generated from individual-level wage regressions from the 1986 CPS Merged Outgoing rotation files. The wage regressions included years of schooling, a quartic in experience, gender, marital status, gender×marriage interactions, race, Standard Metropolitan Statistical Area and regional dummies. The data were kindly provided by Steve Pischke (see Acemoglu and Pischke, 2003, for details).

standard error of 0.172)[33]. In other words, in the ‘high wage premium’ industries, there was no significant difference between the impact of training on productivity and the impact of training on wages. The fact that our results are driven by the wedge in low-paying sectors is tentative evidence in favour of a monopsony/search interpretation.

This evidence is open to the critique that firm-specific training may be systematically more prevalent in the low-wage sectors (although a priori the usual view is that ‘good jobs’ are more likely to have more specific skills). There are several questions in LFS that could be interpreted as general vs. specific training, so we used them to see if the coefficients differed significantly with training type – they did not. For example, there are questions related to off-the-job training (more general) and on-the-job training (more specific). The proportion of off-the-job training produced a coefficient (standard error) of 0.005 (0.018) when added to the wage regression and a coefficient (standard error) of 0.018 (0.029) when added to the production function. We view this not as any rejection of specific human capital theory per se, but rather as an indication the type of human capital is intrinsically difficult to measure. Furthermore, the LFS questions are not asked in all years and have many missing values.

Quantifying the effects of training

Our key qualitative conclusions are, first, that there is a significant impact of training on productivity and, second, that the effects of training on productivity are larger than the effects of training on wages. But, quantitatively, how economically significant is the magnitude of the training effect?

Interpreting the magnitude of the coefficients is difficult, but the implied effects are large. From Tables 1 and 2, we conservatively take the coefficient on training in the productivity regressions to be about 0.6 and the coefficient on training in the wage regressions to be about 0.3. This would imply that a 10 percentage point increase in the training measure is associated with a 6% increase in productivity and a 3% increase in wages.

[33] A test of the equality between the effects of training on wages (λ) and on productivity (γ) can be rejected at the .05 level for the “low wage” industries (p-value = 0.001), but cannot be rejected for the “high wage” industries (p-value = 0.752).

Relative to the returns-to-schooling literature, the training impacts appear high[34]. Card (1999) puts the impact of a year of schooling on wages at about 10%, so our baseline impact of 0.3 is about three times as large. Given that the typical time in training during the four-week period is under a month (the median is two weeks, the mean is higher), the returns to a month of training appear even more impressive. For example, an increase in our key variable, *TRAIN,* of 10% would imply a typical worker only spent 5% extra of his time in training, if training spells were on average two weeks long.

Of course, there may be remaining econometric problems we have not controlled for generating this difference. But assuming the training effect is not a statistical artefact, there remain at least two possible explanations for the training coefficients being larger than conventional estimates of the return to schooling. First, work-related training may have a higher private return than schooling as training is more directed at raising productivity in employment. Training is also likely to have a faster rate of depreciation than schooling, so it requires a higher year-on-year return in order to give incentives for investment.[35] Second, there may be externalities associated with training that are missed in the conventional schooling literature, which focuses on private returns whereas we look at returns to the industry as a whole (cf. Moretti, 2004).

To investigate the externality issue, we estimated some individual-level wage regressions on the LFS panel. If the private returns to training are higher than the social returns, we might expect to see a similarly high coefficient in the individual-level wage regression. We used the individual-level equivalents of the variables in the industry-level regressions. To construct the proportion of the year spent in training, we used the LFS panel which follows individuals for five quarters and asks individuals the training question in each quarter. We defined a dummy variable (*TRAIND*4) indicating whether the individual had been involved in some training in *all* of the previous four quarters. We also defined dummies for if the individual had been in training for three quarters (*TRAIND*3), two quarters (*TRAIND*2), one quarter (*TRAIND*1) or not at all

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[34] Compared with existing UK estimates of the training effects on wages (e.g. Booth, 1993 and Blundell *et al*., 1996), our estimates are actually lower (see Dearden *et al*., 2000, for a detailed comparison).

[35] See Heckman *et al*. (2003) for a recent discussion of interpretation of the schooling coefficient in wage regressions.

(*TRAIND*0). Using *TRAIND*0 as the omitted base, the results we obtained from a typical regression were[36]:

$$\ln(wage) = 0.165(0.033)TRAIND4 + 0.092(0.023)TRAIND3$$
$$+0.125(0.019)TRAIND2 + 0.078(0.015)TRAIND1 \ + \ \text{controls}$$

Longer lengths of time in training are associated with significantly higher wages[37]. The coefficient on receiving training in all four quarters is 0.165; this is comparable with the industry-level coefficient of 0.350. Taken literally, this would suggest that about half of the impact of training on wages at the industry level is attributable to externalities.

If we include a set of industry dummies (which will include potential spillovers), the coefficient on *TRAIND*4 falls from 0.16 to 0.13. If we also include the initial wage in the first quarter (to control for unobserved heterogeneity), the coefficient falls even further to 0.079. So these impacts of a ‘year’ of training are rather similar to the conventional impacts of the returns to a year in school.

Our conclusion from this exercise is that the larger magnitude of the training effects in this paper primarily reflects our strategy of estimating at a level above the individual worker. This was forced upon us by the absence of adequate data on firm productivity and training, but also because of our desire to incorporate externalities. The results are therefore consistent with a story that stresses externalities to training.

Even if there remain econometric problems that have caused us to overestimate the impact of training at the industry level, it is hard to see why this would not also bias upwards the training coefficient in the production function and wage equation to a similar extent. Therefore, even if one disputes the exact quantitative magnitude of the training effect, our key qualitative conclusion that the productivity impact of training is greater than the wage impact should still be valid (this is also a feature of the firm-level results in Appendix B).

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[36] Estimation was by OLS; robust standard errors are given in parentheses. Controls include gender, age, areas (20), employer size, occupational dummies (8), no qualification dummy, and a dummy for part-time status. Results are for the production sector only. The quarterly LFS panel 1997–8 was used as two wage observations per individual did not exist in the LFS prior to this. There were 3,998 observations. Full results are available on request from the authors.

[37] The training effects are not monotonic. There is even a perverse fall in the coefficient on being in training three relative to two quarters, although the coefficients are not significantly different.

VI. Conclusions

In this paper, we have examined the issue of the impact of private sector training on productivity. Rather than simply use wages as a measure of productivity, we have presented (for the first time) estimates of the impact of training on productivity over a long time period. We have assembled a dataset that aggregates individual-level data on training and establishment data on productivity and investment into an industry panel covering 1983–96. We controlled for unobserved heterogeneity and the potential endogeneity of training using a variety of methods including GMM system estimation.

Using these new data, we have identified a statistically and economically significant effect of training on productivity in the UK. An increase of 1 percentage point in the proportion of employees trained is associated with about a 0.6% increase in productivity and a 0.3% increase in wages. The impact of training on productivity is robust to a large number of robustness tests.

We argued that the methodologies in the existing literature may underestimate the importance of training. The focus on wages as the only relevant measure of productivity ignores the additional productivity benefits the firm may capture. The coefficient of training in the production function was around twice as large as the coefficient in the wage equation. This result could occur even under standard specific human capital theory. But it could also arise for a number of other reasons due to imperfect competition in the labour market (and we have presented some evidence consistent with this hypothesis). Clearly, further research is needed to distinguish between these theories.

Finally, a comparison between the industry- and individual-level wage regressions suggests that our industry-level analysis may capture externalities from training that are missed out in the micro-level studies. An important avenue of future research would include probing the returns to training by combining enterprise data with industry-level data to investigate the externalities story in greater detail.

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Appendix A. Data

Data construction for industry panel

The database we construct combines several sources. The critical individual-level source is UK Labour Force Survey (LFS). LFS is a large-scale household interview-based survey of individuals in the UK which has been carried out on varying bases since 1975.[38] Around 60,000 households have been interviewed per survey since 1984. The LFS data are useful for our purposes as they contain detailed information on:

- the extent and types of training undertaken by employees in the survey;
- personal characteristics of interviewees (e.g. age, sex, region);
- the skills of individuals (educational qualifications and occupation);
- some basic workplace characteristics (e.g. employer size, industry);
- job characteristics of employees (e.g. job tenure, hours of work).

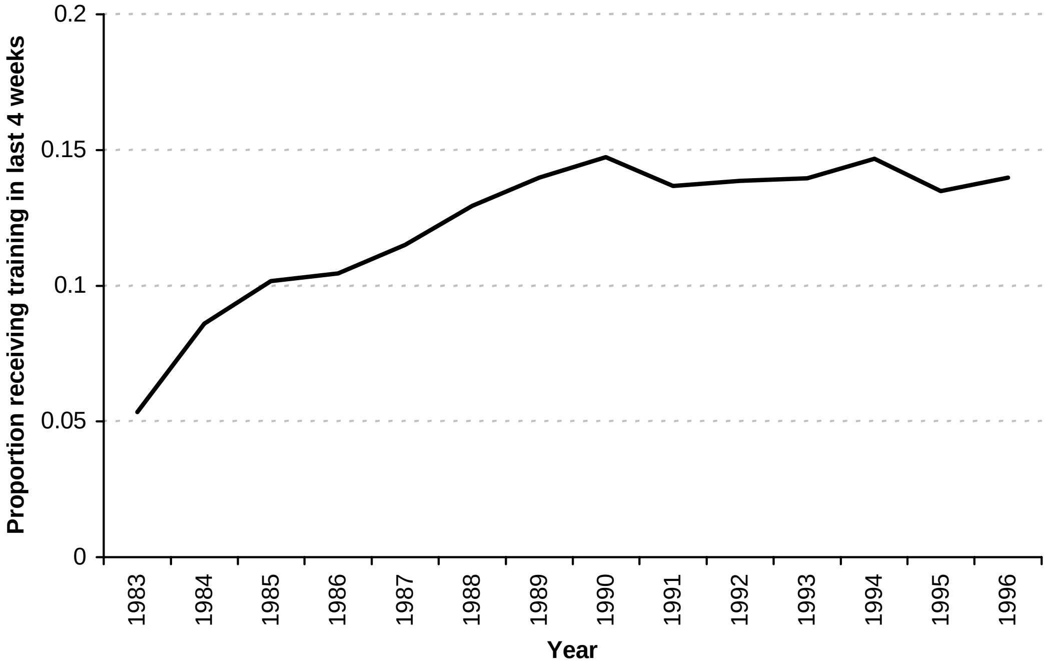
We work with this information aggregated into proportions and/or averages by (broadly) three-digit SIC80 industry. Our sample includes all employed men and women aged between 16 and 64 inclusive (i.e. employees plus the self-employed) for whom there was information on the industry under which their employment was classified.

The main training question asked to employees in the Labour Force Survey between 1983 and 1996 was '*over the 4 weeks ending Sunday … have you taken part in any education or training connected with your job, or a job that you might be able to do in the future … ?*'. Figure A1 presents the average proportions of employees undertaking training in each year of the LFS sample and shows a reasonably steady increase in the 1980s.[39] From 1990 onwards, the proportion of employees receiving training stabilises at around 14% and stays at or around this level for the rest of the sample period.

[38] Between 1975 and 1983, the survey was conducted every two years. From 1984 until 1991, it was conducted annually. Since 1992, the Labour Force Survey has been conducted every three months in a five-quarter rolling panel format.

[39] It should be noted that the figure of around 5% for 1983 is almost certainly an underestimate because in 1983 the four-week training question was only asked of employees under 50, whereas in all subsequent years it was asked of employees over 50 and under 65 as well. However, even if the training measure is calculated as the proportion of employees aged under 50 receiving training in every year, the figure for 1983 is still lower than that for 1984.

Figure A1. Overall training incidence, Labour Force Survey, 1983–96



We did some simple decomposition analyses to investigate whether the increase in aggregate training was due to the growth in size of industries that are (and always have been) relatively more training intensive. It turns out that this is only a minor factor: over 95% of the increase in aggregate training is due to an increase within a large number of different sectors.[40] This is consistent with the findings of other papers, which have found that the aggregate growth of education or occupational skills is essentially a within-industry phenomenon (e.g. Machin and Van Reenen, 1998).

LFS also has further information on the type of training received (although not all of the questions on this are asked in each year). For example, the questionnaire distinguishes between 'on-the-job' training (e.g. learning by example and practice while doing the job) and 'off-the-job' training (training conducted as a formal training course). Whilst the incidence of on-the-job training reported in LFS has been more or less constant since the mid-1980s, the proportion of workers receiving some off-the-job training in the four weeks prior to being surveyed rose from about 5% in 1984 to 8% in the early

[40] The change of training propensity over a given period can be decomposed into a *within-industry* and a *between-industry* component: $\Delta T = \sum_i \Delta S_i \overline{T_i} + \sum_i \Delta T_i \overline{S_i}$ where T_i = proportion of workers in industry i undertaking training, S = share of industry i in total employment, a bar denotes a mean over time and delta is the difference over the given period.

1990s. Other indicators of training (not asked in every year) include the duration of training, whether it was employer funded and whether it was completed or still ongoing. We examined whether there were differential productivity effects for all these different types of training, but could find no significantly different coefficients.[41]

The second major dataset we use is the Annual Census of Production (ACOP). This gives production statistics on capital, labour and output for industries in the manufacturing, energy and water sectors (collectively known as the production sector of the economy). It is based on the ARD (Annual Respondents Database), which is a survey of all production establishments (plants) in the UK with 100 or more employees, plus a subset of establishments with less than 100 employees. We use the ACOP data on value added, gross output, investment, employment and wages for industries in the manufacturing sector and the energy and water industries.

Capital stocks were calculated using the perpetual inventory method drawing on NIESR's estimates of initial capital stocks (see O'Mahony and Oulton, 1990). All the nominal measures were deflated with three-digit industry price indices from the ONS. For the services industries, we drew on the ISDB (InterSectoral DataBase) compiled by the OECD.

There was a change in SIC classification in 1992 which forced us to aggregate some of the industries and prevented us from using some of the industries after the change. Additionally, we insisted on having at least 25 individuals in each cell in each year. After matching the aggregated individual data from LFS, we were left with 94 industry groupings over (a maximum of) 14 years (85 in the production sectors).[42]

Means of the variables are given in Table A1 broken down by high- and low-training industries.

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[41] The measure of training used takes no account of the intensity or length of the training course (except in so far as a longer training course is more likely to fall within the four-week period prior to the survey). There is some evidence that the length of training has been falling since the later 1980s (for a detailed analysis, see Felstead *et al.*, 1997). However, when we estimate training effects separately for each year of the LFS sample, the magnitude of the training effect does not differ significantly over time. One might expect a decrease in the productivity effect of the training measure for the later years if the average quality of training courses had declined (there was no evidence of this in our data).

[42] Full details on how the data were constructe can be obtained from the authors or in Dearden *et al.* (2000).

Calculating the training stock

The main results use the flow in training, but we also report some experiments with an estimate of the stock of trained workers in an industry. If we define the stock of people who have been trained in the industry at time *t* as N^T_t and the flow as M^T_t then if the stock evolves according to the standard perpetual inventory formula, it can be expressed as

$$N^T_t = M^T_t + (1-\delta)\, N^T_{t-1}$$

where δ is the rate at which the stock of effectively trained workers at time *t* decay in their productive usefulness by time *t*+1. This training depreciation rate represents several things. First, individuals will move away from the industry, so their training can no longer contribute to the industry's human capital stock. Second, the usefulness of training will decline over time as old knowledge becomes obsolete and people forget (e.g. knowledge of the DOS operating system). Third, to the extent that training is firm specific, turnover between firms in the same industry may reduce industry productivity. Although we obtain some measures of turnover using the LFS, the second element of depreciation is essentially unknown. Because of this uncertainty, our baseline results simply use the proportion of workers trained in an industry (*TRAIN* in equation (3) in Section II). This will be equal to the stock when δ=1. Nevertheless, we also estimate the training stock. We use the average worker turnover rate as one element of depreciation and then add an extra 'exogenous element' varying between 0.2 and 0.6. We also need to make an assumption about the initial stock in 1983. We assume that the steady-state growth, rate of the training stock *g* is 2% per annum, which enables a first-year approximation of the stock as $M^T_{83}/(g+\delta)$. The qualitative results are quite robust over different measures.[43]

Firm-level data

The firm-level data are based on the 'Survey on Human Resource Practices and Corporate Value in the Modern Corporation' conducted in 1996 by Martin Conyon (see Conyon and Reed, 1999). The relevant training variable was q23f: '*How have the following training strategies used by your company* *changed between 1990 and 1995 … The percentage of company sales turnover spent on training has increased/decreased/not changed*'. The number of firms giving some response to this question was 135. We matched this survey to company accounting data from Datastream (data between 1968 and 1997). We discarded firms with missing values on the training and accounting variables, leaving us with a sample of 119 companies.

Productivity was measured by real sales (Datastream Item 104) per worker (Item 219). Capital is the historical book value (Item 330) and wages were estimated as total remuneration (Item 215) divided by the number of workers (Item 219). All accounts are consolidated.

[43] See the discussion around row 2 of Table 3 in the main text. We also considered looking at the inflows of trained workers from other industries to improve the stock measure, but the LFS sample of industry switchers was too small to construct the full three-digit flow matrix.

TABLE A1

Means of variables by high- and low-training industries

Variable	*Mean, low-training industries*	*Mean, high-training industries*
Proportion of male employees	62.4%	80.9%
Proportion aged:		
16–24	22.7%	15.5%
25–34	24.5%	25.4%
35–44	22.2%	24.0%
45–54	19.1%	22.1%
55–64	11.1%	12.8%
Proportion in occupation:		
professional/managerial	14.7%	27.1%
Clerical	8.5%	10.9%
personal/security	1.9%	1.6%
salesforce/technical	3.4%	2.5%
other occupations	71.5%	58.0%
Highest qualification:		
degree	2.6%	7.3%
sub-degree level	3.7%	9.2%
A level / equivalent	15.5%	22.5%
O level / equivalent	15.6%	14.3%
other/none/missing	62.7%	46.7%
Tenure in current job:		
less than 6 months	10.9%	7.1%
6 months – 1 year	8.4%	5.8%
1 year – 2 years	11.1%	8.0%
2 years – 5 years	21.6%	17.8%
5 years – 10 years	18.8%	19.7%
10 years – 20 years	18.0%	24.3%
more than 20 years	9.0%	16.5%
Proportion in small firm	21.2%	12.6%
Average ln(capital–labour ratio)	2.22	3.02
Average ln(real value added per worker)	2.76	3.19
Average ln(gross output per worker)	3.80	4.27
Average ln(hourly wages)	1.56	1.84
Average hours worked	39.1	40.2
Average R&D spend as proportion of output	0.52	2.99

Note: 'High-training' industries are those that trained on average more than 8.7% of employees (the sample median).

Appendix B. Additional results

Some firm-level results

As a preliminary investigation of the impact of training on productivity, we turned to the only UK dataset we know of that combines training data and objective productivity measures at the firm level. We constructed this by combining a firm survey with company accounting data from Datastream (see Appendix A for details). We estimated an OLS production function in long differences with results summarised in Table B1. The drawback of the firm-level training data is that we only know whether a firm increased the percentage of turnover allocated to training over this time period and not the actual quantitative change, so the table is simply illustrative.

Columns (1) and (2) of Table B1 give results for productivity and columns (3) and (4) have the results for wages. Columns (2) and (4) condition on industry dummies. In columns (1) and (3), the indicator for the change in training is positively and significantly associated with both productivity changes and with wage changes. Interestingly, the magnitude of the association with productivity is about twice as large as the magnitude of the association with wages. We uncovered a similar finding when we examined the industry-level data.[44] A second feature of Table B1 is that much of the association between training, productivity and wages is due to industry-level (growth) effects. When sector dummies are added, the coefficient on training falls by half in the productivity regression (second column) and by three-quarters in the wage equation (fourth column). The training effects are no longer significant. This implies that a substantial element of the training correlation is due to industry growth effects. It is these changes over a much longer time period that we exploited in the main part of the paper.[45]

More detailed industry results

Table B2 reports the unrestricted coefficient estimates underlying Table 2 in the main text and the restricted estimates of the production function parameters after imposing the COMFAC restrictions. If we took the unrestricted coefficients, our preferred models in columns (1) and (4) would suggest a *larger* magnitude of the long-run impact of training on productivity[46] (2.289) than of training on wages (1.529). The qualitative finding that the productivity effect is twice the size of the wage effect remains unaltered.

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[44] Given the size of the sample, it is not surprising that the difference in training coefficients is not significant (p-value=0.23, allowing for cross-equation correlation of the errors through SUR).

[45] In the spirit of looking for complementarities (Ichniowski *et al*., 1997), we investigated allowing for interactions between changes in training and changes in many other features of the firm – merit pay across four different skill groups, employee involvement, retention, flexibility, three types of delayering, share options, teamwork, competition, etc. With the exception of merit pay for managers (which was significant at the 10% level), none of these interactions was statistically significant. It is worth noting that neither Black and Lynch (1997) nor Bartel (1995) find strong evidence of such interactions in their data.

[46] i.e. using $(\pi_2 + \pi_3)/(1-\pi_1)$ in the context of equation (14) in Section III.

TABLE B1

Training, productivity and wages at the firm level
(long differences, 1995 - 1990)

	(1)	*(2)*	*(3)*	*(4)*
Dependent variable:	*Mean change in ln(sales per worker)*		*Mean change in ln(average wage)*	
Growth in training as a percentage of turnover	**.021** **(.009)**	.010 (.010)	**.012** **(.006)**	.003 (.007)
Mean change in ln(capital per worker)	**.139** **(.045)**	**.159** **(.049)**	**.066** **(.032)**	**.103** **(.033)**
Sector dummies (32)	No	Yes	No	Yes
Observations	119	119	119	119
R^2	.41	.62	.22	.63

Notes: Estimation by OLS in long differences (average 1995 - 1990); robust standard errors under coefficients. Bold typeface indicates that the variable is significant at the 5% level. All regressions include controls for the growth in employment and whether there was an increase in the proportion of skills (four groups: managers, clerical, skilled manual, unskilled manual), of females and of part-timers. These are all continuous variables. 'Growth in training as a percentage of turnover' is a dummy variable indicating whether training grew as a percentage of turnover between 1990 and 1995.

TABLE B2

Detailed GMM results

	(1)	*(2)*	*(3)*	*(4)*
	ln(real value added per worker)		*ln(wages)*	
Constrained – COMFAC imposed				
Training	**.602**	**1.043**	**.141**	**.351**
	(.181)	**(.182)**	**(.067)**	**(.074)**
ln(capital/worker)	**.327**	**.325**	**.188**	**.106**
	(.016)	**(.017)**	**(.006)**	**(.011)**
ln(hours/worker)	**.498**	**.519**	**.518**	**.489**
	(.064)	**(.062)**	**(.031)**	**(.027)**
Lagged R&D intensity	**1.905**	**1.538**	.226	**.443**
	(.262)	**(.340)**	(.159)	**(.182)**
Proportion of employees who are professionals or managers	**.306**	**.337**	**.230**	**.160**
	(.068)	**(.074)**	**(.028)**	**(.034)**
Years	1984–96	1985–96	1984–96	1985–96
NT	898	833	898	883
Autocorrelation coefficient (ρ)	.741	.758	.822	.797
	(.015)	(.014)	(.009)	(.013)
LM1(d.f.)	–4.892(85)	–4.513(85)	–5.444(85)	–6.053(85)
[p-value]	[0.00]	[0.00]	[0.00]	[0.00]
LM2(d.f.)	–.940(85)	–.674(85)	–2.003	–1.44(85)
[p-value]	[.347]	[.500]	[.045]	[.158]
Sargan (d.f.)	8.819(121)	6.605(146)	11.03(121)	11.83(146)
Instruments	$(TRAIN)_{t-2,t-3}$, $\ln(Q/N)_{t-2,t-3}$, $\ln(Hrs/N)_{t-2,t-3}$, $\ln(K/N)_{t-2,t-3}$ in differenced equations; $\Delta(TRAIN)_{t-1}$, $\Delta\ln(Hrs/N)_{t-1}$, $\Delta\ln(K/N)_{t-1}$ in levels equations	$(TRAIN)_{t-3,..,t-5}$, $\ln(Q/N)_{t-3,..,t-5}$, $\ln(Hrs/N)_{t-3,..,t-5}$, $\ln(K/N)_{t-3,..,t-5}$ in differenced equations; $\Delta(TRAIN)_{t-2}$, $\Delta\ln(Hrs/N)_{t-2}$, $\Delta\ln(K/N)_{t-2}$ in levels equations	(same as column 1) $(TRAIN)_{t-2,t-3}$, $\ln(Q/N)_{t-2,t-3}$, $\ln(Hrs/N)_{t-2,t-3}$, $\ln(K/N)_{t-2,t-3}$ in differenced equations; $\Delta(TRAIN)_{t-1}$, $\Delta\ln(Hrs/N)_{t-1}$, $\Delta\ln(K/N)_{t-1}$ in levels equations	(same as column 2) $(TRAIN)_{t-3,..,t-5}$, $\ln(Q/N)_{t-3,..,t-5}$, $\ln(Hrs/N)_{t-3,..,t-5}$, $\ln(K/N)_{t-3,..,t-5}$ in differenced equations; $\Delta(TRAIN)_{t-2}$, $\Delta\ln(Hrs/N)_{t-2}$, $\Delta\ln(K/N)_{t-2}$ in levels equations
Unconstrained – COMFAC not imposed				
Lagged dependent variable	**.543**	**.599**	**.707**	**.624**
	(.058)	**(.063)**	**(.035)**	**(.045)**
Training$_t$	**.930**	.652	.248	**.303**
	(.465)	(.412)	(.165)	**(.149)**
Training$_{t-1}$	.116	–.969	–.085	.272
	(.241)	(.648)	(.134)	(.289)
ln(capital/worker)$_t$	**.278**	**.323**	**.120**	.028
	(.073)	**(.056)**	**(.051)**	(.037)
ln(capital/worker)$_{t-1}$	**–.156**	–.104	–.078	.020
	(.063)	(.063)	(.044)	(.033)
ln(hours/worker)$_t$	**.495**	**.434**	**.359**	**.480**
	(.165)	**(.123)**	**(.064)**	**(.079)**
ln(hours/worker)$_{t-1}$	–.422	**–.397**	–.189	**–.362**
	(.231)	**(.169)**	(.114)	**(.100)**
R&D intensity$_{t-1}$	**1.248**	1.137	.549	.551
	(.605)	(.669)	(.323)	(.334)
R&D intensity$_{t-2}$	–1.157	–.671	–.493	.592
	(.673)	(.740)	(.311)	(.354)
Managerial proportion$_t$	**.446**	**.392**	**.125**	.113
	(.129)	**(.136)**	**(.056)**	(.070)
Managerial proportion$_{t-1}$	–.148	–.173	–.044	–.054
	(.129)	(.113)	(.087)	(.088)

Notes: This table reports a fuller set of results than those in Table 2 (which correspond to columns (1) and (4) of Table B2). Estimation by GMM-SYS in Arellano and Bond (1998) DPD-98 package written in GAUSS. All regressions include the current values of all the variables in columns (3) and (6) of Table 1 (i.e. turnover, other occupations, qualifications, age, tenure, gender, region, firm size and time dummies). Capital intensity, training, hours and lagged productivity are always treated as endogenous. The other variables are assumed weakly exogenous. One-step standard errors (robust to arbitrary heteroskedasticity and autocorrelation of unknown form) are given in parentheses under coefficients. LM1 (LM2) is a Lagrange Multiplier test of first- (second-)order serial correlation distributed N[1,0] under the null (see Arellano and Bond, 1991). Sargan is a Chi-squared test of the over-identifying restrictions. Observations are weighted by number of individuals in an LFS industry cell.