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The Union Wage Effect in Late Nineteenth Century Britain

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Abstract
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Keywords
trade unions, union wage effect, Britain, earnings

Disciplines
Economic History | Labor Economics | Labor History | Labor Relations | Regional Economics

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The Union Wage Effect in Late Nineteenth Century Britain

By T.J. Hatton, G.R. Boyer and R.E. Bailey

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This paper offers an historical dimension to the impact of trade unions on earnings by estimating the union wage effect in Britain in 1889-90 using data from the US Commissioner of Labor survey conducted at that time. The determinants of union status are also investigated in terms of a probit estimation using individual characteristics which may be correlated with union membership. The results of this first step are used in the computation of selectivity corrected estimates of the union wage effect. It is found that the effect of union membership on earnings at this time was of the order of 15%-20% and that this effect was similar at different skill levels. A broadly similar pattern is observed for industry groups, although the difference in the impact of unions on earnings across industries was greater than across skill groups.

Introduction

The purpose of trade unions has changed very little in the last century. The Webbs (1894, p. 1) defined a trade union as 'a continuous association of wage earners for the purpose of maintaining or improving the conditions of their employment'. Perhaps the major way in which unions do this is by raising their members' wages relative to those of otherwise similar workers. Several studies undertaken in recent years to estimate the effect of contemporary unions on wages suggest that the current union wage effect in Britain is 12% or less[1].

No similar studies exist for the late nineteenth century, the period that marked the beginning of the modern trade union movement in Britain[2]. This paper attempts to extend our understanding of the impact of trade unions by giving it an historical dimension. We estimate the union wage effect in Britain in 1889-90, using as our data source a survey of 1021 households undertaken by the US Commissioner of Labor. We also use the data to estimate the determinants of workers' decisions to become union members. Recent research using contemporary data has shown that a worker's experience, industry and region are important determinants of union status (Booth 1986). We examine the role of such factors in union membership decisions in 1889-90.

The paper is organized as follows. Section I provides a brief survey of trends in the union movement in the late nineteenth century and the extent of unionism *circa* 1890. The contents of the Commissioner of Labor's survey are discussed in Section II. Section III examines the determinants of union membership. In Section IV we present the ordinary least squares (OLS) and selectivity-corrected OLS estimates of the union wage effect. The main findings are summarized in Section V, and formal supporting analysis is contained in the Appendix.

The Labour Market and Unionism *circa* 1890

The Commissioner of Labor's survey took place during a period of rapid growth in trade unionism. According to E. J. Hobsbawm (1985, p. 15), 'the year 1889 unquestionably marks a qualitative transformation of the British labour movement and its industrial relations'. Union membership increased from approximately 750,000 in 1888 to 1,576,000 in 1892. The large-scale

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1 For estimates of the union wage effect in recent times on either individual or establishment level data, see Stewart (1983, 1990, 1991), Shah (1984), Blanchflower (1984), Blanchflower and Oswald (1990) and Green (1988).

2 The only estimates of the pre-1914 union wage effects are two recent studies of the United States in the 1890s by Eichengreen (1987) and Dillon and Gang (1987) and a study of agricultural labourers' trade unions in late nineteenth-century England by Boyer and Hatton (1994).

organization of unskilled workers in newly established unions, known as the 'new unionism', began in 1889; by 1890 the nine major new unions claimed about 350,000 members (Clegg *et al.* 1964, pp. 1, 83, 489). While the formation of unions of low-skilled workers was the major organizational innovation of the period, the older unions of skilled workers experienced even larger absolute increases in union membership. Table 1 provides estimates of union membership in three major industrial sectors in 1888 and 1892. Union membership in these sectors increased by 380,000 during the period; union density increased from 16-8% in 1888 to 30-6% in 1892. For Great Britain as a whole, union density increased from about one in nine in 1888 to one in five in 1892 (Clegg *etal.* 1964, p. 467).

TABLE 1

TRADE UNION MEMBERSHIP IN BRITAIN, 1888 AND 1892

	1888		1892	
	Union membership	Union density (%)	Union membership	Union density (%)
Metals, engineering and shipbuilding	190,000	19·5	310,200	31·9
Mining and quarrying	150,000	24·2	326,700	52·6
Textiles	120,000	10·5	203,100	17·7
Great Britain	750,000	6·2	1,576,000	13·0

Sources: Union membership data for 1888 are from Clegg *et al.* (1964, p. 1). Membership data for 1892 are from Bain and Price (1980). The union densities were calculated using industry employment data for 1891 from Bain and Price (1980).

The majority of the workers included ion the 1889-90 survey were in iron and steel, coal mining, and cotton and woollen textiles. The importance of trade unionism varied significantly across these sectors. In cotton, overall union density in 1888 was nearly 25%. Among adult male cotton spinners, density was about 90%, compared with about 25% among cotton weavers and cardroom operatives. The number of organized weavers and cardroom operatives more than doubled from 1888 to 1892, so that union density in these sectors was about 50% in 1892. There was a sharp contrast between the strength of unionism in cotton and its weakness in wool. The power-loom weavers and woolcombers achieved some strength between 1889 and 1890, but this was largely lost by 1900. The only workers to maintain a significant degree of union organization were the warp dressers, twisters, drawers, dyers and finishers, all of whom were highly skilled craftsmen (Clegg *et al.* 1964, pp. 112-13, 118, 184, 189).

The unions of coal mines aligned themselves into two groups during the late 1880s. In 1889 the unions in Yorkshire, Lancashire and most of the Midlands, led by the Yorkshire Miner's Association, formed the Miner's Federation of Great Britain. The Federation began with 75,000-100,000 members, which increased to 160,000-170,000 members in 1892. The non-federated unions included the miners of Northumberland, Durham, South Wales and South Staffordshire, areas that produced largely for the export market. The group was dominated by the Northumberland and Durham miners. In 1888 union density in these two counties was over 50%, far above that of any other coalfield, and union membership there increased by 35% from 1888 to 1892 (Clegg *et al.* 1964, pp. 2, 98-9, 102). Union membership in iron and steel also grew rapidly during this period, although union density was not as high as in cotton or coal mining. From 1887 to 1890 membership in the two largest unions, the Associated Iron and Steel Workers and the British Steel Smelters, increased by 183%, from 4100 to 11,600.

Unskilled workers in these industries were not eligible to become members of the trade unions to which their semi-skilled and skilled co-workers belonged. However, they were able to join the 'new' unions of low-skilled workers founded in 1889-90, such as the National Union of Gasworkers and General Labourers; the Dock, Wharf and Riverside, and General Labourers Union; and the Amalgamated Union of Labour. As their names indicate, these were typically general unions in that they recruited lowskilled workers from a number of industries. Unskilled workers within the industries in our sample often formed branches of general unions[3]. The 'new unions' grew exceptionally rapidly. The nine major unions of lowskilled workers founded in the late 1880s claimed to have a membership of 350,000 in 1890. However, their success could not be maintained, and a counter-attack by employers combined with worsening economic conditions caused membership in the new unions to fall under 200,000 in 1892 and further to about 150,000 in 1896 (Clegg *et al.* 1964, pp. 83, 97).

These increases in union membership took place against the background of a long-term trend in union growth. But a major reason for the sudden upsurge was the favourable state of the labour market. Unemployment, which had averaged 8-1% in 1884-87, fell to 2-1% in 1889-90, lower than at any other time from 1875 to 1915. E. H. Hunt (1981, p. 304) contends that 'substantial trade union expansion was inevitable in these conditions'. Another factor may have been a general upward trend in wages which workers may have ascribed to union activity (see Bain and Elsheikh 1976, p. 64). Clearly, many workers rushed to join unions in the hope or expectation that through collective action they could obtain wage increases.

Union militancy was very pronounced during this period. The number of strikes increased from 517 in 1888 to 1211 in 1889 and 1040 in 1890 (Mitchell and Deane 1962, p. 71). According to Cronin (1979, p. 49), 'the industrial distribution of strikes and of workers affected between 1888 and 1892 was remarkably dispersed. Their explosion affected both skilled and unskilled, workers organised in old unions . . . and in "new" unions.' Strike activity was especially pronounced in the industries in our data-set; 32% of the strikes in 1889-90 were in either mining or textiles (Mitchell and Deane 1962, p. 71). Overall the strikes were 'extremely successful'. Moreover, many unions of skilled workers reported achieving wage increases in 1889-90 without having to strike (Clegg *et al.* 1964, p. 126).

The new unions of unskilled workers were more militant than the older unions. According to the Webbs (1894, p. 493), these unions were characterized by 'low contributions, fluctuating membership, and militant trade policy', although many of them quickly adopted a more conciliatory approach. None the less, they contrasted with the traditional craft unions, which offered a wide range of friendly benefits and were less militant[4]. None of the unions in our data-set were craft unions, in the sense that none of them had an apprenticeship system (Lovell 1977, p. 18). There was a fundamental difference between these unions and the major craft unions. By the late nineteenth century craft unions were able to 'enforce their trade policies by a combination of unilateral action and localised collective bargaining, and so avoid large scale clashes with employers' (Lovell 1977, p. 12) On the other hand, 'unions in coal, cotton and iron had to face

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[3] For example, the National Amalgamated Union of Labour contained 11 branches of iron and steel workers and some coal workers in 1893, the Gasworkers' Union contained branches of cotton and iron and steel workers, and the National Labour Federation recruited low-skilled production workers in iron and steel as well as in engineering, shipbuilding and chemicals (Hobsbawm 1964, pp. 186-9; Clegg *et al*. 1964, pp. 65-6).

[4] In the late nineteenth century many unions provided their members with insurance against unemployment, sickness and accidents, old-age benefits (pensions) for retired members, and death benefits to ensure workers and their wives proper funerals. The provision of such insurance policies strengthened union organization by inducing members to join unions and to remain members during downturns. The availability of mutual insurance benefits varied significantly across unions. Several major craft unions such as the Amalgamated Society of Engineers provided unemployment, sickness, old-age, accident and funeral benefits for their members, while most of the 'new unions' of unskilled workers initially provided only funeral benefits or even no benefits at all. See Boyer (1988) and the Webbs (1897, pp. 152-72).

organised groups of employers, some of which were already settling wages and conditions over wide sections of their industries. These unions were therefore almost inevitably driven into extensive strikes' (Clegg *et al.* 1964, p. 43). Perhaps for this reason, some of the unions in cotton, coal, and iron and steel developed relatively sophisticated systems of collective bargaining. The Webbs (1897, pp. 205-6) concluded that 'only the Cottonspinners, Cotton-weavers, and the Boiler-makers, and, to a lesser extent, the Northumberland and Durham Miners, can be said to be adequately equipped with efficient machinery for Collective Bargaining'[5]. In general, wage rates were fixed at the district or local level between unions and one or a few employers, and sometimes at the regional level (often with local variations) by unions and employers' associations.

To some extent the rise in union membership and militancy put upward pressure on the wage rates of all workers in the industries concerned. But there is reason to think that, especially during 1889-90, there would also be a wage gap between union members and non-members. Collective bargaining was still relatively fragmented, and variations in bargaining power could cause union relative wage effects to emerge at the district, local or even firm level. There is some suggestion that, even where union rates were determined by local agreements, non-unionists often received lower rates (Lawrence, 1899).

While no attempt has been made to estimate the size of the union wage effect in the late nineteenth century, historians have generally agreed that the wage gap between unionized and non-unionized workers was larger in 1889-91 than at any other time before 1910. One reason for workers' success was that employers 'were taken by surprise by the elemental outburst of working-class activism and, finding themselves very poorly organised, were forced to grant numerous concessions' (Cronin 1979, p. 50). Many of the successes of 1889-90, in particular those achieved by the new unions of low-skilled workers were short-lived. Sidney Pollard (1965, p. 110) maintains that unions' 'sudden [wage] gains were difficult, if not impossible, to hold against the inevitable counter-offensive'. The wage gains achieved by unionized low-skilled workers in 1889-90 largely were lost after the sharp decline in the new unionism from 1891 to 1896. Another reason why union/non-union wage differentials could not be maintained was that market forces ensured that unions' wage gains 'were diffused fairly quickly among other workers, so that non-unionised towns or works tended to receive most of the increases or other benefits negotiated by the main unions in their trade' (Pollard 1965, p. 112).

E. H. Phelps Brown (1962) maintains that newly formed trade unions had an 'impact effect' on wage rates:

> When [trade unions] come into action for the first time, they raise the rate of pay relative to other rates.... Until the union is formed, the employer may be able to get the labour he needs at a lower rate than he would in fact be willing to pay without having to offer fewer jobs.... In all such cases a trade union will be able to raise the rate of pay substantially without adverse effect upon the numbers employed—such seem to have been the effects, for example, of the two waves of unionization of the unskilled in Britain, in 1888-91 and 1910-13. (Phelps Brown 1962, pp. 180-1)

Phelps Brown contends that unions were often able to retain this wage gap over time, but were seldom able to increase it. The decline in membership of the new unions after 1890 suggests that they may not have been able to hold the wage gains. However, membership of the older unions of skilled workers did not fall, and thus wage effects may have persisted beyond 1890.

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[5] In the iron and steel industry and in coal, wage rates in some areas were set by slidingscale wage agreements which linked wage rates to the product price. Although rising prices of iron and coal were tending to push up wage rates in 1888-92, several coal miners' unions seized the opportunity to repudiate sliding scales in favour of other forms of collective bargaining. However, beginning in 1893, the sliding scales were replaced by conciliation boards. Treble (1987, p. 681) has shown that the conciliation boards were 'a continuation of sliding scales under another name . . . the major distinguishing feature of the conciliation board was the incorporation of maximum and minimum limits [on wages] in the agreements'.

In sum, because our estimates of the union wage effect in Britain are based on wage data for 1889-90, a time of exceptionally rapid growth of unionism, they are likely to overstate the average effect during the late nineteenth century. The magnitude of the overstatement should be larger for unskilled than for skilled workers, since union membership among the unskilled was significantly higher in 1889-91 than at any other time from 1850 to 1900.

The Commissioner of Labor Survey of 1889/90

In sum, because our estimates of the union wage effect in Britain are based on wage data for 1889-90, a time of exceptionally rapid growth of unionism, they are likely to overstate the average effect during the late nineteenth century. The magnitude of the overstatement should be larger for unskilled than for skilled workers, since union membership among the unskilled was significantly higher in 1889-91 than at any other time from 1850 to 1900.

The survey is made up of male household heads employed in firms that were included in the Commission's study of the costs of production. The investigators attempted to 'secure accounts from a representative number of the employees of the establishments covered . . . and also from those families whose surroundings and conditions made them representative of the whole body of employees in any particular establishment' (US Commissioner of Labor 1890, p. 610). However, the sample is probably not random because, as the Report admits, some workers were unwilling or unable to cooperate, and because workers were chosen from a restricted number of (probably large) firms. Unfortunately, the data do not provide information on each worker's firm.

The survey recorded each workers' occupation. Michael Haines has grouped the occupations into seven skill categories, which we have modified and reduced to four categories[6]. A small minority of these were supervisory workers (category 4) whom we have excluded, leaving a total of 956 workers divided between unskilled (263), semi-skilled (409) and skilled (284).

TABLE 2
CHARACTERISTICS OF LABOUR IN BRITAIN:
THE US COMMISSIONER OF LABOR SURVEY, 1889/90

Industry	Total number of workers (1)	Union members (%) (2)	Percentages of sample			Annual income (£) (6)
			Unskilled (3)	Semi-skilled (4)	Skilled (5)	
Pig-iron	64	21·9	56·2	11·0	32·8	77·2
Bar-iron	109	12·8	34·9	22·0	43·1	90·2
Steel	163	47·2	18·4	23·9	57·7	101·0
Coal	157	57·3	20·4	45·9	33·7	76·7
Coke	15	33·3	60·0	33·3	6·7	66·4
Cotton	303	68·0	24·8	63·4	11·8	76·7
Wool	119	48·7	27·7	54·6	17·7	63·6
Glass	26	50·0	38·5	19·2	42·3	87·4

[6] We are grateful to Michael Haines for providing us with this information. His groups are: skilled, semi-skilled, craftsmen, foremen and overseers, apprentices and helpers, and other and unknown. We reallocated craftsmen to the skilled group and foremen and overseers to the supervisory group. We also reallocated apprentices to the skilled group and helpers to the unskilled group. A number of other adjustments were made, the most important of which were the reclassification of ordinary clerks, miners, weavers and spinners as semi-skilled and of watchmen as unskilled.

The average age of workers in the sample was 39-1 years and almost identical for each skill category. Almost all workers (98-1%) were married (spouse present) and 88-9% had children living at home. Table 2 shows the composition of the set of workers by industry. While the range of industries is relatively narrow, the sample is focused on three of the most important sectors of the late nineteenth-century economy: iron and steel, coal and textiles. We have no indication of the geographical concentration of the sample, but the presumption is that the observations were drawn from the major industrial centres of the north of England.

The average annual income of workers in each industry is given in the final column of Table 2. There is some evidence that the annual income data reported in the survey was computed from weekly or monthly earnings data obtained from the employers' wage books (Lees 1979, p. 170). For the sample as a whole, a worker's average annual income was £80.90. The data indicate that steel, bar-iron and glass were high-wage industries and wool and coke were low-wage industries, but the average income estimates are somewhat misleading because of the differences in skill composition across industries shown in columns (3)-(5). Unskilled workers form the dominant group in pig-iron and coke; semi-skilled workers the dominant group in cotton, wool, and coal; and skilled workers the dominant group in steel, bar-iron and glass.

Union status is not identified directly in the records. We infer a worker's union status from whether the household reported any expenditure on union subscriptions. In a minority of cases where there were other household members working, the union status of the head is ambiguous. In cases where the other family members were very young, were female or had very small earnings, we could be reasonably sure that the household head was a union member. In a minority of cases where genuine ambiguity remained, we assumed that the head was the unionist unless there was evidence to the contrary. In some cases amounts were reported for subscriptions which appear too small to represent full union dues. After examining subscription rates for a range of unions in the late nineteenth century, we decided to exclude as members those reporting less than 8s. 8d. {2d. per week). Excluding the observations for which any ambiguity remained made little difference to the results reported below.

The resulting union densities are displayed in column (2) of Table 2. These indicate that union members are considerably over-represented in the sample as compared with the estimates of the true extent of unionism in Table 1. The one exception is coal, where union density in the sample is close to the true figure. The over-representation of union members in general probably reflects the fact that the firms from which the workers were drawn were relatively large and were located in the industrial heartland of the country.

The Determinants of Union Membership

In this section we present probit estimates of the probability of union membership as a function of various individual characteristics. The determination of union membership is an important issue in its own right but it is also important for the analysis of earnings in the following section. Ordinary least squares estimates of the union wage effect will suffer from selectivity bias if workers have unmeasured characteristics which influence both their earnings and the probability that they become union members. The effects of such characteristics are captured by the random errors in the earnings and union membership equations. If, as seems likely, the errors in the two equations are correlated, the dummy variable representing union status will be correlated with the error in the earnings equation and, thus, ordinary least squares will yield inconsistent estimates of the union wage effect[7]. A widely applied technique for

[7] In a simultaneous-equations system, ordinary least squares would generally yield inconsistent estimates even if the errors in the structural equations are uncorrected. The model presented here (and commonly adopted) is, however, recursive in that union status affects earnings but not conversely. In such a case ordinary least squares yields consistent estimates of the parameters of the earnings equation if the errors are uncorrected. Otherwise, some other procedure is

correcting this selectivity bias is that attributed to Heckman (1979). This involves estimating a probit equation and retrieving from this a term (the so-called 'inverse Mills ratio') which is entered as a regressor in the second-stage earnings to correct for the selectivity bias[8].

The model is described formally by the following two equations:

(1)

$$d_i^* = z_i'\gamma + \varepsilon_i \begin{cases} d_i = 1 & if\ d_i^* < 0 \\ d_i = 0 & if\ d_i^* \le 0 \end{cases}$$

(2)

$$w_i = x_i'\beta + \delta d_i + u_i, \quad \mathrm{i} = 1, \ldots, \mathrm{n}.$$

where *i* indexes the individuals in the sample, w_i, is the natural logarithm of annual earnings, W_i; d_i^* represents an individual's (unobserved) intensity of preference for union membership; *dt* takes the value 1 for a union member, 0 otherwise; x, and z, are vectors of non-random exogenous variables for the ith individual; and u_i, and ε_i, are random variables, normally distributed with zero expectations and constant variances.

We turn first to the model for the probability of union membership (equation (1) above). Models of this sort normally include a set of variables that are regarded as proxies for the costs and benefits of union membership, ease of access to union status and the underlying preferences of the workers involved (see Booth 1986). Our data-set offers some variables that might be associated with choice of union status. We include age, age-squared and skill level based on human capital arguments. To the extent that these variables capture Armor occupation-specific human capital, workers might be more inclined to use 'voice' rather than 'exit' to maintain or improve their lot (Freeman and Medoff 1984, Ch. 5). We also include industry dummy variables to account for differences in the costs of (and possibly returns to) union membership. Apart from age and skill, the only personal characteristics included are whether married, whether children were present, and birthplace. We attempt to capture elements of individual preferences by creating variables from data on expenditure patterns. In particular, we infer tastes for the benefits and protection offered by unions from whether the household made contributions for life insurance and for sickness and death benefits. Other characteristics are reflected in whether the household made religious or charitable contributions or made expenditures on books and newspapers. These variables all take on the values 1 or 0 according to whether or not expenditure was positive[9].

The resulting estimates appear in Table 3. The first equation includes interactions between age and age-squared and the skill levels. This is the equation we use to construct the selection correction variable in the subsequent earnings equations, the extra terms being included on the grounds that they also appear in the earnings equations. Omitting these terms, as in the second equation, makes little difference to the other coefficients and makes the coefficients on

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required to obtain consistent estimates of the parameters. The restrictions required for identification of the parameters of the earnings equation typically take the form of the exclusion of a subset of the determinants of union membership from the earnings equation. Less reliably, identification may be sought from the nonlinearity inherent in the measurement of union status as a discrete variable (either an individual is, or is not, a member), rather than as a continuous indicator of the propensity to become a union member.

8 An alternative, maximum likelihood, procedure would allow the joint estimation of the parameters in both the membership and earnings equations, and thus would provide more efficient estimates than the two-step method used here. The least-squares procedure is, however, less sensitive than maximum likelihood methods to specification errors in the model and it also has the virtue of simplicity of implementation.

9 A referee has pointed out that variables derived from information on expenditure are likely to be affected by the wage received. Such variables are, however, necessary to identify the model's parameters at the second step in the estimation of the selectivity-corrected wage equation unless reliance is made solely on the nonlinearity of the functional form of the probit equation for union membership. In order to mitigate, as far as possible, any simultaneous equations bias, dummy variables are used taking a unit value for cases of positive expenditure in a given category rather than the amount spent.

age and skill easier to interpret. The associated marginal probabilities (evaluated at the sample means) appear next to the coefficients[10]. Overall these equations perform well. Each has a pseudo R^2 of approximately 0.24, and a number of variables are individually significant.

In equation (2) of Table 3, the age profile of union membership gives some hint of an inverted-U (H) shape but it is relatively flat and the individual coefficients on age and age-squared are not significantly different from 0. The coefficients on the skill dummy variables indicate that semi-skilled workers were more likely to be union members than either unskilled workers or, surprisingly, skilled workers. The latter result may be due in part to our classification of spinners, weavers and coalminers as semi-skilled. When the skill dummy variables are interacted with age and age-squared, the age profile of membership is U-shaped for skilled workers but has a *O* shape for the unskilled and semiskilled. However, the individual coefficients are not strongly significant and the results still indicate that, at the mean age, semi-skilled workers were more likely to be unionists than unskilled or skilled workers[11]. The coefficients on the industry variables, with coal the excluded group, indicate that the propensity to become a union member was lowest in bar-iron and highest in cotton textiles. However, these effects are likely to be specific to this data-set since, as we have seen, levels of unionization in the data differ in some cases from industry-wide estimates of union density.

Contributions for life insurance have a significant positive effect on the probability of union membership, while contributions for sickness and death benefits have a significant negative effect. These results seem plausible given the mutual insurance functions of nineteenth-century unions. Despite their name, life insurance policies in fact provided death or burial insurance. While the majority of unions of skilled and semi-skilled workers provided funeral benefits for their members, the benefits typically were small and many workers obtained additional insurance through Industrial Assurance Companies[12]. We interpret the purchase of life insurance as an indicator of a worker's preference for insurance in general. Thus, a worker who purchased life insurance was more likely to join a union in order to take advantage of the union's mutual insurance policies.

Workers could obtain sickness benefits, access to medical care and funeral benefits from Friendly Societies. These added benefits were costly; membership in a Friendly Society cost *6d.* to a shilling a week, as compared with the *1d- 2d.* weekly subscription for a life insurance policy (Johnson 1985, pp. 20, 25, 61). The majority of unions in metals and engineering provided sickness and medical benefits for their members, while mining and textile unions often did not (Boyer 1988, p. 335)[13]. Some members of unions that provided sickness benefits obtained

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[10] The marginal probabilities are estimated as $\phi(z_i'\hat{y})\hat{y}$, where z denotes the sample mean of the explanatory variables and *y* represents the probit estimates of the parameters (see Maddala 1983, p. 23). The asymptotic standard errors of the marginal probabilities, given in parentheses, are computed by linearizing the definition of the marginal probability about its estimated value and then calculating the standard deviation of the resulting linear expression.

[11] At age 40 the marginal probabilities for membership (relative to the unskilled) from the first equation in Table 3 are 0147 for the semi-skilled and 0018 for the skilled. Thus, the very large marginal probability on the dummy variable for the skilled is almost totally offset in the middle of the age range by the coefficients on the interaction terms with age and age-squared.

[12] In 1891 there were 12-83 million Industrial Assurance Companies' paid up policies and 408 Friendly Collection Societies' paid-up policies. According to Paul Johnson (1985, pp. 17, 20), 'multiple insurance was common . . . from around 1914 there were more [life insurance] policies in force than there were resident Britons. This was not a new phenomenon.'

[13] Unions in cotton, coal, and iron and steel typically provided some benefits, but not as many as the major craft unions. For example, the Durham Miners' Association provided generous sickness benefits, out-of-work benefits 'when collieries are idle for a clear week or more in consequence of alteration or breakage of machinery', and funeral benefits. The Amalgamated Association of Operative Cotton Spinners (Bolton District) provided temporary and permanent accident benefits, out-of-work, old-age and funeral benefits, and subscribed to certain hospitals and infirmaries which members could use when sick. On the other hand, the Associated Iron and Steel Workers provided only funeral benefits and 'legal expenses for obtaining compensation' to members injured at work, and the Northumberland Miners' Mutual

additional benefits by joining Friendly Societies. However, because of Friendly Societies' high subscription rates, the practice of obtaining multiple sickness insurance policies was far less prevalent than that of obtaining multiple life insurance policies. The large negative coefficient on the variable for sickness and death benefits supports the hypothesis that Friendly Society policies were viewed as an alternative to union-provided mutual insurance policies.

Among the other variables derived from the information on expenditure patterns, only that for expenditure on books and newspapers has a significant effect on the probability of union membership. The variable's large coefficient suggests that workers who were more literate or more aware of the wider issues were more likely to be union members. It probably reflects the fact that, then as now, trade unions were also political organizations and the decisions to join depended on more than narrow economic criteria.

The other coefficients indicate that those who made religious contributions were marginally less likely, and those who made charitable contributions marginally more likely, to join unions. Marriage and the presence of children both had a small and insignificant effect on the probability of membership. Finally, although the effects are not significant, it appears that those of Scottish and Irish origin were more likely, and those of Welsh origin less likely, to be union members than the English.

Earnings Functions and the Union Wage Effect

The model we use to estimate the union wage effect has been widely applied to modern data-sets. The specification and interpretation of such models is discussed by Lewis (1986). We take as our dependent variable the natural logarithm of annual earnings and regress this on a set of variables representing human capital attributes of the individual worker, his industry and national origin, and the dummy variable for union status. As an alternative, we estimate separate equations for union and non-union members and infer the union wage effect from the predicted values for the two equations.

In the light of (1) and (2) above, we introduce the selectivity-correction variable constructed from the union membership equation to correct for the potential bias in the earnings equation. As noted in the Appendix (equation (A4)), this variable takes the form

(3)

$$S_i = \frac{(d_i - \Phi(z_i'\hat{y}))\phi(z_i'\hat{y})}{1 - \Phi(z_i'\hat{y}))\Phi(z_i'\hat{y})}$$

where Φ and ϕ are, respectively, the cumulative normal distribution function and the standard normal density each evaluated at the probit estimate, *y*, and z, for each individual.

The series for S_i, is then included in (2) as an additional regressor. The Appendix shows thats its coefficient can be interpreted as the covariance between the disturbances in (1) and (2).

In the alternative, 'split-sample', specification, for which there are separate equations for union members and non-members, the S, series for members and non-members are included in their respective equations. The interpretation of the coefficients on these terms is similar to that for the whole-sample specification, namely that they represent the covariances between the disturbances in the earnings equations and the disturbance in the union membership (probit) equation.

The regressions in which the S, variable is omitted are referred to as ordinary least squares estimates, OLS; those for which it is included are referred to as the selectivity-corrected

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Confident Association provided only out-of-work benefits and a very small funeral benefit and made payments to medical charities.

Table 3

Probit Estimates for Union Membership

	Coefficient (1)	Marginal probability (2)	Coefficient (3)	Marginal probability (4)
Constant	−2·206	−0·880	−1·357	−0·541
	(1·143)	(0·446)	(0·742)	(0·296)
Age/10	0·386	0·154	0·025	0·010
	(0·554)	(0·221)	(0·034)	(0·136)
$Age^2/10$	−0·037	−0·010	−0·001	−0·000
	(0·070)	(0·030)	(0·040)	(0·020)
Semi-skilled	0·067	0·027	0·423	0·169
	(1·561)	(0·622)	(0·114)	(0·045)
Skilled	3·457	1·378	0·114	0·045
	(0·079)	(0·675)	(0·127)	(0·051)
(Age × semi-skilled)/10	0·275	0·110		
	(0·788)	(0·314)		
(Age^2 × semi-skilled)/100	−0·045	−0·020		
	(0·100)	(0·040)		
(Age × skilled)/10	−1·554	−0·620		
	(0·825)	(0·329)		
(Age^2 × skilled)/100	0·169	0·070		
	(0·100)	(0·040)		
Industry				
Pig-iron	−0·614	−0·245	−0·616	−0·245
	(0·232)	(0·093)	(0·231)	(0·092)
Bar-iron	−1·382	−0·551	−1·423	−0·567
	(0·206)	(0·082)	(0·205)	(0·082)
Steel	−0·170	−0·068	−0·183	−0·073
	(0·182)	(0·073)	(0·182)	(0·072)
Coke	−0·637	−0·254	−0·614	−0·245
	(0·368)	(0·147)	(0·367)	(0·146)
Cotton	0·375	0·150	0·365	0·146
	(0·148)	(0·059)	(0·147)	(0·059)
Wool	−0·326	0·130	−0·342	−0·136
	(0·165)	(0·066)	(0·164)	(0·065)
Glass	−0·132	−0·052	−0·117	0·080
	(2·83)	(0·113)	(0·280)	(0·111)
Preference indicators				
Life insurance	0·489	0·195	0·481	−0·192
	(0·113)	(0·045)	(0·112)	(0·045)
Religious contributions	−0·136	−0·054	−0·140	−0·056
	(0·109)	(0·044)	(0·108)	(0·430)
Charitable contributions	0·125	0·050	0·132	0·053
	(0·113)	(0·045)	(0·113)	(0·045)
Books/newspapers	0·994	0·396	0·998	0·398
	(0·159)	(0·063)	(0·158)	(0·063)
Sickness/death contributions	−0·371	−0·148	−0·371	−0·148
	(0·104)	(0·042)	(0·103)	(0·041)
Wife present	0·128	0·051	0·101	0·040
	(0·343)	(0·137)	(0·337)	(0·135)
Children present	0·156	0·062	0·162	0·064
	(0·159)	(0·063)	(0·159)	(0·063)
National origin				
Scottish	0·265	0·106	0·255	0·102
	(0·179)	(0·071)	(0·178)	(0·071)
Welsh	−0·347	−0·138	−0·313	−0·125
	(0·202)	(0·081)	(0·200)	(0·080)
Irish	0·213	0·085	0·200	0·080
	(0·275)	(0·110)	(0·275)	(0·109)
log-likelihood	−1058·9		−1065·1	
pseudo R^2	0·243		0·238	
% classified correctly	71·5		71·8	

estimates. Of course, the method of estimation in both cases is that of ordinary least squares, the second with an additional regressor

The variables included in the earnings equation to represent human capital are age and skill level. (We have no direct measures of education or training.) Age is introduced in the usual quadratic form to represent the returns to experience, and skill levels enter as dummy variables. We interact age and age-squared with the skill dummies to allow for different age-earnings profiles across skill levels. To the extent that skills were accumulated on the job, we would expect the age-earnings profiles to be more 'humped' for the higher skill levels. The union status dummy variable is also interacted with skill level in order to examine potential differences in the union wage effect by skill which might be inferred from the literature on the 1890s noted earlier. Finally, we include dummy variables for industry and birthplace but exclude the other variables for types of expenditure and family circumstances which appeared in the membership equation. Although firm-level variables, such as establishment size, might have been important, no information is available for each individual worker's firm[14].

The estimated coefficients appear in Table 4. The two equations for the whole sample give a reasonably good fit, with 40% of the variation in earnings explained. In the first equation the age-earnings profile for the unskilled (the excluded skill group) exhibits a n shape, as expected, with a maximum at age 39.4 years. The age-earnings profile for semi-skilled workers is very similar, but there is some evidence of a more humped profile for skilled workers with a peak at 41 -8 years. The estimated earnings differentials across skill levels also seem plausible. At age 40 the differential for the semi-skilled over the unskilled is 17-4% and for the skilled over the unskilled is 311%. The coefficients on the industry dummies indicate that iron, steel and glass were relatively high-wage industries while wool was the relatively low-wage industry. The effects of age, skill and industry on earnings are similar in the second equation, which includes the selectivity correction. In particular, the skill differentials at age 40 are 14-2% and 31-4%, respectively.

Before considering the union wage effects from these equations, we turn to the separate estimates for unionists and non-unionists. There are significant differences between the two groups; a test for the restrictions implied by the whole-sample selectivity-corrected model against the two separate equations rejects the restrictions[15]. One difference is in the coefficients on age and age squared, which give a flatter age-earnings profile for unskilled non-unionists than for unskilled unionists. This feature is less marked for the skilled and reversed for the semi-skilled. Other differences are in the industry coefficients, which (relative to coal, the excluded group) are larger for unionists than non-unionists in bar-iron and glass and larger for non-unionists in pig-iron.

We turn now to the effect of unions on earnings. Table 5 reports six alternative estimates derived from the regressions in Table 4 together with the estimates from OLS regressions for the split-sample which are not reported in Table 4[16]. For the whole-sample estimates the predicted wage effect is measured by the coefficient on the union membership dummy, while for the split-

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14 Recent research suggests that there may be some increase in the union wage differential with firm size (Stewart 1991). More importantly, the wage rates of all employees tend to rise with firm size, and hence, if large firms were more heavily unionized, this could bias upwards the estimated union wage effect.

15 The x^2 statistic for this restriction with 17 degrees of freedom is 57-7 compared with the critical 5% value of 30-2. We also estimated for unionists and non-unionists separately but without the selectivity variables. This was rejected as a valid restriction of equation (1) in Table 4 with a computed x^2 statistic of 56-3.

16 Each specification of the model was also analysed using a data-set which excluded all observations where there was any ambiguity with respect to union status; in particular, cases for which the recorded union dues might have been attributed to working members of the household other than the head were omitted. The resulting subsample comprised 682 individuals, of whom 203 paid union dues and thus were classified as union members. In these estimations the union wage effects were very close to those presented in Table 4. For example, the comparable OLS coefficients on the three dummy variables representing union membership were approximately 0-20 for unskilled, 016 for semi-skilled and 016 for skilled workers.

TABLE 4

ESTIMATES FOR LOGARITHM OF ANNUAL EARNINGS

	OLS Whole sample (1)	Selectivity corrected estimates Whole sample (2)	Union members (3)	Non-members (4)
Constant	5·094	5·093	5·207	5·267
	(0·196)	(0·211)	(0·286)	(0·267)
Age/10	0·315	0·258	0·368	0·167
	(0·097)	(0·106)	(0·134)	(0·134)
$\text{Age}^2/100$	−0·040	−0·034	−0·047	−0·023
	(0·011)	(0·012)	(0·016)	(0·016)
Semi-skilled	0·142	0·186	0·706	−0·393
	(0·284)	(0·309)	(0·363)	(0·433)
Skilled	−0·345	−0·625	−0·494	−0·748
	(0·320)	(0·354)	(0·430)	(0·475)
(Age × semi-skilled)/10	0·016	−0·027	−0·031	0·265
	(0·144)	(0·156)	(0·018)	(0·219)
(Age^2 × semi-skilled)/100	−0·002	0·004	0·037	−0·032
	(0·017)	(0·019)	(0·022)	(0·026)
(Age × skilled)/10	0·296	0·419	0·365	0·445
	(0·155)	(0·171)	(0·207)	(0·229)
(Age^2 × skilled)/100	−0·033	−0·046	−0·045	−0·045
	(0·018)	(0·020)	(0·024)	(0·027)
Union × unskilled	0·163	0·409		
	(0·035)	(0·070)		
Union × semi-skilled	0·122	0·358		
	(0·028)	(0·065)		
Union × skilled	0·145	0·385		
	(0·032)	(0·068)		
Industry				
Pig-iron	0·069	0·143	−0·071	0·193
	(0·042)	(0·049)	(0·079)	(0·061)
Bar-iron	0·166	0·268	0·532	0·234
	(0·035)	(0·046)	(0·075)	(0·060)
Steel	0·086	0·116	0·084	0·109
	(0·035)	(0·038)	(0·051)	(0·052)
Coke	−0·043	0·014	0·070	−0·054
	(0·072)	(0·079)	(0·104)	(0·100)
Cotton	−0·017	−0·034	−0·001	−0·049
	(0·028)	(0·030)	(0·032)	(0·047)
Wool	−0·216	−0·188	−0·202	−0·190
	(0·033)	(0·037)	(0·041)	(0·054)
Glass	0·110	0·123	0·261	−0·004
	(0·057)	(0·062)	(0·070)	(0·089)
National origin				
Scottish	0·163	0·143	0·265	0·094
	(0·033)	(0·036)	(0·053)	(0·044)
Welsh	−0·138	−0·099	0·038	−0·151
	(0·036)	(0·040)	(0·054)	(0·049)
Irish	−0·094	−0·097	0·035	0·107
	(0·046)	(0·050)	(0·073)	(0·060)
Selection coefficient	—	−0·159	−0·084	−0·210
		(0·039)	(0·049)	(0·055)
R^2	0·395	0·407	0·505	0·351
Residual sum of squares	63·545	62·255	22·155	36·455
Standard error of estimate	0·261	0·258	0·220	0·282

TABLE 5
ESTIMATED UNION WAGE EFFECTS, BY SKILL GROUP

	Skill group*					
	Whole-sample			Split-sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Unskilled	0·163	0·409	0·216	0·200	0·404	0·252
	(0·035)	(0·070)	(0·037)	(0·034)	(0·069)	(0·037)
Semi-skilled	0·122	0·358	0·170	0·130	0·367	0·174
	(0·028)	(0·065)	(0·031)	(0·029)	(0·067)	(0·032)
Skilled	0·145	0·385	0·181	0·158	0·367	0·193
	(0·032)	(0·068)	(0·033)	(0·031)	(0·066)	(0·034)

* (1) and (4): OLS for whole-sample and split-sample, respectively. (2) and (5): selectivity-corrected regression, raw wage effects. (3) and (6): Selectivity-corrected regression, adjusted wage effects.

sample estimates the effect is obtained as the difference between the predicted earnings of the two equations evaluated at common points (the sample means) for the regressors. In the OLS regression for the whole sample, reported in column (1), the estimated coefficient on each of the union dummies is highly significant.

The union wage effect is similar across skill levels: 16- 3% for unskilled workers, 12-2% for semi-skilled workers and 14-5% for skilled workers. A test for the restriction that the union wage effects were the same across skill levels could not be rejected[17]. The estimates from the split-sample OLS regression are given in column (4). They also indicate similar union wage effects across the skill levels with a somewhat larger effect for unskilled workers than for semi-skilled and skilled workers. For each different skill level, the wage effects obtained from the split-sample regressions are slightly larger than those obtained from the whole-sample regression equation.

As discussed in Section II, neglect of the sample-selection effect of trade union membership may lead to biased estimates of the wage effect in the OLS equations which omit the S_i, variable constructed from the probit estimates. An alternative approach is simply to report the coefficient on the union membership dummy in the selectivity-corrected estimates (Lewis 1986, p. 49). However, this ignores the unmeasured characteristics, mentioned earlier, which influence whether a particular individual is, or is not, predicted to be a union member. It amounts to taking a randomly selected non-member and predicting his earnings as a member without also taking into account that, on average, the unmeasured characteristics of members and non-members have different effects on their expected earnings (given a non-zero correlation between the disturbances in the union membership equation and the earnings equation). For this reason, these estimates are referred to as *raw* union wage effects and appear in columns (2) and (5) in Table 5. For the split-sample equations the effect is measured, once again, as the difference in the predictions evaluated at a common point.

A third approach involves using the selectivity-corrected estimates, suitably adjusted to take account of the different effects of the unmeasured characteristics on average earnings for union members and non-members. A formal explanation of the procedure used to derive what we shall refer to as the *adjusted* union wage effects is contained in the Appendix, which also presents

17 The x^2 statistic for this restriction with 2 degrees of freedom was 0-9 for the OLS equation compared with the 5% critical value of 60. For the selectivity-corrected equation discussed below, the test statistic was 1-4.

some alternative ways of estimating the effects. For the experiments considered here, the resulting adjusted wage effects are reported in columns (3) and (6) of Table 5.

The interpretation of the adjusted union wage effect is based on a conceptual experiment in which a hypothetical non-unionist becomes a union member and in which account is taken of the unmeasured characteristics of such an individual represented by the random error in the union membership (probit) equation. Given that what is observed is the discrete indicator of membership or non-membership, the average value of this error, conditional upon union status, is given by the selectivity-correction term, S_i, defined in equation (3) above (see also Appendix equation (A4)). If, as the estimations reveal, the error in the prediction of union membership is correlated with that of the earnings equation, the raw union wage effect fails to reflect the fact that the unmeasured characteristics of unionists are, on average, different from those of non-unionists[18]. The adjusted effect allows for this difference by estimating the values of the selectivity-correction term corresponding to union members and non-members, respectively, and, by isolating the effect of union membership on earnings, separates it from the effect of differences in unmeasured characteristics (both of which are conflated in the raw wage effect). As outlined in the Appendix, a variety of plausible methods for calculating the selectivity correction terms are available. The estimates reported in Table 5 correspond to the evaluation of 5 in equation (3) at the sub-sample means of the conditioning, z, vector for union members and non-members respectively (and separately for each skill group). The most obvious alternative method, of averaging the *Si* terms evaluated at the observed individual z, values for union members and non-members, yields very similar estimates for the union wage effect in each case.

The raw union wage effects are very similar in the two selectivity-corrected models—36%-41% in the single-equation model and 37%-40% in the split sample model. The fact that these wage effects are more than twice as large as the wage effects in the corresponding OLS regressions indicates that the union members in our sample had very different characteristics from those of the non-members. On the other hand, the adjusted union wage effects derived from the selectivity-corrected models are much smaller than the raw wage effects, although they remain somewhat larger than the estimates obtained using OLS. The two selectivity-corrected models again yield similar results—a union wage effect of 21-6%-25-2% for unskilled workers, 170%-17-4% for semi-skilled workers, and 181%-19-3% for skilled workers.

It is of some interest to compare the estimates in column (1) with those in column (3), and also to compare column (4) with column (6). The difference in each case can be interpreted as the bias in estimating the parameters of the model using ordinary least squares (resulting in the estimates reported in columns (1) and (4), respectively) when the selectivity correction is called for that is, when $\sigma_{ue} \neq 0$. Thus, for unskilled workers in the whole-sample case, it appears that use of the OLS estimates results in a bias of approximately —5-3% (= 0.163 — 0.216) in the union wage effect, on the assumption that a selectivity correction is warranted. This is almost exactly the same as the bias, -5-2%, obtained from the split-sample estimates for the unskilled group. The biases are of the same sign and similar magnitude for the other skill groups (4-8% and 3-6% for the semi-skilled and skilled groups, respectively, in the whole sample estimations, and 4-4% and 3-5%, respectively, in the split-sample estimations). It is difficult to determine whether these differences are significant in a statistical sense, for when one set of estimates is appropriate the other set is generated from a mis-specified model. To the extent that the results reported in Table 4 support the selectivity-correction procedure, this is evidence in favour of the estimates presented in columns (3) and (6) of Table 5 compared with columns (1) and (4).

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18 The selection coefficients reported in Table 4 are estimates of the covariance between the errors in the union membership equation and that in the wage equation. That they are significantly different from 0 in two of the three equations supports the computation of adjusted union wage effects.

The results in Table 5 indicate that the size of the union wage effects did not differ significantly across skill levels. We also tested for differences in the union wage effect across industries. We re-estimated the whole-sample models with the skill level-union status interaction terms replaced by variables for the interaction of industry and union status. In the revised regressions, pig-iron, bar-iron and steel were combined into one group, and coal and coke into another. It was not necessary to re-estimate the split-sample models in order to calculate the union wage effects by industry. For the grouped industries, weighted averages of the coefficients for the relevant industry dummy variables were used in the calculation of the union wage effect.

Six separate estimates of the union wage effect for each industry are reported in Table 6. These correspond to the estimates reported in Table 5: precisely analogous procedures have been used to compute the estimates in each case. In the OLS whole-sample and split-sample regressions, underlying columns (1) and (4), the estimated union wage effect is significantly different from 0 for four of the industry groups, and insignificant only for wool. The size of the union wage effect differs sharply across industries, being highest for glass, 34%- 35%, and (apart from wool) lowest for coal and coke, approximately 10%. However, one might not want to put too much weight on the estimated wage effect for glass, as it is based on a very small number of observations. As before, the selectivity-corrected raw union wage effects in columns (2) and (5) are much larger than the wage effects in the corresponding OLS regressions. The estimated raw wage effect for wool is significantly different from 0 and nearly as large, 31%-33%, as the raw wage effect in cotton and coal and coke. The adjusted union wage effects derived from the selectivity-corrected models are much smaller than the raw wage effects, but slightly larger than the OLS estimates. Once again, the estimated wage effect for wool is not significantly different from 0. The lack of a significant union wage effect for wool is consistent with the relative weakness of unionism in wool in the late nineteenth century discussed by Clegg *et al.* (1964, pp. 118, 184, 189).

TABLE 6
ESTIMATED UNION WAGE EFFECTS, BY INDUSTRY GROUP

	Industry group[a]					
	Whole-sample			Split-sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Iron and steel	0·202	0·440	0·259	0·235	0·421	0·286
	(0·034)	(0·069)	(0·037)	(0·033)	(0·068)	(0·037)
Coal and coke	0·103	0·353	0·127	0·103	0·344	0·127
	(0·041)	(0·074)	(0·040)	(0·041)	(0·074)	(0·042)
Cotton	0·125	0·344	0·146	0·121	0·355	0·142
	(0·033)	(0·065)	(0·033)	(0·034)	(0·069)	(0·036)
Wool	0·060	0·326	0·057	0·068	0·312	0·066
	(0·049)	(0·081)	(0·047)	(0·048)	(0·080)	(0·049)
Glass	0·344	0·604	0·363	0·351	0·591	0·369
	(0·105)	(0·121)	(0·102)	(0·103)	(0·123)	(0·106)

[a] (1) and (4): OLS for whole-sample and split-sample, respectively. (2) and (5): selectivity-corrected regression, raw wage effects. (3) and (6): selectivity-corrected regression, adjusted wage effects.

Finally, comparison of column (1) with (3) and of column (4) with (6) reveals a similar pattern of biases as that found for skill groups in Table 5. In the case of iron and steel the bias is —5-7% (= 0-259-0-202) for the whole sample experiment and - 5 1% (= 0-286-0-235) for the split-sample experiment. For the other industries the bias is somewhat smaller in absolute magnitude but, again, almost identical between whole-sample and split-sample experiments. Only for the wool industry is the bias negligible (—0.2% to —0-3%), reflecting the absence of any significant union wage effect for this industry.

Conclusion

This paper offers several estimates of the union wage effect in Britain in 1889-90. Three conclusions stand out from the estimates. (1) In 1889-90 trade unions had a significant positive effect on wages for workers of all skill levels. (2) The magnitude of the union wage effect was similar for the three skill levels. (3) The union wage effect was significant in all of the industries, except wool, for which data are available. The OLS estimates and the adjusted estimates from the selectivity-corrected model, which we prefer to the raw estimates, yield roughly similar union wage effects. Thus, we feel safe in concluding that the overall union wage effect in Britain in 1889-90 was of the order of 15%-20%.

Our results can be compared with estimates of the union wage effect in the United States in the 1890s. In his study of Iowa workers in 1894, Eichengreen (1987, p. 512) found an OLS union wage effect of 18%[19]. Dillon and Gang (1987, p. 523), also using the US Commissioner of Labor survey, obtained OLS wage effects of 20% for unskilled workers, 23% for semi-skilled workers and 29% for skilled workers. These studies suggest that union wage effects were of the same magnitude in the two countries in the 1890s, although perhaps slightly larger in the United States.

The estimates for 1889-90 are somewhat larger than the 8%-12% union wage effect typically found using contemporary microdata. Why was the union wage effect so large, particularly for unskilled workers, in 1889-90? The explanation must lie in the exceptionally rapid growth in trade union membership from 1888 to 1890. Our results, along with the evidence of a large number of successful strikes in 1889-90, support the contention of Phelps Brown (1962) and others that newly organized trade unions often have an initial 'impact effect' on wages, a conclusion that we also reached in our analysis of the wage effect of agricultural labourers' trade unions in late nineteenth-century England (Boyer and Hatton 1994).

Such findings as these must be tempered by the fact that we were unable to control for the characteristics of the firms in which the workers worked, particularly firm size. Although we did not find differences in union wage effects across skill levels, we did find substantial differences across industry groups. These broadly correspond with the views of observers who have commented on the strength of unions in different industries. In addition, firm size and product market structure may help explain the large wage effects in the iron and steel industries and the comparatively modest effects in textiles.

19 Eichengreen also reported estimates of the selectivity-corrected raw union wage effect. Estimating the earnings equation with the selectivity term added yielded a raw union wage effect of 34%, similar to our estimated selectivity-corrected raw union wage effects in Table 5.

Appendix

The model applied in this paper is well known in labour economics (Lee 1978; Heckman 1979; Maddala 1983), although more attention has been paid to the estimation problem than to the ways in which the parameter estimates should be interpreted. This Appendix focuses on an interpretation in terms of the so-called 'treatment effect' for which the paper by Barnow *et al.* (1981) provides an instructive starting point.

The underlying framework can be expressed concisely as follows:

(A1)

$$d_i^* = z_i'\gamma + \varepsilon_i \begin{cases} d_i = 1 & if\ d_i^* < 0 \\ d_i = 0 & if\ d_i^* \leq 0 \end{cases}$$

(A2)

$$w_i = x_i'\beta + \delta d_i + u_i, \quad \mathrm{i} = 1, \dots, \mathrm{n}.$$

where *i* indexes the individuals in the sample, w, is the natural logarithm of annual earnings, *Wi*; *d** represents an individual's (unobserved) intensity of preference for union membership; *d*, takes the value 1 for a union member, d_i otherwise; *xi* and *zi*,
for i= 1,. . . , *n*, are vectors of non-random exogenous variables for the *i*th individual; *u*, and ε_i, are random variables, normally distributed with expectations 0, constant variance and covariance σ_{ue}, β, γ and *S* are parameters to be estimated. The variance of *e* is unidentified and is set to unity, a common assumption in probit estimation. Where no ambiguity is involved, the index, *i*, is omitted.

It is straightforward (Barnow *et al.* 1981, p. 54) to obtain the expectation of *e* conditional upon *d* and z:

(A3)

$$E\left(\varepsilon \mid z, d = 1\right) = \frac{\phi(z'\gamma)}{\Phi(z'\gamma)} \ \ and\ \ E\left(\varepsilon \mid z, d = 1\right) = \frac{\phi(z'\gamma)}{1 - \Phi(z'\gamma)},$$

where Φ (•) denotes the normal distribution function and ϕ (•) denotes the normal density function. More compactly,

(A4)

$$S(z'\gamma, d) \equiv E(\varepsilon \mid z, d) = \frac{\left(d - \Phi(z'\gamma)\right)\phi(z'\gamma)}{\left(1 - \Phi(z'\gamma)\right)\Phi(z'\gamma)}.$$

Now, using the standardization of the variance of e to unity, the bivariate normal distribution of *e* and *u* implies that the expectation of *u* conditional upon z and *d* is given by

(A5)

$$E(u \mid z, d) = \sigma_{ue} E(\varepsilon \mid z, d) = \sigma_{ue} S(z'\gamma, d).$$

This expression suggests a simple amendment of (A2) which effectively purges the random error of *u* of its correlation with *d*, namely to define $v = u - \sigma_{ue} S$ and write the earnings determination equation

(A6)

$$w_i = x_i'\beta + \delta d_i + \sigma_{ue} S(z_i'\gamma, d_i) + v_i, \qquad i = 1, \dots, n$$

The construction of (A6) also suggests a two-step least squares method of estimating the parameters of (Al) and (A2): first, obtain a probit estimate of the parameter vector *y*; second, use a least-squares method to estimate the parameters, β, δ and σ_{ue}, in (A6) where S_i, is constructed by evaluating the expression for *S* at the probit estimate of *y* and the value of z_i, for individual *i*.

(Note that, since the expectation of 5 in (A4) is zero, the sample mean of the constructed values, 5, should be approximately zero. The approximation turns out to be very close for this application.) The parameters of the selection-adjusted earnings equation (A6) are estimated by ordinary least squares. Bearing in mind that the values of *St* are constructed using the probit estimates, asymptotic standard errors for the parameter estimates are computed according to a procedure outlined in Maddala (1983, pp. 252-5).

The common starting-point for different measures of the union wage effect is to envisage taking a randomly selected individual from among the non-unionists and to ask how much the individual would earn in the event of his (or her) becoming a union member. The parameter estimates reflect the fact that the union status of individuals is affected by unmeasured characteristics absorbed in the random error, ε: on the basis of the imperfect information captured by the conditioning variables, z, some individuals will be incorrectly predicted to be unionists or non-unionists according to the value of the random error. If the potential effect of this error on earnings (via a non-zero correlation of *e* with *u)* is ignored, the experiment amounts to treating measured union status as a fixed, non-random, variate which can take on the values of 1 or 0. Alternatively, it is possible to interpret the observed d_i as being the outcome of a random occurrence and, thereby, to allow for the influence of the inherent randomness in the determination of union status on the level of earnings, occasioned by the correlation between ε and u, and reflected in the selectivity correction term, $\sigma_{ue}S$.

More formally, the first experiment can be based solely on the estimate of δ. The second experiment compares the expectations of *w*, conditional upon union membership, *d*, and z:

(A7)

$$E\left(w \mid z,d\right) = x'\beta + \delta d + \mathrm{E}(u \mid z,d) = x'\beta + \delta d + \sigma_{ue}S(z'\gamma,d).$$

Evaluated at a common value of *x*, together with given values of *d* and z, the difference in expected earnings can be written as

(A8)

$$E\left(w \mid z_u, d=1\right) - E\left(w \mid z_n, d=0\right) = \delta - \sigma_{ue}(S_u - S_n).$$

where the subscripts *u* and *n* refer to unionists and non-unionsits respectively, and $Su\text{-}S(z_u' \; y, 1)$, $Sn = S(z'_n \; y, 0)$. Typically, z_u and z_n would be measured by representative values (such as sub-sample means) of the personal characteristics of unionists and nonunionists, although a case can be made for the evaluation to be made at the same point (such as the whole-sample mean) for unionists and non-unionists alike. The issue is whether a typical non-unionist carries his measured personal characteristics, z_n, with him into union membership or whether the personal characteristics should be measured at some common value.

With respect to the evaluation of the *Su* and S_n terms, there are additional options. One obvious contender is simply to take the sub-sample means, for unionists and nonunionists, of the constructed S, values used in the second stage regression. (Clearly, there would be little sense in using the whole-sample mean of S_i, which, as noted, is approximately zero.) Alternatively, it is possible to evaluate the expression for *S* (see (A4)) at *d*= 1 and *d=0* with a common value (such as the whole-sample mean) of z. In this case, $Su — S_n$ reduces to

(A9)

$$S_u - S_n = \frac{\phi(z'\gamma)}{\left(1-\Phi(z'\gamma)\right)\Phi(z'\gamma)},$$

where the value of the *y* vector would be replaced by its probit estimate. Yet another option is to evaluate the terms S_n and *Su* at *d*= 1 and *d=0* together with different values of z, z_u, and z_n (such as the sub-sample means of the observed z variables) for the unionists and non-unionists, respectively. In this case the *Su* and *Sn* terms would be of the form

(A10)

$$S_u = \frac{\phi(z_u'\gamma)}{\Phi(z_u'\gamma)} \quad and \quad S_n = -\frac{\phi(z_n'\gamma)}{1-\Phi(z_n'\gamma)}.$$

When the values of *Su* and *S„* have been obtained, the second-stage estimates of δ and σ_{ue}can be substituted in (A8) to obtain an estimate of the effect of trade union membership.

It is straightforward to generalize the framework to allow for two separate earnings equations for unionists and non-unionists, respectively. This involves replacing (A2) with

(A11)

$$w_{ui} = x_{ui}'\beta_u + u_{ui} \qquad w_{ni} = x_{ni}'\beta_n + u_{ni},$$

where the *u* and *n* subscripts refer to unionists and non-unionists, respectively. The analysis of the more general model is very close to that of the simpler case considered in detail above. (See, e.g. Lee (1978) or Maddala (1983, pp. 223-8), for a detailed explanation of the estimation principles.)

An exactly analogous expression to (A8) can now be constructed for the union wage effect:

(A12)

$$E\left(w \mid z_u, d=1\right) - E\left(w \mid z_n, d=0\right) = x'(\beta_u - \beta_n) + \sigma_{ue}^{u} S_u - \sigma_{ue}^{n} S_n,$$

where σ_{ue}^{u} = cov (u_{ni}, e,) and σ_{ue}^{u}= cov (u_{ni},ε_i). A useful interpretation of this formulation is provided by Dolton and Makepeace (1987). No new issues of principle are raised in the calculation of the union wage effect expressed in (A12). As before, the S_u and S_n terms can be estimated either from their sample values used in the second-stage regression or from the evaluation of (A4) at suitably chosen values of z_i, for union members and non-members, respectively.

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