Abstract
This paper estimates the size of the union membership wage premium by comparing wage outcomes for unionised workers with 'matched' non-unionised workers. The method assumes selection on observables. For this identifying assumption to be plausible, one must be able to control for all characteristics affecting both union status and wages. This requires very informative data. We illustrate the value of the rich data offered by the linked employer-employee Workplace Employee Relations Survey (WERS) 1998 in implementing this methodology. We estimate the union membership premium for the whole private sector, among workers in workplaces where at least some workers are covered by collective bargaining, and in occupations with pay set by collective bargaining. We find a raw 17-25% union premium in gross hourly wages for the private sector in Britain, depending on the sub-group used. However, post-matching this difference falls to between 3% and 6%. This indicates that the higher pay of unionised workers is largely accounted for by their better underlying earnings capacity, which is associated with their individual characteristics, the jobs they do and the workplaces they find themselves in.

Key words: trade unions, wage premium, treatment effect, matching, propensity score
JEL classification: C14, C81, J31, J51

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Alex Bryson is a member of the Policy Studies Institute, London and a Research Associate at the Centre for Economic Performance, London School of Economics.
Contact: a.bryson@psi.org.uk

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The Union Membership Wage Premium: An Analysis Using Propensity Score Matching

Alex Bryson

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1. Introduction

This paper addresses the question: how much of the wage differential between union members and non-members is attributable to union membership, and how much is due to differences in personal, job and workplace characteristics across members and non-members? The question is prompted by two recent developments, one substantive, and one methodological.

The substantive development is the apparent decline in the union membership premium in Britain in the 1990s. Studies for the United States and Britain have traditionally found union members' earnings to be 10-20% higher than non-members'. However, Machin's (2001) analysis of longitudinal data from the British Household Panel Survey (BHPS), indicates that, although there was a wage gain for people moving into union jobs in the early 1990s, this had disappeared by the late 1990s. The findings complement workplace-level analyses which indicate that, on average, there was no union wage premium in the private sector by 1998 arising from workplace bargaining coverage (Forth and Millward, 2000a) and that union-bargained pay settlements in the period 1997-98 were no higher than settlements which did not involve unions (Forth and Millward, 2000b). Our analysis contributes to the body of knowledge about the size of the union membership premium by the late 1990s.

The methodological development is the advent of new data and relatively new estimation techniques permitting a fresh look at the nature of the union wage premium. The membership differential is often attributed to the rent-seeking behaviour of unions who, through negotiation with employers, are able to procure a wage premium for their members. However, studies also find a membership premium even among workers whose pay is set through collective bargaining ('covered workers'). In explaining this phenomenon, some have argued that employers may conspire to pay lower wages to covered non-members than to members in return for union co-operation, since this may increase the size of the surplus to be shared between workers and the firm (*eg.* Blakemore *et al.*, 1986). However, even if this sort of collusion occurs in some cases, it seems unlikely that this could account for the size of membership differentials identified in the literature. Since there appear to be no obvious mechanisms by which members should command higher wages than non-members other than coverage, the membership premium may be accounted for by unobserved differences between members and non-members which boost members' relative earnings. Biases in estimates of the union membership premium may be accounted for by data deficiencies and, in particular, the paucity of employer controls in the household and employee data sets often used to generate them. For instance, the membership premium among covered workers could be explained if the union differential is positively correlated with union density since the conditional probability of high density given membership is higher than that given coverage. We address this deficiency in employer controls with linked employer-employee data in the Workplace Employee Relations Survey 1998 (WERS). As well as information on individual employees' union membership, WERS contains rich information on the employer, including pay bargaining arrangements at workplace and occupational level.

A second possible source of bias in the estimates of union membership effects on wages is the potential endogeneity of union status if membership is governed by a selection process. Following Farber (2001), there are two possible selection processes. The first is 'worker choice' in which workers only choose membership if the union wage is greater than the wage available to the individual outside the union. It is often assumed that workers with a lower underlying earning capacity have more to gain from membership than higher quality workers, in which case this selection process will understate the union wage premium. The second selection process arises through 'queuing' since not all workers desiring union employment can find union jobs (see Bryson and Gomez, 2002 for empirical validation of this model in Britain). Under this model, union employers may choose the best of the workers among those desirous of a union job. This employer selection implies a positive bias in the union premium but, a priori, it is not clear whether this bias is greater or less than the negative bias implied by worker selection. Either way, if there is endogenous selection the membership mark up estimated using standard cross-sectional regression techniques 'can be interpreted as the average difference in wages between union and non-union workers, but it can not be interpreted as the effect of union membership on the wage of a particular worker' (Farber, 2001: 11). Causal inference is problematic because, where workers who become members differ systematically from those who do not become members in ways which might affect their earnings independent of membership, we can not infer the non-union wage for union members simply by comparing union members' wages with those of non-members. In the literature for the United States, the problem of selection bias is usually tackled by modelling union status determination simultaneously with earnings and estimating an econometric model that takes account of the simultaneity. This usually involves a Heckman estimator where the earnings function and union status determination function are assumed to have errors that are jointly normal. This technique relies on untestable exclusion restrictions whereby variables assumed to affect union status have no direct effect on earnings. In his review of the literature, Lewis (1986) concluded that, because of these arbitrary functional form assumptions and untestable exclusion restrictions, results from these studies were unreliable. Until recently, the selection problem was usually ignored in the British literature (Andrews *et al.*, 1998, review this literature). Using panel data for the first half of the 1990s, Hildreth (1999) accounts for selection into union membership using fixed effects estimation and finds a large but declining membership premium among covered workers. However, Booth and Bryan (2001), using the richer employer controls available in WERS, also account for endogenous selection into membership and find no membership premium in 1998.[1]

Our analysis takes a different approach. We use a semi-parametric statistical matching approach known as propensity score matching (Heckman *et al.*, 1999) to compare wage outcomes for unionised workers with 'matched' non-unionised workers to infer the causal effect of union membership on wages. The method avoids the need for functional form assumptions and reliance on exclusion restrictions. However, as with all non-experimental estimators, causal inference relies on an untestable assumption. In this case, the assumption is that the selection process is captured with observable data. For this key identifying assumption to be plausible, one must be able to control for all characteristics affecting both union status and wages. This requires very informative data. We illustrate the value of the rich data offered by the linked employer-employee Workplace Employee Relations Survey (WERS) 1998 in implementing this methodology.

The remainder of this paper is set out as follows. Section 2 introduces the propensity score matching method. Section 3 describes our data. Section 4 describes the empirical implementation of matching. Section 5 presents results and Section 6 concludes.

2. Causal Inference Through Statistical Matching

To establish whether the union membership wage premium is due to membership, or is due to systematic differences in personal, job and workplace characteristics across members and non-members, we need to isolate the causal effect of union membership on wages. Let us

[1] They use two methods to tackle endogenous selection. The first method involves substituting a predicted probability of union membership for actual membership status in the wage equation. The second method takes the generalised residuals from a probit estimation of union membership and adds them to the wage equation, along with some instruments that are hypothesised to affect membership but not the unobserved determinants of wages.

conceive of union membership as if it were a 'treatment' that the individual receives. We wish to evaluate the causal effect of this treatment (treatment *1*) relative to non-membership (treatment *0*) on an outcome variable, *Y*, gross earnings. Let Y_1 be earnings if the individual received treatment 1 (that is, where the individual is a union member) and Y_0 be the earnings that would result if the same individual received treatment 0 (non-membership). Let us denote the binary indicator of the treatment actually received as $D \in \{0,1\}$, while *X* is a set of attributes which are not affected by the treatment (demographic, job and workplace-related).

The effect of treatment 1 on individual *i* as measured by *Y* and relative to treatment 0 is:

(1) $? = Y_{1i} - Y_{0i}$

which is simply the difference between the individual's potential outcome if 'exposed' to membership and the individual's potential outcome from non-membership. To estimate the impact of membership on members' earnings, it is necessary to know what the outcome would have been if the individual had *not* been a member. The problem is that we can not observe the counterfactual, namely the outcome which would have resulted if an individual had made an alternative choice (that is, if members had chosen non-membership, and *vice versa*). Either Y_{1i} or Y_{0i} is missing for each i. Thus our problem is one of estimating missing data. This counterfactual cannot be inferred directly from the outcomes of non-members since they are likely to differ substantially in their characteristics from members.

To overcome this selection problem, researchers must choose from a range of evaluation methods, the choice being determined by a number of factors including the richness of the data and the nature of the treatment. Because it is impossible to observe the individual treatment effect, each method relies on generally untestable assumptions to make causal inferences (Holland, 1986). In order to identify individual treatment effects, it is necessary to make very strong assumptions about the joint distribution of Y_{1i} and Y_{0i}. However, the *average* treatment effect at the population or sub-population level can be identified under generally less stringent assumptions, some of which are set out below. Among the parameters that only depend on the marginal distributions of Y_{1i} and Y_{0i} is the parameter most commonly estimated and the one estimated in this paper, namely the mean impact of treatment on the treated:

(2) $q = E(Y_1 – Y_0 \mid D = 1, X)$

$$= E(Y_1 \mid D = 1, X) - E(Y_0 \mid D = 1, X)$$

where D=*1* denotes treatment (membership), D=*0* denotes non-treatment (non-membership) and X is a set of conditioning variables. In assessing the expected treatment effect for individuals who are union members, we are addressing the question of how members' earnings compare with what they would have received had they not been members, on average.[2]

For members we observe Y_1 so that the average observed outcome for participants is an unbiased estimate of the first component of the effect of treatment on the treated $E(Y_1 \mid D = 1, X)$. The evaluation problem arises from the term $E(Y_0 \mid D = 1, X)$. This is the mean of the counterfactual which, since it is unobservable, must be identified and estimated on the basis of some usually untestable identifying assumptions justifying the use of the observable pairs $(Y_1 , D = 1) , (Y_0 , D = 0)$.

As noted above, members may not be a random sample of all employees. If there are systematic differences in characteristics across members and non-members that are likely to influence earnings, failure to take account of these will bias any estimate of the union membership effect on earnings. Thus, $E(Y_1 \mid D = 1) - E(Y_0 \mid D = 0)$ would in general be biased for the effect of treatment on the treated. An exception is when the independence assumption Y_0 ? D can be invoked. This is credible where the random assignment of individuals to treatment ensures that potential outcomes are independent of treatment status. In this situation, $E(Y_0 \mid D = 1) = E(Y_0 \mid D = 0) = E(Y \mid D = 0)$ so that the treatment effect can be consistently estimated by the difference between the observed mean of the outcome variable for the treatment group and the observed mean for the non-treatment group.

In the absence of random assignment, one option is to construct a comparison group based on statistical matching. Matching estimators try to resemble an experiment by choosing a comparison group from all non-participants such that the selected group is as similar as possible to the treatment group in observable characteristics. Matching can yield unbiased estimates of the treatment impact where differences between individuals affecting the outcome of interest are captured in their observed attributes. This assumption, which is

[2] To obtain the average treatment effect on the non-treated $E(Y_1 - Y_0 \mid D = 0)$ the procedure is applied symmetrically. The average treatment effect $E(Y_1 - Y_0)$ is a weighted average of the treatment effects for the treated and non-treated.

often referred to as the Conditional Independence Assumption (CIA), is the key identifying assumption underpinning the matching methodology. The precise form of the CIA depends on the parameter being estimated. For the treatment on the treated parameter, the CIA requires that, conditional on observable characteristics, potential non-treatment outcomes are independent of treatment participation. Formally,

(3) $$E(Y_0 \mid X, D = 1) = E(Y_0 \mid X, D = 0)$$

Thus, CIA requires that the chosen group of matched controls does not differ from the group of treated by any variable which is systematically linked to the non-participation outcome Y_0, other than on those variables that are used to match them. This permits the use of the matched non-participants to measure how participants would have fared, on average, had they not participated.

The plausibility of the CIA depends on the informational richness of the data since the set of X's should contain all the variables thought to influence *both* participation (that is, membership) *and* the outcome (earnings) in the absence of participation. We discuss how likely it is that the CIA is met in this analysis in Sections 4 and 5.

Under CIA,

(4) $$E(Y_1 \mid D = 1) - E(Y_0 \mid D = 1)$$

$$= E_{x|D=1}\{E(Y|X, D = 1) - E(Y|X,D = 0)\}$$

Hence, after adjusting for observable differences, the mean of the no-treatment (potential) outcome is the same for those receiving treatment as for those not receiving treatment. This allows non-participants' outcomes to be used to infer participants' counterfactual outcomes. However, this is only valid if there are non-participants for all participants' values of X (this is known as the support condition):

(5) $$0 < Pr(D = 1 \mid X) < 1$$

This ensures that all treated individuals have a counterpart in the non-treated population for each X for which we seek to make a comparison. If there are regions where the support of X does not overlap for the treated and non-treated groups, matching can only be performed, and the treatment parameter, q, retrieved, over the common support region. If treated individuals have no support in the non-treated population, they are dropped from analysis and the estimated treatment effect is redefined as the mean treatment effect for those treated falling within the common support.

Matching operates by constructing, for those participants with support, a counterfactual from the non-participants. There are a number of ways of defining this counterfactual.[3] Once the counterfactuals are identified, the mean impact of the programme can be estimated as the mean difference in the outcomes of the matched pairs.

A refinement to the matching approach was introduced by Rosenbaum and Rubin (1983). If the CIA is met and there is common support then:

(6) Y_0 ? $D \mid P(X)$ for X in $\mathbf{X}$

where $P(X)$ is the propensity score, the conditional probability of participating in the programme – in our case, the probability of being a union member – given a vector of observed characteristics X.[4] Formally,

(7) $P(X_i) = Pr(D_i = 1 \mid X_i)$

Rosenbaum and Rubin show treatment and the observed covariates are conditionally independent given the propensity score, that is:

(8) D_i ? $X_i \mid P(X_i)$

The advantage of Rosenbaum and Rubin's innovation is that the dimensionality of the match can be reduced to one. Rather than matching on a vector of characteristics, it is possible to match on just the propensity score. This is because, as Rosenbaum and Rubin show, by definition treatment and non-treatment observations with the same value of the propensity score have the same distribution of the full vector of regressors X. Having matched on the propensity score, the mean impact of the programme is estimated as the mean difference in the outcomes of the matched pairs.

[3] See, for example, Heckman *et al*., (1997) for a comparison of alternative matching schemes.
[4] $P(X)$ is shorthand notation for $P(D=1|X)$.

If the CIA is satisfied, matching offers an attractive means of identifying the impact of union membership on earnings. The main attraction is that it is non-parametric, avoiding the need to define a specific form for the outcome equation, selection process or unobservables in either equation. In addition, it avoids extrapolation beyond the common support which occurs with simple linear estimators. Heterogeneous treatment effects are allowed for, so no assumption of constant additive treatment effects for different individuals is required. Effects for sub-groups can be estimated by running the match on sub-populations (see Section 5.1). Matching estimators also highlight the problem of common support and thus the short-comings of parametric techniques which involve extrapolating outside the common support. Matching is thus able to eliminate two of the three sources of estimation bias identified by Heckman, Ichimura, Smith and Todd (1998): the bias due to difference in the supports of X in the treated and control groups (failure of the common support condition) and the bias due to the difference between the two groups in the distribution of X over its common support. The other source of bias is the one due to selection on unobservables. This highlights the importance of the CIA since, if this holds, selection on unobservables ceases to be a problem. The appropriateness of the CIA is dependent on the richness of the available data.

3. Data

We use the linked employer-employee data from the Workplace Employee Relations Survey 1998 (WERS). WERS is a nationally representative survey of workplaces with 10 or more employees covering all sectors of the economy except agriculture (Airey *et al*, 1999).

We use two elements of the survey. The first is the management interview, conducted face-to-face with the most senior workplace manager responsible for employee relations. Interviews were conducted in 2,191 workplaces between October 1997 and June 1999, with a response rate of 80%. The second element is the survey of employees where a management interview was obtained. Self-completion questionnaires were distributed to a simple random sample of 25 employees (or all employees in workplaces with 10-24 employees) in the 1,880 cases where management permitted it. Of the 44,283 questionnaires distributed, 28,237 (64%) usable ones were returned.

The sample of workplaces is a stratified random sample with over-representation of larger workplaces and some industries (Airey *et al.*, 1999). Employees' probability of selection for the survey is a product of the probability of their workplace being selected and the probability of the employee's own selection. To extrapolate from our analyses to the population from which the employees were drawn (namely employees in Britain in workplaces with 10 or more employees) we weight the analysis using the employee weights.[5]

Our estimating sub-sample is all private sector employees with complete information on the variables used in the analysis. By estimating the union membership premium for the whole private sector, we obtain an average return to membership, irrespective of whether the individual – member or non-member – is covered by collective bargaining. This sample contains 10,694 non-members and 4,323 members.[6]

We also exploit the bargaining coverage information in WERS based on management classifications of the way pay is set for each occupational group in the workplace. The eight possible responses include collective bargaining at industry, organisation or workplace-level; from these, we identify employees at workplaces where there is collective bargaining for any occupational group at any level. Estimates of the membership premium for covered workers defined in this way are based on 2,489 non-members and 3,352 members. We use the same information to identify employees whose own occupational group is covered by collective bargaining at any level. This sample comprises 1,605 non-members and 2,765 members.

Using the survey weights to obtain population estimates, 29% of employees in the private sector are union members, a similar percentage (28%) belong to a covered occupation, but 37% are located in a workplace where there is some collective bargaining. However, only 59% of those employed in covered workplaces are members, and only 65% of those in covered occupations are members, indicating that between 35% and 41% of covered workers are free-riders.

[5] The weighting scheme used in this paper compensates for sample non-response bias which was detected in the employee survey (Airey *et al*., 1999: 91-92).
[6] Membership is derived from individual employees' response to the question: 'Are you a member of a trade union or staff association?'

Table 1: Bargaining Coverage Among Union Members and Non-Members in the Private Sector

	Member	Non-member
Workplace covered?		
Yes	.78	.21
No	.22	.79
Occupation covered?		
Yes	.64	.13
No	.36	.87

Note: these percentages are based on 4,323 members and 10,694 non-members in our estimation sample for the whole private sector

Table 1 sets out more clearly the strong but imperfect correlation between membership and coverage. If we focus on workplace coverage first, we see that four-fifths of members are covered, compared with only one-fifth of non-members. However, workplace coverage is a fairly loose definition of coverage since one-quarter of those in covered workplaces belong to occupations which are not themselves covered. Using the stricter definition of coverage, namely occupational coverage, we find that only about two-thirds of members are actually covered, as are 13% of non-members.

Since we expect any membership premium to be generated by wage bargaining, we anticipate that the premium should be smallest where the sub-sample consists solely of workers in covered occupations since, in general, all these workers should benefit from pay bargaining, unless employers discriminate between members and non-members, or members are located in covered workplaces where unions have greater bargaining power. We test for this second possibility with sub-group analyses. *A priori*, it is not certain whether the membership premium should be larger in the whole private sector or within the covered workplace sample. This is because, although the probability of occupational-level coverage is higher for members in covered workplaces than it is in the whole private sector (81% against 64%), this is also true for non-members (62% against 13%). Indeed, the 'coverage gap' for members versus non-members is much larger in the whole private sector than it is among covered workplaces (51 percentage points against 19 percentage points). This might imply a higher premium in the whole private sector, at least in the raw data.

3.1 The dependent variable

Our dependent variable is log gross hourly wages. Although the employee questionnaire contains continuous hours data, it only contains banded weekly earnings data, so that we only know the lower and upper bounds for each individual's wage. Furthermore, the data are top-coded so that we only have a lower bound for the highest earners. Therefore, we generate a predicted hourly wage for each individual using interval regression, a generalisation of the tobit model for censored data, initially developed by Stewart (1983) for banded earnings data.[7] This estimation was undertaken for each of the three samples (whole private sector, employees in covered workplaces, and employees in covered occupations). The estimation for the whole private sector is presented in Appendix Table A1. The purpose of this model is to generate an accurate estimate of individuals' actual earnings, which is why union membership and union recognition enter the equation. As a measure of fit we use the percentage of employees correctly classified within their original gross weekly earnings bands once the predicted wage is multiplied by their total hours. We find that in 36% of cases the band is correctly predicted[8] and, in 75% of cases, the prediction is exact or within one earnings band.

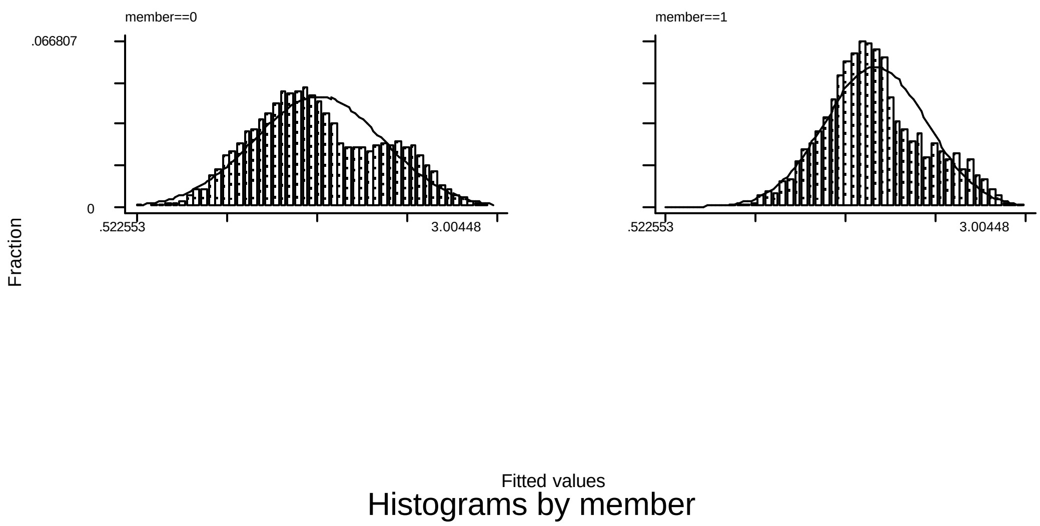
Graph 1 shows the wage distribution is much more peaked for members than non-members, as one would expect given unions' propensity to compress the wage distribution (Metcalf *et al*, 2001). The membership premium, measured by the difference in average log hourly wages between members and non-members, is 24% (exp(1.92-1.70)).

Union membership wage differentials are usually higher when measured in terms of hourly earnings than when measured in weekly earnings because union workers work fewer hours per week than non-union workers, on average (Andrews *et al.*, 1998). This proves not to be the case in WERS: mean hours worked per week are 39.4 for members and 35.4 for non-members. This is because the incidence of part-time working is much higher among non-members: part-timers were usually excluded in previous analyses (Stewart, 1983; Green, 1988; Blackaby *et al.*, 1991; Andrews *et al.*, 1998). We investigated this issue further by estimating the independent effect of membership and union recognition on log hours worked per week with a model identical to that used to estimate log hourly wages but without the

[7] We use the SVYINTREG procedure in STATA 7 which is a robust estimation procedure which makes allowance for the complex sample structure (clustering, stratification and sample weighting) when calculating point estimates and standard errors. See Forth and Millward (2000a, Appendix B) for the log likelihood function and details of the estimation methodology.
[8] This is similar to Forth and Millward (2000a: 36).

continuous hours variable. The model accounts for a substantial amount of the variance in hours (r-squared = 0.51). The membership effect is positive but not significant (0.38, t = 0.84) and workplace union recognition has a significant negative effect (-1.53, t = 3.17). So, although we observe an hours differential in the data, it is not attributable to membership per se, and suggests that measurement of the membership differential should vary little whether measured in weekly or hourly wages. This is confirmed when we estimate a regression-adjusted membership premium using the interval regression technique on banded weekly earnings. The differential is 22%, compared to the 24% for hourly earnings.

Graph 1: Predicted Log Hourly Wages for Members and Non-Members in the Private Sector



4. Empirical Implementation of Matching

As noted above, the regression-adjusted estimate of the membership wage premium in the private sector is 24%. However, for reasons discussed in Section 2, these estimates involve extrapolating beyond the common support and can not be interpreted as the causal effect of membership on wages. Here we describe the empirical implementation of propensity score matching in WERS to yield an unbiased estimate of membership's effects on the wages of union members.

Since the propensity to be a union member is unknown, the first task in matching is to estimate the propensity to be a union member. We do this with a probit estimating a (0,1) variable identifying individuals' union membership status. The estimation accounts for the complex sample design, that is, sampling weights, clustering and stratification. The CIA requires that all variables influencing membership and wages should be included in the estimate.[9] Our choice of variables is informed by previous empirical work (Bryson and Gomez, 2002) and the theory underpinning the worker choice and queuing models of membership. Variables entering the model are demographics (age, gender, marital status, health, ethnicity, qualifications), job-related (occupation, nature of contract, hours worked, gender segregation), workforce composition (by age, gender, occupation, hours worked), workplace (size, activity, industry, ownership, location) and local labour market conditions. Although our linked employer-employee data provide much of the requisite information, it is arguable that we are missing some data. For example, we have no data on motivation which, it has been argued, is positively correlated with membership and the desire to invest in workplace-specific human capital, thus raising wages (Budd and Na, 2000). Our data set does contain workplace tenure and the amount of employer-provided training undertaken, both of which may be correlated with this tendency. However, because these variables may be influenced by membership itself, and are thus endogenous with respect to membership, their incorporation in the estimation of the propensity score could undermine the interpretability of estimated effects (Heckman, LaLonde and Smith, 1999). They are therefore excluded from our estimates. Although the absence of data on motivation may violate the conditional independence assumption, the absence of workplace tenure and employer-provided training would only bias our estimates if they influenced both membership and wages. It is certainly the case that longer workplace tenure is independently associated with higher earnings (Appendix Table 1) and an increased likelihood of union membership.[10] But the empirical literature suggests that membership increases tenure by reducing the likelihood of voluntary quits.[11] There is little reason to believe that longer tenure might lead to membership since, unlike the United States (Budd and Na, 2000) there are no institutional factors that increase the likelihood of joining the union after the end of a probationary period. So, in the British context, workplace tenure is best thought of as a

[9] Variables that affect neither membership nor wages are clearly irrelevant. If a variable influences membership but not wages, there is no need to control for differences between members and non-members because wages are unaffected. Conversely, if a variable influences only wages, there is no need to control for it since it will not be significantly different between members and their matched comparators. This just leaves variables that affect membership and wages.

[10] Two-thirds (66%) of members had been working at the workplace for at least five years, compared with 37% of non-members. Conversely, 39% of non-members had been at their workplace for under two years compared with only 7% of members. Regression analysis revealed an independent association between membership and tenure (results available from the author).

mediating variable between membership and wages. As such, it can be omitted without biasing estimates. Similarly, it is hard to see how employee take-up of training can influence union membership – unless, that is, non-members are discriminated against by employers or unions who ensure privileged access for members, whereupon poorly trained non-members may have an incentive to join the union. In fact, the distribution of days spent in employer-provided training was nearly identical across members and non-members and the variable is not statistically significant in models estimating union membership status.[12] So, in spite of the independent effect it had on employees' earnings (see Appendix Table 1), there is little empirical or theoretical justification for its inclusion in the propensity score estimation.

The model estimating the probability of union membership for the whole private sector is presented in Appendix Table A2.[13] Among non-members, the predicted probability of union membership ranges from .00001 to .9768508, with a mean of .17 and a median of .11. Among members, the predicted probability ranges from .0004702 to .9910685, with a mean of .48 and median of .47. Thus, the zone in which there is no common support given by non-members is above .9768508: enforcing common support at the extremes results in the loss of only 5 of the 4,240 members.[14] Thus the sub-group of members for whom we are unable to estimate the membership premium is very small. The resultant distributions of propensity scores are presented in Graph 2 which shows that, although non-members' scores are bunched in the lower quartile of the distribution, they nevertheless offer support for members throughout the distribution.

As noted in Section 2, matching operates by constructing, for those participants with support, a counterfactual from the non-participants. There are a number of ways of defining this counterfactual using the propensity score. We use nearest neighbour: of the available matching methods, nearest neighbour matching produces estimates with least bias, but at the cost of the highest variance (because only part of the comparison sample is utilised). The procedure involves taking each treated individual (member) and identifying the non-treated individual (non-member) with the most similar propensity score. The matches were made

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[11] These findings are consistent with the theory that unions provide a 'voice' alternative to quitting for dissatisfied workers (Freeman and Medoff, 1984).

[12] Our measure of employer provided training is employees' responses to the question: 'During the last 12 months, how much training have you had, either paid for or organised by your employer? Include only training away from your normal place of work, but it could be on or off the premises.'

[13] The equivalent models for the covered workplace and covered occupation sub-samples are available from the author. In what follows, we illustrate the matching process with detail from the estimates for the whole private sector. Identical information for all the matching undertaken is provided in the notes reporting results in Tables 2-4.

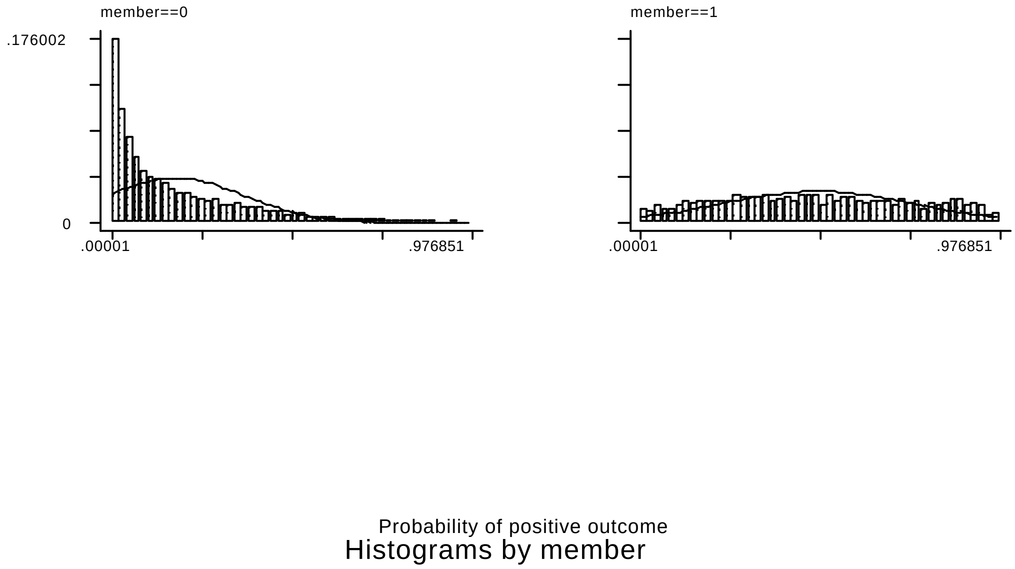
[14] A further 226 of the 14,932 employees are dropped because they have no valid predicted log hourly wage.

with replacement so that, in some cases, a non-treated individual provides the closest match for a number of treated individuals, whereupon they feature in the comparison group more than once.[15] We seek to ensure the quality of our matches by setting a tolerance when comparing propensity scores. We do so by imposing a 0.002 caliper: where the propensity score of a treated individual falls beyond this bound for a near comparator, the treated individual remains unmatched. This second means of enforcing common support results in the discarding of a further 150 members from our analysis. So, in total, 155 members are lost through the enforcement of common support, that is, 3.7% of all members. The advantage of nearest neighbour matching is that the match is as good as it is possible to achieve in the sense that the bias across the treatment and comparison groups is minimised. In our case, the matches are very close: the mean difference in propensity scores between treated individuals and their matched comparators is .0001934, and ranges between 0 and .0019938. On the other hand, nearest neighbour matching disregards potentially useful information by not considering any matches of slightly poorer quality. Over-reliance on a reduced number of observations can result in effects being less precisely identified. Of the 10,551 non-members who could potentially have been matched to our 4,085 members, 2,235 were used as matched comparators. In nearly two-thirds of cases (65.5%) these matched comparators have a match weight of 1 because they are matched to a single treated case. The largest weight is 19, and in only 25 cases is a non-member used as a match for 10 or more members. The mean match weight for non-members is 1.83.

To be effective, matching should balance characteristics across the treatment and comparison groups. Appendix Table 3 presents comparisons of the means in the characteristics used to match members and non-members, as well as a measure of the 'distance' of the marginal distributions of relevant characteristics in both groups (Rosenbaum and Rubin, 1985). For a given covariate, the standardised difference after matching is defined as the difference of the sample means in the treated and matched non-treated subsamples as a percentage of the square root of the average of the sample variances in the treated and non-treated groups (Sianesi, 2001). Overall, the quality of the match seems good, the mean absolute standardised bias for all covariates being –0.29. Standardised bias for each variable tends to range from –6% to +6%, and only three times does it exceed 10%.[16]

[15] Dehijia and Wahba (1998) find that allowing the non-treated to be used more than once as comparators improves the performance of the match.

Graph 2: Predicted Union Membership Probability for Members and Non-Members in the Private Sector



5. Results

Results are presented in Tables 2, 3 and 4 for the whole private sector, those in covered workplaces and those in covered occupations respectively. Our estimates of mean wages are for the population from which our sample was drawn, taking account of the complex survey design when comparing mean differences across members and matched non-members.[17]

Beginning with results for the whole private sector, Table 2 shows a membership wage premium of 24% pre-matching. Post-matching, the union membership differential falls to a statistically insignificant 3.5%.[18] This tells us that, in the private sector as a whole, observable differences in personal, job and workplace characteristics between members and their matched counterparts can account for nearly all of the raw membership differential.

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[16] Although achieving a reasonable balance on the *X*'s entering the participation equation is an indicator of how good the match is on observables, it cannot provide an indication as to whether the CIA is plausible.

[17] In these population estimates, the survey sample weight of each treatment group member is applied to the corresponding matched comparator(s) (Frölich *et al*., 2001: 12). Hence, population estimates of the union membership differential are based on a weight incorporating both the matching weight and sampling weight. Population differences in mean earnings between members and their non-member comparators also account for variance arising from sample stratification and clustering.

[18] A complication that arises in the case of matching is that the estimation of the propensity score and the matching itself both add variation beyond normal sampling variation (Heckman, Ichimura and Todd, 1998). We ran bootstrap estimates to account for this in the calculation of standard errors. The bootstrap estimates for the post-match differential based on 200 repetitions are contained in footnote (3) to the table.

Table 2: Mean Differences In Log Earnings (%) Between Members and Non-Members Pre- and Post-Matching, Whole Private Sector

	Pre-match	Post-match
Member log wage	1.919	1.917
	N=4,235	N=4,085
Non-member log wage	1.704	1.883
	N=10,551	N=2,235
% differential	24.0	3.5
Significance	F(1,1043)=110.75 Prob>F=0.0000	F(1,865)=1.68 Prob>F=0.1954

Notes: (1) Propensity scores derived from probit estimation accounting for complex survey design (that is, sampling weights, clustering and stratification). (2) % differential is exp(mean log wage of members – mean log wage of non-members). (3) The 95% confidence interval for the post-match bootstrapped estimates is 7.1% to -0.01%. (4) Diagnostics for matching. 3.7% members lost through common support enforcement (5 at extremes, 150 through caliper). Mean difference in propensity scores for treated and matched non-treated: .0001934. Mean match weight for non-members: 1.83, maximum = 19. Mean absolute standardised bias post-matching: -0.29.

Table 3 presents results for the same type of analysis focusing on those employees in workplaces with at least some covered workers. The raw differential pre-matching is smaller than for the private sector as a whole, as we anticipated in Section 3. But the matching reduces the differential by a smaller amount than in the case of the private sector as a whole. Thus, post-matching the union membership premium is a statistically significant 6.4% in the population of employees in covered workplaces.

Table 3: Mean Differences in Log Earnings (%) Between Members and Non-Members Pre- and Post-Matching, Covered Workplaces

	Pre-match	Post-match
Member log wage	1.956	1.956
	N=3,320	N=3,180
Non-member log wage	1.795	1.894
	N=2,477	N=1,182
% differential	17.5	6.4
Significance	F(1,341)=22.67 Prob>F=0.0000	F(1,338)=4.91 Prob>F=0.0274

Notes: See Table 2 for notes (1) – (2). (3) The 95% confidence interval for the post-match bootstrapped estimates is 10.3% to 2.6%. (4) Diagnostics for matching: 4.4% members lost through common support enforcement (6 at extremes, 140 through caliper). Mean difference in propensity scores for treated and matched non-treated: .0003258. Mean match weight for non-members: 2.69, maximum = 28. Mean absolute standardised bias post-matching: -0.51.

In Section 3 we reasoned that, since it is bargaining coverage that is most likely to generate a membership wage premium, this premium should be smallest where the sub-sample consists solely of workers in covered occupations. This is because, in general, all these workers should benefit from pay bargaining, unless employers discriminate between members and non-members, or members are located in covered workplaces where unions have greater bargaining power. In fact, Table 4 shows the raw membership differential is similar among those in covered occupations to the premium in the whole private sector at around 24-25%. However, matching reduces the membership premium from 24.5% to a statistically insignificant 2.7%, a figure that is lower than the matched estimates for the whole private sector or for those in covered workplaces.

Table 4: Mean Differences in Log Earnings (%) Between Members and Non-Members Pre- and Post-Matching, Covered Occupations

	Pre-match	**Post-match**
Member log wage	1.927 N=2,717	1.927 N=2,592
Non-member log wage	1.708 N=1,594	1.900 N=834
% differential	**24.5**	**2.7**
Significance	F(1,334)=55.20 Prob>F=0.0000	F(1,325)=0.67 Prob>F=0.4149

Notes: See Table 2 for notes (1) – (2) (3) The 95% confidence interval for the post-match bootstrapped estimates is 8.0% to -2.4%. (4) Diagnostics for matching: 5.4% members lost through common support enforcement (23 at extremes, 125 through caliper). Mean difference in propensity scores for treated and matched non-treated: .0003984. Mean match weight for non-members: 3.11, maximum = 25. Mean absolute standardised bias post-matching: -0.84.

We tested the sensitivity of our results to the way we implemented the matching procedure and estimated predicted wages. First, we removed the caliper setting the tolerance for an acceptable match, allowing the matching process to produce different counterfactuals for union members. This made no difference to our results. Second, estimates of the union membership premium can differ when separate wage estimates are conducted for members and non-members (Andrews *et al.*, 1998). Running separate equations for members and non-members confirms that the slopes on predictor variables do differ across membership status in ways noted by Farber (2001). However, our results are similar whether we use a single equation or separate equations to predict hourly earnings.[19]

5.1 Is the membership premium among covered employees explained by members' employment in conditions where unions are better able to extract rents?

Our analysis above indicates that union members earned roughly 3-6% more than their matched counterparts. However, covered non-members may be located in workplaces where

[19] Using separate member and non-member wage equations to estimate hourly wages, the membership premium post-matching was 4.0% for the whole private sector, 5.5% for employees in covered workplaces, and 1.3% for employees in covered occupations, relative to 3.5%, 6.4% and 2.7% respectively when using a single equation. Full model specifications are available from the author.

unions find it harder to extract rents from the employer. There are two reasons for thinking that this might be so. First, as noted earlier, if the size of the union premium is linked to union strength, covered members are likely to receive a higher premium than covered non-members. This is because the conditional probability of high density given membership is higher than that given coverage. In our data, 38% of covered non-members were employed in workplaces with union density below 25%, compared with only 4% of covered members. Conversely, over half (52%) of covered members were employed in workplaces with density of 75% or more, compared with 12% of covered non-members. There is empirical evidence from Britain (Stewart, 1987) and the United States (Schumacher, 1999) that the union premium is indeed higher where density is higher. Second, covered members are more likely than covered non-members to be working in the oldest workplaces (Table 5). Research for Britain indicates that unions' inability to command a wage premium among newer workplaces was part of the reason for the decline in the union premium during the 1980s (Stewart, 1995). If older, more established workplaces have higher rents to offer, perhaps due to their relative success in the market place, then the average union premium should be higher for covered members than it is for non-members. With these considerations in mind, we investigated the membership effect on wages in four sub-populations of covered workers: strong and weak union employees, and employees in younger and older workplaces.

Table 5: Distribution of Union Members and Non-Members in Covered Workplaces, by Age of Workplace (Column %)

Age of workplace:	Non-members	Members
Less than 3 years	5	4
3-4 years	3	3
5-9 years	16	10
10-20 years	26	14
More than 20 years	50	68
Weighted N	*2943*	*4282*
Unweighted N	*2994*	*3989*

A. Union strength

To assess the impact of union strength on the size of the union membership wage premium among covered workers, we ran the matching process separately for employees in workplaces with union density of 50% or more and those with under 50% density. We anticipated that, conditioning on union strength, any membership differential would be smaller than those presented above because matched cases would share a common union strength environment. In fact, this proved not to be the case. Where unions were weak, the post-match membership premium was similar to the premium estimated above – roughly 6.5% for employees in covered workplaces, and 2% among those in covered occupations (Table 6). The premium was substantially higher where unions were strong (12.3% among those in covered workplaces and 9.5% among those in covered occupations). It is not surprising to find that there is a larger union premium where unions are strong. As others have noted, a higher incidence of free-riding can weaken a union's bargaining power. Causation may also work the other way: if the incentive to join a union is higher where the union commands a larger premium, then we should observe higher union density (Schumacher, 1999). What is surprising is that strong unions seem able to target those rents at their members to the exclusion of non-members. It may be that the institutional strength of a union which enables it to procure rents also permits it to enforce discrimination against non-members. In any event, the union membership wage premium among covered workers is only statistically significant where unions are strong.

Table 6: Mean Difference in Log Earnings (%) Between Covered Members and Non-Members Pre- and Post-Matching, by Workplace Union Density

	Pre-match	Post-match
Employees in covered workplaces:		
50%+ union density	4.8%	12.3%****
<50% union density	12.5%**	6.4%
Employees in covered occupations:		
50%+ union density	9.4%***	9.5%**
<50% union density	19.8%****	2.1%

Notes: * significant at a 90% confidence level. ** significant at a 95% confidence level. *** significant at a 99% confidence level. **** significant at a 99.9% confidence level.

B. Age of workplace

We undertook a similar exercise, this time running the matching estimator on sub-sets of covered employees according to whether they were employed in an older workplace (aged 21 years or more) or a younger workplace. It seems that unions could only extract a wage premium for their members relative to matched covered non-members where they were located in older workplaces (Table 7). Among those in younger workplaces, members appeared to earn a little less than matched non-members, although the difference is statistically insignificant.

Table 7: Mean Difference in Log Earnings (%) Between Covered Members and Non-Members Pre- and Post-Matching, by Age of Workplace

	Pre-match	Post-match
Employees in covered workplaces:		
Workplace aged 21+ years	13.8%**	8.5%*
Workplace<21 years	16.7%****	-2.5%
Employees in covered occupations:		
Workplace aged 21+ years	25.4%****	2.2%
Workplace <21 years	17.9%****	-2.5%

Notes: * significant at a 90% confidence level. ** significant at a 95% confidence level. *** significant at a 99% confidence level. **** significant at a 99.9% confidence level.

These sub-group analyses indicate that covered members only received a union membership wage premium relative to 'like' covered non-members where unions were strong and where employees were employed in older workplaces. Although there are good reasons, discussed above, as to why a union wage premium is more likely in these workplaces, compared to workplaces which are younger and bargain with weaker unions, it is unclear how unions translate these rents into a membership premium.

6. Conclusions

We use a semi-parametric matching approach (propensity score matching) to compare union members' wages with the wages they would have received if they had not been members. We do so by comparing mean wage outcomes for unionised workers with 'matched' non-unionised workers. The method assumes selection on observables. For this identifying assumption to be plausible, one must be able to control for all characteristics affecting both union status and wages. We maintain that this assumption, although untestable, is more credible with the very informative data offered by the linked employer-employee Workplace Employee Relations Survey (WERS) 1998. In implementing the technique, we are able to effect good matches between members and non-members across most of the propensity distribution. Very few members are lost through the enforcement of common support and the mean differences in propensity scores between members and their matched counterparts are small. We are also able to achieve a good balance in observable characteristics between members and matched non-members.

The union membership wage premium falls substantially after matching. The raw differential is around 24% in the private sector as a whole, 18% among employees in covered workplaces, and 25% among those in covered occupations. The only membership premium that is statistically significant in the population after matching is the 6.4% among employees in covered workplaces. The post-matching differentials of 3.5% and 2.7% in the whole private sector and those in covered workplaces respectively are not statistically significant.

In Section 2, we suggested that whether individuals received a union wage premium was likely to depend on whether their pay was set by collective bargaining. If so, then one would expect the size of the membership premium in the private sector as a whole, covered workplaces and covered occupations to be driven by the 'coverage gap' between members and non-members in each of the sub-samples. This would lead us to anticipate the smallest membership premium among workers in covered occupations, and the largest premium in the private sector as a whole (where the coverage gap between members and non-members is largest). This proved not to be the case. Pre-matching, the largest membership premium was within covered occupations. Although this proved to be somewhat illusory, with the gap becoming insignificant post-matching, the largest membership premium post-matching was found among employees in covered workplaces. It is not obvious why this should be so. One possible explanation is that, if the membership premium among covered workers arises through discrimination on the part of unions, employers, or both against non-members, as others have suggested, there may be more scope for this discrimination in covered workplaces than among a sub-population of employees, all of whom belong to a covered occupation. This is because the dimensions across which discrimination may occur are more numerous across occupations than within occupation.[20]

Further investigations as to the source of the membership premium revealed that the premium was larger where unions were stronger (as measured by union density) and among older workplaces relative to younger ones. These findings conform to our expectations about the likely availability of rents which unions can extract from the employer, but they do not explain why it is that unions are successful in channelling those rents to their members at the expense of 'like' non-members.

[20] I would like to thank Dorothe Bonjour for this point.

Although not always statistically significant, our preferred population estimates for the membership premium range from 2.7% in covered occupations to 6.4% in covered workplaces. If there is a real membership wage premium, even among covered workers, why don't non-members join? Could it be that non-members face higher costs than members in joining a union? In the private sector as a whole this is quite likely since non-members are more likely to be located in non-unionised workplaces than members, and the costs of becoming a member are higher in non-unionised workplaces than they are in joining an already established union (Farber, 2001). However, it is less likely that there is a cost differential facing non-members where they are already located in covered workplaces. An alternative is that non-members are simply less desirous of membership than members, perhaps because their tastes differ. Bryson and Gomez (2002) present evidence to suggest that this is indeed the case, since the market for union membership is segmented along ideological lines. These tastes may have an independent effect on the likelihood of joining a union, or else they may affect the weight employees attach to the costs and benefits of membership. If this characteristic, which is unobserved in our data, is also likely to influence employees' wages, then its omission from our analysis will bias our estimates of the union membership wage premium.

Of course, if we focus on our estimates for the whole private sector or covered occupations, where no significant premium is apparent, we face a different question: why don't union members leave the union? Well, they have been leaving: union density is in decline, even within unionised workplaces (Millward *et al.*, 2000, Chapter 5). However, evidence for the period 1983-1998 indicates that the rate at which employees have left membership has not risen and that the decline in membership is due to an increase in employees who have never been members (Bryson and Gomez, 2002). It may well be that the returns to membership have declined. However, there are at least three reasons why it is not possible to read off employees' likely membership intentions from the wage premium they face. The first is that the premium may rise under, say, more favourable economic conditions. In any event, one can make the case for remaining in a union if – as we show - union density has a role to play in the size of the wage premium unions can extract from the employer. If members were to leave, the prospects of bargaining for better wages will deteriorate. Second, members are unlikely to value membership purely in terms of the wage mark up unions command. Members also benefit directly by unions' efforts to improve non-pecuniary benefits, job security, the handling of grievance and disciplinary matters, and by encouraging management to treat all employees more fairly. Third, as alluded to above, many join and remain members because they are ideologically committed to doing so.

Appendix Table 1: Gross Hourly Earnings in the Private Sector

Union member	0.083
	(4.98)**
Workplace union recognition	0.030
	(1.99)*
Age (ref.: under 20)	
20-24 years	0.155
	(5.68)**
25-29 years	0.269
	(9.37)**
30-39 years	0.334
	(12.15)**
40-49 years	0.346
	(12.24)**
50+ years	0.341
	(11.89)**
Highest academic qualification (ref: none)	
CSE	0.054
	(3.35)**
GCSE	0.095
	(7.39)**
A-level or equivalent	0.155
	(10.19)**
Degree or post-graduate	0.263
	(12.41)**
Any vocational qualifications	-0.021
	(2.17)*
Workplace tenure (ref: under 2 years)	
2-4 years	0.053
	(3.84)**
5-9 years	0.072
	(4.59)**
10+ years	0.092
	(5.71)**
Employer provided training in last 12 months (ref: none)	
Less than 1 day	0.004
	(0.27)
1 < 2 days	0.045
	(3.24)**
2 < 5 days	0.056
	(4.41)**
5 < 10 days	0.040
	(2.04)*
10+ days	0.015
	(0.90)
Female	-0.082
	(6.24)**
Dependent children	0.026
	(2.37)*
Married or living as married	0.050
	(5.18)**
Health problem	-0.064
	(3.06)**
Member of non-white ethnic group	-0.048
	(1.50)
Occupational classification (ref.: operative)	
Manager/senior administrator	0.550
	(19.79)**
Professional	0.488
	(16.62)**
Associate professional and technical	0.324
	(11.75)**

Clerical and secretarial	0.151
	(6.76)**
Craft and skilled service	0.142
	(5.95)**
Personal and protective service	-0.018
	(0.49)
Sales	0.062
	(2.01)*
Other unskilled occupations	-0.100
	(3.68)**
Permanent contract	0.093
	(4.02)**
Part-time (under 30 hours per week)	0.073
	(2.70)**
Overtime hours (ref.: no overtime)	
Overtime, voluntary	0.017
	(0.76)
Overtime, required	0.051
	(1.90)
Occupational gender segregation (ref.: equal between men and women)	
Only men	0.045
	(2.23)*
Mainly men	0.027
	(1.97)*
Mainly women	-0.070
	(4.77)**
Only women	-0.088
	(4.44)**
50%+ of workforce are part-timers	-0.167
	(7.08)**
Under 25% workforce is female	0.038
	(2.04)*
Percentage of workforce manual (ref.: 50%+)	
Under 10% of workforce is manual	0.146
	(7.27)**
10-49%	0.066
	(3.58)**
Workplace size (ref.: 10-24 employees)	
25-49 employees	0.036
	(1.32)
50-99 employees	0.051
	(2.02)*
100-199 employees	0.050
	(1.85)
200-499 employees	0.091
	(3.30)**
500+ employees	0.168
	(5.16)**
Foreign owned	0.133
	(5.09)**
Single independent workplace	-0.030
	(1.66)
Industrial classification (ref.: manufacturing, utilities, construction)	
Wholesale and retail distribution	-0.063
	(2.87)**
Hotels and restaurants	-0.136
	(4.05)**
Transport and communication	-0.038
	(1.24)
Financial Services	-0.040
	(1.54)
Other business services	-0.043
	(1.58)
Other	-0.037

	(1.16)
London	0.194
	(7.86)**
Unemployment rate of 5%+	-0.058
	(3.39)**
Constant	0.928
	(21.15)**
Sigma	0.333
	(41.76)**
F stat	(60,989) = 101.31 Prob>F = 0.0000
Observations	14875

Note: * significant at 5% level; ** significant at 1% level. Absolute value of t-statistics in parentheses

Appendix Table 2: Individual Union Membership Status in the Private Sector

Demographics:	
Age (ref.: under 20)	
20-24 years	0.222
	(1.98)*
25-29 years	0.495
	(4.91)**
30-39 years	0.712
	(6.99)**
40-49 years	0.821
	(7.81)**
50+ years	0.808
	(7.55)**
Highest academic qualification (ref: none)	
CSE	0.096
	(1.65)
GCSE	-0.060
	(1.13)
A-level or equivalent	-0.125
	(1.86)
Degree or post-graduate	-0.317
	(3.88)**
Female	-0.013
	(0.24)
Married or living as married	0.105
	(2.67)**
Health problem	0.080
	(1.19)
Member of non-white ethnic group	0.169
	(1.90)
Job-related:	
Occupational classification (ref.: operative)	
Manager/senior administrator	-0.979
	(8.77)**
Professional	-0.492
	(4.77)**
Associate professional and technical	-0.524
	(5.22)**
Clerical and secretarial	-0.917
	(9.30)**
Craft and skilled service	-0.033
	(0.41)
Personal and protective service	-0.938
	(6.16)**
Sales	-0.544
	(5.16)**
Other unskilled occupations	-0.547
	(5.66)**
Permanent contract	0.275
	(2.84)**
Hours worked (continuous)	0.023
	(3.37)**
Hours worked squared	-0.000
	(3.28)**
Occupation performed solely by men	0.122
	(2.07)*
Workforce composition:	
Percentage female is <25%	0.476
	(4.96)**
Percentage part-time is <10%	-0.321
	(3.25)**
No workers aged under 20 years	0.365

	(4.03)**
No manual workers	0.506
	(4.91)**
Workplace:	
Size (ref: 10-99 employees)	
100-199 employees	0.479
	(4.30)**
200-499 employees	0.941
	(9.98)**
500+ employees	1.104
	(9.59)**
Foreign-owned	-0.153
	(1.56)
Single independent establishment	-0.533
	(5.40)**
Workplace activity (ref: producers of goods/services for consumers, producers for other parts of organisation, non-producers)	
Administrative office only	-0.364
	(2.14)*
Supplier to other companies	-0.371
	(4.08)**
Industrial classification (ref.: manufacturing, utilities, construction)	
Wholesale and retail distribution	-0.481
	(4.20)**
Hotels and Restaurants	-0.434
	(2.29)*
Transport and communication	0.142
	(1.00)
Financial Services	0.258
	(1.98)*
Other business services	-0.738
	(4.42)**
Other	0.073
	(0.57)
Location (ref: East, East Midlands, London, South East, Yorkshire and Humberside, North East)	
North	0.336
	(2.08)*
North West	0.426
	(3.50)**
Scotland	0.178
	(1.49)
South West	0.391
	(3.32)**
Wales	0.194
	(0.91)
West Midlands	0.387
	(2.90)**
Local labour market conditions:	
Unemployment rate of 5%+	0.282
	(3.75)**
Constant	-2.076
	(9.06)**
Observations	14932
F-stat	(49,995) = 20.26 Prob>F=0.0000

Note: * significant at 5% level; ** significant at 1% level. Absolute value of t-statistics in parentheses

Appendix Table 3: Imbalance in Means Between Treated and Matched Comparators, Plus Standardised Differences (%)

	Non-members pre-match	Non-members matched	Members	% bias before match	% bias after match
Age					
20-24 years	.11	.04	.03	-30.31	-3.26
25-29 years	.16	.12	.12	-12.65	-2.38
30-39 years	.27	.31	.31	10.10	0.38
40-49 years	.21	.27	.30	22.70	6.90
50+ years	.20	.24	.23	7.27	-3.05
Highest academic qualification					
CSE	.12	.14	.13	3.94	-3.25
GCSE	.28	.28	.28	-0.48	0.38
A-level or equivalent	.17	.13	.15	-3.82	5.97
Degree or post-graduate	.22	.16	.16	-17.63	1.07
Female	.49	.38	.35	-30.94	-4.52
Married or living as married	.64	.74	.76	28.11	4.44
Health problem	.05	.06	.06	7.85	1.73
Member of non-white ethnic group	.04	.03	.03	-1.67	0.54
Occupational classification					
Manager/senior administrator	.15	.09	.09	-20.92	-2.23
Professional	.09	.09	.10	-0.16	1.60
Associate professional and technical	.08	.11	.10	7.61	-6.00
Clerical and secretarial	.23	.16	.20	-9.79	9.79
Craft and skilled service	.08	.17	.17	30.38	1.64
Personal and protective service	.08	.03	.02	-25.92	-1.36
Sales	.12	.09	.07	-18.43	-5.83
Other unskilled occupations	.09	.06	.06	-13.02	-2.87
Permanent contract	.94	.96	.97	15.54	3.87
Hours worked (continuous)	37.59	39.49	39.77	19.46	2.43
Hours worked squared	1581.37	1673.33	1682.22	11.94	1.03
Occupation performed solely by men	.15	.26	.26	30.90	1.22
Percentage female is <25%	.26	.41	.46	47.32	10.65
Percentage part-time is <10%	.53	.61	.65	25.96	7.59
No workers aged under 20 years	.22	.27	.30	18.94	6.96
No manual workers	.21	.25	.23	5.04	-4.88
Workplace size					
100-199 employees	.18	.19	.20	5.94	3.82
200-499 employees	.17	.34	.33	38.59	-1.08
500+ employees	.08	.22	.18	29.33	-13.14
Foreign-owned	.18	.21	.20	5.47	-2.31
Single independent establishment	.29	.14	.10	-49.41	-8.54
Workplace activity					
Administrative office only	.07	.03	.04	-11.31	6.69
Supplier to other companies	.34	.27	.24	-23.06	-6.68
Industrial classification					
Wholesale and retail distribution	.23	.12	.11	-33.58	-3.41
Hotels and Restaurants	.07	.02	.01	-28.83	-4.61
Transport and communication	.05	.12	.12	30.00	-.97
Financial Services	.07	.17	.14	22.12	-9.29
Other business services	.16	.03	.03	-43.70	-.09
Other	.15	.10	.10	-17.37	-1.28
Location					
North	.04	.06	.08	15.96	7.83
North West	.08	.12	.13	18.92	5.97

Scotland	.09	.11	.11	7.07	1.72
South West	.09	.10	.11	4.85	1.81
Wales	.04	.05	.05	4.43	-5.29
West Midlands	.08	.12	.08	4.13	-10.95
Unemployment rate of 5%+	.48	.57	.56	17.15	-2.90
Average absolute standardised bias pre-match, whole sample					18.16
Average absolute standardised bias post-match, whole sample					4.21
Average absolute standardised bias pre-match, matched sample					2.12
Average absolute standardised bias post-match, matched sample					-0.29
Absolute bias reduction					41.00

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