ILR Review

Volume 57 | Number 3 Article 5

April 2004

The Union Membership Wage-Premium Puzzle: Is There a Free Rider Problem?

Alison L. Booth
Australian National University

Mark L. Bryan
University of Essex

The Union Membership Wage-Premium Puzzle: Is There a Free Rider Problem?

Abstract
Economists have long suggested that labor unions suffer a free rider problem. The argument is that, since union-set wages are available to all workers covered by unions irrespective of their union status, and union membership entails costs, workers will only join if they are coerced or are offered non-wage goods that they value above membership costs. Yet U.S. and British empirical research has found a substantial union membership wage premium among private-sector union-covered workers, implying that there is no free rider problem. The authors of this study hypothesize that these findings arise due to selectivity problems associated with identifying the union membership effect. Their analysis, which uses rich data from a new linked employer-employee survey for Britain and exploits the within-establishment variation in wages as a function of individual union membership status, demonstrates that the apparent wage premium for members is illusory. Hence, a potential free rider problem remains.

Cover Page Footnote
This research was supported by funds from the Leverhulme Trust under Award F/00213C "Work-Related Training and Wages of Union and Non-Union Workers in Britain." For helpful comments on earlier drafts of this paper, the authors thank Martyn Andrews, Wiji Arulampalam, René Böheim, John Budd, Lorenzo Cappellari, John Forth, Marco Francesconi, Andrew Hildreth, and participants in seminars at the University of Essex, the London School of Economics, and the 2001 conferences of the European Association of Labour Economics and the Education and Employment Economics Group. The authors acknowledge the Department of Trade and Industry, the Economic and Social Research Council, the Advisory, Conciliation, and Arbitration Service, and the Policy Studies Institute as the originators of the 1998 Workplace Employee Relations Survey, and the Data Archive at the University of Essex as the distributor of the data.

THE UNION MEMBERSHIP WAGE-PREMIUM PUZZLE: IS THERE A FREE RIDER PROBLEM?

ALISON L. BOOTH and MARK L. BRYAN*

Economists have long suggested that labor unions suffer a free rider problem. The argument is that, since union-set wages are available to all workers covered by unions irrespective of their union status, and union membership entails costs, workers will only join if they are coerced or are offered non-wage goods that they value above membership costs. Yet U.S. and British empirical research has found a substantial union membership wage premium among private-sector union-covered workers, implying that there is no free rider problem. The authors of this study hypothesize that these findings arise due to selectivity problems associated with identifying the union membership effect. Their analysis, which uses rich data from a new linked employer-employee survey for Britain and exploits the within-establishment variation in wages as a function of individual union membership status, demonstrates that the apparent wage premium for members is illusory. Hence, a potential free rider problem remains.

Economists have long suggested that there is a free rider problem associated with organizations such as labor unions. The argument, broadly, is that since the union-set wage is a public good applying to all workers in the union-covered sector regardless of their individual union status, and there are monetary or psychic costs to membership, workers will behave like rational economic agents faced with a public good and take a free ride on union membership—unless they are coerced into joining, or offered excludable goods (goods unavailable to non-members) that they value.

By law, union-covered members and non-members receive equal wages within the

*Alison Booth is F. H. Gruen Professor at the Australian National University, and Mark Bryan is Senior Research Officer at the University of Essex. This research was supported by funds from the Leverhulme Trust under Award F/00213C "Work-Related Training and Wages of Union and Non-Union Workers in Britain." For helpful comments on earlier drafts of this paper, the authors thank Martyn Andrews, Wiji Arulampalam, René Böheim, John Budd, Lorenzo Cappellari, John Forth, Marco Francesconi, Andrew Hildreth, and participants in seminars at the University of Essex, the London School of Economics, and the 2001 conferences of the European Association of Labour Economics and the Education and Employment Economics Group. The authors acknowledge the Department of Trade and Industry, the Economic and Social Research Council, the Advisory, Conciliation, and Arbitration Service, and the Policy Studies Institute as the originators of the 1998 Workplace Employee Relations Survey, and the Data Archive at the University of Essex as the distributor of the data.

Copies of the computer programs used to generate the results presented in the paper are available from Mark L. Bryan at the Institute for Social and Economic Research, University of Essex, Wivenhoe Park, CO4 3SQ, United Kingdom.

Industrial and Labor Relations Review, Vol. 57, No. 3 (April 2004). © by Cornell University.
0019-7939/00/5703 $01.00

same workplace.[1] Yet empirical analysis typically finds a union membership wage advantage. In this paper we attempt to uncover the economic and empirical linkages that explain this anomaly.

Empirical research for both the United States and Great Britain has shown that private-sector union-covered workers enjoy a substantial union membership wage premium (see, *inter alia,* Blakemore, Hunt, and Kiker 1986; Hildreth 2000; Budd and Na 2000). These studies take explicit account of membership endogeneity using a variety of techniques, and find large statistically significant member/non-member wage effects. The conclusion of those studies is that there are substantial economic gains in the form of higher wages for union members. One implication reached by these authors is that a "rethinking of the 'free rider' literature is warranted" (Budd and Na 2000:804), since the union-set wage appears not to be a collective good.

Using data from a new British linked employer-employee survey with a particularly rich set of industrial relations variables, in this paper we address that apparent challenge to the free rider hypothesis. Our examination of the within-establishment variation in wages as a function of individual union membership status provides a novel way of addressing the usual selectivity issues associated with identifying the union membership effect.

Background

Why Join a Union?

The free rider problem is only relevant to workplace settings in which individuals can exercise choice with regard to union membership, that is, in which there is no coercion. Such choice exists both in Britain and in those U.S. states where closed or union shops are illegal. Starting in the late 1980s, legislation in Britain effectively outlawed closed shop arrangements, while in the United States right-to-work laws prohibit union shops in some 20 states.[2] In Britain, the proportion of private-sector union-covered workers who are also members currently stands at 73%, and in U.S. right-to-work states it is 87%.[3]

According to Olson (1965), a reason workers might join a union in the absence of coercion is that unions may offer excludable goods or services to their members to encourage them to join. Booth (1985) showed that social customs, such as a group norm of membership enforced by a threat of reputational damage to the noncompliant, could also overcome the free rider problem. Moreover, there is considerable historical evidence that friendly society benefits have been important in attracting workers into unions (see, for example, Boyer 1988). More recent examples of excludable goods include legal advice, pension advice, and seniority rules.[4] However, it is not easy to find, in available data sets, appropriate proxies for such excludable goods to facilitate structural estimation of wages and membership.

[1]In the United States, federal laws stipulate that union-covered members must receive the same wage as covered non-members. In other countries, unions typically do not try to negotiate lower wages for covered non-union workers, perhaps because such activity encourages under-cutting by non-union workers. If otherwise identical non-union members are paid less than union members, then ultimately the credibility and survival of the union will be undermined, and the union driven out of existence, as firms substitute non-member labor for costlier union members. We return to this issue later.

[2]See Boeri, Brugavini, and Calmfors (2001) for details of the Europe-wide situation. The huge variation in the extent of free riders—from more than 70% in France to a low of just 2% in Finland—reflects the heterogeneity across countries in collective bargaining institutions and labor laws, as well as benefits provided by unions to their members.

[3]The British figure is calculated from WERS 98 data on workers whose pay is set by collective bargaining. We discuss this definition of coverage in more detail later in the text. The figure for the United States is based on Current Population Survey data for 1983–93, reported by Budd and Na (2000).

[4]Booth (1985) and Naylor (1989) focused on social custom sanctioned by loss of reputation for noncomplying individuals. Booth and Chatterji (1995) emphasized grievance procedures. Willman (1990) and Booth (1991) looked in detail at what unions actually provide.

What Might Explain the Member/ Non-Member Covered Wage Premium?

In spite of the free rider problem, there is evidence from some empirical studies of a positive member/non-member wage differential for covered workers. Although Jones (1982), using the National Longitudinal Survey (NLS), found a very small member/non-member covered worker wage premium, subsequent investigation typically found quite large effects—see, for example, the studies using the NLS by Blakemore et al. (1986) and Hunt, Kiker, and Williams (1987); the more recent studies using the Current Population Survey (CPS) by Hundley (1993), Schumacher (1999), and Budd and Na (2000); and Hildreth (2000), which used the British Household Panel Survey (BHPS).[5]

Reasons advanced to explain this observed wage premium fall into two broad categories. The first includes selectivity or omitted variable explanations; the second, explanations based on discriminatory behavior by the union, the firm, or both.

According to explanations in the first broad category, members and non-members may differ systematically in some productivity-augmenting characteristic that is unobservable to the researcher. To the extent that this characteristic is positively correlated with membership, the estimated coefficient to union membership status in a covered wage equation will be upward-biased: there will be a positive selection effect. For example, Budd and Na (2000) suggested that workers who unionize are those who are more motivated or more prepared to stay with their firm and invest in firm-specific human capital. Insofar as the data set does not include information on "motivation" and training, there will be an omitted variable problem. A related explanation is that only permanent workers will face pressure from union shop stewards to join. Younger temporary or probationary workers, who almost certainly earn less, will not be targeted in the same way. There will again be positive omitted variable bias if this temporary status cannot be observed in the data. We are fortunate that our data set contains a particularly rich collection of individual-level variables, some of them unavailable to previous studies (for example, tenure and training were not in the CPS data used by Budd and Na).

There may also be selection at the workplace level, with members systematically being found in higher-paying firms. The underlying mechanism might be a causal effect of density on wages (as found by Reilly 1996 and Barth et al. 2000). Alternatively, workers may be more likely to join where other bargaining structures (such as multiple union bargaining) help unions achieve high wages. In both cases, the omission of appropriate controls for union power, both observed and unobserved, will lead to biased estimates of the membership effect. Unions may also concentrate their organizing activity in firms that are already high payers. Suppose, for example, that unions divert all their organizing efforts to firms in which there is a large surplus, making the payment of high wages more feasible. Then membership will be greater in the higher-paying firms through this selection effect. Analogously, if high-paying firms are also high-turnover firms, then workers may put a greater value on private goods offered by unions—such as grievance procedures that are available only to members—and hence be more likely to join. These selection effects in estimation arise because of the inherent difficulties in identifying the underlying structural relationships. Booth and Chatterji (1995) constructed a theory (based on the behavior of workers, the union, and the firm) in which there is a membership demand curve and a wage-setting curve. Failure to identify these correctly can lead to a spurious correlation between membership and wages—a classic identification problem.

This discussion suggests that controlling for workplace heterogeneity will be impor-

[5]See Budd and Na (2000) for a comprehensive survey of these papers. See also the methodological paper by Andrews, Stewart, Swaffield, and Upward (1998), who discussed the positive membership wage premia that have typically been found in British studies.

tant in any empirical analysis of covered wages and membership. Our data set contains union bargaining variables, such as the presence of multiple trade unions, as well as other rich plant-level controls. Crucially, we are also able to control for unobserved workplace-level effects by examining the within-workplace relation between membership and wages. It may be noted that this method also helps deal with individual selection insofar as it differs across workplaces. For example, less productive workers (who are also less likely to be union members) may be less able to find a job with a strong union firm.

The second broad set of explanations advanced in the empirical literature for the observed positive member/non-member wage differential relates to discriminatory behavior by unions or employers. For example, Blakemore et al. (1986) suggested that the wage premium might be the result of cooperation between the union and the firm. The essence of this argument is that a union's cooperative behavior can increase the size of the surplus to be shared between workers and the firm.[6] Since both parties gain from union cooperation, the firm will be willing to assist or to turn a blind eye to the union's discriminatory behavior in ensuring that its members are paid more than non-members. For example, the firm might target training programs systematically toward one group, thereby conferring on that group a wage advantage. As another example, firms might, in return for union cooperation, attempt to pay non-members less, or, if equal pay laws preclude this, pay from a point lower down the union wage scale.

Notice that the conventional explanation of why unions do not want to see members and covered non-members paid different rates—to avoid under-cutting by non-members—disappears in this cooperative scenario. This is because the firm is now unlikely to substitute cheaper non-member labor for more costly union members. If it were to do so, the cooperative behavior of the union—which is held to increase the overall surplus and reduce labor turnover—would be withdrawn, and the firm would be made worse off.[7]

Of course a problem with such explanations for the member/non-member covered wage premium is that they do not make clear why union non-members do not take appropriate action to improve their lot—by, for example, joining the union to obtain higher wages. Therefore these explanations are not very logically convincing as an explanation of the wage premium.[8] This is especially the case since, in some of the studies outlined above, the wage premium actually increases once appropriate care has been taken to account for the potential selectivity or omitted variable bias to the union status variable in the covered wage equation. Why non-members in the covered sector abstain from membership then becomes even more of a puzzle, since the economic gains are so large.

Against this background, the purpose of our paper is to investigate possible explanations for the observed member/non-member wage differential using data from a new linked employer-employee survey with a particularly rich set of industrial relations variables. These data include the key individual and firm-level characteristics—training incidence, tenure, temporary work status, and union power—whose omission, as already noted, may lead to bias. Our main innovation is to exploit the within-establishment variation in union membership for covered workers, which allows us to address selectivity issues associated with estimating the union membership effect.

---

[6]For a summary of the quite rich body of literature discussing the conditions under which unions can enhance efficiency, see Freeman and Medoff (1984) and Booth (1995:66–71).

[7]The available empirical evidence shows that while union presence reduces labor turnover, the impact of unions on productivity and profitability is typically negative.

[8]Hildreth (2000) acknowledged this point in discussing his 30% average member/non-member wage premium, and suggested instead that there remains the selectivity issue.

Data and Key Variables

The Data Set

We use the new linked employer-employee data from the 1998 Workplace Employee Relations Survey (WERS 98), the first comprehensive survey of its kind for Britain. This is a nationally representative survey of workplaces with ten or more employees, covering the private and public sectors.[9] The sample of workplaces was obtained through a process of stratified random sampling, with over-representation of larger workplaces and some industries (see Forth and Kirby 2000 for details).[10]

WERS98 contains three linked cross-sectional components: the management interview questionnaire, the worker representative interview questionnaire, and the individual self-completion questionnaire. The management interview was carried out face-to-face with the most senior workplace manager responsible for personnel or employee relations (see Cully et al. 1999). Interviews were conducted in 2,191 workplaces over the period October 1997 to June 1998, with a response rate of 80.4%. Additional interviews were carried out with worker representatives where such interviews were relevant and permitted by management. This occurred in 947 workplaces, representing a response rate of 82% of eligible cases. The third component of WERS98, the Survey of Employees, comprised survey questionnaires presented to 25 randomly selected employees at each workplace (or all employees in smaller workplaces). The questionnaire was distributed to the 1,880 workplaces where management permitted it, with a response rate of 64% (28,237 employees).[11] In this paper we use the individual-level responses, to which we linked workplace characteristics from the management survey. Our estimating subsample is all private sector men and women who are union-covered and employed in workplaces with at least 25 employees and with complete information on the variables of interest.[12] This represents 2,162 full-time men and 1,224 part-time and full-time women, where full-time workers are defined, following government definitions, as those whose usual total weekly hours exceed 30.[13] We further disaggregate our data into manual and non-manual subsamples, since testing showed that it is inappropriate to pool manual and non-manual workers.[14]

[9]It does, however, exclude agriculture, fishing, mining, private households with employed persons, and extra-territorial organizations.

[10]The oversampled industries are the SIC 92 major groups covering electricity, gas and water, construction, hotels, financial services, and other community services.

[11]For descriptive analysis in which the aim is to make inferences about the population, the worker responses should be weighted to account first for the probability of an employee's workplace being selected, and secondly for the probability of the employee's own selection (which is greater in smaller workplaces). However, in regression analysis in which the regression model is viewed as the structural model, the data need not be weighted (see, for example, Cameron and Trivedi 2002).

[12]We lose 3.8% of our employees in the covered sector by dropping establishments with fewer than 25 employees. We also estimated all our models retaining workers in these smaller workplaces, and the main results of the paper are unchanged.

[13]The sample contained only 51 part-time men, 47 of whom were manual workers, and their observed characteristics differed substantially from those of full-time men. The same contrasts were not apparent between part-time and full-time women. We therefore excluded part-time men from the main analysis, although we included them as a sensitivity check at a later stage (see footnote 29). Examples of the observed difference in characteristics of part-time compared to full-time men are as follows: part-time men were less likely to have vocational qualifications (34% against 50%) and more likely to be educated to A-level (19% against 8%) or degree level (4.3% against 2.1%). They were less than a third as likely to be in the craft or operative occupations; instead, over 70% were in sales or the "other" unskilled category. On average they worked only 17 hours per week, and only 43% were union members, compared to 85% of full-timers.

[14]After preliminary testing, we rejected the hypothesis that the manual and non-manual samples could be combined. The Wald statistics from this test were 327 for men and 191 for women, distributed as χ^2 (62) (pooled model); and 183 for men and 55 for women, distributed as χ^2 (27) (workplace dummy model). All statistics are significant at better than the 0.01% level.

Table 1. Panel Characteristics of the Subsamples (Covered Private Sector Workers).

Sample	*No. Establishments*	*No. Individuals*	*Mean Individuals per Establishment*	*Percent of Observed Individuals in Establishment*
Covered	278	3,386	12.2	74.4
Male, FT, Manual	190	1,357	7.1	44.2
Male, FT, Non-Manual	164	805	4.9	29.2
Female, FT/PT, Manual	101	441	4.4	30.0
Female, FT/PT, Non-Manual	160	783	4.9	27.5

Union coverage. WERS 98 contains much more detailed information on the union coverage status of employees' workplaces than is usual in individual-level data. Our union coverage variable takes the value 1 if the employee's occupational group is covered by collective bargaining over pay at any level and 0 otherwise. We construct this variable by matching management and worker responses concerning coverage and occupation.

The question addressed to management inquires about the extent of union involvement in negotiating pay for each of up to nine occupational groups in the workplace. The questions asks:

Which of the following statements most closely characterizes the way that pay is set [for each occupational group present at the workplace]?

- — Collective bargaining for more than one employer (for example, industry-wide agreement)
- — Collective bargaining at an organization level
- — Collective bargaining at this workplace
- — Set by management at a higher level in this organization
- — Set by management at this workplace
- — Negotiation with individual employees
- — Some other way (for example, pay review body)
- — None of these

If a manager selected any of the first three statements to describe an occupational group, we define that group as covered.

The question addressed to workers in the Survey of Employees asks, "Which of the following occupation groups best describes your job at present?" and is followed by a list (with examples) of the nine one-digit occupations. We match these individual replies to the workplace–occupation-level coverage-status variable derived from the management responses to obtain a coverage indicator for each worker. Note that the construction of this indicator differs from the more usual procedure, based on individual replies to a direct coverage question. It is commonly suggested (see, for example, Jones 1982) that, in individual–level surveys, union coverage is measured with more error than is membership. The reason is that while employees are aware of their own membership status, they are less likely to know the exact role played by the union, especially if they are non-members who play no part in union governance. On the other hand, management respondents probably do have first-hand knowledge of the pay determination process. Assuming that there is not too much misreporting of individuals' occupational group, we might expect the indicator derived here to be more accurate than the traditional variable. A further assumption is that bargaining does not vary within one-digit occupational groups.

The Between- and Within-Establishment Nature of the Data

One possible explanation for the union membership wage premium observed in previous studies is that workers are more likely to join the union in establishments where unions succeed in negotiating above-

Table 2. Variation in Individual Membership across Establishments by Subgroup.

	Number of Establishments				
Mean Membership	*Covered*	*Male, Manual*	*Male, Non-Manual*	*Female, Manual*	*Female, Non-Manual*
0	7	11	25	16	36
0–0.2	9	0	3	3	7
0.2–0.4	30	8	14	6	24
0.4–0.6	36	12	22	14	23
0.6–0.8	63	16	21	8	17
0.8–1	81	50	18	9	9
1	52	93	61	45	44
Total	278	190	164	101	160
Overall Mean Membership	0.70	0.85	0.66	0.57	0.54
Overall Mean Membership (Weighted)[a]	0.73	0.86	0.63	0.62	0.49
Proportion of Variation between Workplaces[b]	0.34	0.38	0.39	0.47	0.43

[a]Weighted mean membership is calculated using the individual weight variable EMPWT_NR. All other figures are calculated without weights.
[b]The proportion of variation between workplaces is calculated as the R^2 from a regression of individual membership on the establishment dummies.

average mark-ups—a classic selection problem. To explore this possibility, we exploit the variation in wages and union membership observed within covered establishments as well as between establishments. Table 1 summarizes the between- and within-establishment dimensions of the data for our sample of covered individuals within private sector workplaces. The first row shows that we observe 3,386 employees in 278 establishments.[15] The mean number of covered individuals per establishment is 12.2, and on average they represent 74% of the individuals observed in the establishment, with the remaining 26% of individuals belonging to uncovered occupations.

The remaining rows break up the covered sample into the four estimating subsamples used in the analysis of this paper. The largest is the group of full-time manual men, for which on average we observe just over 7 individuals per establishment, representing 44% of individuals sampled in the establishment. The smallest group is manual women, for which we observe a mean of 4.4 individuals per establishment, which is 30% of sampled individuals.

[15]They represent 28% of the workplaces and 23% of the individuals in the private sector sample.

Union Membership and Its Distribution across Establishments

In Table 2 we report how membership density varies across establishments in the sample. Individual membership status is obtained from the Employee Survey, which asks respondents, "Are you a member of a trade union or staff association?" Density is then defined as the percentage of the covered group, or the four subgroups as appropriate, who are members.

Aggregate establishment-level membership density figures could mask considerable heterogeneity. At one extreme, one might have a population comprising a mix of establishments with either 0% or 100% membership, while at the other extreme density might be equal across establishments, with the only variation being between individuals in the establishment. The aggregate density figures are reported in the lower panel of Table 2. The top row of

Table 3. Mean Wages and Raw Membership Differentials.

Variable	*Covered*	*Male, Manual*	*Male, Non-Manual*	*Female, Manual*	*Female, Non-Manual*
Member Log Wage	1.93	1.83	2.33	1.48	1.99
Non-Member Log Wage	1.83	1.69	2.22	1.29	1.91
Raw Differential	0.10	0.14	0.10	0.19	0.07
Raw Differential (Weighted)	0.11	0.16	0.08	0.20	0.10
Between Coefficients	0.22	0.34	0.17	0.27	0.11
Within Coefficients	0.06	0.06	0.08	0.03	0.11

Notes: The weighted raw differential is the difference between the weighted mean wage of members and the weighted mean wage of non-members. They are calculated using the individual weight variable EMPWT_NR. All other estimates are calculated without weights.

The wage measure used is the log of the midpoint of the hourly wage interval for each individual.

this panel shows that 70% of individuals in the covered sample are members. From the weighted proportion, shown in the next row, we can infer that 73% of covered workers in the population as a whole are members (or, alternatively, 27% are free riders). The similarity of the two figures suggests that conditional on coverage, the distribution of members in our sample is reasonably representative of that in the population.[16] For the four subgroups, both the weighted and unweighted figures show that density is highest among manual men and lowest among non-manual women.

The bottom row of Table 2 shows the percentage of membership variation between establishments, rather than within establishments. Between a third and nearly a half of membership variation is due to establishment rather than individual characteristics. More detail is given in the upper panel of Table 2, which shows the distribution of density over establishments. Consider, for example, covered male, manual workers, who are observed in 190 establishments. In 11 of these establishments there are no members, and in 93 establishments all members of the subsample are members. Thus in 104 establishments there is no variation in membership among this subsample. However, in the remaining 86 establishments we observe both members and non-members, and so we can identify the membership effect on wages controlling for the establishment effect.

[16]This conclusion does not hold if uncovered workers are included. The unweighted and weighted density figures are then 0.29 and 0.18, reflecting the concentration of members in larger establishments, which are over-sampled. The fact that, conditional on coverage, this difference seems to disappear suggests that our sample of free riders is representative of free riders in the overall population. Analysis of density also reveals no systematic pattern of free riding by establishment size.

Raw Membership Wage Differentials

Table 3 reports the mean wages and mean raw wage differentials between union members and non-members. In WERS, the hourly wage is not directly reported. Instead it has to be inferred from employee-provided information on gross earnings, reported in bands, not continuously, and the total number of hours usually worked. The calculation of hourly wage measures for each individual is described at the end of the Appendix, and in the following section ("Estimates for Private Sector Men and Women") we discuss the complications that arise when this information is exploited in a multivariate structural model. In these raw comparisons, we calculate the wage simply as the log of the midpoint of the hourly wage interval for each individual.

The first two rows of Table 3 report the mean wages of members and non-members, with the difference between the two, the raw membership differential, shown in

Table 4. Summary Statistics.

(A) Individual-Level Variables

Variable	*Male Manual*			*Male Non-Manual*			*Female Manual*			*Female Non-Manual*		
	Mean	*Between Std. Dev.*	*Within Std. Dev.*	*Mean*	*Between Std. Dev.*	*Within Std. Dev.*	*Mean*	*Between Std. Dev.*	*Within Std. Dev.*	*Mean*	*Between Std. Dev.*	*Within Std. Dev.*
Log Hourly Wage	1.806	0.327	0.226	2.291	0.299	0.266	1.396	0.323	0.303	1.955	0.315	0.271
Training Incidence	0.508	0.346	0.402	0.781	0.321	0.343	0.444	0.378	0.394	0.756	0.334	0.359
Trade Union Member	0.850	0.278	0.280	0.665	0.367	0.368	0.569	0.386	0.361	0.542	0.388	0.375
Age	42.169	6.918	9.693	39.850	7.680	8.303	39.692	8.542	10.311	37.881	7.345	9.119
Tenure (Yrs.)	10.892	4.320	5.707	8.768	5.347	5.543	7.466	5.665	4.884	7.417	4.809	5.231
CSE or Equivalent or Less	0.634	0.300	0.426	0.129	0.270	0.282	0.658	0.344	0.407	0.183	0.292	0.337
O-Level or Equivalent	0.264	0.259	0.402	0.220	0.325	0.333	0.220	0.287	0.362	0.437	0.342	0.432
A-Level	0.081	0.171	0.243	0.258	0.304	0.380	0.095	0.201	0.255	0.194	0.238	0.357
Degree	0.021	0.121	0.123	0.393	0.322	0.388	0.027	0.166	0.129	0.186	0.305	0.301
Vocational Qualification	0.500	0.317	0.430	0.493	0.355	0.409	0.166	0.283	0.318	0.292	0.338	0.391
Vocational Qual. Missing	0.004	0.014	0.058	0.012	0.045	0.101	0.014	0.056	0.106	0.013	0.028	0.107
Manager	—	—	—	0.171	0.331	0.279	—	—	—	0.083	0.222	0.232
Professional	—	—	—	0.278	0.306	0.335	—	—	—	0.080	0.193	0.209
Associate Prof. / Technical	—	—	—	0.343	0.475	0.366	—	—	—	0.086	0.280	0.248
Clerical or Secretarial	—	—	—	0.207	0.349	0.290	—	—	—	0.751	0.357	0.324
Craft or Skilled Service	0.388	0.386	0.342	—	—	—	0.082	0.206	0.178	—	—	—
Personal or Protective Serv.	0.011	0.107	0.068	—	—	—	0.034	0.209	0.105	—	—	—
Sales	0.029	0.203	0.089	—	—	—	0.415	0.422	0.236	—	—	—
Operative or Assembly	0.512	0.384	0.350	—	—	—	0.329	0.458	0.201	—	—	—
Other Occupation	0.059	0.255	0.180	—	—	—	0.141	0.344	0.268	—	—	—
Black	0.007	0.025	0.076	0.009	0.060	0.081	0.018	0.111	0.102	0.009	0.043	0.087
Indian Subcontinent	0.019	0.067	0.120	0.017	0.060	0.116	0.020	0.094	0.116	0.014	0.071	0.104
Fixed Term/Temp. Contract	0.026	0.079	0.143	0.045	0.171	0.177	0.043	0.113	0.186	0.047	0.115	0.188
Health Problems	0.071	0.119	0.241	0.067	0.144	0.225	0.057	0.130	0.206	0.040	0.091	0.178
Married	0.786	0.266	0.369	0.738	0.281	0.390	0.705	0.338	0.399	0.699	0.285	0.413
Dependent Children	0.146	0.204	0.320	0.171	0.259	0.331	0.129	0.229	0.299	0.156	0.247	0.324
Children Data Missing	0.021	0.097	0.129	0.015	0.083	0.110	0.011	0.060	0.092	0.017	0.047	0.120
Work Mainly Female	0.007	0.138	0.055	0.056	0.183	0.182	0.533	0.391	0.396	0.476	0.350	0.424
Work Only Female	—	—	—	—	—	—	0.079	0.185	0.228	0.106	0.253	0.256
Part-Time Job	—	—	—	—	—	—	0.490	0.408	0.332	0.208	0.258	0.354
Number of Observations	1,357			805			441			783		
Mean Number of Individuals	7.1			4.9			4.4			4.9		

the third row.[17] The differentials in row 4 are equivalent but calculated from weighted means. They do not differ very much from the unweighted figures, except for female, non-manual workers, for whom the unweighted calculation (7%) appears to understate the raw differential in the overall population (10%). The wage means show the expected pattern: men earn more than women and non-manual workers earn more than manual workers. Looking at the unweighted differentials more closely, we see that, for example, on average, manual male members earn 14% more than non-members and manual female members earn 19% more than manual female non-members.

The last panel of Table 3 presents estimates from simple bivariate regressions of the log wage on a constant and the membership variable. The first row shows the estimated coefficient from a regression of the establishment mean wage on establishment proportionate membership and a constant, that is, the between estimator. The second row shows the estimated coefficient from a fixed effects model of the individual wage as a function of individual membership, that is, the mean difference between members' and non-members' wages within the same establishment.

The between estimates are generally much larger than the within estimates, with the exception of female non-manuals.[18] These simple decompositions of the raw data therefore constitute some evidence that observed membership differentials may be driven partly by a higher union membership in high-paying establishments.

[17]For non-manual men and women, the small discrepancies are due to rounding error in the subtraction.

[18]For an indication of whether these differences are statistically significant, we estimated a random effects specification and performed a Hausman test of it against the fixed effects model. The results suggested that the differences are significantly different at conventional levels for all groups except non-manual men and women. (The latter finding is not surprising, as the between and within estimates are almost identical.)

Sample Characteristics

Table 4 reports summary statistics, which reveal considerable differences in observed characteristics across the four union-covered subgroups. The definitions of these variables are given in the Appendix. The descriptive statistics in Table 4 are unweighted, since our purpose is to evaluate the means and variability of the estimating sample rather than make inferences about the population. For the individual-level variables in Panel (a) of Table 4, we present the between- and within-workplace standard deviations. These show a good deal of variability along both dimensions. Manual men are, on average, the oldest (42.2 years), with the longest job tenure (10.9 years), and non-manual women are the youngest (37.9 years), with the shortest time on the job (7.4 years).

Within the manual and non-manual groups, there are some striking differences between men and women. For example, non-manual women are less than half as likely as non-manual men to have a degree, and a large majority (75%) are found in clerical or secretarial occupations. The professional and associate professional categories predominate, on the other hand, among non-manual men. The figures for manual workers also suggest that women are in less skilled jobs: only 16.7% have a vocational qualification, and nearly 75% are in sales or lesser-skilled operative positions. Half of manual men, by contrast, have a vocational qualification and just under 40% have skilled craft jobs (compared to 8.2% of manual women). Part-time work is twice as common among manual women as among non-manual women (40.8% against 20.8%).

Turning to the workplace-level variables in Panel (b) of Table 4, we see that manual women are much less likely to be covered by multiple-union bargaining (the proportion of workplaces is 0.44, compared to more than 0.6 for the other three subgroups). Around a quarter of their workplaces are in the wholesale and retail sector, against about 10% in the other subgroups. The proportion of the work force

(B) Workplace-Level Variables—Means

Variable	*Male Manual*	*Male Non-Manual*	*Female Manual*	*Female Non-Manual*
Closed Shop	0.111	0.073	0.089	0.081
Multiple Unions	0.611	0.677	0.436	0.650
Manufacturing (SIC 1)	0.479	0.274	0.475	0.288
Electrical (SIC 2)	0.179	0.287	0.000	0.250
Construction (SIC 3)	0.021	0.012	0.000	0.013
Wholesale and Retail (SIC 4)	0.116	0.085	0.267	0.081
Hotels and Restaurants (SIC 5)	0.016	0.000	0.000	0.000
Transport, Storage, and Communications (SIC 6)	0.142	0.140	0.119	0.113
Financial Intermediation (SIC 7)	0.021	0.116	0.050	0.150
Real Estate, Renting, Business (SIC 8)	0.005	0.030	0.020	0.038
Public Administration and Defense (SIC 9)	0.000	0.000	0.000	0.000
Education (SIC 10)	0.000	0.006	0.010	0.013
Health and Social Work (SIC 11)	0.000	0.030	0.050	0.044
Other Social and Personal Services (SIC 12)	0.021	0.018	0.010	0.013
Workplace Size	515	512	425	449
UK Organization Size	30393	30227	47176	31400
UK Organization Size Missing	0.037	0.030	0.059	0.019
Proportion Part-Time	0.101	0.100	0.249	0.112
Proportion Female	0.248	0.333	0.457	0.393
Proportion Manual	0.666	0.378	0.602	0.346
Imperfect Competition	0.484	0.402	0.426	0.394
Domestic Market	0.558	0.561	0.594	0.550
Tight Labor Market	0.221	0.256	0.287	0.238
East	0.042	0.037	0.059	0.031
East Midlands	0.074	0.073	0.079	0.081
London	0.037	0.110	0.069	0.113
North East	0.063	0.043	0.059	0.038
North West	0.153	0.134	0.139	0.138
Scotland	0.132	0.104	0.099	0.106
South East	0.126	0.140	0.129	0.119
South West	0.095	0.122	0.089	0.106
Wales	0.053	0.043	0.040	0.031
West Midlands	0.121	0.110	0.139	0.106
Yorkshire and Humberside	0.105	0.079	0.089	0.125
Number of Observations	190	164	101	160

Notes, Panel (A): The age and tenure figures were calculated by taking the midpoint of each reported band. The upper age band (60 years or more) was set to 65 years; the upper tenure band (10 years or more) was set to 17.5 years.

Notes, Panel (B): UK organization size applies to workplaces that are part of a larger organization (subsample sizes are 160, male manual; 147, male non-manual; 80, female manual; 143, female non-manual).

The UK organization size figures were calculated by taking the midpoint of each reported band. The upper size band (100,000 employees or more) was set to 200,000.

that is part-time is also much higher for manual women than for the other groups.

In summary, our preliminary analysis of the data suggests that there are significant differences in wage premia between occupational groups and that covered members appear to earn more than covered nonmembers, especially when no account is taken of between-establishment variation. We now turn to the multivariate analysis in order to estimate the union membership wage premium controlling for other factors, including workplace effects.

The Estimates for Private Sector Men and Women

Consider the wage equation

$$(1) \qquad w_{ij} = x_{ij}'\beta + z_j'\delta + \gamma U_{ij} + \phi_j + \varepsilon_{ij},$$

where w_{ij} is the natural logarithm of hourly wages for the i^{th} union-covered worker at the j^{th} workplace; x_{ij} is a vector of individual characteristics determining wages; β is the associated parameter vector; z_j is a vector of workplace characteristics with associated parameter vector δ; U_{ij} is a dummy variable equal to one if individual i at establishment j is a union member and zero otherwise; and γ is our parameter of interest, the effect of union membership on wages. Unobservable influences on wages are captured by a workplace effect ϕ_j and a random error ε_{ij}. Note that a full specification would include an additional unobserved effect associated purely with the individual. With cross-sectional data, however, such an effect cannot be identified, as individuals are not observed to move between firms. When the effect is omitted, as in (1), δ_j combines the "true" workplace effect and the workplace mean of individual unobserved characteristics. The random effect ε_{ij} then includes the deviation of the individual effect from ϕ_j. For a more detailed discussed of these issues relating to matched employer-employee data, see Abowd and Kramartz (1999).[19]

The vector x_i contains individual variables assumed to influence human capital formation (including training incidence in the last 12 months, age, tenure, highest educational qualification, race, occupation, and marital status).[20] We have argued that some of these variables—training, tenure, and temporary job status, for example—are also likely to be related to membership status, and so are important controls. The vector z_j contains workplace characteristics like size and the proportions of female and part-time workers. We also control for one-digit industry and region of workplace, as well as imperfect competition in the product market and a low unemployment to vacancy ratio in the labor market.[21] Finally, we include dummies indicating the presence of a *de facto* closed shop and multiple trade unions to proxy union power.[22] Table A1 in the Appendix defines the variables and their source.

The dependent variable in (1) is the log of hourly wages. As noted above (under "Data and the Key Variables") and in the Appendix, we do not observe the hourly wage, but only the upper and lower bounds of this wage that are particular to each individual. One could infer the true wage

[19]Although the union coverage/non-coverage wage differential is not the focus of the present paper, the reader may be interested in knowing our estimated coefficients of the coverage/non-coverage wage effects for each of the four subgroups (with SEs in parentheses): 0.096 (0.027) for male manual workers; 0.050 (0.028) for male non-manual workers; 0.050 (0.025) for female manual workers; and 0.054 (0.023) for female non-manual workers.

[20]There is no information on individuals' work experience.

[21]Wages are likely to be higher in firms or industries in imperfectly competitive product markets than in firms or industries operating under perfect competition, to the extent that there is rent-sharing of the surpluses generated by imperfectly competitive product markets. For this reason we include controls for product market competition obtained from the Management Questionnaire, as described in the Appendix.

[22]Although closed shops are illegal in Britain, some managers reported that pre- or post-entry closed shops were present, or that management recommended that workers join the union. We combined responses to these questions into the single "*de facto* closed shop" variable.

Table 5. The Effect of Individual Membership on Wages—Interval Regression.

	Male Manual			*Male Non-Manual*			*Female Manual*			*Female Non-Manual*		
Independent Variable	*Pooled [1]*	*Workplace Dummies [2]*	*Modeled Workplace Effect [3]*	*Pooled [1]*	*Workplace Dummies [2]*	*Modeled Workplace Effect [3]*	*Pooled [1]*	*Workplace Dummies [2]*	*Modeled Workplace Effect [3]*	*Pooled [1]*	*Workplace Dummies [2]*	*Modeled Workplace Effect [3]*
Trade Union Member	0.067**	0.034*	0.035	0.042*	0.024	0.024	0.017	–0.016	–0.015	0.054**	0.035*	0.036
	(2.52)	(1.66)	(1.39)	(1.90)	(1.11)	(1.05)	(0.52)	(0.47)	(0.39)	(2.45)	(1.65)	(1.63)
Training Incidence	0.046***	0.022	0.021	0.038	–0.006	–0.005	–0.006	–0.032	–0.031	0.017	0.002	0.003
	(2.66)	(1.55)	(1.41)	(1.35)	(0.25)	(0.18)	(0.15)	(0.99)	(0.76)	(0.72)	(0.09)	(0.12)
Fixed Term or Temp.	–0.007	0.067	0.065	–0.111*	–0.113**	–0.112	0.115*	0.154**	0.158*	–0.085**	–0.062	–0.062*
	(0.11)	(1.62)	(1.24)	(1.75)	(2.45)	(1.63)	(1.70)	(2.27)	(1.71)	(2.39)	(1.40)	(1.69)
Closed Shop	0.033		0.042	–0.035		–0.005	0.040		0.023	0.045		–0.053
	(0.73)		(0.97)	(0.65)		(0.09)	(0.83)		(0.44)	(0.93)		(0.98)
Multiple Unions	0.127***		0.092***	–0.044		–0.059	0.087		0.112**	0.000		0.048
	(3.80)		(2.80)	(0.97)		(1.28)	(1.58)		(2.06)	(0.01)		(1.30)
Tenure 1–2 Yrs.	–0.002	0.024	0.024	0.066	0.076**	0.076*	0.010	0.014	0.024	0.030	0.062*	0.059
	(0.05)	(0.71)	(0.58)	(1.62)	(2.10)	(1.66)	(0.15)	(0.25)	(0.37)	(0.74)	(1.76)	(1.32)
Tenure 2–5 Yrs.	0.053	0.062**	0.062*	0.083***	0.102***	0.105***	0.073	0.028	0.030	0.064**	0.073**	0.071**
	(1.46)	(2.16)	(1.75)	(2.62)	(3.06)	(3.13)	(1.44)	(0.59)	(0.56)	(1.97)	(2.34)	(2.15)
Tenure 5–10 Yrs.	0.086**	0.080***	0.080**	0.128***	0.142***	0.140***	0.140***	0.107**	0.112*	0.149***	0.174***	0.170***
	(2.20)	(2.82)	(2.41)	(3.49)	(4.15)	(3.55)	(2.72)	(2.13)	(1.79)	(5.08)	(5.05)	(5.46)
Tenure > 10 Yrs.	0.125***	0.113***	0.112***	0.130***	0.143***	0.143***	0.124**	0.135**	0.134**	0.129***	0.158***	0.156***
	(3.29)	(4.04)	(3.35)	(4.12)	(4.11)	(3.94)	(2.50)	(2.43)	(2.37)	(4.44)	(4.51)	(4.93)
Constant	1.010***	1.099***	0.682***	1.501***	1.708***	1.200***	1.454***	2.104***	1.413***	1.302***	1.669***	0.994***
	(5.26)	(4.91)	(2.80)	(9.90)	(6.78)	(7.24)	(9.61)	(8.45)	(4.40)	(10.64)	(7.20)	(4.35)
Observations	1,357	1,357	1,357	805	805	805	441	441	441	783	783	783

Notes: Dependent variables are the upper and lower bounds of the log (hourly wage), constructed as described in the Appendix. Estimation is by unweighted interval regression. Asymptotic *t*-ratios (adjusted for workplace clustering in methods [1] and [3]) are in parentheses.

Other controls are six age dummies, six education dummies, 1-digit occupation and industry, workplace size, region, race, presence of health problems, marital status, dependent children, UK organization size if the workplace is not independent, work done mainly/only by women, proportions of part-time, female, and manual workers, indicators of imperfect product market competition, domestic product market and tight labor market, and part-time status.

*Statistically significant at the 10% level; **at the 5% level; ***at the 1% level.

from this information by taking the midpoint of the wage interval (as we did for the raw data in "Data and the Key Variables"), and then estimate (1) by ordinary least squares (OLS). However, Stewart (1983) demonstrated that such a procedure is likely to produce inconsistent estimates.[23] Best practice in this case is to use the interval regression technique, whereby, on the assumption that the error term is normally distributed, it is possible to write down the contribution of each individual observation to a likelihood function. The likelihood function can then be maximized by standard software routines (for example, intreg in Stata). As a robustness check, and a feasible way of obtaining between-workplace estimates, we also ran OLS equations.

We proceed with the interval regression by three methods. We first estimate equation (1) on pooled individual data for each subsample. The coefficient estimates from this method are consistent under the assumption that the regressors are not correlated with the error term $\phi_j + \varepsilon_{ij}$.[24] However, if union members are more likely to be found in high-paying workplaces because of the selection mechanisms discussed above, then this positive correlation between the membership dummy U_{ij} and ϕ_j will bias the estimate of γ upward.

To control for all workplace-level influences on individual wages, we therefore add workplace dummy variables:

$$w_{ij} = x_{ij}'\beta + \gamma U_{ij} + D_j'\alpha + \varepsilon_{ij}. \tag{2}$$

The vector D_j contains the set of workplace dummies (where the k^{th} dummy takes the value 1 if $k = j$ and zero otherwise) associated with a vector of parameters α. In this specification, all observable (z_j) and unobservable (ϕ_j) workplace characteristics are captured by the dummies. Identification of the membership premium then comes from within-workplace variation only, eliminating the across-workplace selection problem. Equation (2) is a fixed-effects (FE) specification. While this is an appealing strategy, it is generally robust only when the model is estimated by linear regression techniques. With the non-linear methods required for interval regression (necessary because of the banded earnings data), the coefficient estimates will be inconsistent (see Arellano and Honore 2001, Section 4). In practice, the bias may be small, especially if the bands are quite narrow. Nevertheless, to guard against the possibility that the results are an artifact of the estimation method, we use a third and alternative technique that also exploits within-workplace variation. Here, following Chamberlain (1984), we model ϕ_j as a linear combination of workplace means,[25]

$$\phi_j = \bar{x}_j'\kappa + \lambda \bar{U}_j + \eta_j, \tag{3}$$

where $\bar{x}_j'$ and $\bar{U}_j$ are the means of individual characteristics and union membership, calculated over all observed individuals in the workplace, κ and λ are parameters, and η_j is the unexplained part of the workplace effect, assumed to be orthogonal to the workplace means. With the workplace means added to (1), the estimates of β and γ are now based on deviations of the variables from their means. If the estimates of κ and λ are jointly statistically significant, then this model is preferred over the simple pooled model (1).

Table 5 reports the results obtained from estimation using the three methods described above for each subsample.[26] The full set of controls is listed in the notes

[23]Meaning that the coefficient estimates generally do not approach the true parameter values as more observations are added to the sample.

[24]Observations on individuals in the same workplace are not independent, as they have the workplace effect ϕ_j in common, so we adjust the standard errors to account for this clustering.

[25]For a "fixed effects" application, see Kawaguchi (2002), and for a random effects application, see Arulampalam, Booth, and Taylor (2000).

[26]The equation is only specified over the covered sector. Initial estimation indicated there were statistically significant differences between the β vectors in the covered and uncovered sectors, and this finding is consistent with several other studies—for example, Blakemore et al. (1986), Budd and Na (2000), and Hildreth (1999).

Table 6. The Effect of Workplace Membership on Wages—Between-Workplace Estimates.

Description	*Male Manual*	*Male Non-Manual*	*Female Manual*	*Female Non-Manual*
Proportion of Union Members	0.177**	0.083	0.216*	0.102*
	(2.45)	(1.39)	(1.87)	(1.70)
Observations (Workplaces)	190	164	101	160
R-Squared	0.72	0.80	0.86	0.74

The dependent variable is the workplace mean of the log of the midpoint of the hourly wage interval, constructed as described in the Appendix. In parentheses are *t*-ratios.

Other controls are dummies for training incidence, fixed term or temporary contract, closed shop, and the presence of multiple trade unions, four tenure dummies, six age dummies, six education dummies, 1-digit occupation and industry, workplace size, region, race, presence of health problems, marital status, dependent children, UK organization size if the workplace is not independent, work done mainly/only by women, proportions of part-time, female, and manual workers, indicators of imperfect product market competition, domestic product market and tight labor market, and part-time status.

*Statistically significant at the 10% level; **at the 5% level; ***at the 1% level.

under the table, and the full set of estimates is available from the authors on request.

First consider the estimates in Table 5 obtained from the pooled model using method [1] and presented in the first column for each subsample. A male manual worker who is a union member earns exp(0.067) – 1 = 6.9% more than a comparable non-member. This coefficient is well defined with a t-statistic of 2.5. For male non-manuals, the membership effect is exp(0.042) – 1 = 4.3%, but this is statistically significant only at the 10% level. For female manuals, the effect is exp(0.017) – 1 = 1.7%. This is a tiny effect that is statistically insignificant. Finally, a female non-manual union member earns around exp(0.054) – 1 = 5.5% more than a comparable non-member (t = 2.5).

Thus for only two subgroups—manual men and non-manual women—is there evidence of a membership premium from the cross-sectional pooled estimates. The manual male and non-manual female premia are of a magnitude similar to that of the ordinary least squares (OLS) estimates of other studies. For example, using pooled British data over 1991–94, Hildreth (2000) found an OLS membership premium of 7% for full-time men and women. Andrews et al. (1998), in their replication of alternative specifications using the first wave of the BHPS, found a premium of 9.4% for full-time male manual workers when using a specification reasonably similar to ours.[27]

Before discussing the FE estimates, we report the results of investigating whether or not wages are higher in workplaces where more covered workers are members. In Table 6 we present our OLS "between" estimates of the wage impact of the establishment-level proportion of union members. As noted above, a caveat applies, because these estimates are based on the log of the midpoint of the wage interval.[28] The results do, however, suggest that higher wages are associated with higher covered density in all subgroups except non-manual men. For example, a 10 percentage point rise in membership among covered manual men would be associated with a wage increase of exp(0.018) – 1 = 1.9%. For manual and non-manual women, the same increase would raise wages by 2.4% and by 1.1%, respectively, though these effects are only significant at the 10% level.

[27]The sample included uncovered workers, but they allowed all parameters to vary for the covered subgroup with respect to the uncovered workers. The control vector differed somewhat, however, and the wage measure was weekly earnings, not hourly earnings.

[28]Between estimates cannot be obtained by interval regression, as one cannot simply take the mean of the wage intervals.

We now return to Table 5, where we report the within-workplace estimates of the membership impact on wages. The estimates based on a dummy variable for each establishment—method [2]—are given in the second column for each subsample, while those based on modeling ϕ_j as a linear combination of workplace means—method [3]—are given in the third column for each subsample. The latter estimates are, as we noted above, identified by the deviations of the independent variables from their means.

For each of the four subsamples, the estimates of the union membership premium obtained from methods [2] and [3] are remarkably similar. Moreover, they are considerably smaller than the estimates obtained using the pooled approach of method [1]. For manual men, for example, method [2] produces an estimated effect of $\exp(0.034) - 1 = 3.5\%$, while the effect from method [3] is $\exp(0.035) - 1 = 3.6\%$. This compares with an estimated effect of 6.9% from method [1]. For non-manual men the estimated membership coefficient is now 0.024 from methods [2] and [3], as compared with an estimated coefficient of 0.042 from method [1]. For non-manual women, the point estimate falls from 0.054 by method [1] to 0.035 and 0.036 by methods [2] and [3]. In all four estimations by method [3], the coefficients on the workplace means are jointly significantly different from zero. This model is, then, preferred over the simple pooled model. Because method [2] (addition of workplace dummies) may produce inconsistent estimates, method [3] is also preferred overall. In practice, however, the within estimates are remarkably robust with respect to the method used.[29]

These differences between the estimates obtained from method [1] and the within-workplace methods suggest that workplace fixed effects are important in explaining why reliance on method [1] results in the finding of a membership premium. In a recent reassessment of union wage effects in Britain using the WERS98 data but very different estimation techniques, Bryson (2002) also concluded that workplace characteristics explain a large part of the membership premium. Using a propensity score estimator to match individuals on individual and workplace characteristics, he found no statistically significant premium.

Conclusion

The free-rider hypothesis rests on the assumption that any union-bargained wage is available to all covered workers whether or not they are members. Empirical studies that have found a substantial membership wage premium imply, on the contrary, that workers have a positive incentive to join the union and therefore the free rider problem disappears. Using a new data source representing British workplaces and their employees, we have shown that union-covered private sector male manual workers and female non-manual workers who are union members do indeed appear to earn 5.5–7% more than comparable non-members. However, once unobservable workplace-level influences on wages are controlled for by exploiting the workplace-employee linked nature of the data, we find that the premium falls sharply and is no longer statistically significant. The reduction is

[29]As a check on the robustness of our results, we also estimated the model by alternative methods. The alternatives were (i) interval regression with weighted log-likelihood function to allow for the non-random selection of observations; and (ii) within estimation using the midpoint of each earnings band and OLS. Our results for membership remained unchanged. We also added the part-time men to the sample (47 manual and 4 non-manual workers). Unsurprisingly, the point estimates were almost identical for non-manual men. For manual men, method [1] produced a membership coefficient of 0.045, statistically significant at 10%, and method [3] produced an estimate of 0.016, which was statistically insignificant at conventional levels. We noted above that the subsample of part-time men appears very different from the full-timers. It seems that the membership effect is substantially weaker among this subsample, and that this reduces the estimate for the combined sample. Note, however, that the within estimate still shows a sharp fall relative to the pooled estimate.

particularly dramatic for male, manual workers.

These results highlight the different possible selection mechanisms and the need to examine workplace-level pay determination in more detail. They suggest that previous estimates of the union membership wage premium for covered workers are an artifact arising from unobserved workplace differences being correlated with union membership, a pattern for which we have outlined some potential explanations. Our results also indicate that—for Britain at least—it is premature to suggest that "a rethinking of the 'free rider' literature is warranted." Moreover, the puzzle noted in the literature surveyed at the start of this paper—as to why non-members in the covered sector abstain from membership when the wage gains from membership were so large—is not a puzzle, at least for our data. Instead, it seems likely that covered individuals base their membership decisions on their valuation of other benefits associated with membership, resulting in heterogeneous outcomes. Individuals are therefore not behaving irrationally in their union membership decisions, since they get the covered wage regardless of their membership decision. Those workers who do join the union are likely to be attracted by other, non-wage benefits.

Appendix

Table A1
Variable Definitions and Sources

Employee Questionnaire		
log (hourly wage)		See below.
Trade Union Member	=	1 if employee is a member of a trade union (TU) or staff association.
Fixed Term or Temporary	=	1 if job is temporary or for a fixed term.
Tenure Dummies		Derived from banded responses. Total years working at this workplace.
Age Dummies		Derived from banded responses.
Education Dummies		Highest educational qualification. Derived from categorical responses. Omitted category is CSE or equivalent.
Vocational Qualification	=	1 if employee holds any recognized vocational qualifications (for example, NVQ).
Voc. Qual. Missing	=	1 if information on vocational qualification is missing.
Training Incidence	=	1 if employee received any employer-financed training away from normal place of work in last 12 months.
Black	=	1 if employee is black (Caribbean, African or other).
Indian Subcontinent	=	1 if employee is Indian, Pakistani, or Bangladeshi.
Non-White	=	1 if employee is non-white.
Occupational Dummies		Derived from categorical responses (1-digit SOC). Omitted category is other occupations.
Health Problems	=	1 if employee has long-standing health problems or disabilities that limit activity at work, home, or in leisure.
Married	=	1 if employee is living with spouse or partner.
Dependent Children	=	1 if employee has dependent children under 5 years.
Children Info Missing	=	1 if information on children is missing.
Part Time	=	1 if employee works less than 30 hours per week.
Work Mainly Women	=	1 if respondent's type of work is mainly done by women.
Work Only Women	=	1 if respondent's type of work is only done by women.
Female Work	=	1 if respondent's type of work is mainly or only done by women.
Management Questionnaire		
Industry Dummies		Derived from 1-digit SIC92 codes.
Closed Shop	=	1 if any employees in the workplace have to be union members to get or keep their jobs; or if management strongly recommends membership.
Multiple Unions	=	1 if more than one union is recognized by management for negotiating pay and conditions for any section of the work force in this establishment.
UK Org. Size Dummies		Derived from size of UK organization if the workplace is part of a larger organization.
UK Org. Size Missing	=	1 if UK organization size is missing and the workplace is part of a larger organization.
Imperfect Competition	=	1 if 5 or fewer competitors or organizations dominate the product market.
Domestic Market	=	1 if the market for the main product is local, regional, or national.
***Employee Profile Questionnaire*—Completed by Management Respondent before Interview**		
Size Dummies		Derived from total number of employees at the workplace.
Proportion Part-Time		Proportion of employees working less than 30 hours per week at the work place.
Proportion Female		Proportion of female employees at the workplace.
Proportion Manual		Proportion of employees in manual occupations at the workplace.
Additional Data		
Tight Labor Market	=	1 if the unemployment to vacancy rate ratio in travel-to-work-area ≤ 3.
Regional Dummies		Derived from Government Office Regions.

Derivation of the Hourly Wage

The banded hourly wage measures were derived from the following questions about banded earnings (D11) and hours (A3, A4, and A5) in the Employee Questionnaire.

D11 "How much do you get paid for your job here, before tax and other deductions are taken out? If your pay changes before tax from week to week because of overtime, or because you work different hours each week, think about what you earn on average."

Earnings bands:
Less than £50 per week / less than £2,600 per year
£51–£80 per week / £2,601–£4,160 per year
£81–£140 per week / £4,161–£7,280 per year
£141–£180 per week / £7,281–£9,360 per year
£181–£220 per week / £9,361–£11,440 per year
£221–£260 per week / £11,441–£13,520 per year
£261–£310 per week / £13,521–£16,120 per year
£311–£360 per week / £16,121–£18,720 per year
£361–£430 per week / £18,721–£22,360 per year
£431–£540 per week / £22,361–£28,080 per year
£541–£680 per week / £28,081–£35,360 per year
£681 or more per week / £35,361 or more per year

A3 "How many hours do you usually work each week, including any overtime or extra hours?"

A4 "How many overtime or extra hours do you usually work each week, whether paid or unpaid?"

A5 "Are you normally paid or given time off later when you work overtime or extra hours?"

From D11, we constructed variables as follows. Let WAGE1 = the lower weekly earnings bound of the selected band; for the bottom band, set WAGE1 equal to 0.01 (a trivially small value) and let WAGE2 = the upper weekly earnings bound of the selected band; for the top band, set WAGE2 equal to missing. Now define WAGE = (WAGE2 – WAGE1) / 2—the midpoint of the earnings band. (For the top band, assume WAGE2 = £750.)

We now define TOTHRS as total hours as reported in response to Question A3, and OVRTMHRS as hours of overtime from Question A4. From A5 we know whether or not the respondent was paid for any overtime hours worked. Assuming an overtime premium of 1.5, which is the rate widely used in Britain, we then calculate effective paid hours as

$$\text{EFCTVHRS} = (\text{TOTHRS} - \text{OVRTMHRS}) + 1.5 * \text{OVRTMHRS} \quad \text{if paid for overtime,}$$

and

$$\text{EFCTVHRS} = \text{TOTHRS} \quad \text{if not paid for overtime.}$$

Our dependent variable in the earnings equation is then

$$\text{Lower bound of log (hourly wages)} = \log (\text{WAGE1} / \text{EFCTVHRS})$$
$$\text{Upper bound of log (hourly wages)} = \log (\text{WAGE2} / \text{EFCTVHRS})$$
$$\text{Midpoint of log (hourly wages)} = \log (\text{WAGE} / \text{EFCTVHRS})$$

REFERENCES

Abowd, John M., and Francis Kramartz. 1999. "The Analysis of Labor Markets Using Matched Employer-Employee Data." In Orley Ashenfelter and David Card, eds., *Handbook of Labor Economics,* Vol. 3. Amsterdam: Elsevier Science.

Andrews, Martin J., Mark B. Stewart, Joanna K. Swaffield, and Richard Upward. 1998. "The Estimation of Union Wage Differentials and the Impact of Methodological Choices." *Labour Economics,* Vol. 5, pp. 449–74.

Arellano, Manuel, and Bo Honore. 2001. "Panel Data Models: Some Recent Developments." In James J. Heckman and Leamer Edward, eds., *Handbook of Econometrics,* Vol. 5. Amsterdam: Elsevier Science, pp. 3229–96.

Arulampalam, Wiji, Alison L. Booth, and Mark P. Taylor. 2000. "Unemployment Persistence." *Oxford Economic Papers,* Vol. 52 (January), pp. 24–50.

Barth, Erling, Oddbjorn Raaum, and Robin Naylor. 2000. "Union Wage Effects: Does Membership Matter?" *Manchester School,* Vol. 68, No. 3 (June), pp. 259–75.

Blakemore, Arthur E., Janet C. Hunt, and B. F. Kiker. 1986. "Collective Bargaining and Union Membership Effects on the Wages of Male Youths." *Journal of Labor Economics,* Vol. 4, No. 2 (April), pp. 193–211.

Boeri, Tito, Agar Brugavini, and Lars Calmfors, eds. 2001. *The Role of the Unions in the Twenty-First Century.*

London: Oxford University Press (June).

Booth, Alison L. 1985. "The Free Rider Problem and a Social Custom Model of Trade Union Membership." *Quarterly Journal of Economics,* Vol. 100, No. 1 (February), pp. 253–61.

____. 1991. "What Do Unions Do Now? A Study of the Provision by British Trade Unions of Benefits and Services to their Members." *Labor Studies Journal,* Vol. 16, No. 2 (Summer), pp. 50–64.

____. 1995. *The Economics of the Trade Union.* Cambridge: Cambridge University Press.

Booth, Alison L., and Monojit Chatterji. 1995. "Union Membership and Wage Bargaining When Membership Is Not Compulsory." *Economic Journal,* Vol. 105 (March), pp. 345–60.

Boyer, George R. 1988. "What Did Unions Do in Nineteenth-Century Britain?" *Journal of Economic History,* Vol. 48, No. 2 (June), pp. 319–32.

Bryson Alex. 2002. "The Union Membership Wage Premium: An Analysis Using Propensity Score Matching." Centre for Economic Performance Discussion Paper No. 530, London School of Economics.

Budd, John W., and In-Gang Na. 2000. "The Union Membership Wage Premium for Employees Covered by Collective Bargaining Agreements." *Journal of Labor Economics,* Vol. 18, No. 4 (October), pp. 783–807.

Cameron, A. Colin, and Pravin K. Trivedi. 2002. "Microeconometrics: Methods and Applications." Mimeo, University of California–Davis.

Chamberlain, Gary. 1984. "Panel Data." In Zvi Griliches and Michael D. Intriligator, eds., *Handbook of Econometrics,* Vol. 2. Amsterdam: Elsevier Science, pp. 1247–1318.

Cully, Mark, Stephen Woodland, Andrew O'Reilly, and Gill Dix. 1999. *Britain at Work.* London: Routledge.

Forth, John, and Simon Kirby. 2000. *Guide to the Analysis of the Workplace Employee Relations Survey 1998.* London: NIESR, WERS98 Data Dissemination Service (http://www.niesr.ac.UK/niesr/wers98).

Forth, John, and Neil Millward. 2000. "The Determinants of Pay Levels and Fringe Benefit Provision in Britain." Discussion Paper No. 171, National Institute for Economic and Social Research, London.

Freeman, Richard B., and James L. Medoff. 1984. *What Do Unions Do?* New York: Basic Books.

Hildreth, Andrew K. G. 1999. "What Has Happened to the Union Wage Differential in Britain in the 1990s?" *Oxford Bulletin of Economics and Statistics,* Vol. 61, No. 1, pp. 5–31.

____. 2000. "Union Wage Differentials for Covered Members and Nonmembers in Great Britain." *Journal of Labor Research,* Vol. 21, No. 1 (Winter), pp. 133–47.

Hundley, Greg. 1993. "Collective Bargaining Coverage of Union Members and Nonmembers in the Public Sector." *Industrial Relations,* Vol. 32, No. 1, pp. 72–93.

Hunt, Janet C., B. F. Kiker, and Glyn C. Williams. 1987. "The Effect of Union Type on Member-Nonmember Wage Differentials." *Journal of Labor Research,* Vol. 8, No. 1 (Winter), pp. 59–65.

Jones, Ethel B. 1982. "Union/Nonunion Differentials: Membership or Coverage?" *Journal of Human Resources,* Vol. 17, No. 2 (Spring), pp. 276–85.

Kawaguchi, Daiji. 2002. "Compensating Wage Differentials among Self-Employed Workers: Evidence from Job Satisfaction Scores." Mimeo, Osaka University. Forthcoming in *Labour Economics.*

Metcalf, David, Kirstine Hansen, and Andy Charlwood. 2000. "Unions and the Sword of Justice: Unions and Pay Systems, Pay Inequalities, Pay Discrimination, and Low Pay." Discussion paper No. DP0452, Centre for Economic Performance, April.

Millward, Neil, Alex Bryson, and John Forth. 2000. *All Change at Work.* London: Routledge.

Naylor, Robin. 1989. "Strikes, Free Riders, and Social Customs." *Quarterly Journal of Economics,* Vol. 104, No. 4 (November), pp. 771–86.

Olson, Mancur, Jr. 1965. *The Logic of Collective Action.* Cambridge, Mass.: Harvard University Press.

Reilly, Kevin T. 1996. "Does Union Membership Matter? The Effect of Establishment Union Density on the Union Wage Differential." *Review of Economics and Statistics,* Vol. 78, No. 3 (August), pp. 547–57.

Schumacher, Edward J. 1999. "What Explains Wage Differences between Union Members and Covered Nonmembers?" *Southern Economic Journal,* Vol. 65, No. 3 (January), pp. 493–512.

Stewart, Mark B. 1983. "On Least Squares Estimation When the Dependent Variable Is Grouped." *Review of Economic Studies,* Vol. 50, No. 4 (October), pp. 737–53.

Willman, Paul. 1990. "The Financial Status and Performance of British Trade Unions, 1950–1988." *British Journal of Industrial Relations,* Vol. 28, No. 3 (September), pp. 313–27.