The Union Wage Premium in the US and the UK

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Abstract

This paper presents evidence of both counter-cyclical and secular decline in the union membership wage premium in the US and the UK over the last couple of decades. The premium has fallen for most groups of workers, the main exception being public sector workers in the US. By the beginning of the 21st Century the premium remained substantial in the US but there was no premium for many workers in the UK. Industry, state and occupation-level analyses for the US identify upward as well as downward movement in the premium characterized by regression to the mean. Using linked employer-employee data for Britain we show estimates of the membership premium tend to be upwardly biased where rich employer data are absent and that OLS estimates are higher than those obtained with propensity score matching.

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The decline in union density in the United States and the United Kingdom has prompted some commentators to wonder whether unions matter anymore. In particular, there has been speculation that the intensification of competition since the 1980s, coupled with a diminution of union bargaining strength, has prevented unions from obtaining the sort of wage premium they achieved in the past. It is evident that unions are not as central to the economy as they used to be, but union decline is not apparent everywhere: many employers continue to contend with strong unions, raising important questions about union effects in those sectors.

This paper estimates trends in the union wage premium over the last few decades in the UK and the US. We identify both counter-cyclical and secular decline in the union membership wage premium in the US and the UK over the last couple of decades. The premium has fallen for most groups of workers, the main exception being public sector workers in the US. By the beginning of the 21st Century the premium remained substantial in the US but there was no premium for many workers in the UK. Industry, state and occupation-level analyses for the US identify upward as well as downward movement in the premium characterized by regression to the mean. We also indicate the need for some caution in interpreting the magnitude of the premium estimated using standard OLS techniques. Using linked employer-employee data for Britain, we show estimates of the membership premium tend to be upwardly biased where rich employer data are absent and that OLS estimates are higher than those obtained with propensity score matching.

Background

In his definitive empirical work, H. Gregg Lewis (1986) found that the overall impact of unions in the US economy was approximately 15 per cent and showed relatively little variation across years – varying between 12 per cent and 19 per cent between 1967 and 1979. Subsequent work confirmed constancy of the differential until the 1990s. For example, Hirsch and his co-authors have produced a series of papers estimating changes in the differential over time and concluded there has been some decline in the premium in recent years (e.g. Hirsch, Macpherson and Schumacher, 2002; Hirsch and Schumacher, 2002; Hirsch and Macpherson, 2002). Bratsberg and Ragan (2002) come to similar conclusions in their examination of private sector union wage differentials. They also conclude that dispersion in the wage premium across industries has substantially declined as the US economy has become more competitive.

Counter-cyclical movement in the union wage premium may occur when unions can protect their members from downward wage pressures workers in general face when market conditions are unfavorable (Freeman and Medoff, 1984). Conversely, when demand for labor is strong, employees rely less on unions to bargain for better wages because market rates rise anyway. A second factor that might induce counter-cyclical movement in the premium is the length of union contracts relative to non-union ones, which means union wages are less responsive to the cycle. However, if the union premium comes from employers sharing rents, it is plausible that the premium will be higher when those rents are higher, in which case the wage gap would be pro-cyclical. Empirical evidence suggests pro-cyclical movement in union wages in the 1970s (Moore and Raisian, 1980; Grant, 2001), disappeared in the 1980s (Grant, 2001; Wunnava and Okunade, 1996). Taking a longer-time frame through to 1999, Bratsberg and Ragan (2002) find clear evidence of a counter-cyclical union wage premium.

A factor that should reduce the cyclical sensitivity of the union wage premium is the cost-of-living-adjustment (COLA) clauses in union contracts that increase union wages in response to increases in the consumer price level. According to Freeman and Medoff (1984: 54), despite a dramatic rise in COLA coverage in the 1970s, their wage estimates for manufacturing suggest that COLA provisions "contributed only a modest amount to the rising union advantage" in the 1970s. Bratsberg and Ragan (2002) revisit this issue and find an increased sensitivity of the premium to the cycle that they attribute, in part, to reduced COLA coverage from the late 1980s.

The recent spate of studies that have looked at the impact of union membership on wages in the UK has been occasioned by a growing belief that the union wage premium may be falling. Some argue that a decline in the average union premium is consistent with diminishing union influence over pay setting. There is certainly evidence pointing in that direction. First, case studies suggest the scope of bargaining has narrowed substantially in companies that continue to bargain with unions (Brown et al., 1998). Second, pay settlements in the private sector by the end of the 1990s were no greater where trade unions were involved than in their absence (Forth and Millward, 2000a). Third, even where managers say employees have their pay set through workplace-level or organization-level collective bargaining, union representatives and officials are either not involved or are only consulted in a minority of cases (Millward, Forth and Bryson, 2001). But there is also evidence to the contrary. For example, unions continue to have a substantial effect on pay structures, bringing up the wages of the lowest paid and thus narrowing pay differentials across gender, ethnicity, health and occupation (Metcalf, Hansen, and Charlwood, 2001). These studies, which indicate union effects despite substantial declines in union density, might suggest that those unions that have survived are the stronger and, as such, better able to command a wage premium (thus raising the "batting average" of unions).

The consensus in the earlier literature is that the mean union wage gap was approximately 10 per cent (Blanchflower, 1999). Despite the rapid decline in union density experienced in the UK since 1979, the gap remained roughly constant from 1970 – the year for which the earliest estimate is available (Shah, 1984) – to 1995 (see Blanchflower, 1999).[1] However, while the union effect was persisting, the premium declined for some workers (Blanchflower, 1999; Hildreth, 1999). Hildreth (1999: 7) argues that stability in the union premium for blue-collar male workers in 1991-95 compared with a declining premium for their white-collar counterparts may reflect their respective abilities to maintain their bargaining power. The picture emerging from research through to 1998/99 is suggestive of a more widespread decline in the premium. Machin's (2001) analysis of longitudinal data from the British Household Panel Survey 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. Booth and Bryan (2004) using linked employer-employee data for 1998 also find no significant wage premium. Forth and Millward (2002b) find the premium was confined to workers in workplaces with high bargaining coverage or multiple unions.

On the basis of this evidence for the UK, it is difficult to establish what has happened to the trend in the premium over time because, as others note (Andrews et al., 1998; Lanot and Walker, 1998: 343) there have been no studies estimating the premium over the 1980s and 1990s with a consistent methodology and comparable data.

Trends in the union wage premium in the United States

Table 1 presents estimates of the wage gap using separate log hourly earnings equations for each of the years from 1973 to 1981 using the National Bureau of Economic Research's (NBER) May Earnings Supplements to the Current Population Survey (CPS) and for the years

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[1] However, there is some dispute on this question. Establishment-level analyses indicate that the union wage premium in the early 1980s was most evident where unions were strong, as indicated by the presence of a closed shop (Stewart, 1987). This premium seems to have declined in the second half of the 1980s, a trend which has been attributed to a decline in the incidence and impact of the closed shop, coupled with unions' inability to establish differentials in new workplaces (Stewart, 1995).

since then using data from the NBER's Matched Outgoing Rotation Group (MORG) files of the CPS.[2] For both the May and the MORG files a broadly similar, but not identical, list of control variables is used, including a union status dummy, age and its square, a gender dummy, education, race and hours controls plus state and industry dummies.

(Table 1 near here)

The first column of Table 1 reports time-consistent estimates of the union wage premium for the union coefficient in log hourly earnings equations for the total sample whereas the second and third columns report them for the private sector. Results obtained by Hirsch and Schumacher (2002) with a somewhat different set of controls are reported in the final column of the table.[3] The time series properties of all three of the series are essentially the same.

The wage gap averages between 17 and 18 percent over the period, and is similar in size in the private sector as it is in the economy as a whole. What is notable is the high differential in the early-to-mid 1980s and a slight decline thereafter, which gathers pace after 1995, with the series picking up again as the economy started to turn down in 2000.

(Figure 1 near here)

Figure 1 plots the point estimates of the US whole economy and private sector union wage premia, taken from the first and second columns of Table 1, against unemployment for 1973-2002. The premium moves counter-cyclically.

The US private sector union wage premium by worker type.

(Table 2 near here)

[2] Table 1 is taken from Blanchflower and Bryson (2003a) which also contains full details of problems regarding data imputation in CPS. The May extracts of the CPS extracts in Stata format from 1969-1987 are available from the NBER at http://www.nber.org/data/cps_may.html. There was no CPS survey with wages and union status in 1982.

[3] For a discussion of the reason for these differences, see Blanchflower and Bryson (2003a).

Table 2 presents union wage gaps obtained from estimating a series of equations for sub-groups of private sector employees since the mid-1970s. To ensure large sample sizes we pooled together six successive May CPS files from 1974-1979 and compare those to wage gaps estimated for the years 1996-2001 using data from the Matched Outgoing Rotation Group (MORG) files of the CPS. Two points stand out from these analyses. First, no group of workers in the private sector sample has experienced a substantial increase in their union premium. Clearly, unions have found it harder to maintain a wage gap over time. Second, with the exception of the manual/nonmanual gap, those with the highest premiums in the 1970s saw the biggest falls, so there has been some convergence in the wage gaps. Nevertheless, with the exception of the most highly educated and non-manual workers, the wage premium remains around 10 percent or more.

The US public sector union wage premium by worker type.

By 2001, public sector unions accounted for 44 percent of all union members in the US compared with 32.5 percent in 1983.

(Table 3 near here)

Table 3 is comparable to Table 2 for the private sector in that it presents disaggregated union wage gap estimates but, due to data constraints, the base period is from the early 1980s. Because sample sizes in the public sector are small using the May CPS files we use data from the ORG files of the CPS for the years 1983-1988 for comparison purposes with the 1996-2001 data.[4] The private sector union wage gap has fallen over the two periods (21.5 percent to 17.0 percent) whereas a slight *increase* was observed in the public sector (13.3 percent to 14.5

[4] Data for the years 1979-1982 could not be used, as no union data are available. A further advantage of the 1983-1988 data is that information is available on individuals whose earnings were allocated who were then excluded from the analysis.

percent, respectively). Furthermore, the majority of the worker groups in the public sector experienced increases in their union wage premium over the period.

Industry, Occupation and State-Level Wage Premia[5]

The conventional assumption is that unions can procure a wage premium by capturing quasi-rents from the employer (Blanchflower et al., 1996). If this is so, there must be rents available to the firm arising from its position in the market place, and unions must have the ability to capture some of these rents through their ability to monopolize the firm's labor supply. Individual-level data can tell us little about these processes. Instead, the literature has concentrated on industry-level wage gaps. In this section we model the change in the union wage premium at three different units of observation: industry, state, and occupation.

Industries. We used our data to estimate separate results for 44 two digit industries for 1983-1988 and 1996-2001.[6] In contrast to the analysis by worker characteristics, which reveal near universal decline in the premium – at least in the private sector – we found that the wage gap rose in 17 industries and declined in 27. The decline in the wage gap for the whole economy, presented earlier, is due to the fact that the industries experiencing a decline in their wage gap make up a higher percentage of all employees than those experiencing a widening gap. The results are similar to those presented by Bratsberg and Ragan (2002) who found that, over the period 1971-1999, the regression-adjusted wage gap closed in 16 industries and increased in 16 others.

The gap rose by more than ten percentage points in autos (+12 percent) and leather (+19 percent). It declined by more than twenty percentage points in other agriculture (-33 percent)

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[5] See Blanchflower and Bryson (2004) for full details on the estimations used in this section.

[6] We chose these years as it was possible to define industries identically using the 1980 industry classification.

retail trade (-20 percent) and private households (-29 percent). Many of the industries experiencing a fall in the union premium between 1983 and 2001 would have been subject to intensifying international trade (such as textiles, apparel and furniture) but this was equally true for those experiencing rising premiums (such as machinery, electrical equipment, paper, rubber and plastics, leather).

Some of the biggest declines in the premium have been concentrated in the three industries with more than a 10 percent share in private sector union membership in 2002. In construction and transport, which both make up an increasing proportion of all private sector union members, the premium fell by around 10 percentage points. In retail trade, where the share of private sector union membership has remained roughly constant at 10 percent, the premium fell 20 percentage points.

(Table 4 near here)

To explore these changes in the private sector industry union wage premium over time we ran panel fixed effects estimates. Our first step was to estimate separate first-stage regressions for each of our industries in each year from 1983-2001 with the dependent variable the log hourly wage along with controls for union membership, age, age squared, male, 4 race dummies, the log of hours, and 50 state dummies. Three sectors with very small sample sizes (toys, tobacco, and forestry and fisheries) were deleted. We extracted the coefficient on the union variable, giving us 19 years * 42 industries or 798 observations in all. The coefficient on the union variable was then turned into a wage gap taking anti-logs, deducting 1 and multiplying by 100 to turn the figure into a percentage. We used the ORG files to estimate the proportion of workers in the industry who were union members both in the private sector and overall and mapped that onto the file. Unemployment rates at the level of the economy are used as industry-specific rates are not meaningful: workers move a great deal between industries and considerably more than they do between states.

Regression results, reported in columns 1 and 2 of Table 4, estimate the impact of the lagged premium, lagged unemployment, and a time trend on the level of the industry-level wage premium. The number of observations is 756 as we lose 42 observations in generating the lag on the wage premium and the union density variables.

In the unweighted equation in column (1) the lagged premium is positively and significantly associated with the level of the premium the following year indicating regression to the mean. Unemployment and the time trend are not significant. However, once the regression is weighted by the number of observations in the industry in the first-stage regression, (column (2)) lagged unemployment is positive and significant, indicating counter-cyclical movement in the premium, while the negative time trend indicates secular decline in the premium.

Bratsberg and Ragan (2002) reported that the industry-level premium was influenced by a number of other variables.[7] In particular they found that COLA clauses reduced the cyclicality of the union premium and that increases in import penetration were strongly associated with rising union premiums. They also found some evidence that industry deregulation had mixed effects. Their main equations (their Table 2) did not include a lagged dependent variable. Table 5 reports results using their data for the years 1973-1999 using their method and computer programs that they kindly provided to us. Column 1 of Table 5 reports the results they reported in column 2 of their Table 2. Column 2 reports our attempt to replicate their findings. We are

[7] Bratsberg and Ragan (2002) also use CPS data. But their analysis differs in several ways. First, they assess trends over the period 1971-1999 whereas we present trends over the period 1983-2001. Second, we adjust for wage imputation as recommended by Hirsch and Schumacher (2002) whereas Bratsberg and Ragan do not. Third, specifications producing the regression-adjusted estimates differ somewhat. Fourth, the samples differ. In particular, Bratsberg and Ragan exclude government workers, and they present results for some different industries. Fifth, their wage premium relates to weekly wages whereas all of our estimates are derived from (log) hourly wages.

unable to do so exactly but there are several similarities – we find import penetration both in durables and nondurables, COLA clauses, deregulations in communications, and the unemployment rate all have positive and significant effects. We also found, as they did, that deregulation in finance lowered the premium. In contrast to Bratsberg and Ragan, however, the inflation rate and the two interaction terms with the unemployment rate were insignificant. The model is rerun in column 3, but without the insignificant interaction term. A linear time trend is added in column 4: this is negative and significant, and eliminates the COLA effect and the negative effect of deregulation in the finance sector. Column 5 adds the lagged union wage premium, which is positive and significant. Its introduction makes inflation positive and significant. In columns (6) to (8) models are run without the four insignificant deregulation dummies. Column (6) indicates that using an unweighted regression, the size of the lagged premium effect drops markedly and the time trend and inflation lose significance, showing these results are sensitive to the weighting of the regression. The smaller coefficient on the lagged dependent variable is unsurprising given that there is much less likely to be variation in the union wage gap estimates in industries with large sample sizes that have higher weights in the former case. We are able to confirm Bratsberg and Ragan's finding that the unemployment rate, deregulation in communications, and import penetration in both durables and nondurables have positive impacts on the premium but not the findings on COLA, inflation, or any of the other deregulations identified.

That import penetration in durable and nondurable goods sectors increases the premium suggests that union wages are more resilient than nonunion wages to foreign competition. Import penetration is likely correlated with unmeasured industry characteristics that depress the premium inducing a negative bias that is removed once industry characteristics are controlled for. Import penetration has likely reduced demand for union and nonunion labor, with union wages holding up better than nonunion wages, but at the expense of reduced union employment. There are theoretical and empirical reasons as to why this might occur. For instance, since union wages tend to be less responsive to market conditions generally, union wages may be sluggish in responding to increased import competition. Alternatively, industries characterized by "end-game" bargaining may witness perverse union responses to shifts in product demand as the union tries to extract maximum rents in declining industries (Lawrence and Lawrence, 1985). Another possibility is that increased import penetration reduces the share of union employment in labor -intensive firms and increasing it in capital-intensive firms. Greater capital intensity reduces elasticity of demand for union labor, allowing rent maximizing unions to raise the premium (Staiger, 1988).

It is not obvious that weights should be used if we regard each industry as a separate observation. Columns (1) to (6) are GLS estimates accounting for potential correlation in error terms. Column (7) switches to a weighted OLS and shows that results are not sensitive to the switch. The unweighted OLS in column (8) gives broadly the same results as the unweighted GLS in column (6). Taking off the weights has a much bigger effect than switching from GLS to OLS.

Furthermore, the industries defined by Bratsberg and Ragan are *very* different in size. Some industries are very broadly defined – for example industry 32 Services covers SIC codes 721-900 whereas tobacco covers one SIC code (#130). Retail trade averaged 19,075 observations. Column 9 of Table 5 illustrates the sensitivity of the results to industry exclusions. It is exactly equivalent in all respects to column 5 of Table 5 except that it drops the 32 observations from retail trade. The lagged dependent variable falls dramatically from .60 to .32.

The COLA variable is now significantly *positive* while the inflation variable moves from being significantly positive to insignificant. The unweighted results (not reported) are little changed. Bratsberg and Ragan's results appear to be sensitive to both the use of weights and the sample of industries used.

States. A similar procedure was adopted to estimate state-level premia over time for the 50 states plus Washington D.C. We compare results using merged samples of the CPS's MORG for 1983-1988 and 1996-2001 files.[8] The mean state union wage gap was 23.4 percent between 1983 and 1988, falling to 17.2 percent in 1996-2001. The premium fell in all but five states. The premium only rose markedly in Maine, where it increased 9 percentage points (from 7 percent to 16.1 percent). We then ran 969 separate first-stage regressions, one for each state in each year from 1983-2001 with the dependent variable the log hourly wage along with controls for union membership, age, age squared, male, 4 race dummies, the log of hours, and 44 industry dummies. The sample was restricted to the private sector. We extracted the coefficient on the union variable, giving us 19 years * 51 states (including D.C.), 969 observations in all. We then mapped to that file the unemployment rate in the state-year cell.[9] Once again we ran a series of second-stage regressions where the dependent variable is the one-year level of the premium (obtained by taking anti-logs of the union coefficient and deducting one) on a series of RHS variables including the lagged premium and lagged unemployment and union density rates.[10]

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[8] The source of the data is the Union Membership and Coverage Database which is an Internet data resource providing private and public sector union membership, coverage, and density estimates compiled from the Current Population Survey (CPS) using BLS methods. Economy-wide estimates are provided beginning in 1973; estimates by state, detailed industry, and detailed occupation begin in 1983; and estimates by metropolitan area begin in 1986. The *Database*, constructed by Barry Hirsch (Trinity University) and David Macpherson (Florida State University), is updated annually. The *Database* can be accessed at http://www.unionstats.com.

[9] Source: http://data.bls.gov/labjava/outside.jsp?survey=la.

[10] We experimented with both the level of the unemployment rate and the log and the latter always worked best.

Results are reported in columns (3) and (4) of Table 4. The number of observations is 918 - we lose 51 observations in generating the lag on the wage premium and the union density variables. Both unweighted and weighted results are presented where the weights are total employment in the state by year. Controlling for state fixed effects with 50 state dummies we find that with an unweighted regression (column (3)), the lagged premium is positive and significant, as it was at industry level. Again, as in the case of industry-level analysis, the effect is apparent when weighting the regression (column (4)). The positive, significant effect of lagged state-level unemployment confirms the counter-cyclical nature of the premium - the effect is apparent whether the regression is weighted or not. There is also evidence of a secular decline in the state-level premium, but only where the regression is unweighted.[11]

Occupations. Finally, we moved on to estimate wage gaps at the level of the occupation pooling six years of data for each of the time periods 1983-1988 and 1996-2001. In each case we used files from the Outgoing Rotation Group files of the CPS. Out of the 44 groups, 13 showed increases in the size of the differential over time while the remainder had decreases. We used the same method described above for industries and states, with occupations defined in a comparable way through time. Columns (5) and (6) of Table 4 show that whether the occupation-level analysis is weighted or not, there is clear evidence of regression to the mean, with the lagged premium positive and significant, as well as evidence of a secular decline in the premium. A significant counter-cyclical effect is evident when the regression is weighted, but not in the unweighted regression.

[11] State fixed effects account for state-level variance in union density where the effect is fixed over time. However, Farber (2003) argues that there remain potential unobserved variables which simultaneously determine density and wages, but which are time-varying, and thus not picked up in fixed effects, which might bias our results.

In all three units of observation– industry, state, and occupation – there is evidence that the private sector premium moves counter-cyclically and that it has been declining over time. In all three cases the lagged level of the premium entered significantly positively and was larger when the weights were used than when they were not. The size of the lag was greatest when industries were used as the unit of observation and least when occupations were used. Translating the results from levels into changes – that is by deducting t-1 from both sides – leaves all of the other coefficients unchanged. Using the weighted results in Table 4 the results reported below imply mean convergence.

State level	$\Delta \text{Premium}_{t\text{ - }t\text{-}1}$	=	$-.7949\text{Premium}_{t\text{-}1}$
Industry level	$\Delta \text{Premium}_{t\text{ - }t\text{-}1}$	=	$-.6457\text{Premium}_{t\text{-}1}$
Occupation level	$\Delta \text{Premium}_{t\text{ - }t\text{-}1}$	=	$-.8254\text{Premium}_{t\text{-}1}$

The *higher* the level of the premium in the previous period the *lower* the *change* in the next period.

Trends in the union wage premium in the United Kingdom

Table 6 presents the union membership wage premium over the period 1985-2002. Column 1 estimates the premium for the UK since 1993 using the Labour Force Survey (LFS), while column 2 estimates it for Britain since 1985 using the British Social Attitudes Surveys (BSAS). Both series are based on standard specifications for each separate year (details are contained in the Data Appendix available on request). In identifying the union effect over time, we make what we think is the reasonable assumption that any bias in our estimates arising through unobserved heterogeneity is constant over time.

The LFS estimates tend to be above the BSAS estimates, but in both series there has been a decline in the log hourly union wage premium since 1994 (with the BSAS estimate for 1997 being an outlier, perhaps due to the much smaller sample that year).[12] Although the premium remains roughly 10 per cent in the 2000 LFS, it falls to a statistically insignificant 5 per cent in BSAS 2000, and falls even further in 2001. However, it recovers to a statistically significant 6.4% in 2002 as unemployment rises, further evidence of counter-cyclical movement in the premium which is brought out more clearly in Figure 2.

(Tables 6 and Figure 2 near here)

Trends by worker type.

In analyses not presented here we find a large fall in the wage premium across most types of worker, indicated by the sub-group regressions (see Blanchflower and Bryson, 2003a for details). In 1993, analyses of LFS indicate only one group of employees (the highly educated) had a premium well below 10 per cent. In 2000, all but three out of the 17 types of worker had a premium below 10 per cent. Those worse affected were manufacturing workers, men, private sector workers and non-whites, all of whom had no significant premium by 2000. Results are similar when using BSAS data. In 1993-95, only two types of worker (non-manuals and the highly qualified) had a union premium of less than 10 per cent. By 1999-2001, eleven types of worker had a premium of less than 10 per cent. For five types of worker (men, younger workers, those in the private sector, non-manuals, and the highly educated) the membership premium was no longer statistically significant.

The impact of data richness and estimation method on the magnitude of the union membership wage premium

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[12] The estimate for 1997 is smaller (13%) when the data are weighted. Further support for the proposition that the BSAS 1997 point estimate is an outlier comes from the authors' calculations of the log hourly wage premium using the same methodology (unweighted estimates of the mid-point earnings) for individual level data from the Workplace Employee Relations Survey 1998, the fieldwork for which spanned 1997 and 1998. The raw membership premium is .226 (25.4%). This shifts with the addition of controls as follows: + demographics = .121; + job = .114; + establishment = .076; + geographical = .091. In short, these estimates point to a premium of around 10% in 1997/98.

Our knowledge of the size of the union membership wage premium in the US and the UK derives largely from analyses of individual and household survey data. There will be an upward bias in these estimates, induced by the paucity of employer controls in such data, if unionized employers are better payers than non-unionized employers. This deficiency in employer controls is addressed directly in this section with linked employer-employee data from the Workplace Employee Relations Survey 1998 (WERS). As well as information on individual employees' union membership, WERS contains rich information on the employer, including workplace-level union density and pay bargaining arrangements for occupations within the workplace. The regression coefficient for the union membership dummy in an OLS can be interpreted as the causal effect of union membership on wages if the variables entering the regression equation account fully for endogenous selection into membership status. This requires very informative data. We assess the sensitivity of results to this assumption by varying the information set entering the estimation – first utilizing individual-level data only, and then introducing workplace-level data.

An alternative to OLS to control for bias on observables is the semi-parametric statistical matching approach known as propensity score matching (PSM) (Rosenbaum and Rubin, 1983; Heckman et al., 1999) which compares wage outcomes for unionized workers with 'matched' non-unionized workers. The method shares the causal identification assumption of the OLS in that it yields unbiased estimates of the treatment impact where differences between individuals affecting the outcome of interest are captured in their observed attributes (the conditional independence assumption, or CIA).[13] However, matching has three distinct advantages relative to regression in identifying an unbiased causal impact of membership on wages. First, it is semi-

[13] For a full description of the PSM technique and its application in this case see Bryson (2002).

parametric, so it does not require the assumption of linearity in the outcome equation. Second, it leaves the individual causal effect completely unrestricted so heterogeneous treatment effects are allowed for and no assumption of constant additive treatment effects for different individuals is required. Thirdly, matching estimators highlight the problem of common support and thus the short-comings of parametric techniques which involve extrapolating outside the common support (Heckman, et al., 1998). The appropriateness of the CIA is dependent on having data that account for selection into membership. As in the case of the OLS estimates, the sensitivity of results to data quality is assessed by altering the information set entering estimation.

Table 7 reports OLS estimates of the union membership wage premium in the private sector as a whole and for coverage, gender and occupational sub-groups. Using individual-level data only, the estimated membership premium for the whole private sector is 15% (the exponentiated coefficient for union member in column 1). Column 2 introduces data collected from the employer. The employer variables are jointly significant and improve the model fit. What is more, they reduce the membership premium by over half to 6.1%. This pattern, whereby the premium estimated using individual-level only data is substantially reduced with the introduction of workplace-level data, is repeated across the sub-group analyses. The impact of workplace-level data is particularly marked for men and manual workers. These findings suggest that, at least in the British case, OLS estimates of the membership premium based on individual-level and household survey data are upwardly biased because some of the positive wage effect attributed to membership is actually due to members being employed at better paying workplaces. There are many possible reasons why union workplaces might be better payers than non-union workplaces. Unions may target organizing efforts on employers with the biggest rents to share. ‘Better’ employers may chose to unionize to create stable firm-employer conditions conducive to investment in human capital. Alternatively, if union members are 'better' workers than their non-member counterparts in ways unobservable to the analyst but observable to employers, members may be able to sort themselves into the best employers, or may be chosen by the best employers (Abowd et al., 1999).

These analyses for the whole private sector condition on whether the individual is located in a workplace where the employer engages with a union in pay bargaining, as well as on union density at the workplace where the employee works.[14] However, if the biggest component of any membership premium is that generated by collective bargaining, the premium should be much smaller where the sub-sample consists solely of workers in covered occupations. In general, all these workers should benefit from pay bargaining, unless employers discriminate between members and non-members. In fact, the OLS estimates for covered occupations presented in row 2 differ little from those for the whole private sector. Again, the size of the premium falls substantially once account is taken of workplace heterogeneity, but it remains sizeable and statistically significant at 6.7%. On this evidence, the membership premium among covered workers, evident in other recent studies using individual-level data only (for the US, Schumacher, 1999, Budd and Na, 2000; and for Britain, Hildreth, 2000), persists having accounted for workplace heterogeneity.

Intriguingly, the 14% of employees who are members in uncovered occupations receive a similar membership premium of 5.7% when the OLS is run with individual and workplace-level controls. However, almost three-quarters (71%) of these members are located in workplaces

[14] The full union-nonunion wage differential combining membership and coverage is obtained by exponentiating the sum of the membership and workplace-level union recognition coefficients exp(.059 + -.018) = 4.2%. The union recognition dummy is never statistically significant in the equations presented in Table 7, but wages rise with union density.

where other workers have their pay set through collective bargaining. This suggests that these members benefit from the spillover effects of collective bargaining at their workplace.[15]

The most striking evidence that union membership effects are heterogeneous comes from analyses by broad occupation. Running analyses for manual and non-manual employees separately, results confirm those from other studies in showing a larger membership premium among manual workers (Booth, 1995; Forth and Millward, 2002b). Indeed, with the introduction of workplace controls, non-manual workers are the only group of workers for whom the OLS estimates do not produce a statistically significant membership premium.

Table 8 presents the PSM analyses. These are run on identical samples to those used in the OLS estimates in Table 7. The sample sizes are smaller than those appearing in Table 7 because, in the process of matching members to their nearest neighbors, PSM leaves many non-members out of the estimation sample (the Data Appendix, available on request, contains details). In addition, a small number of members have no support in the non-member population, so it is not possible to estimate membership effects for this subset. Fortunately this group tends to be small, ranging between 3%-6% in most cases. This means common support is not a problem, so PSM can estimate the effect of membership on members for nearly all of the member population. As in the case of the OLS estimates in Table 7, PSM estimates are run with individual controls only, and with individual plus workplace controls.

The results are striking. The first row in column 1 shows the membership premium based on matching with individual data alone is estimated to be 8.9%. When workplace data are used in the matching this premium disappears and is even negatively signed. As in the case of the OLS estimates in Table 7, the introduction of workplace-level data always reduces the membership premium, confirming the potential for upward bias in estimates based on individual-

[15] Forth and Millward (2002b) find evidence of such spillover effects in their analysis of WERS.

level data – whether estimated via OLS or PSM. But, in stark contrast to the OLS estimates in Table 7, there is no significant membership premium for any type of employee where matching is based on individual and workplace-level data. The premia are always statistically significant if one runs OLS on the matched data (see Data Appendix for details), so one can discount sample size differences and common support enforcement as reasons for differences in the OLS and PSM results. Rather, the OLS estimates are upwardly biased due to the linear functional form assumption. Of course, it is arguable that the OLS models are simply misspecified and that results could be reconciled through the addition of appropriate interaction terms. In practice, this requires a great deal of trial and effort and, in any event, OLS will still be linear, albeit in non-linear covariates. This is illustrated by the fact that the OLS-generated premium remains large and significant across coverage, gender and broad occupation, three dimensions where one is most likely to find heterogeneous membership effects. Yet, in each case, the PSM-generated premia are not significant. In estimates not shown, the membership premium for uncovered workers where matching is based on individual and workplace-level data is –2.4%. It is true that, at 6.9%, the PSM estimate of the premium for manual workers comes close to statistical significance (with the bootstrapped 95% confidence interval presented in the Data Appendix only just straying into negative territory). So, if there was a membership premium for anyone in Britain in 1998, it was for manual workers.

Table 9 presents four sensitivity analyses. The first row reproduces results from Tables 7 and 8 for the 'baseline' estimates. The first two sensitivity analyses involve alterations to the *X* vector used in the OLS estimation and estimation of the propensity score. The third sensitivity analysis estimates effects on weekly wages, as opposed to hourly wages. The fourth involves splitting the analyses according to union strength at the workplace employing the worker. In each case, the first column presents results from the OLS using individual-level and workplace-level controls. The second column presents the PSM results, and the third column presents the OLS results run on the matched data.

Although there is a sizeable union wage premium literature conditioning on bargaining coverage and union density, it is at least arguable that density and union recognition are endogenous with respect to membership in that these workplace features are, in part, a function of individuals' decisions to unionize. A comparison of results in row 2 Table 9 with the whole private sector estimates in row 1 shows the premium estimates rise in the absence of density and recognition controls. However, the pattern of results remains the same, with OLS producing sizeable and statistically significant premia, whereas the PSM estimate is small and statistically non-significant.

Workplace training and workplace tenure may also be endogenous with respect to membership, which is why they were omitted from the earlier estimates. Their inclusion in row 3 of Table 9 makes no difference at all to the PSM estimates, and very little difference to the OLS using matched data. The premium estimated with OLS on unmatched data falls a little.

There is the potential for measurement error in the hourly earnings measure and estimates of the wage premium can differ across hourly and weekly earnings measures due to different working patterns of members and non-members. Row 4 in Table 9 therefore shows the sensitivity of results to the use of a weekly earnings measure. Again, the pattern of results is largely unchanged.

Empirical evidence for Britain (Stewart, 1987) and the United States (Schumacher, 1999) indicates that the union premium is higher where union density is higher. This may be because a higher incidence of 'free-riding' can weaken union bargaining strength, or else causation may work the other way if the incentive to join a union is higher where the union commands a larger premium. Splitting the analysis into employees working in lower and higher density workplaces offers some support for the proposition that the membership premium is higher where the union is stronger. Using OLS to estimate the membership effect on unmatched data, the premium is much larger among employees in workplaces with 50%+ density than it is among those located in workplaces with less than 50% density. There is also a differential using PSM although the premium is not significant in either case.

Across all these sensitivity analyses, OLS identifies a sizeable and statistically significant membership premium whereas PSM finds no significant premium, supporting the main conclusion from the baseline analyses. In the case of OLS and PSM, the introduction of rich employer controls substantially reduces the size of the membership premium, sometimes rendering it statistically non-significant. Using the same data set Booth and Bryan (2004) came to a similar conclusion having controlled for workplace fixed effects. The two studies confirm the importance of controlling for workplace heterogeneity in accurately estimating the union membership wage premium.

Conclusions

The union membership wage premium has been higher in the US than the UK in the last couple of decades. In both countries the premium was untrended in the years up to the mid-1990s, but it has fallen since then. Much of this is due to counter-cyclical movement and thus, as we might expect, the premium rose with unemployment in both countries in 2001 and 2002 after a number of years of decline. However, we also find clear evidence in the US of a secular decline in the premium. Even so, in 2002, the premium in the US economy was 16.5%, just a little below the 17.1% average for the period 1973-2002. In the private sector it was 1 percentage point above the average of 17.6% for the period. In the UK, on the other hand, there are real questions as to whether there is a significant union wage premium for workers at the beginning of the 21st Century. Standard OLS estimates of the premium show no statistically significant premium for many types of worker. The analyses for 1998 using linked employer-employee data suggest that LFS and BSAS estimates of the premium in the UK may even overstate the size of the premium, as do the analyses using PSM instead of OLS.

In the US and the UK the premium has fallen for virtually all types of private sector employee, with those with the largest premiums at the outset suffering the greatest declines. These include more vulnerable workers such as the lower educated and women, raising questions about unions' ability to bid up the wages of those who with lower marginal productivity and those who may be earning below their marginal product as a result of discrimination or labor market segmentation. The picture was very different when we estimated the US premium at the level of industry, state and occupation. The premium went up in many industries and occupations, and down in others, but again there was regression to the mean.

US analyses for the public sector revealed quite a different picture. Here the premium rose a little and did so for all types of public sector worker.

What are the implications for trade unions? The size of the premium in the US might suggest that the benefits of membership, net of dues and other costs, remain sizeable. So why has density been declining in the private sector? One possibility is that the premium comes at the cost of union jobs – evidence for the US and the UK shows unionized establishments grow at a slower rate than non-unionized establishments (Blanchflower, et al., 1991; Leonard, 1992; Bryson, 2004). Unionized companies face greater competition from nonunion employers at a time when increasing price competitiveness means employers are less able to pass the costs of the premium onto the consumer. Declining union density, by increasing employers' opportunities to substitute nonunion products for union products, fueled this process. So too did rising import penetration: if imports are nonunion goods, regardless of US union density, they increase the opportunity for nonunion competition. These pressures have increased the employment price of any union wage premium. A second possibility – not inconsistent with the first – is that the costs of membership have risen, most notably through increasing employer opposition to union organizing (Kleiner, 2002). That opposition may even be fuelled, in part, by the size of the wage premium if employers might view it as the price tag attached to successful union organizing campaigns. Either way, it is clear that unions' relative success in the bargaining arena is not going to bring about a reversal in union fortunes. In the UK, the problem is that unions are struggling to procure any premium for members. At a time when the new cohort of employers has turned away from unions (Bryson, Gomez and Willman, 2004), raising the costs of employees joining unions, this dip in the premium means a further reduction in the net benefits of membership, making it increasingly difficult for unions to recruit new members.

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Table 1. *Union Wage Gap Estimates for the United States, 1973-2002 (%)*
(excludes workers with imputed earnings)

	All Sectors	Private Sector	Private Sector
Year	Blanchflower/Bryson	Blanchflower/Bryson	Hirsh/Schumacher
1973	14.1	12.7	17.5
1974	14.6	13.8	17.5
1975	15.1	14.3	19.2
1976	15.5	14.6	20.4
1977	19.0	18.3	23.9
1978	18.8	18.6	22.8
1979	16.6	16.3	19.7
1980	17.7	17.0	21.3
1981	16.1	16.3	20.4
1983	19.5	21.2	25.5
1984	20.4	22.4	26.2
1985	19.2	21.0	26.0
1986	18.8	20.1	23.9
1987	18.5	20.0	24.0
1988	18.4	19.1	22.6
1989	17.8	19.2	24.5
1990	17.1	17.6	22.5
1991	16.1	16.6	22.0
1992	17.9	19.2	22.5
1993	18.5	19.6	23.5
1994	18.5	18.2	25.2
1995	17.4	18.0	24.5
1996	17.4	18.4	23.5
1997	17.4	17.7	23.2
1998	15.8	16.1	22.4
1999	16.0	16.9	22.0
2000	13.4	14.3	20.4
2001	14.1	15.1	20.0
2002	16.5	18.6	
1973-2002 average	17.1	17.6	22.4

Note: See Data Appendix for details of sample and controls.

Table 2. *Private Sector Union/Nonunion Log Hourly Wage Differentials, 1974-1979 and 1996-2001, in Percent*

	1974-1979	1996-2001
Men	19	17
Women	22	13
Ages 16-24	32	19
Ages 25-44	17	16
Ages 45-54	13	14
Ages >=55	19	16
Northeast	14	11
Central	20	15
South	24	19
West	23	22
< High school	33	26
High school	19	21
College 1-3 years	17	15
College >=4 years	4	3
Whites	21	16
Non-white	22	19
Tenure 0-3 years	20	20
Tenure 4-10	16	15
Tenure 11-15	10	11
Tenure 16+	17	8
Manual	30	21
Non-manual	15	4
Manufacturing	16	10
Construction	49	39
Services (excl. construction)	34	16
Private sector	21	17

Note: See Data Appendix for details of samples and controls.

Table 3. *Union Wage Differentials in the Public Sector, in Percent*

	1983-1988	1996-2001
Private	22	17
Public	13	15
Federal	2	8
State	9	10
Local	16	20
Male	8	10
Female	17	16
Age <25	28	23
Age 25-44	13	15
Age 45-54	8	11
Age >=55	13	14
New England	17	17
Central	16	16
South	10	12
West	10	13
<High School	26	18
High School	15	13
College 1-3	13	11
College >= 4 Years	8	11
Whites	13	14
Non-whites	15	16
Manual	18	18
Non-manual	13	14
Registered Nurses (95)	5	6
Teachers (156-8)	15	21
Social workers (174)	12	12
Lawyers (178)	5	17
Firefighters (416-7)	15	19
Police & correction officers (418-424)	16	18

Notes: sample excludes individuals with allocated earnings. Controls and data as in Table 2.

Table 4. Industry, State, and Occupation Level Analysis of the Private Sector Union Wage Premium, 1983-2001

	(1)	(2)	(3)	(4)	(5)	(6)
Level of Analysis	Industry	Industry	State	State	Occupation	Occupation
Premium$_{t-1}$	.2584*	.3453*	.2051*	.2366*	.0907*	.1746*
	(.0367)	(.0350)	(.0337)	(.0333)	(.0379)	(.0374)
Unemployment rate$_{t-1}$	.6333	.5866*	.4373*	.5366*	.3799	.5823*
	(.4035)	(.2821)	(.1449)	(.1175)	(.5084)	(.2900)
Time	-.0463	-.2344*	-.1547*	-.0651	-.3419*	-.2416*
	(.1056)	(.0762)	(.0468)	(.0379)	(.1343)	(.0788)
State/industry/occupation dummies	50	50	41	41	41	41
Weighted by # obs at 1st stage	No	Yes	No	Yes	No	Yes
R^2	.6187	.7749	.5071	.5861	.7345	.8453
N	756	756	918	918	756	756

Source: Outgoing Rotation Groups of the CPS, 1984-2001. Samples exclude individuals with imputed earnings. Standard errors in parentheses.

Table 5. *Industry Level Analysis of the Union Wage Premium in the Private Sector, 1973-1999*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Premium$_{t-1}$					.6030*	.2759*	.6001*	.2468*	.3196*
					(.0274)	(.0350)	(.0284)	(.0361)	(.0333)
Time				-.0019*	-.0012*	.0002	-.0011*	-.0001	-.0009*
				(.0004)	(.0003)	(.0004)	(.0003)	(.0004)	(.0003)
Unemployment rate	.0187*	.0131*	.0108*	.0083*	.0064*	.0064*	.0061*	.0070*	.0052*
	(.0017)	(.0017)	(.0011)	(.0014)	(.0010)	(.0021)	(.0011)	(.0022)	(.0010)
COLA	.0763*	.0767*	.0403*	.0155	-.0065	.0139	.0041	.0156	.0141*
	(.0313)	(.0303)	(.0126)	(.0134)	(.0090)	(.0140)	(.0108)	(.0144)	(.0096)
Inflation	-.0182*	-.0077	.0012	.0006	.0024*	.0026	.0020*	.0032	.0002
	(.0065)	(.0069)	(.0008)	(.0008)	(.0007)	(.0015)	(.0008)	(.0016)	(.0008)
*Unempt rate*COLA*	-.0092*	-.0047							
	(.0038)	(.0036)							
*Unempt rate*Inflation*	.0026*	.0012							
	(.0009)	(.0009)							
Import penetration	.2048*	.2201*	.2362*	.3090*	.1688*	.1234*	.1738*	.1668*	.1811*
Durables	(.0427)	(.0414)	(.0441)	(.0424)	(.0326)	(.0416)	(.0461)	(.0549)	(.0302)
Import penetration	.1655*	.1459*	.1491*	.1698*	.0939*	.0880*	.0914*	.0945*	.1043*
Non-durables	(.0513)	(.0525)	(.0509)	(.0488)	(.0302)	(.0208)	(.0419)	(.0265)	(.0314)
Dereg.Communications	.0752*	.0609*	.0589*	.0612*	.0451*	.0625*	.0506*	.0734*	.0532*
	(.0316)	(.0244)	(.0246)	(.0248)	(.0200)	(.0307)	(.0234)	(.0261)	(.0193)
Deregulation Rail	.0329	.0400	.0394	.0580	.0200				.0333
	(.0905)	(.0844)	(.0855)	(.0839)	(.0616)				(.0606)
Deregulation Trucking	-.0716	-.0617	-.0630	-.0394	-.0139				-.0332
	(.0560)	(.0570)	(.0565)	(.0518)	(.0429)				(.0398)
Deregulation Air	.0554	.0684	.0661	.0815	.0087				.0214
	(.1262)	(.1217)	(.1190)	(.1161)	(.0852)				(.0804)
Deregulation Finance	-.0614*	-.0599*	-.0587*	-.0329	.0179				-.0174
	(.0191)	(.0188)	(.0195)	(.0203)	(.0160)				(.0150)
Weighted	.Yes	Yes	Yes	Yes	Yes	No	Yes	No	Yes
Method	GLS	GLS	GLS	GLS	GLS	GLS	OLS	OLS	GLS
Wald Chi^2/ R^2	2325.01	2781.32	2686.37	3190.74	10961.71	1220.21	.8973	.6516	6189.4
N	832	832	832	832	832	832	832	832	806

Notes: all equations also include a full set of 31 industry dummies. Data are taken from Bratsberg and Ragan 2002. GLS regression estimated with industry specific AR(1) process in error term. Where indicated ach observation in the GLS regressions is weighted by the industry observation count of the first step following Bratsberg and Ragan (2002). Column 9 excludes Retail Trade. Standard errors in parentheses.

Table 6
Time-Series Estimates of Union Wage Premium (%), UK and Britain

	LFS	BSA
1985		3.5
1986		11.1
1987		7.9
1989		6.3
1990		6.3
1991		4.8
1993	14.9	11.4
1994	17.5	13.7
1995	14.6	13.1
1996	14.8	7.3
1997	11.4	17.7
1998	12.2	11.0
1999	10.2	9.5
2000	10.3	5.0
2001		4.4
2002		6.4

Note: See Data Appendix for samples and controls used.

Table 7: Estimated coefficients on union membership dummy from hourly pay equations

	Individual	Individual + workplace
Whole private sector	.140*	.059*
Covered occupations	.125*	.065*
Men	.165*	.060*
Women	.098*	.061*
Manual	.204*	.075*
Non-manual	.055*	.019

Note: * = significant at 95% confidence interval or above. Details of estimation procedure, controls, samples and diagnostics are contained in the Data Appendix

Table 8: Mean percentage hourly wage premium for union members using propensity score matching		
	Individual	Individual + workplace
Whole private sector	8.9*	-1.5
Covered occupations	2.8*	-1.0
Men	11.1*	2.1
Women	1.3	-3.3
Manual	17.7*	6.9
Non-manual	3.6	-1.0
Note: * = significant at 95% confidence interval or above. Details of estimation procedure, controls, samples and diagnostics are contained in the Data Appendix		

Table 9: Sensitivity analyses

	OLS, unmatched data	PSM	OLS, matched data
1. Original estimates for whole private sector	6.1*	-1.5	5.1*
2. Exclude union recognition and union density from workplace variables	11.7*	1.8	8.6*
3. Add workplace training and workplace tenure to individual variables	4.8*	-1.5%	5.7*
4. Change dependent variable to log gross weekly wages	7.8*	-2.5%	5.4*
5a. Employees in workplaces with 50%+ union density	8.9*	2.6	4.7
5b. Employees in workplaces with <50% union density	5.0*	-1.9%	4.9%*

Note: Figures are percentage differentials based on exponentiated differences in log wages between members and non-members. * = significant at 95% confidence interval or above. Details of estimation procedure, controls, samples and diagnostics are contained in the Data Appendix

Figure 1: Movement in the US union membership wage premium, 1973-2002

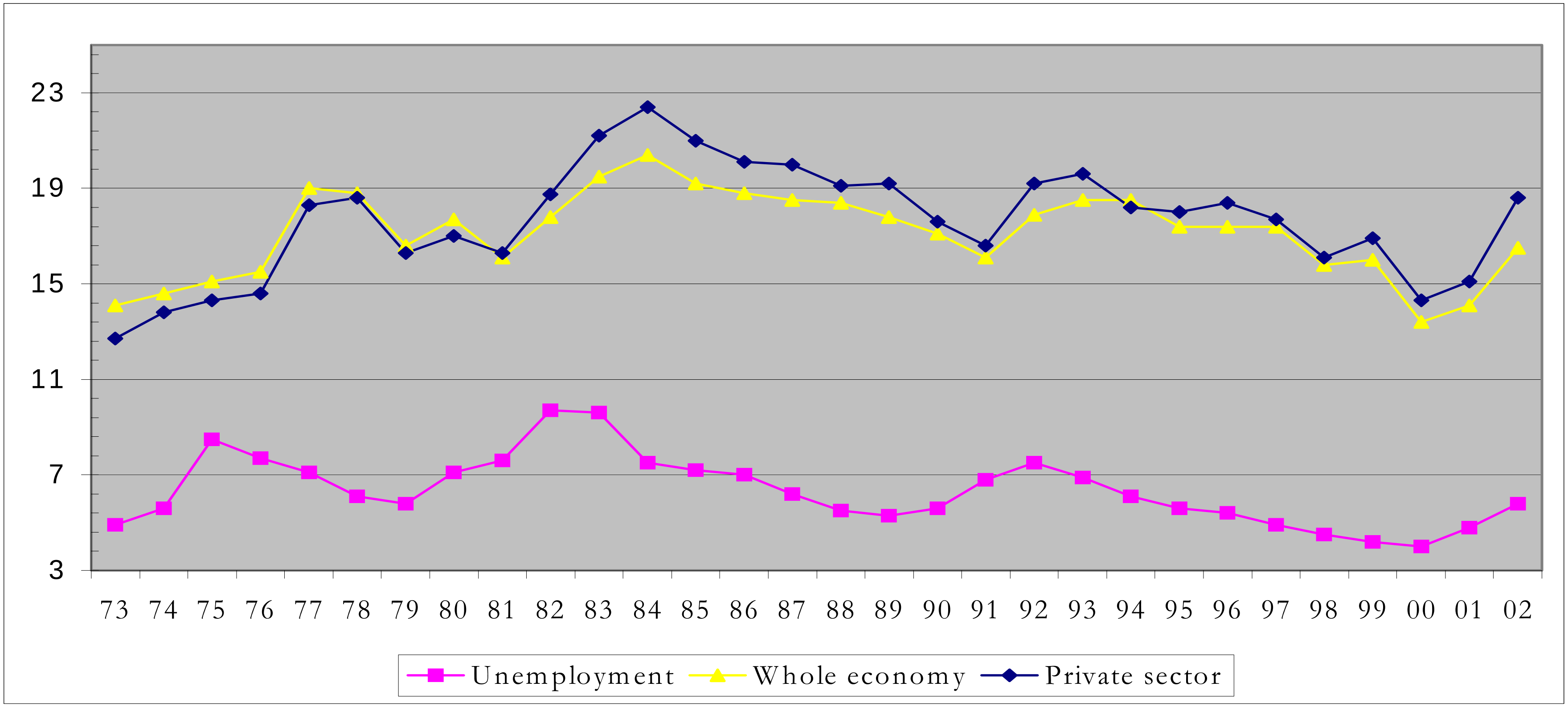
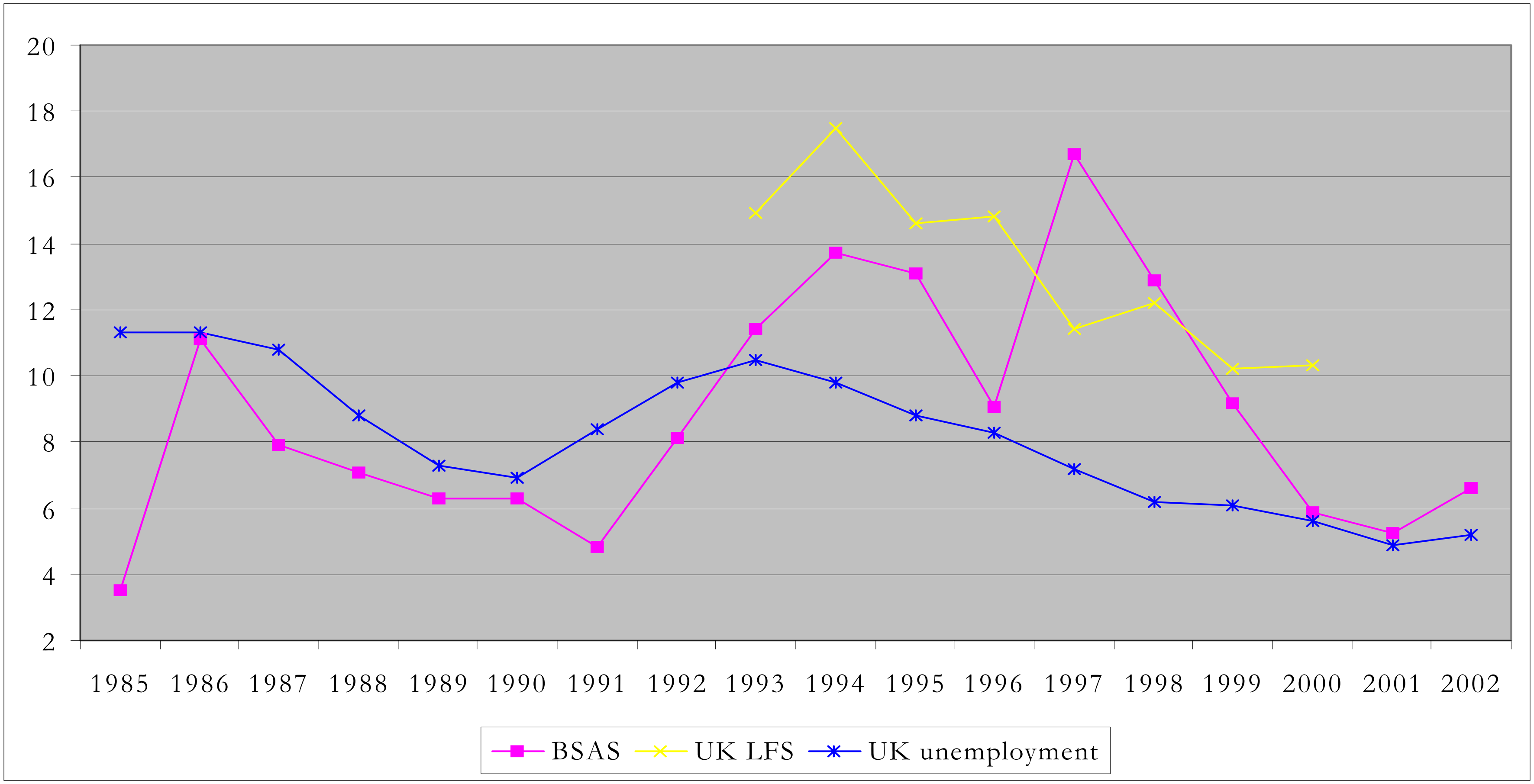


Figure 2: Movements in the UK/British Union Membership Wage Premium, 1985-2002



Note: Unemployment using ILO definition. BSAS figures are unweighted mid-point estimates using banded earnings data.