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# Not So Dissatisfied After All? The Impact of Union Coverage on Job Satisfaction

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## Abstract

The links between unionisation and job satisfaction remain controversial. In keeping with the existing literature we find strong statistically significant negative correlations between unionisation and overall job satisfaction. However, in contrast to the previous literature we find that once one accounts for fixed unobservable differences between covered and uncovered employees, union coverage is positively and significantly associated with satisfaction with pay and hours of work. Failure to account for fixed unobservable differences between covered and uncovered employees leads to a systematic underestimate of the positive effects of coverage on job satisfaction for both union members and non-members. It seems union coverage has a positive impact on job satisfaction that is plausibly causal.



Key words: Unions, union coverage, union membership, job satisfaction
JEL: C35; J28; J51

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## 1. Introduction

There is a well-established negative correlation between union membership and overall job satisfaction. The association is apparent in most data sets, across country and time. This has puzzled analysts who anticipate union efforts to improve members' wages and working environment should, if anything, lead to an improvement in employee job satisfaction. However, there are a number of reasons why we might anticipate a negative correlation. First, since it is costly to unionise, it is likely that those who do so are among those most dissatisfied with their jobs and, perhaps, other aspects of their life. If this worker heterogeneity is unaccounted for it could bias the estimates of union effects on job satisfaction downwards. Second, unions may be better able to gain a foothold in workplaces where the working environment is most disadvantageous to workers: it is these poor conditions that trigger unionisation. If this workplace (or job) heterogeneity is not fully accounted for in analyses this will also downwardly bias the relationship between unionisation and job satisfaction. Third, it is conceivable that the union effect is a true causal effect arising from unions' voice function. This function, as described by Freeman and Medoff (1984) and others, leads unions to foment dissatisfaction with a view to strengthening the bargaining hand of the union in negotiations with the employer.

Many efforts have been made to isolate the causal effect of unionisation on job satisfaction. The majority of these have sought to account for fixed unobservable differences across unionised and non-unionised workers by estimating panel models which identify the impact of individuals switching in and out of union membership status. Results from these papers, which are discussed in Section Two, are mixed. A second group of papers tends to use cross-sectional data and instrument for unionisation so as to account for the potential endogeneity of union status. Some of these papers find that the negative association between membership and job satisfaction disappears when this is done. Until recently the literature focused almost exclusively on union *membership* as the measure of unionisation. This is problematic since in countries like Britain it is not very strongly correlated with coverage by a trade union that has bargaining rights at the workplace. As such, it does not capture the potential causal impact of union bargaining coverage. Recognising this, some recent papers have sought to distinguish between members in covered and uncovered environments, comparing them both to non-members. Again, results are contested.

In this paper we take union coverage, rather than union membership, as our point of departure, in recognition of the fact that any causal impact of union bargaining on employees' job satisfaction will arise through the bargaining process leading to alterations in employees' terms and conditions of employment. The causal effect arising from voice-induced complaining is also likely to be stronger where the union is engaged in bargaining. Collectively bargained terms and conditions of employment are normally extended to non-members in a covered environment, and so we might not anticipate differential effects of union coverage on the job satisfaction of members and non-members unless non-members derive added satisfaction from receiving the benefits of coverage without paying union dues. Nevertheless, cognisant of the literature on differences between members and non-members, we consider whether – having accounted for the fixed unobservable differences between covered and uncovered employees – there remain significant differences in the coverage effects on members' and non-members' job satisfaction.

In keeping with the existing literature which uses membership as a proxy for unionisation, we find strong statistically significant negative correlations between union coverage and job satisfaction in an OLS regression framework. However, having accounted for fixed unobservable differences between covered and uncovered employees, we find union coverage is positively and significantly associated with satisfaction with pay and hours of work. These effects are apparent for covered members and non-members. Furthermore, shifting from OLS to fixed effects estimates results in the union coverage coefficients becoming more positive for job satisfaction across a range of job satisfaction measures, something that happens for both union members and non-members. Of course, panel fixed effects estimates do not tackle the potential endogeneity of union status. Nor do they account for time-varying unobservable features of the individual, such as changes in preferences over time. Nevertheless, it seems reasonable to conclude, therefore, that union coverage has a positive impact on some aspects of job satisfaction that is plausibly causal. At the same time, coverage is associated with lower satisfaction with job security, although this is partly accounted for by the nature of jobs undertaken by covered employees.

Section Two reviews the existing literature and how this paper seeks to contribute to it. Section Three describes the estimation strategy. Section Four presents the data and our empirical approach. Section Five presents the results. In Section Six we reflect on these findings and discuss why they appear to be at odds with most of the existing literature.

## 2. Individual Differences and Their Implications for Union Status and Job Satisfaction

In their review of the psychology literature Diener and Lucas (1999: 226) conclude "subjective well-being reports do not completely reflect arbitrary decisions based on temporally unstable factors. Instead, the affective and cognitive components are consistent across time and across situations and can be reliably predicted from a number of personality traits and constructs". Twin studies confirm that a large proportion of inter-individual variation in subjective wellbeing

(SWB) ratings is attributable to genomic variation (Lykken and Tellegen, 1996). These fixed differences across individuals in SWB can bias estimated relationships between SWB outcomes (of which job satisfaction is an instance) and other variables of interest. For example, when Ferrer-i-Carbonell and Frijters (2004) compared their OLS estimates of factors affecting life satisfaction with those accounting for fixed differences across individuals they found that the latter were substantially smaller (closer to zero) for important variables such as income and family composition. Similarly, Gerlach and Stephan (1996) analysed the negative effect of unemployment on happiness ratings and found that estimates shrank toward zero when accounting for fixed differences across individuals, possibly because people with unhappy dispositions are more likely to lose their jobs. When analysing the influence of financial capability on psychological wellbeing Taylor et al. (2011) argued for the use of a fixed effects estimator on the basis that unobserved personality traits such as internal versus external locus of control could confound their estimates.

Similar considerations apply in relation to the link between job satisfaction and union status. To our knowledge there is no empirical evidence regarding personality traits and individuals' propensity to be unionised. However, it seems reasonable to argue that workers are more likely to organise the more dissatisfied they are with their jobs and this may be a part of a systemic tendency to dissatisfaction for the reasons just noted. It is those who are least satisfied with their job who will perceive the greatest net returns to unionisation, leading them to organise or join an existing union while others may be less prepared to incur the costs. Put another way, those with a higher propensity for job dissatisfaction are more likely to desire unionisation for a given level of "poor" working conditions. There is undoubtedly a time-varying component to this dissatisfaction. Consistent with this, using the data used in this study Powdthavee (2011) finds evidence of a dip in job satisfaction in the period just prior to union coverage - a bit like the Ashenfelter Dip in the evaluation literature - followed by a bounce back on becoming unionised.[1] However, there is also likely to be a time invariant component to this job satisfaction differential between covered and uncovered employees. Our hypothesis is that fixed unobservable differences across covered and uncovered employees are liable to downwardly bias the effects of unionisation on job satisfaction such that, once one accounts for those differences, the underlying association between coverage and job satisfaction is likely to be more positive.

[1] Powdthavee (2011) uses waves 5-15 of the British Household Panel Survey and confines his analysis to those in the private sector who do not switch workplace arguing that he wishes to avoid confounding job changers and the newly organised. His model specification means he focuses on the subset of respondents with at least 4 years of data before unionisation and 3 years afterwards.

It is nevertheless uncertain a priori whether the coverage effect conditional on fixed individual differences will be positive and statistically significant since unionisation may have its own causal impact on job satisfaction and this effect may be either positive or negative. The positive effect will dominate where union bargaining has improved workers' terms and conditions relative to what they might have achieved in a non-union environment. The negative effect will dominate where bargaining relies on voice-induced complaining to strengthen the bargaining hand of the union, where union information provision improves employees' knowledge about managerial failings, where unions "over-sell" what they can achieve to employees in an effort to increase or maintain membership levels, or where unions prove ineffectual in a bargaining setting. Equally unions may have no discernible impact on employees' job satisfaction where the union wage premium simply compensates employees for poorer conditions than those they might face in a non-union setting.

The empirical literature remains split as to whether there is a negative effect of unionisation on job satisfaction. In one of the first studies for Britain Bender and Sloane (1998) find the negative association between unionisation and job satisfaction disappears when accounting for the industrial relations climate, leading them to argue that union dissatisfaction may be genuine and arises from the poorer working environment in a unionised setting. Using cross-sectional linked employer-employee data Bryson et al. (2004) find a negative association between union membership and job satisfaction but this effect becomes statistically non-significant when they instrument for union status, leading them to argue that the effect is driven by selection into union status. In a follow up study using the same data they seek to account for selection into both union membership and union coverage (Bryson et al., 2010). In doing so they find a negative relationship between unionisation and job satisfaction which is confined to uncovered union members. They suggest membership may increase the 'taste' for coverage, leading to member dissatisfaction in an uncovered environment.

A further set of studies for Britain are more directly linked to the current study because they use longitudinal panel data to account for fixed unobservable differences across unionised and non-unionised workers. All use various waves of the *British Household Panel Survey* (BHPS). These studies also come to different conclusions about the underlying link between unionisation and job satisfaction. In an early study using the first four waves of the BHPS which pooled data for the private and public sectors Heywood et al. (2002) found links between union membership and job dissatisfaction persisted controlling for person fixed effects. Indeed, contrary to expectations outlined above, the negative union membership coefficient became much larger in the fixed effects models estimating overall job satisfaction and satisfaction with "relations with the boss" compared with their OLS equivalents (op. cit.: 603). On the other hand, the negative union membership coefficient became smaller with the introduction of person fixed effects into the models estimating satisfaction with "the work itself" and pay. In the case of pay the union membership coefficient became statistically non-significant (op. cit.: 606).

Powdthavee (2011) finds the initial positive impact of being newly unionised dies out quickly, a finding he argues is consistent with the voice-induced complaining needed to support union bargaining, an effect that counters the initial positive effect of becoming unionised. In a similar study for the United States using the National Longitudinal Survey of Youth (NLSY) Artz (2010) finds an initial positive impact of unionisation on job satisfaction for those with no previous union experience. Nevertheless, Powdthavee's results are sufficient for him to raise questions as to whether there is really any job dissatisfaction "puzzle" associated with unionisation.

Using the BHPS data for 1996-2007 Green and Heywood (2010) find that, having accounted for both fixed individual and job effects, covered members are significantly less satisfied with their jobs than uncovered employees. However, the effect is confined to satisfaction with "the work itself" and job security. There are no differences by union status with respect to satisfaction with pay or hours. Furthermore, the introduction of worker fixed effects systematically reduces the size of the negative coefficients for satisfaction with all aspects of the job (op. cit.: Table 5).

This paper contributes to the literature by focusing on the association between job satisfaction and union coverage within individuals over time. In doing so we establish the degree to which exclusion of person fixed effects biases the association. We focus on collective bargaining coverage, rather than union membership, since it is coverage that is likely to capture any causal effect of unionisation via the bargaining process and related voice-induced complaining. As noted below, others have begun to consider union coverage links to job satisfaction, but their primary focus has remained the distinction between union members and non-members. We also test for differences between the job satisfaction of covered union members and non-members. We anticipate coverage effects to be small due to the non-excludable nature of union goods. If covered members are less satisfied than covered non-members having accounted for unobservable fixed traits across individuals, this may reflect non-members' ability to benefit from coverage without having to pay union dues.[2]

[2] The situation in Britain is quite different from that in the United States where, even in right-to-work states, non-members are often required to pay union dues if covered by collective bargaining, even if they choose to remain union non-members. Partly because of this there are relatively few union non-members in covered workplaces in the United States (Bryson and Freeman, 2006: 3 and 32).

We make explicit comparisons between OLS models and person fixed effects models to examine the difference made to estimated coverage effects by accounting for fixed unobserved heterogeneity across employees. We present average effects of coverage for the period individuals remain in the panel. In doing so we run a specification test to see if attrition is non-ignorable (see Section Three).

In the spirit of Powdthavee (2011) and Artz (2010) who explore links between satisfaction and the dynamics of unionisation, we examine changes in job satisfaction with the time employees spend in a union covered environment. To avoid confounding the job satisfaction effects of becoming newly covered with those associated with obtaining a new job we control for new job entry.

We estimate union effects on five job satisfaction measures. This is important because one can anticipate trade-offs between bargaining objectives. In the traditional right-to-manage model the bargaining object is wages whereas employment is set unilaterally by the employer conditional on those wages. Although there is evidence that unions negotiate over employment as well as wages in many instances (van Wanrooy, 2013) the right-to-work model is usually viewed as a reasonable approximation to the British case. The implication is that the union wage premium should raise satisfaction with pay if the employee's reference point is her outside market wage. However, the wage premium may come at the cost of increased concerns regarding job security since wage pressures may encourage employers to substitute capital for labour and, in extremis, threaten workplace survival.

We run estimates for 17 waves of the BHPS through to 2007, thus extending analyses by some years compared to previous contributions to the literature. Although we would like to extend the analysis to additional years the switch from the BHPS to Understanding Society introduced a number of data discontinuities that make this problematic. (In addition the financial crisis begins just after the end of our data, and this may have altered a number of things). It is worth reflecting on the stability of the assumed fixed effects as this may affect judgments about the length of the panel that is desirable. Soto et al. (2011) gathered data from 1.27 million respondents and found that there are cross-sectional age differences in the mean levels of three of the ‘Big Five’ personality domains and in facets within those domains. Neuroticism tends to be lower at higher ages, while conscientiousness (especially its ‘orderliness’ facet) and agreeableness (altruism, compliance) tend to be higher with age. The amount of mean-level variation across the life-course is sufficiently slight to be compatible with constancy of estimates at the individual adult level over an observation period of around 15 years, but it might be problematic for panels of much longer duration since over-time variation in personality variables would be detectable over such a period.

## 3. Estimation

Panel data consist of observations on individuals, indexed by i, repeated at regular intervals, indexed by t; here the interval is one year and the years are designated t=1,....T. The outcome variables in the present case are attitudinal and are measured on 7-point satisfaction scales. In the past it was common for economists to treat such scales as ordinal, or to collapse them to binary responses, so that analysis by non-linear methods was required. Psychologists however have generally treated the measures as cardinal (equal interval), and this has now been quite widely adopted by economists as well. The psychological case for the cardinality assumption depends on evidence suggesting individuals interpret response scales as implying an equal-spacing metric and are capable of responding accordingly. The case for adoption of the cardinality assumption in economics has been made by van Praag (1991) and by Ferrer-i-Carbonell and Frijters (2004), among others, and recent applications using this approach include Clark et al. (2008) and Taylor et al. (2011); van Praag and Frijters (1999) provide an extended discussion of well-being and welfare functions in relation to the utility concept.

Making the cardinality assumption, we obtain estimates from two kinds of models, pooled OLS and FE. With time indexed by t={1,...T},under pooled OLS, the model is usually written as

$$y_{it} = X_{it}\beta + \nu_{it} \qquad (1)$$

where $y_{it}$ is the outcome, the $X_{it}$ are the regressors, including the intercept. Unobserved individual effects (heterogeneity) are included within the disturbance term $\nu_{it}$ that sums both the unobserved individual fixed effect (say, $c_i$) and the time-specific disturbance (say, $u_{it}$). The OLS model is appropriate if both the $c_i$ and the $u_{it}$ are uncorrelated with the observable regressors $X_{it}$; we may regard the use of OLS in panel analysis as implying this as an assumption.

To implement the FE estimator, equation (1) is first re-written in its full form

$$y_{it} = X_{it}\beta+c_i + u_{it} \qquad (2)$$

Then from each term is subtracted the mean value for each individual averaged over the T observations, viz. $y_{it} – y_{i.}$, $x_{it}$-$x_{i.}$, $u_{it} – u_{i.}$ (the subscript dot indicates the term over which averaging is applied). As the $c_i$ are by definition constant, they are eliminated. If (2) is an appropriate model, then so also is this de-meaned transformation. The resulting ‘within regression', i.e. the regression using the within-person over-time variation, provides estimates of the parameters β while allowing that the unobserved effects $c_i$ can be correlated with the explanatory variables $X_{it}$, as is intuitively appropriate. The estimator assumes that $E(u_{it}|X,c)=0$ (Wooldridge 2002: 275-84) and that union status is strictly exogenous.

The foregoing outline stresses the underlying similarity of the pooled OLS and FE methods: indeed FE is correctly described as pooled OLS on de-meaned variables. Additionally, panel analysis, whether by OLS or FE, should take account of the correlation of observations within each respondent over time, since otherwise t-statistics would be inflated. For both the OLS and FE analyses, therefore, standard errors are computed by means of a robust variance estimator that takes account of clustering and also of heteroskedasticity.

Panel data can be either balanced or unbalanced, where balanced means that each unit is observed at every time-point. The balanced panel is easy to conceptualize and analyse, and is appropriate where there is little problem of missing data (e.g. drawing on administrative data or statutory returns). The data used here (see below) are obtained from a longitudinal survey that is affected by non-response and attrition, and analysing the balanced panel would lead to large losses in data and an unacceptable dilution of the sample's representativeness. We therefore analyse the unbalanced panel. This makes formal definition of the FE model somewhat more cumbersome (Wansbeek and Kapteyn 1989) but computation remains straightforward. More problematically, the unbalanced nature of the panel involves potential selectivity effects. An individual's leaving the panel presumably reflects varied reasons or circumstances. To model this selectivity is a very difficult task and is here not attempted. However, as suggested by Verbeek and Nijman (1992), also by Wooldridge (2002), it is possible to get some indication of the likely influence of selectivity on the estimates by including a dummy representing the out-movement of the individual in the following observation period and considering its statistical significance. The specification adopted here marks the last observation on the individual with a dummy variable (a possible psychological interpretation of this variable is in terms of moving or leaving intentions). These tests for sample attrition bias suggest that any bias resulting from attrition has been reasonably well controlled in our specification.[3] Further details of the unbalanced panel construction are given in the next section.

[3] There is only weak evidence of attrition bias affecting the results for satisfaction with job security. There is no evidence of serious attrition bias affecting results for the other satisfaction outcomes. It is reasonable to conclude that the extensive controls used in the specification have tended to compensate for sample attrition. These results are available on request.

A further problem arising from  the unbalanced nature of the panel is that it is not possible to weight the data.  However, as detailed later, as an alternative to weighting, the analysis included a range of control variables that were used by the originators in stratifying the sample and in constructing the weights for the balanced panel (see Taylor et al. 2011 and the BHPS documentation).

## 4. Data set, variable and analyses

### 4.1 Data

The research uses the British Household Panel Survey (BHPS) dataset. The initial sample for BHPS was drawn in 1990 and consisted of 9,912 full interviews with individuals from 5,538 households drawn as a stratified sample from all British households.[4] Members are interviewed annually. Representativeness has been maintained by following individuals who set up or join new households and by admitting as new panel members those who form a family relationship to existing members. At various stages booster samples were added to the original sample design, e.g. to contribute to the European Community Household panel (ECHP), or to provide sufficient numbers for separate analysis of country sub-samples for Wales, Scotland and Northern Ireland. As these booster samples change the nature of the original sample, and as this cannot be corrected in the type of analysis we perform by re-weighting, we have excluded them entirely from all aspects of the analysis.[5]

We analyse data from the first 17 waves (years 1991-2007).  We analyse the unbalanced panel incorporating those who either leave or join during the observation period.  However, we exclude observations on leavers if they subsequently re-join the panel; leaving is treated as an 'absorbing state' (see Wooldridge 2002).[6]  We also exclude the 3,940 observations who were only present for one period as they cannot contribute to the FE estimates.  Only years in which individuals are employees appear, since the dependent variables are not available for the non-employed and

[4] Userguide, 5151userguide_vola.pdf, Tables 16 & 17 (page A4-28).

[5] This entails the removal of 75,959 person-year observations, equivalent to 31.8% of the person-year observations. In principle oversampling can be corrected by inverse probability re-weighting.  However, the weights must be constant within the panel, something that is not possible since the supplementary samples are only present in certain waves.  If, on the other hand, one simply includes supplementary samples without weighting, two main kinds of bias are introduced:  first, the years when the supplements are present have an increased influence on model estimates; second, the results are no longer representative of British employees, or, in the case of the ECHP supplements, they are no longer representative of the British employee income distribution.

[6] 14780 observations (9.1% of the original sample) are removed because they follow a gap resulting from non-interview at one or more waves (i.e., we ignore observations after these sample members return because to include them would lead to irregular spacing of the interviews with various adverse consequences – e.g. no consistent definition of leads and lags for dynamic analysis.

some control variables are not available for the self-employed. Further we limit the analysis to observations when individuals are aged 20-60. This latter choice is made to reduce problems of selection and self-selection into employee status: ages 16-19 being peak student years, and ages 61-65 being peak years for (early) retirement and disability/incapacity claims.[7] Around 52% of the original sample observations are employees. Of these 70,870 observations 5,655 are dropped because the respondents are outside the 20-60 year age range. Movement out of employee status is *not* treated as an absorbing state: subsequent observations of resumed employee status are covered in the analysis. After exclusions for missing data[8] we are left with somewhat more than 58,000 person-year observations on a little more than 8,000 individuals.

### 4.2 Variables

*Dependent variables*

Five outcome variables are separately analysed. *Overall job satisfaction* is often interpreted as a single-item measure of the subjective utility of a job. It is the most widely used measure in occupational psychology and has well established associations with behavioural outcomes including performance ratings, absence, lateness and quit (Judge et al. 2001; Harrison et al. 2006). Four further measures obtain ratings of *satisfaction with facets of jobs*: job security, the work itself, hours, and pay. Each of these relates to an area in which British trade unions have been actively engaged and also to areas that are currently discussed as aspects of deteriorating employment conditions under pressures from globalized competition and technological change (Green 2006; Gallie 2007). Satisfaction with security relates to precarious employment contracts and to risk of job loss, satisfaction with work relates to such issues as autonomy and task discretion, satisfaction with hours and pay relate to work intensification as well as to unions' core business of negotiating a favourable effort-reward bargain for members. All five outcome measures have 7-point response scales. Table 1 provides further descriptive details.

*Explanatory variables*

There are two chief explanatory variables that are used alternately in parallel analyses: union recognition and union membership. Employee respondents are first asked 'Is there a trade union or a similar body such as a staff association, recognized by your management for negotiating pay

[7] The age range 20-60 has been adopted in several major British surveys of employment conditions including Social Change and Economic Life (SCELI) 1987, Employment in Britain 1992, Working in Britain 2000/1 and the Skills Surveys.
[8] 7,536 observations (4.6% of the original sample) are removed due to missing data arising from proxy or telephone interviewing. Small numbers of observations have missing data on union status or job satisfaction.

or conditions for the people doing your sort of job in your workplace?' It is notable that this question not only focuses on workplace trade union recognition, but also on recognition that covers the respondent's job or occupation at that workplace. Union recognition is a dummy variable taking value 1 when the respondent answers 'Yes' to the foregoing question, and 0 when the answer is 'No'. At waves 2 and 3, the question was only asked of employees who had changed their job (including through promotion in the same workplace), so that union recognition is missing for employees who had not changed job. In the next section we report robustness of our results to the exclusion of these years.

If the respondent stated that a union was recognized, she was then asked 'Are you a member of that trade union or association?'. We construct a union membership variable with three categories: trade union member with recognized union/association; non-member where there is a recognized trade union/association (sometimes referred to as 'free-rider'); no trade union recognized. Table 2 shows the frequency distributions of the union recognition and membership variables.

For waves 1 to 7, BHPS identified individuals as members of a union that did not provide them with representation at work. From wave 8 this question was discontinued. A more general question about union membership was also included in some subsequent waves but it cannot be regarded as equivalent to the earlier question. We therefore decided that it was not possible to construct a consistent membership variable for uncovered employees. We estimate that our membership variable, although limited to the case where the member is covered by a union recognized at the workplace, none the less represents at least 90 per cent of all union membership.

*Control variables*

Control variables were included in all analyses. These controls, which are listed in the note to Table 3, are commonly used in models of labour market participation and earnings. We also include certain employment conditions that are likely to affect employee attitudes. These are also listed in the note to Table 3. The inclusion of these employment conditions variables entails some risk of endogeneity bias as they may partly reflect individual choices, but if omitted the union effects may be distorted.[9] We therefore re-ran all analyses excluding the controls for working conditions. However, results were very similar so we only report those results where

[9] Working conditions might be endogenous to the extent that they are choice variables. However, this is less likely to be the case if employers fix them by virtue of asymmetric power.

they differed significantly from main results.  We also incorporate variables that were used in the original construction of the strata and weights for the survey sample. Finally, in the 'dynamic' models that conclude our analyses we add control variables for movement to a different employment in the current spell and for the cumulative number of years observed in employee status.

*Sub-sample analysis*

We ran all analyses for the whole economy and for the market sector since the nature of unionisation and the institutional settings in the market and non-market sectors are fundamentally different. Perhaps the chief difference is that public sector bargaining occurs at national or sectoral level, as opposed to organization or workplace level, within parameters that are largely set outside the organization. As such, public sector collective bargaining is far removed from the traditional rent extraction model one might think of in a market setting. Furthermore, individuals choosing employment in the public (private) sector differ (eg. with respect to their risk preferences) such that one might anticipate potential for systematic differences in union effects across the two sectors. In practice, differences were small so we focus on the whole economy analysis here only reporting on significant differences when they arise.  Sector of employment (market v. non-market) was included as a control variable in the whole economy analyses and it seems that this was sufficient to stabilise the results.

## 5. Results

The effects of union coverage on job satisfaction in the whole economy are presented in Table 3. Coefficients from OLS estimates are presented in the left-hand columns and those from person FE models are presented in the right-hand columns.  All estimates should be interpreted in terms of the proportions of the unit response on a response scale of 1-7 with 7 being high (more satisfied).

The OLS estimates reveal a negative statistically significant association between union coverage and three of the five job satisfaction measures, namely overall satisfaction, job security and satisfaction with the work itself.  There is no statistically significant association between union coverage and satisfaction with pay or hours.

Conditioning on person fixed effects, so that we are comparing the effects of union coverage *within* individuals over time, the effects of union coverage become more positive.  This is apparent from a direct comparison of the union coverage coefficients  in the last column of the table.[10] Under the FE model union coverage is associated with significantly higher satisfaction with pay and hours, which are often the subject of direct bargaining between unions and employers (van Wanrooy et al., 2013). However, coverage continues to be associated with significantly lower job security.[11] [12]

To establish whether coverage effects differ across covered members and non-members we run models with the same controls but this time distinguish employees according to whether they are members of the union recognised for pay bargaining. The reference category is all uncovered employees, regardless of their membership status.

Across all five job satisfaction measures covered non-members (sometimes called "free-riders") have higher job satisfaction than their member counterparts, though not always statistically significantly so. This member satisfaction differential among covered employees persists in the FE models, indicating that it is not simply driven by fixed unobservable differences between members and non-members. However, the coefficients for both covered members and covered non-members become more positive when we take account of fixed unobservable differences across employees, as indicated in the final column of the table.

The OLS estimates for covered members indicate that their job satisfaction is significantly lower than uncovered employees' for overall job satisfaction, and satisfaction with job security, work itself and hours. These effects persist but the size of the coefficients falls by more than half when introducing the person fixed effects. Furthermore, pay satisfaction becomes positive and statistically significant, as does satisfaction with hours (albeit at a 90 per cent confidence level).

In OLS estimates, free-riders have significantly lower satisfaction with job security and work itself than their uncovered counterparts, but they have higher hours satisfaction. When accounting for unobserved differences across individuals, their dissatisfaction with work itself

[10] The significance of the differences between the OLS and fixed effects estimates cannot be established but the sign of the OLS-FE difference is positive without exception.
[11] We obtain very similar results if we confine the analysis to the market sector. The only notable difference is that the negative association between coverage and satisfaction with job security is larger in the market sector and robust to the exclusion of job characteristics.

[12] We reran the analyses on waves 5 to 17 of BHPS, thus avoiding potential problems regarding measurement error in union status in early waves. But the results are similar. For instance, in the fixed effects model with job controls the union coverage effect on pay satisfaction is 0.088, t=2.97, for hours satisfaction it is 0.068, t=2.57. Dropping the job characteristics from the specification the pay satisfaction coefficient is .093 (t=3.14),while the coefficient in the hours satisfaction equation is .072 (t=2.71). The coverage effects on overall satisfaction estimates are positive and non-significant while those for job security and work satisfaction are negative and non-significant.

and job security is no longer apparent, while they are significantly more satisfied than uncovered employees as measured by overall satisfaction, pay satisfaction and hours of work.

The effects of coverage on members, relative to being an uncovered employee, are similar when we exclude the potentially endogenous job characteristics, except that the effects of coverage on satisfaction with job security are no longer statistically significant in the FE model.[13] The implication is that the significant negative association between covered membership and satisfaction with job security is driven by the nature of the working environments that are organised by unions, rather than coverage per se. [14]

*Results from dynamic models*

Finally we present analyses which are dynamic in the sense that they take account of the interactions of current union coverage with coverage in prior years .  We first present a model, in which prior experience of union coverage is reduced to a single dummy variable (0=no prior within-panel years covered, 1=one or more prior years covered). Subsequently we go on to models where prior coverage experience is treated as a continuous variable.

Before looking at the model results, it is useful to look at the way current coverage is associated with prior coverage. We construct a variable for 'any previous experience of union coverage' and a continuous variable representing the number of previous waves (years) of previous union coverage called UNISUM. One third (34 percent) of the employee-year observations are for employees who are uncovered and have never been covered.  Sixteen percent of all observations are for employees who have past coverage experience but are no longer covered (unisum | covered=0); almost half (46 percent) are currently covered and have prior coverage experience (unisum | covered=1); while the remaining 4 percent are covered for the very first time (covered==1 | unisum==0).  These figures suggest considerable persistence in union coverage status, which accords with the known tendency for unionised employees to have relatively long tenures.[15]

Table 5 shows the results from models where the prior-coverage dummy is interacted with current-year coverage status.  Looking first at the model estimates (top three rows of results),

[13] These results are not shown in the table but are available on request.

[14] All analyses were run separately for women. Results were similar to those reported.

[15] Note that becoming a newly covered employee could be due to moving to a unionised workplace, or to a union-covered job in the same workplace, or through new union recognition at same workplace.  Although the data do not permit us to distinguish reliably between these circumstances we are able to net out the positive and statistically significant effect of moving to a new job.

current coverage in the absence of prior coverage (equating to new coverage) has significantly positive effects on satisfaction with pay, work, and hours, and with overall satisfaction. The same applies to prior coverage in the absence of current coverage, albeit with estimated effects of somewhat smaller magnitude. For both coverage variables, the estimated effects on satisfaction with security are also positive, but not significant. It is however the results for the interaction term that are most striking, since these are always *negative and significant*, indicating that when there is both present and previous coverage, the effects of each form of coverage experience on satisfaction are shifted in a negative direction. For instance, looking at the column of estimates for pay satisfaction, the effect of current coverage when there has also been previous coverage experience is (0.271-0.190)=0.081; similarly, the effect of prior coverage experience when there is also current coverage is (0.141-0.190)=-0.049.

It is useful to compute the partial (or marginal) effects of each variable across all conditions, and these partial effects are shown in the last two rows of Table 5. In these whole-sample estimates, the effects of both current and prior coverage status have positive signs for all the satisfaction outcomes except satisfaction with security, whereas the estimates for current coverage remain significant for pay, hours and overall satisfaction, those for prior coverage are always non-significant. The partial effects of current coverage in the present model can also be directly compared with the coverage estimates of Table 3: the present model provides a somewhat more positive view of the effects of union coverage on job satisfaction.

Table 6 provides the results from a similar model in which, however, the dummy variable for 'any previous experience of union coverage' is replaced by a continuous variable representing the number of previous waves (years) of previous union coverage. This variable, labeled UNISUM, is interacted with the dummy for current union coverage status, UNION. The estimated effects of current union coverage when there is no prior coverage experience are, just as in Table 5, positive and significant for satisfaction with pay, work, hours and overall satisfaction. The linear effect estimates for years of coverage conditional on not currently being covered (second row of results) are always positive, and significant for pay, work, hours and overall satisfaction, though only at the 10 per cent level for pay. The estimates for the interaction term however, are always negative and significantly so except in the case of satisfaction with security. So, for example, the effect on pay satisfaction is 0.191 when there is no prior coverage experience (i.e. unisum=0), but this is reduced by 0.02 for each year of prior coverage. Similarly, the slope of unisum is shifted in the negative direction when there is current union coverage as well. For instance for pay satisfaction, the slope is 0.02 when union=0 but approximately zero when union=1.

The whole-sample partial (or marginal) effects are shown in the bottom two rows of the table. Over the whole sample the partial effect of current union coverage remains positive for all the satisfaction outcomes except security, and significantly so for satisfaction with pay and hours. However the partial effect on overall satisfaction becomes non-significant and the effect on satisfaction with security becomes *significantly negative*. The effects of having previous time in a unionised situation are positive, except in the case of security, and significant in the case of work, hours, and (at the 10 per cent level) overall satisfaction. But the effect is non-significant in the case of pay. One can conclude then that taking account of the amount of prior experience of union coverage somewhat reduces the estimates of current union coverage on satisfaction, but suggests that prior experience of coverage itself has positive effects on satisfaction, especially when the individual is no longer in a covered situation.

## 6. Conclusions

In keeping with the existing literature which uses membership as a proxy for unionisation, we find strong statistically significant negative correlations between unionisation and job satisfaction when individual fixed effects are assumed to be uncorrelated with regressors. However, in contrast to the previous literature, having accounted for fixed unobservable differences between covered and uncovered employees, union coverage is positively and significantly associated with satisfaction with pay and hours of work. These effects are apparent for covered members and non-members. Furthermore, shifting from OLS to fixed effects estimates results in the union coverage coefficients becoming more positive for job satisfaction across a range of job satisfaction measures, something that happens for both union members and non-members. It seems reasonable to conclude, therefore, that union coverage has a positive impact on some aspects of job satisfaction that is plausibly causal. At the same time, coverage is associated with lower satisfaction with job security, although this is partly accounted for by the nature of jobs undertaken by covered employees.

These results are consistent with union bargaining effects which result in higher pay and hours schedules that better suit covered employees' preferences, relative to what they might have received in the uncovered sector. In keeping with the literature on the non-excludable nature of collectively bargained terms and conditions, the positive benefits of coverage are not confined to union members. On the contrary, satisfaction is higher among non-members than it is among members, even having accounted for fixed unobservable differences across individuals. This may reflect the fact that the net returns to coverage are highest for non-members who are able to avoid the financial costs of membership and reduce the potentially adverse effects of voice-induced complaining that members engage in to strengthen the union's bargaining hand in negotiation.

Our results are, perhaps, most similar to those of Powdthavee (2011). He also uses the BHPS. He observes an increase in job satisfaction once covered by a union, as we do, although the effect does not persist over time. However, his use of lagged job satisfaction measures means his analysis is confined to employees appearing in many years in the BHPS, so his sample is much smaller than ours. Furthermore, he excludes those switching workplace such that his analysis only captures changes in coverage within a particular workplace. Our analysis, on the other hand, includes coverage changes due both to changes within and across workplaces while controlling for the effect of entering a new job. In doing so we find the impact of past years of coverage differs systematically with current coverage status. Previous coverage experience has some positive effects on current job satisfaction, especially when employees are no longer covered. This might be due to employees transferring union gains across to non-union jobs through individual-level bargaining (McGovern et al., 2007). The effects of current coverage weaken with increasing years of prior coverage, an effect which might be explained if most of the gains for a union-covered employee arise early on, leading to disappointment if further gains do not accrue to the same extent.

Finally we show - as do Powdthavee (2011) and others - that union coverage effects on job satisfaction differ markedly across facets of the job in a way that is consistent with union bargaining effects.

Perhaps the biggest limitation to the existing study is our inability to account for the potential endogeneity of switches in union coverage status. Employees can move from the uncovered to the covered sector for one of three reasons. First, they may find that their existing job is organised by a union, and that union obtains bargaining rights. In such circumstances employees have usually been directly involved in obtaining coverage and, as such, are operating on the basis that they anticipate improvements to their terms and conditions via coverage. Although job satisfaction may rise in anticipation of the benefits of coverage, this is not what we capture in this paper: the FE models identify satisfaction differentials based on periods of coverage versus non-coverage for all who have switched status. The implication is that, on average, the satisfaction benefits of coverage for pay and hours satisfaction persist over time. Second, employees may become covered if they switch to a different job in the same workplace that is union covered. Some jobs are in occupations where coverage is very high, such as nursing or teaching, in which case coverage comes with the occupational choice, whereas in other cases the worker may be deliberately seeking out coverage in anticipation of benefits. Third, the worker may switch to a workplace with union coverage. Again, this may be an attribute of the new job that the worker was specifically seeking, or else it may have been coincidental to the move. It is uncertain, a priori, how this might affect employees' job satisfaction. We are able to partial out the impact of entering a new job, but the current analysis does not distinguish between the sources of coverage change. Future work could usefully explore how different sorts of switching might influence job satisfaction outcomes.

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**Table 1: The job satisfaction dependent variables**

| Satisfaction with: | security | work | hours | pay | overall |
|---|---|---|---|---|---|
| mean | 5.297 | 5.458 | 5.180 | 4.810 | 5.352 |
| standard deviation | 1.568 | 1.344 | 1.469 | 1.588 | 1.324 |
| skewness | -1.064 | -1.212 | -0.853 | -0.731 | -1.216 |
| kurtosis | 3.503 | 3.503 | 3.136 | 2.673 | 4.279 |
| N of observations | 59978 | 60374 | 60380 | 60316 | 60433 |

Notes: Unweighted sample statistics for employee respondents aged 20-60. All satisfaction questions are answered on a response scale scored 1 to 7: ‘completely satisfied’, ‘very satisfied’, ‘satisfied’, ‘neither satisfied not dissatisfied’, ‘dissatisfied’, ‘very dissatisfied’, ‘completely dissatisfied’. The scoring has been reversed so that 1 represents ‘completely dissatisfied’ while 7 represents ‘completely satisfied’.

**Table 2: Descriptives for union recognition and union membership variables**

| Union or staff association recognized at workplace? | n of observations | column % |
|---|---|---|
| yes | 27838 | 40.4 |
| no | 26945 | 39.1 |
| missing data | 14094 | 20.5 |
| Total | 68877 | 100 |
| If union recognized, is respondent a member? | | |
| yes | 17459 | 25.3 |
| no | 10351 | 15.0 |
| membership data missing | 28 | 0.1 |
| no union recognized | 26945 | 39.1 |
| union recognition missing | 14094 | 20.5 |
| Total | 68877 | 100 |

Notes: Unweighted sample statistics for employee respondents aged 20-60. About two thirds of missing data for union recognition arises in waves 2-4 where respondents were only asked this question if they had changed jobs (including by promotion) since the previous wave.

**Table 3 – Union Coverage Effects on Job Satisfaction**

| model | OLS | | | FE | | | $b_{fe}$-$b_{ols}$ |
|---|---|---|---|---|---|---|---|
| | b (s.e.) | t | Nit | b (s.e.) | t | Ni | |
| - overall sat | -.11 (.02) | 4.82 | 49996 | .01 (.02) | 0.46 | 8077 | 0.122 |
| - pay sat | -.02 (.03) | 0.77 | 49958 | .12 (.03) | 4.16 | 8071 | 0.136 |
| - job security | -.25 (.03) | 9.64 | 49779 | -.07 (.03) | 2.66 | 8050 | 0.180 |
| - work sat | -.15 (.02) | 6.08 | 49979 | -.02 (.02) | 0.85 | 8074 | 0.062 |
| - hours sat | .00 (.03) | 0.16 | 49992 | .09 (.03) | 3.60 | 8074 | 0.087 |

Notes:All analyses include controls as follows (+ indicates reference category): time (wave) dummies; 5 age-bands (20-29+...50-60); any professional qualification;highest educational qualification (degree/higher degree,sub-degree/a-level/equiv.,olevel/equiv.,below o-level or none+);labour income last year; non-labour income last year; have or can use car/van;housing tenure (own outright,own on mortgage,rent public housing, rent private housing+);separately for female/male respondent, marital status/spouse employment (no partner+, partner not employed,partner employed);separately for female/male respondent, age of youngest dependent child (no dependent child+,0-2,3-4,5-11,12-15,16-18); size of workplace (<25+,25-49,50-99,100-199,200-499,500-999,1000 or more), employer is in market sector,SIC group of workplace (agric.,utilities, manufcturing or extractive, distribution,transport and communications, financial and business services,government administration, health, education, other services),dummy indicating last wave present in panel=leaves panel next wave. Results shown also include controls for job characteristics (omitted in variant analyses) as follows: hours, incentive participation, and contract (permanent+/fixed term/casual).

**Table 4: Union Membership Effects on Job Satisfaction**

| model | | OLS | | | FE | | | $b_{fe}$-$b_{ols}$ |
|---|---|---|---|---|---|---|---|---|
| | | b (s.e.) | t | Nit | b (s.e.) | t | Ni | |
| - overall sat | mem | -.17 (.03) | 6.36 | 49970 | -.07 (.03) | 2.15 | 8074 | 0.105 |
| | fre | -.03 (.03) | 1.04 | | .06 (.03) | 2.40 | | 0.088 |
| - pay sat | mem | -.04 (.03) | 1.49 | 49932 | .09 (.04) | 2.7 | 8068 | 0.138 |
| | fre | .02 (.03) | 0.50 | | .13 (.03) | 4.41 | | 0.116 |
| - job security | mem | -.29 (.03) | 9.31 | 49753 | -.14 (.04) | 3.89 | 8047 | 0.155 |
| | fre | -.19 (.03) | 6.91 | | -.03 (.03) | 1.06 | | 0.164 |
| - work sat | mem | -.21 (.03) | 7.29 | 49953 | -.08 (.03) | 2.51 | 8071 | 0.127 |
| | fre | -.07 (.03) | 2,41 | | .02 (.03) | 0.67 | | 0.082 |
| - hours sat | mem | -.07 (.03) | 2.21 | 49966 | .06 (.03) | 1.83 | 8071 | 0.124 |
| | fre | .10 (.03) | 3.71 | | .11 (.03) | 4.23 | | 0.011 |

Notes:mem=member in covered workplace employment; fre=nonmember in covered workplace employment; reference category is not covered. Controls are those listed below Table 3.

**Table 5: Estimated effects on facet and overall job satisfaction of current union covereage and any previous union coverage: fixed effect panel regressions**

| Full controls for all models | Satisfaction with (dependent variable) | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Pay | | Security | | Work | | Hours | | Overall | |
| | b | t | b | t | b | t | b | t | b | t |
| covered now, but not previously | 0.27 | 5.06 | 0.03 | 0.56 | 0.19 | 4.01 | 0.25 | 4.99 | 0.17 | 3.59 |
| covered previously, but not now | 0.14 | 2.59 | 0.04 | 0.80 | 0.15 | 3.05 | 0.16 | 3.26 | 0.10 | 2.25 |
| interaction between previous coverageand current coverage | -0.19 | -3.06 | -0.10 | -1.65 | -0.23 | -4.23 | -0.23 | -4.09 | -0.18 | -3.28 |
| Ni | 6913 | | 6889 | | 6914 | | 6916 | | 6919 | |
| *partial effects* | | | | | | | | | | |
| covered now | 0.15 | 5.05 | -0.03 | -1.16 | 0.04 | 1.59 | 0.11 | 3.82 | 0.06 | 2.18 |
| covered previously | 0.05 | 1.20 | -0.01 | -0.26 | 0.03 | 0.91 | 0.05 | 1.30 | 0.01 | 0.42 |

Notes: controls are those listed for the full models (including job characteristics) below Table 3, *plus* a dummy for change to a different employment in the current wave, plus a continuous variable for the number of waves observed as an employee up to the current wave.

**Table 6: Estimated effects on facet and overall job satisfaction of current union coverage and years of prior coverage: fixed effects panel models**

| | ***Satisfaction with (dependent variable):*** | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | **Pay** | | **Security** | | **Work** | | **Hours** | | **Overall** | |
| | *b* | *t* | *b* | *t* | *b* | *t* | *b* | *t* | *b* | *t* |
| covered now | 0.19 | 5.22 | -0.05 | -1.36 | 0.10 | 3.19 | 0.17 | 4.92 | 0.13 | 4.14 |
| Previous coverage | 0.02 | 1.94 | 0.01 | 0.92 | 0.03 | 3.29 | 0.03 | 2.51 | 0.02 | 2.72 |
| Current x past coverage | -0.02 | -2.15 | -0.01 | -0.56 | -0.04 | -4.63 | -0.03 | -3.31 | -0.04 | -4.39 |
| Ni | 8087 | | 8063 | | 8090 | | 8090 | | 8094 | |
| *partial effects* | | | | | | | | | | |
| covered now | 0.13 | 4.73 | -0.07 | -2.37 | -0.01 | -0.58 | 0.08 | 3.22 | 0.02 | 0.99 |
| years of previous coverage (unisum) | 0.01 | 1.31 | 0.11 | 1.62 | 0.02 | 2.64 | 0.01 | 2.14 | 0.01 | 1.84 |

Notes: Previous coverage is a continuous variable measured by the number of waves (years) observed in a covered status. Current x past coverage means the interaction of current coverage with the number of years in a covered status. Controls are as described in the note to Table 5

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