Unions, Dissatisfied Workers and Sorting

Colin P. Green* and John S. Heywood**

* Corresponding Author, Lancaster University, c.p.green@lancaster.ac.uk

** University of Wisconsin – Milwaukee, heywood@uwm.edu

Abstract

A persistent and sizeable literature argues that the reported job dissatisfaction of union members is spurious. It reflects either the sorting of workers across union status or the sorting of union recognition across jobs. We cast doubt on this argument presenting the first estimates that use panel data to hold constant both worker and job match fixed effects. The estimates demonstrate that covered union members report greater dissatisfaction even when accounting for sorting in both dimensions. Moreover, covered union members are less likely to quit holding job satisfaction constant and their quit behaviour is far less responsive to job satisfaction. The paradox of the discontented union member remains intact.

JEL: J28, J51

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1. INTRODUCTION

The last two decades have witnessed an explosion of interest by economists in the job satisfaction of workers. While studying the influence of unions on job satisfaction has been a crucial part of that explosion, it actually predates it (Hammermesh 1977, Freeman 1978 and Borjas 1979). The longevity of this interest stems from what many see as a basic contradiction. Unions are associated with better earnings, better benefits and, arguably better means for dealing with worker grievances but at the same time unions are associated with lower job satisfaction. This has given rise to a wide variety of alternative explanations for this apparent paradox. These often start by recognizing the voice function of unions, but conclude that while unions may be *associated* with dissatisfaction it is not because unions *cause* dissatisfaction. The findings and their implications are diverse and often contradictory justifying yet more empirical investigation.

Previous resolutions of the paradox fall within two broad categories of arguments related to the sorting of workers and of union recognition itself. First, unions attract inherently dissatisfied workers and once this is corrected for, unions are not associated with dissatisfaction. These union workers would have been dissatisfied even in non-union jobs. Second, union jobs are genuinely worse and the dissatisfaction reflects negative characteristics that outweigh the wages, benefits and protection. Critically, the jobs that are genuinely worse attract unions and once this is corrected for, unions are not associated with dissatisfaction. Workers who were not members of a union would be just as dissatisfied with these jobs. While we will review studies within these broad categories, our ultimate objective is to provide new evidence on both the influence of unions on job satisfaction and the influence of unions on quit behaviour. Using longitudinal data for the United Kingdom we find that neither of these two categories of explanations prove powerful enough to explain the basic paradox.

Our individual fixed effects estimates suggest that sorting and the attraction of the inherently dissatisfied seems unlikely. Job satisfaction drops as a given worker moves from a non-union to a union position. While union jobs may genuinely be worse in some dimensions, if this was pervasive, one would anticipate union workers to be more likely to want to change jobs and more likely to quit. Our evidence shows just the reverse, they are less likely. More critically, we find that when workers change status while staying in specific jobs, the satisfaction paradox remains. This status change continues to influence the job satisfaction of the workers matched to those jobs. Thus, neither sorting of union status across jobs nor of workers seems the source of the paradox. Finally, we show that the expression of dissatisfaction is most, and perhaps only robustly, evident among those union members covered by collective bargaining. Thus, we suggest that the classes of theory proposed to date do not satisfactorily hold together all the relevant empirical evidence. While voice phenomenon are no doubt important, the way in which they work to generate superior working conditions, reduced quits and job dissatisfaction remains inadequately captured.

2. SETTING THE CONTEXT

The idea of union voice sits at the base of most explanations of the paradox of union discontent. Trade unions provide a collective voice alternative to quitting (Freeman and Medoff 1984) and must mobilize or encourage discontent as a necessary prequisite for successfully making demands of the firm (see Booth 1995). Unions create a “climate of complaint” as a tool to improve the work environment for workers. Borjas (1979) suggests the discontent may not be “genuine” but a “device” designed so the workforce can be heard.

Bender and Sloane (1998) disagree, arguing that employees recognize that survey interviewers will not reveal answers to the firm and thus have no incentive to exaggerate dissatisfaction. Freeman (1980) clearly suggests that the discontent is genuine, claiming that the voice function encourages workers to stay in jobs they do not like and improve their working conditions from within. Yet, the jobs, as measured by worker satisfaction, do not get better. Indeed, Artz (2010, 2011) uses longitudinal data to confirm that dissatisfaction actually grows as a worker's tenure in a unionized job grows.

The first category of explanation for the paradox is that union members express dissatisfaction because naturally dissatisfied workers are more likely than others to join a union. As Clark (1996: 202) argues, "If unions address issues of worker dissatisfaction, the more dissatisfied will be the most attracted to union membership." In this view, there exists an individual-specific element of job satisfaction that has a distribution across potential workers. If unions create a "climate of complaint," expect their ranks to be filled with complainers.

Several researchers have studied this sorting problem by instrumenting the union membership variable in cross-sectional individual data. Using U.S. data, Borjas (1979) and Kochan and Helfman (1981) found that the predicted union membership variable continued to be a statistical significant and negative partial correlate of satisfaction. They conclude that worker sorting is not a crucial issue. Miller (1990) using Australian data, found that the predicted union membership variable became statistically insignificant arguing in favour of sorting. Using UK data, Bender and Sloane (1998) use a key variable on worker perceptions of management to instrument membership. They suggest that the negative effect of membership is greatly reduced when accounting for sorting. Using the matched employee-employer data of the UK WERS, Bryson et al. (2004) use simultaneous estimation and strong establishment controls to instrument for union status finding that the dissatisfaction of union members is “a selection effect, rather than a causal effect.” In an ambitious study using a later wave of WERS, Bryson *et al*. (2010) pay particular attention to the interplay of bargaining coverage and membership finding that membership effects on satisfaction depend on that interplay. They confirm that sorting explains the apparent difference between members and non-members who are covered but find that among uncovered workers union members have lower satisfaction. This is a variant on previous results in that it argues that for the core of covered members satisfaction is no lower. It is only for the uncovered members who presumably want a covered job that dissatisfaction does not reflect sorting.

As an alternative to instrumenting membership in cross-sectional estimates, Heywood *et al.* (2002) use rudimentary longitudinal techniques to account for the supposed individual dissatisfaction effect. Using the first four waves of the British Household Panel Study (1991 – 94), they show that the dissatisfaction of union members remains in pooled regressions, simple difference equations that should remove the fixed effect and in linearized fixed effect models. They conclude that “sorting and individual effects do not explain reduced satisfaction in union jobs (605).”

This may not be surprising if the key element of sorting is not across individuals but across jobs. The second broad explanation of the paradox argues that union jobs are inherently less pleasant than non-union jobs. Indeed, that is the reason they are unionized. The union didn’t create the unpleasant aspects but, instead, was a response to them. Duncan and Stafford (1980) argue that unions arise in exactly those circumstances in which workers share adverse working conditions.[1] Thus, Hirsch (1993) and Hirsch and Macpherson (1998) present evidence suggesting that the union wage premium in the US trucking industry largely reflects a compensating differential for poor working conditions. In an effort to bring this logic to the issue of job satisfaction, Gordon and DeNisi (1995) examine the difference in job satisfaction and quit rates within three U.S. bargaining units containing both union and non-union workers. They found no differences by union membership. Thus, either the unions provide "voice" to non-union members or, more likely they claim, non-union members in the bargaining units share job conditions that generate similar levels of satisfaction for otherwise equal union and non-union members.

To the extent that many of the worker sorting models include establishment or job level controls as instruments, they are implicitly recognizing this second type of sorting across jobs. Indeed, both Borjas (1979) and Bender and Sloane (1998) explicitly identify this as reverse causation. Inherently worse jobs are more likely to be unionized. This logic would seem to require not a mixture of firm and individual controls to examine membership but a focus on the firm characteristics that lead to unions winning elections (Saks and Farber 1983). Moreover, the individual membership decision ultimately depends on a worker both searching for a union job and being offered a union job (Abowd and Farber 1983). As a result, reduced form probability estimates of the sort used in the literature cannot model which characteristics cause a worker to "choose" a unionized job. Instead, it is a mixture of worker and firm characteristics that tend to be associated with membership with little hope of disentangling the two.

Yet, the separate ideas of sorting across workers and firms may profitably inform longitudinal estimates in a fashion not previously considered in this context. Individual worker fixed effects allow the examination of how a change in membership or coverage status influences the job satisfaction of a specific worker hopefully eliminating the influence of sorting across workers. Similarly, match specific fixed effects (capturing all years a given worker has held a particular job) allow the examination of how a change in membership or coverage influences the satisfaction of a worker with the job (and presumably its characteristics) held constant. This may eliminate the influence of sorting across jobs. Our study is the first to use a relatively long panel of UK workers to investigate the influence of both membership and coverage on the job satisfaction of workers. We do so controlling for worker fixed effects, then match fixed effects and ultimately for both types of fixed effects. Our conclusion is that the fundamental paradox of covered union members reporting relative dissatisfaction persists. This leads us to suggest that sorting of workers or of union coverage is not a large part of the explanation.

Finally, we show that the same covered members who express dissatisfaction even when accounting for fixed effects are also those least likely to want to look for new jobs and least likely to actually quit. Previous work confirms that lower job satisfaction is associated with both an increased probability of intending to quit and of actually quitting (Clark et al. 1998, Clark 2001 and Kristensen and Westergard-Nielsen 2004). While not without exception, union members are typically found to be less likely to quit holding job satisfaction constant.[2] We also report that the quit responses of covered union members are the least responsive to greater job dissatisfaction. Thus, the paradox remains. Union members (especially covered members) express the least satisfaction, are the least likely to quit and their quit decisions are the least responsive to changes in job satisfaction.

3. DATA

We draw data from the British Household Panel Survey (BHPS), a nationally representative sample annually interviewing approximately 10,000 individuals from roughly 5,500 households. We use all twelve waves from 1996-2007. Earlier waves (1992-1995) only asked questions regarding certain key job characteristics, including some related to union membership and coverage, for individuals who changed jobs. As a consequence, we omit these earlier waves rather than utilise data where key variables may suffer from measurement error.

Information on union status comes from two sources in the BHPS.[3] First, the employment section annually asks workers (in sequence) whether they are in workplace that is covered by a union (cover) and then whether they are a member of this union (covered member). While this allows a distinction between covered members and non-members, all other workers are simply grouped into uncovered. Second, the BHPS values and opinions section (biannually) asks whether individuals are a member of a union (union(v)). The information from across these two sources can be combined to create an indicator of uncovered union members (uncovered member) with uncovered non-members being the base. In our results we demonstrate and discuss the robustness of our estimates to the choice of membership information used.

All job satisfaction questions in the BHPS are reported on a 7 point Likert scale, 1 being the least satisfied, 7 the most satisfied. At different times a variety of job satisfaction questions have been included in the BHPS. The overall job satisfaction question ("How satisfied are you with your job") is asked of respondents throughout the BHPS sample period, but the questions regarding certain domains of job satisfaction changed markedly from 1998 onwards. Four domain specific job satisfaction questions are available for this period: satisfaction with pay, satisfaction with hours worked, satisfaction with job security, and satisfaction with the work itself.[4] We restrict our sample to those aged 20 to 65 and exclude the self-employed and those with missing data. This yields an unbalanced panel of 73,515 individuals. Sample statistics are provided in Table A1. The variables displayed represent typical controls from the literature on job satisfaction.

<INSERT TABLE 1>

Table 1 presents average job satisfaction levels according to different categories of union membership and coverage. The first three columns reflect the union membership and coverage categories derived from the employment section of the BHPS. These demonstrate that covered union members are significantly (p-val = 0.00) less satisfied with their work than both covered non-members and workers without coverage. The second three columns reflect the categories of the values section. These allow disaggregation of workers without union coverage into members and non-members at the cost of a smaller sample size. Again, covered union members are less satisfied with their jobs than covered non-members. They are also less satisfied than uncovered non-members. Finally, union members without coverage are less satisfied than union members with coverage.

4. METHODOLOGY

Job satisfaction of worker *i* at time *t* can be represented as the following wellbeing (W_i) function (*f*) :

$$W_i = f(U_i, Z_i, X_{i,}) \qquad (1)$$

Where *Z* is a vector of hedonic work characteristics and *X* is a vector of personal characteristics. *U* is a vector of union membership and coverage variables. Given the centrality of controlling for worker and job match fixed effects, we take the ranking of job satisfaction to be more nearly cardinal. We rely primarily on probit adapted ordinary least squares (POLS) as developed by Van Praag and Ferrer-i-Carbonell (2008 p. 29 - 34). This approach follows the demonstration by Ferrer-i-Carbonell and Frijters (2004) that while fixed effects can be critical in estimating the determinants of satisfaction, the assumption of cardinality instead of ordinality of the responses to satisfaction questions is typically unimportant. While we briefly confirm this for our own results, the computational ease of POLS allows greater ease in accounting for fixed-effects and running robustness checks when utilising panel data.

Implementing POLS begins by deriving Z values of a standard normal associated with the cumulative frequencies of the *k* different categories of the dependent variable. Then the expectation of a standard normally distributed variable is taken for an interval between any two adjacent Z values. Thus if the true unobserved continuous variable is W^* where the observed $W_i = j$ if $u_{j-1} < W_i^* < u_j$ for $j=1,2..k$ then the conditional expectation of the latent variable is given by:

$$\overline{W} = E(W_i^* \mid u_{j-1} < W_i^* < u_j) = \frac{n(u_{j-1}) - n(u_j)}{N(u_j) - N(u_{j-1})} = \frac{n(u_{j-1}) - n(u_j)}{p_j} \qquad (2)$$

Where *n* is the standard normal density and $p_j=N(u_j)- N(u_{j-1}),j=1,...k-1$. This approach allows the application of ordinary least squares on the conditional expectations. Critically, with panel data POLS easily allows for the inclusion of fixed effects.

This ultimately leads to the estimation of the following equation:

$$\overline{W}_{itk} = \phi + \beta \mathbf{U}_{it} + \gamma \mathbf{X}_{it} + \delta \mathbf{Z}_{it} + \alpha_i + \eta_{ik} + \sigma \mathbf{Y}_t + \varepsilon_{itk} \qquad (3)$$

Where ϕ is the intercept, ***U*** is a vector of union membership and coverage covariates, ***X*** is a vector of personal characteristics, ***Z*** is a vector of work characteristics, α_i are individual specific fixed effects, η_{ik} are match specific effect for work *i* in job match *k*,***Y*** a series of year dummies and ε_{itk} an IID error term. To aid interpretation we use trade-offs within estimated equations to identify the relative magnitude of variables of interest, unionization in our case.

5. BASIC RESULTS

The initial results in Table 2 pool all years 1996 to 2007 presenting estimates of job satisfaction with clustered standard errors at the worker level. The parsimonious estimate in column 1 reveals a statistically significant negative coefficient for covered union members. The estimates of the union coefficients increase with the controls which initially include those from Table A1, one-digit industry and occupational controls and year dummies. As a judge to the magnitude, the estimate in column 2 implies that the dissatisfaction of covered union members can be eliminated only with an increase in earnings of 3.43 log points (.144/.042). While this suggests a large degree of dissatisfaction by union members, it might be argued that wages and union status should not both be explanatory variables as they are related. Yet, we find no evidence that the union influence on wages is obscuring the influence of unions on job satisfaction. While higher wages increase job satisfaction, removing wages from the estimates in Table 2 does not change the size or significance of the union coefficients.

The final estimate in Table 2 adds to the controls a series of working conditions.[5] Two points seem pertinent. First, while in this more complete specification, covered non-union members report lower satisfaction, the far larger negative influence is among those who are covered members. Second, the addition of controls for working conditions in the final estimate does not alter the coefficient for covered members. If a large portion of union dissatisfaction flowed from underlying poor working conditions, one might have anticipated a decline in the union coefficients. The coefficient for covered members is unchanged and that for covered non-members actually increases substantially.

<INSERT TABLE 2>

In further unreported estimates (available on request) we investigate the robustness of our results across various relevant sub-samples. First, in light of the high proportion of covered public sector workers evident in Table A1, we re-estimate model (III) for public sector and non-public sector workers separately. The pattern of sign and significance of the union variables remained the same as that reported in Table 2. We then estimated the models separately by gender and could not reject the null that the point estimates are identical by gender for the two union coefficients. Finally, we re-estimated the models by size of the firm, splitting at greater or less than 100 workers. While the negative effect of being a covered union member remains, it was noticeably larger in smaller firms, -0.165 [0.021] compared to -0.094 [0.023]. The negative influence of being a covered non-member was only present for workers in small firms.[6]

A basic concern with even these simple pooled estimates is that the excluded reference group is all uncovered workers regardless of union membership. Ideally, one would want to separate this reference group into uncovered members and uncovered non-members. Bryson *et al*. (2010) argue this distinction is important as it uncovered members who are the only union members genuinely dissatisfied. In order to make this distinction we supplement the coverage information from the employment section of the BHPS with the additional indicator of union membership drawn from the values section. Using this additional indicator we can identify both covered and uncovered union members as well as both covered and uncovered non-union members.

Table 3 shows estimates using these alternative union variables while including all the other controls as in the last column of Table 2. For brevity we report only the relevant estimates. In the first column, we include the new indicator of union membership taken from the values section, member(v). The estimate reveals a large negative association between membership and job satisfaction. In the next column we use the new membership variable and the original coverage variable mimicking our earlier estimate of the influence of covered members and covered non-members on job satisfaction. Again a large negative relationship between being a covered member and job satisfaction is apparent, with a smaller negative coefficient for covered non-members. In the final column we show the full specification identifying each of the membership and coverage categories. The new omitted category is uncovered non-members. The estimates reveal that members (either covered or not) report far less job satisfaction than uncovered non-members. Covered non-members also continue to show less job satisfaction but the result is more muted.

<INSERT TABLE 3>

In the pooled data we have reported a robust negative relationship between job satisfaction and unions, one that is more strongly related to actual membership rather than coverage. Yet, as emphasized, such associations may be spurious due to unobserved variation in the types of workers who choose to join unions. If the inherently dissatisfied join unions, the association reflects sorting not causation. Here we exploit the panel dimension of the BHPS introducing worker fixed effects. The results in the top panel of Table 4 report worker fixed effect estimates that otherwise mimic the final columns from Table 2 and Table 3. In the first estimation the original split of covered members and non-members are compared to all uncovered workers. The point estimate for covered members falls as a result of the fixed effects but retains a similar order of magnitude and high statistical significance. The coefficient on covered members drops substantially from the pooled estimate and is not significantly different from zero. Thus, the primary implication remains that membership is associated with dissatisfaction. Moreover, the dissatisfaction does not flow from union members being inherently less satisfied as the current estimate is identified by observing the same worker changing membership status over time.

<INSERT TABLE 4>

These estimates are based on a reasonably large number of status changers and on transitions that are not all in one direction (away from membership for example). We present the transition matrix for the status changers in Appendix Table A2. There are 7419 transitions between the three union states and 897 that move from non-member in to covered membership. Thus, the fixed effect estimates are based on a reasonably large small size. Moreover, we found little indication that the sample that made transitions was overly selected or unrepresentative. We estimated our pooled estimate from the last column of Table 3 revealing that the sign and relative magnitudes of all the union indicators remained identical to those in the full panel. Moreover, despite the loss of sample size the pooled estimate indicated a large significant negative coefficient for covered members.

The second column presents a similar pattern using the four-way split of membership and coverage. The covered members remain significantly less satisfied than the uncovered non-members. Indeed, the coefficient is very similar to that in the first column. The coefficient associated with covered non-members again drops substantially when compared to the estimate of its size that did not account for worker fixed effects. Now this is matched by a similar drop for the coefficient associated with uncovered members which drops to a fifth of the point estimate size without accounting for worker fixed effects. Neither the coefficient for covered non-members nor that for uncovered members is statistically different from zero. Thus, accounting for worker fixed effects makes clear that the focus for a union influence on job satisfaction should be among covered union members. The fact that this relationship persists in the face of worker fixed effects suggests that the sorting of workers across union status is not the source of the apparent union dissatisfaction.

The second theoretical contention is that union dissatisfaction reflects the fact that jobs more likely to be unionized have working conditions associated with lower job satisfaction. Thus, workplaces where conditions are more dangerous or less pleasant generate lower job satisfaction and it is these conditions that led to the jobs becoming unionized in first place. In this view, workers would report lower job satisfaction in these jobs independent of union status and it is the job, not the union, causing the dissatisfaction. Controlling for the non-random sorting of workers will not eliminate the spurious negative correlation in this case. It is necessary to hold the job constant and watch a workers change status within that job. While the BHPS does not identify jobs as units of observation, the job history files allow for the identification of job matches. A job match is identified every wave that a given worker holds the same job. We generate fixed effect estimates that hold job matches constant. Thus, the variation in the role of unionization on job satisfaction is measured by status changing within specific matches. As the time frame is not overly long, it seems unlikely that job conditions change dramatically over the match and the fixed effect estimate should hold these job conditions constant.

In Table 4 we now present the fixed effect estimates using the match specific fixed effects and the original coverage based definitions of unionization.[7] The first set of results in the lower panel includes fixed effects for job matches but not for workers. These estimates demonstrate a large and significant negative coefficient associated with being a covered union member. Indeed, the size of the coefficient remains nearly the size from the pooled analysis in Table 2. The size of the coefficient on the covered non-members drops in half relative to the pooled estimate but does remain statistically different from zero. Finally, in the last estimate in Table 4 we simultaneously account for worker fixed effects and job match fixed effects. This represents an attempt to control for both sorting of workers and the sorting of union status across jobs. The coefficient for covered union members declines relative to previous results but remains negative and with a t-statistic of over 3. Accounting for the combination of sorting by workers and the sorting of union coverage across jobs does not alter the conclusion that covered union members report lower job satisfaction. This increases the likelihood that the dissatisfaction is genuinely and directly associated with covered membership.[8]

Again we present the transition matrix but this time focusing exclusively on those that change union status while remaining in their existing job. As shown in panel B of Table A2, these are the majority of all status changers, 5751. There continues to be a reasonable number of observations for estimating the fixed effects and it remains clear that the transitions are not unidirectional. Moreover, even on this smaller subset of within job transitions, the pooled estimates mimic those from Table 3 and that for covered union members is negative and significant.

The result for covered non-members in Table 4 is also noteworthy. When controlling for both worker and match fixed effects, the coefficient retains size and a t-statistic of over 1.5 but *switches sign* indicating that covered non-members report, if anything, greater job satisfaction. This switch requires accounting for both types of sorting but hints that the most satisfied are those who have the benefits of coverage but not the costs of membership. Our examination of satisfaction with various job aspects will repeat this pattern. Such results fit with the view that covered non-members free-ride on the gains of unions without contributing the effort, time and energy required as a member of the union. Past empirical literature typically estimates free-riding as the extent to which covered workers can fully gain the union membership wage premium without joining. The evidence on this appears mixed (contrast Booth and Bryan 2004 with Budd and Na 2000) but the issue might better be thought of in terms of job satisfaction. If participating in the activities of the union requires time and effort, the advantages of free-riding may not be captured in wages alone. Seen this way, the difference between the two coefficients in this final column estimates a very large job satisfaction advantage to free-riding (.023 - -.047 = .070). This advantage may help explain the growing incidence of such free-riding in Britain (Bryson 2008).

The critical point of this section is that while cross-sectional estimates suggested that many types of worker associations with unions might diminish job satisfaction, the fixed-effect estimates confirm that only covered members have lower job satisfaction. Thus, the true effect that is obscured by sorting is not the neutral or positive influence of membership as suggested by several of the instrumental variable estimates in past literature. If anything the true effect obscured by sorting is the increased job satisfaction associated with free-riding by covered non-union members.[9]

6. DOMAINS, QUITS AND ROBUSTNESS CHECKS

Respondents in the BHPS identify their satisfaction with various aspects, or domains, of their job. The responses use the same Likert scale and examining these responses provides evidence on the source of the dissatisfaction expressed by union members. Table 5 summarizes fixed effect estimates on the extent of satisfaction with these aspects. Note that there is variation in the number of years various aspects appear within our time frame. The estimates that control for only individual fixed effects suggest that covered union members are less satisfied than uncovered workers with each aspect of their work. Yet, this moderates with the inclusion of the match fixed effects. Accounting for both types of fixed effects reveals that covered union members are significantly less satisfied with their job security and the work itself. They are insignificantly different in their satisfaction with pay and hours. Interestingly, in the same estimates, the covered non-members are significantly more satisfied than uncovered workers with their pay and hours. Again, we think these latter results hint at the potential advantages of free-riding.

<INSERT TABLE 5>

If union members are genuinely dissatisfied they might be anticipated to express this by quitting. Yet, the conjecture that they stay put to improve their current jobs is a critical part of the exit-voice hypothesis. As emphasized by Artz (2010, 2011), there is no evidence that their job satisfaction increases over time with a unionized employer so workers are either unable to improve their current job but continue to stay put or they improve their current job and it does not result in increased reported job satisfaction. While we cannot distinguish these alternatives, we do examine quits as part of isolating the group of workers for which the seeming paradox applies most strongly.

In quit models, Clark (2001) confirms that greater job satisfaction plays a role in diminishing the probability of quits even after controlling for a long set of reasonable controls including earnings, hours, demographics and family structure. Clark shows that satisfaction with job security is particularly important in explaining quits but that this differs by age and gender. As part of his examination of quit behaviour, he explores the role of unionization asking "is union job dissatisfaction real?" His results indicate that holding job satisfaction (and all other controls) constant union members are less likely to quit. He also presents mixed evidence on whether there is a difference by union status in quitting as a function of job satisfaction (Clark 2001 p. 237).

We return to these issues first estimating the influence of union status on both looking for a new job and on quits holding job satisfaction and our other set of controls constant. The results in upper panel of Table 6 show that it is the covered union members who are the least likely to report they are looking for a new job. Indeed, the uncovered members are actually more likely to be looking for a new job than uncovered non-members perhaps to find a covered job. The quit results in the lower panel of Table 6 estimate the probability of quitting next period as a function of this period's characteristics. The only significant negative coefficient is that for covered union members. We take from these two estimations that the paradox should be fully focused on covered union members.[10] They are the only category that consistently reports lower satisfaction when accounting for sorting. They are the only category that is significantly less likely to quit.

<INSERT TABLE 6>

As a note we examine the samples separately across the different types of union status (we do not simply add interactions). In each case we examine the role of job satisfaction on quits within a particular union status using the full set of controls. Covered members have the smallest reduction (these results are available upon request). We mention these results as they again emphasize the importance of covered union members. Not only are they the least likely to quit for a given level of job satisfaction but their quit decisions are the least responsive to changes in job satisfaction.

7. CONCLUSION

A series of papers have tested whether or not the dissatisfaction of union members results from sorting. Many find evidence that it does and so suggest there is no paradox as union members react to their job conditions in similar ways as non-members. The basis for this suggestion has routinely been instrumental variable models (of varying sophistication) that identify variation in one or more dimensions of unionization. We provide a contrasting method to account for sorting by controlling for individual and job match fixed effects in panel data. The evidence is robust and convincing that controlling for these fixed effects does not cause the dissatisfaction to vanish. This evidence diminishes the likelihood that reported dissatisfaction results from either sorting by workers or sorting of union status across jobs.

The second key result from this analysis of job satisfaction is the clear indication that the dissatisfaction is uniquely and robustly associated with covered union members. The other categories of union status had coefficients that varied greatly with estimation and tended toward small and insignificant when controlling for fixed effects. Most dramatic was the positive result for covered non-members, especially for some of the domains, hinting at the benefit of free-riding.

The intention to look for work and the quit data continues to round out the paradox. Covered union members are significantly less likely to quit with job satisfaction held constant and, in a new test, are less sensitive to job satisfaction in their quit behavior. Thus, the dissatisfaction is not generated by sorting but the quit behavior does not reflect the dissatisfaction. Thus, our evidence argues that paradox remains with union members less satisfied but less willing to quit and the issue remains explaining how this can be a long-term equilibrium.

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Table 1 Job Satisfaction by Union Status.

	Employment Section			Values Section			
	Covered Union Member	Covered Non-Member	Not Covered	Covered Member	Uncovered Member	Covered Non Member	Uncovered Non Member
Job Satisfaction	5.306	5.376	5.381	5.259	5.164	5.413	5.393
Obs	23,534	13,687	36,256	9512	468	10073	18577

Source: BHPS 1996-2007.

Table 2 Job Satisfaction POLS Estimates, BHPS 1996-2007, Employees 20-65 years old

	(I)	(II)	(III)
Covered Member	-0.087* [0.016]	-0.144* [0.018]	-0.168* [0.018]
Covered Non-Member	-0.024 [0.016]	-0.043* [0.016]	-0.067* [0.017]
Male		-0.107* [0.015]	-0.098* [0.017]
Age		-0.041* [0.004]	-0.039* [0.004]
Age^2		0.0005*[0.000]	0.0005*[0.000]
Tenure		-0.001* [0.0003]	-0.001* [0.0003]
Married		0.091* [0.016]	0.093* [0.016]
Dependent Child		0.113* [0.020]	0.110* [0.020]
A Level		-0.126* [0.017]	-0.128* [0.018]
Diploma		-0.144* [0.027]	-0.148* [0.027]
Degree or higher		-0.207* [0.021]	-0.212*[0.021]
Log Wage		0.042* [0.008]	0.034* [0.008]
Public Sector		0.088* [0.020]	0.049* [0.020]
Work Hours		-0.008* [0.001]	-0.008* [0.001]
Overtime Hours		0.002*** [0.001]	0.002** [0.001]
Temporary Job		-0.162* [0.026]	-0.137* [0.026]
Manager/Supervisor		0.038* [0.014]	0.033** [0.012]
Employer Funded Training		0.075* [0.013]	0.066* [0.013]
Firm Size 50-99		-0.099* [0.016]	-0.102* [0.015]
Firm Size 100-499		-0.153* [0.017]	-0.156* [0.017]
Firm Size 500+		-0.132* [0.020]	-0.133* [0.019]
Year Controls		X	X
Industry Controls		X	X
Occupation Controls		X	X
`Work Conditions' Controls			X
r^2	0.001	0.042	0.047
Observations	73513	73513	73513

*Standard errors in parentheses clustered at the individual level. *,** and *** indicate statistical significance at the 1%, 5% and 10% level, respectively.*

TABLE 3 Alternative Measures of Unionisation, Values Section, POLS, BHPS 1997-2007.

	(I)	(II)	(III)
Member (V)	-0.146*		
	[0.018]		
Covered Member (V)		-0.165*	-0.171*
		[0.021]	[0.021]
Covered Non-Member(v)		-0.038**	-0.044*
		[0.019]	[0.019]
Uncovered Member (v)			-0.167*
			[0.062]
r^2	0.042	0.042	0.042
observations	38644	38644	38644

*The included controls are all of those in model (III) of table 2. Standard errors in parentheses clustered at the individual level. * and ** indicate statistical significance at the 1% and 5% level, respectively.*

TABLE 4 Fixed Effects Estimates of Job Satisfaction, BHPS 1996-2007.

	Worker Fixed Effects	
Covered Union Member	-0.095*	
	[0.017]	
Covered Non-Member	0.003	
	[0.015]	
Covered member(v)		-0.094*
		[0.027]
Covered non member (v)		-0.012
		[0.022]
Uncovered member(v)		-0.030
		[0.066]
r^2	0.015	0.014
Observations	73,515	38,644

	Match FE	Match and Worker FE
Covered Member	-0.142*	-0.062*
	[0.012]	[0.018]
Covered Non-member	-0.041*	0.023
	[0.012]	[0.015]
Adjusted r^2	0.068	0.058
Observations	73,513	73,513

*Standard Errors in Parentheses, * indicates statistical significance at the 1% level. The included controls are all of those in column 3 of table*

Table 5 Fixed Effects Estimates of Union Coverage and Domains of Job Satisfaction, BHPS 1996-2007

	Match FE	Match & Worker FE
	Security	
Covered Member	-0.146* [0.013]	-0.032*** [0.019]
Covered Non Member	-0.121*[0.014]	-0.024 [0.016]
r^2	0.105	0.080
	Work Itself	
Covered Member	-0.177*[0.012]	-0.098* [0.018]
Covered Non Member	0.061*[0.013]	0.006 [0.016]
r^2	0.076	0.059
	Pay	
Covered Member	-0.038* [0.012]	-0.010 [0.017]
Covered Non Member	-0.006 [0.012]	0.039*[0.014]
r^2	0.071	0.059
	Hours	
Covered Member	-0.055* [0.012]	0.006 [0.018]
Covered Non Member	0.040* [0.013]	0.042* [0.015]
r^2	0.126	0.085
Observations	73515	

*The included controls are all of those in model (III) of table 2. Standard errors in parentheses. *, ** and *** indicate statistical significance at the 1%, 5% and 10% level, respectively.*

TABLE 6 Unions and Quitting, BHPS 1996-2007, Probit Marginal Effects

	Looking For a New Job	
	(I)	(II)
Covered Member	-0.020* [0.005]	
Covered Non-Member	-0.004 [0.005]	
Covered Member(v)		-0.018** [0.009]
Covered Non-Member (v)		-0.015** [0.008]
Uncovered Member(v)		0.086* [0.031]
Pseudo r^2	0.090	0.060
Observations	62306	27,437
	Quit in Next Year	
	(i)	(II)
Covered Union Member	-0.014*[0.002]	
Covered Non-Member	-0.002 [0.002]	
Covered Member(v)		-0.009** [0.004]
Covered Non-Member (v)		-0.001 [0.003]
Uncovered Member(v)		0.020*** [0.013]
Pseudo r^2	0.076	0.058
Observations	58380	27,811

*All other controls as per (III) in Table 2. Standard errors in parentheses clustered at the individual level. *,** and *** indicate statistical significance at the 1%, 5% and 10% level, respectively.*

TABLE A1- Summary Statistics, BHPS 1996-2007. UK Employees

	Covered Member	Covered Non-Member	Not Covered
Male	0.452	0.449	0.528
Age	40.879	37.364	36.696
tenure2	18.663	16.838	16.418
Married	0.629	0.535	0.503
Dependent Child	0.176	0.178	0.133
A Level	0.209	0.230	0.218
Diploma	0.096	0.090	0.071
Degree or Higher	0.226	0.199	0.143
Log Pay	6.742	6.550	6.303
Public sector	0.586	0.430	0.046
Hours	34.961	33.736	35.820
Overtime Hours	4.195	3.406	3.793
Temporary Job	0.025	0.074	0.042
Manager/supervisor	0.389	0.336	0.369
Employer provides training	0.207	0.190	0.138
Firm Size 50-99	0.277	0.236	0.275
Firm Size 100-499	0.274	0.276	0.183
Firm Size 500+	0.252	0.276	0.082
Annual Increment	0.636	0.579	0.315
Night Shift	0.025	0.020	0.016
Shift Work	0.113	0.063	0.051
Other Non-usual hours	0.075	0.082	0.081
Flexitime	0.154	0.186	0.094
Annualised Hours	0.049	0.043	0.033
Term Time Work	0.040	0.028	0.007
Job Sharing	0.006	0.005	0.004
Observations	23545	13692	36276

TABLE A2- Transitions between Union Status, UK Employees 1996-2007, BHPS

PANEL A: ALL STATUS CHANGES

	Transitions		
	Covered Member [t]	Covered Non-Member [t]	Non Member [t]
Covered Non-Member[t-1]	1,086		1,812
Non Member [t-1]	897	2,067	
Covered Member [t-1]		689	868
n=7419			

PANEL B: STATUS CHANGES WITHIN THE SAME JOB

	Entrants		
	Covered Member [t]	Covered Non-Member [t]	Non-Member [t]
Covered Non-Member[t-1]	877		1378
Non Member [t-1]	709	1,569	
Covered Member [t-1]		555	663
n=5751			

Endnotes

[1] See Heywood (1988) for further evidence supporting this view

[2] Hersch and Stone (1990) provide an exception with their case study of the US state of Oregon in which union members were insignificantly different than non-members with respect to quitting.

[3] See Swaffield (2001) for a discussion of this.

[4] Prior to 1998 questions were also asked regarding satisfaction with the boss and promotion.

[5] Specifically a range of controls available in the BHPS that describe timing of work and pay arrangements; whether the individual worked shift work, night shifts or other non-usual hours, flexitime, annualised hours, job sharing and received an annual pay increment.

[6]More generally, we emphasize that the basic size and significance of the estimates in column 3 of Table 2 are robust to a wide variety of variable inclusions and exclusions. Moreover, the sign and significance remain virtually identical in actual ordered probit estimates.

[7] The biannual asking of the values definitions greatly reduces both the sample size and the variation needed for properly estimating the models.

[8]In addition to their complexity, fixed effect ordered probits have the difficulty of potential inconsistency (Green 2001). Nonetheless, the sign and significance of the key estimates are not affected by accounting for both individual and match fixed effects in a specialized ordered probit routine run in Limdep (E.18.5.10) although convergence depends on the exact specification.

[9] Again we estimated the match and worker fixed effects for a series of sub-samples, males *vs* females, public sector *vs* non-public sector and large *vs* small firms. In all these estimates (available on request) the pattern of sign and significance of the covered union member remains as reported in table 4.

[10] We have no confidence in quit estimates with our two kinds of fixed effects. The resulting condition logit estimation is estimated on those who either move from not quitting to quitting or from quitting to not quitting. There is no reason to anticipate symmetry. Moreover, the variation is minimal and most crucially every quit is perfectly associated with a change in the match specific fixed effect. In short, the estimations seem meaningless.