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Do Unions Cause Job Dissatisfaction? Evidence from a Quasi-Experiment in the United Kingdom

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Abstract

Unionized workers tend to be less satisfied with their jobs than their non-union counterparts. Despite 40 years of research that has sought to explain this phenomenon, the causes of this relationship are not fully understood. Drawing on nationally representative panel data from the UK, this study uses quasi-experimental methods to compare how the job satisfaction of union members and their non-union counterparts changes in response to an exogenous event. Results suggest that working conditions rather than the behaviour of unions are the more likely cause of union member job dissatisfaction.

It is a well-established empirical regularity that union members are less satisfied with their jobs than their non-union counterparts (Borjas 1979; Bryson *et al*. 2010; Freeman 1978; Green and Heywood 2015; Laroche 2017, 2016). This is widely described as an apparently puzzling or anomalous finding because it is not clear why a worker would join a union if it reduces her job satisfaction (Bryson *et al*. 2010; Freeman 1978; Freeman and Medoff 1984; Green and Heywood 2015; Powdthavee 2011). The original theoretical explanation for this seeming paradox is that unions make workers dissatisfied with their terms and conditions of employment in order to galvanize them into pressing for improvements. Unions are typically successful in this endeavour, so union workers tend to be paid more and enjoy better protection from arbitrary dismissal than non-union counterparts, and it is these job characteristics which explain why union workers quit less despite the higher job dissatisfaction that results from a culture of protest against management

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(Borjas 1979; Freeman 1980, 1978; Freeman and Medoff 1984). Because union workers quit less, there is also likely to be a greater stock of dissatisfied workers in the union sector. There is some disagreement about whether such dissatisfaction is 'true' or 'voiced' (Freeman and Medoff 1984: 139) but the basic underpinning idea is that unions have a causal impact on job dissatisfaction.

This theory has been challenged by two alternative explanations. First, that union membership and job satisfaction are jointly influenced by working conditions that extant studies have not been able to measure adequately (Bender and Sloane 1998; Gordon and Denisi 1995; Pfeffer and Davis-Blake 1990). Once working conditions are properly taken into account, the supposed relationship between job satisfaction and union membership disappears because it is not causal (or may even become positive). Second, workers who are more prone to experience job satisfaction because of aspects of their personality or personal values are more likely to sort themselves into union membership (Heywood *et al*. 2002). Once these endogenous variables are properly accounted for, any relationship between union membership and job satisfaction will be trivial.

Which of these theories is correct matters because being satisfied with time spent at work has an inherent value to the worker (Steel *et al*. 2019). Job satisfaction is also a higher-level job attitude which is predictive of a range of desirable (from a management perspective) worker behaviours (Harrison *et al*. 2006; Harter *et al*. 2002). Consequently, the management of worker satisfaction is a global industry worth hundreds of millions of dollars a year (Kowske 2012). If unions act to undermine job satisfaction, it is plausible that they are undermining these large investments by reducing the incidence of the desirable behaviours which job satisfaction is associated with.

In this context the novel contribution of this article is to test the theory that it is union membership (as opposed to sorting or working conditions) that causes job dissatisfaction using quasi-experimental methods (difference in differences and discontinuity analysis) that examine whether an exogenous change in working conditions was associated with changes in job satisfaction and whether the size of the association differed according to workers' union membership status. The strength of quasi-experimental methods is that they allow us to make much stronger inferences about causality than purely observational analysis. Against this strength needs to be set the limitation that our results will not necessarily generalize beyond the population of our study; the nature of the causal relationships may be different in other countries and other sectors. By utilizing a quasi-experimental approach, this article addresses a significant limitation of the extensive literature on this issue identified by a recent article in this journal (Green and Heywood 2015: 597).

The exogenous change is a change in the pension arrangements for five million UK public sector workers that was announced in March 2011, triggering a large-scale industrial dispute involving unions representing 2.1 million workers. The dispute led to negotiated changes in the proposals which the unions accepted, bringing the dispute to an end in December

2011. Increased worker pension contributions equivalent to 3 per cent of salary for most workers were then implemented from April 2015. Was the announcement of the pensions changes associated with a fall in job satisfaction among those affected and was the size of any change greater for union members? To answer these questions the studies draw on the United Kingdom Household Longitudinal Study (UKHLS, sometimes referred to as 'Understanding Society', Knies 2015; University of Essex *et al*. 2016) and its predecessor, the British Household Panel Survey (BHPS), a high-quality, nationally representative panel dataset. The article begins by considering theory and existing empirical evidence. It then explains the data and methods used in the study before presenting results.

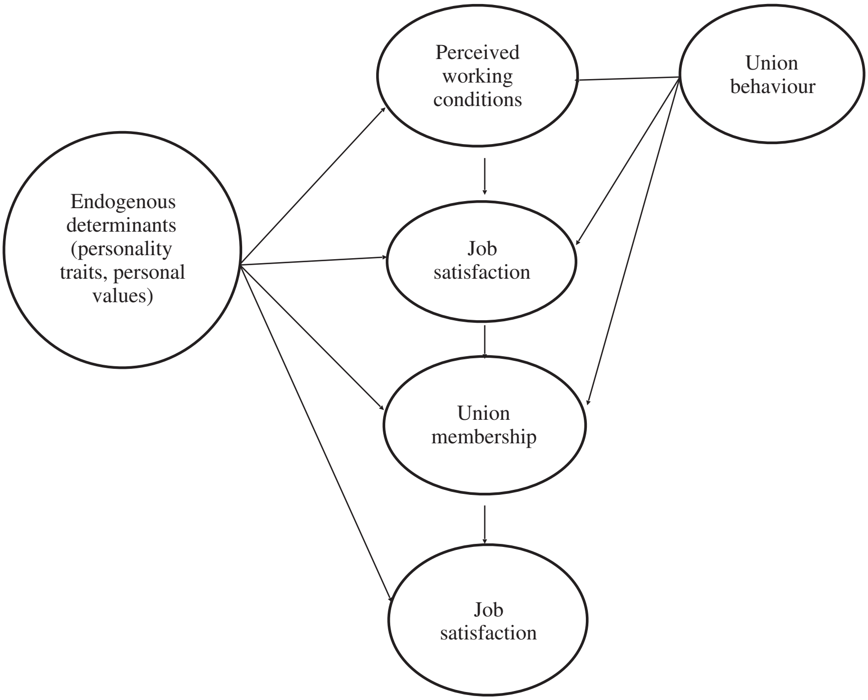
1. Theory and Past Research

There is an abundance of evidence suggesting that union workers (or union members in sectors and societies where open shop unionism predominates) are less satisfied with their jobs. This was first observed by Hamermesh (1977) and Freeman (1978) in data from the USA with other US studies finding similar results (Artz 2010, 2012; Borjas 1979). Similar evidence has been uncovered in the UK (Bender and Sloane 1998; Clark 1996; Green and Heywood 2015; Guest and Conway 2004; Powdthavee 2011). The extent to which union workers are more dissatisfied in other countries with different institutional arrangements has been questioned (Hipp and Givan 2015). A recent meta-analysis of 59 published studies suggested that the relationship does not generally hold outside of the US and UK (Laroche 2016), although the same author has subsequently found evidence for the relationship in France (Laroche 2017). Laroche's meta-analysis also found that cross-sectional studies tend to over-estimate the size of the union–job dissatisfaction relationship because they do not adequately account for individual workers' values, attitudes and personality traits. Nevertheless, a number of studies suggest a link between unionization and job dissatisfaction even after taking these factors into account (Artz 2010, 2012; Green and Heywood 2015; Heywood *et al*. 2002; Powdthavee 2011).

As we explained above, the original explanation for this empirical regularity is that unions induce job dissatisfaction (Borjas 1979; Freeman and Medoff 1984). This idea has been challenged by those who argue that union worker job dissatisfaction reflects poorer working conditions for union workers (Bender and Sloane 1998; Gordon and Denisi 1995; Pfeffer and Davis-Blake 1990). It has also been suggested that those predisposed to job dissatisfaction because of personality and values might be more likely to become union members (Bryson *et al*. 2010, 2004; Clark 1996; Green and Heywood 2015; Heywood *et al*. 2002; Laroche 2017).

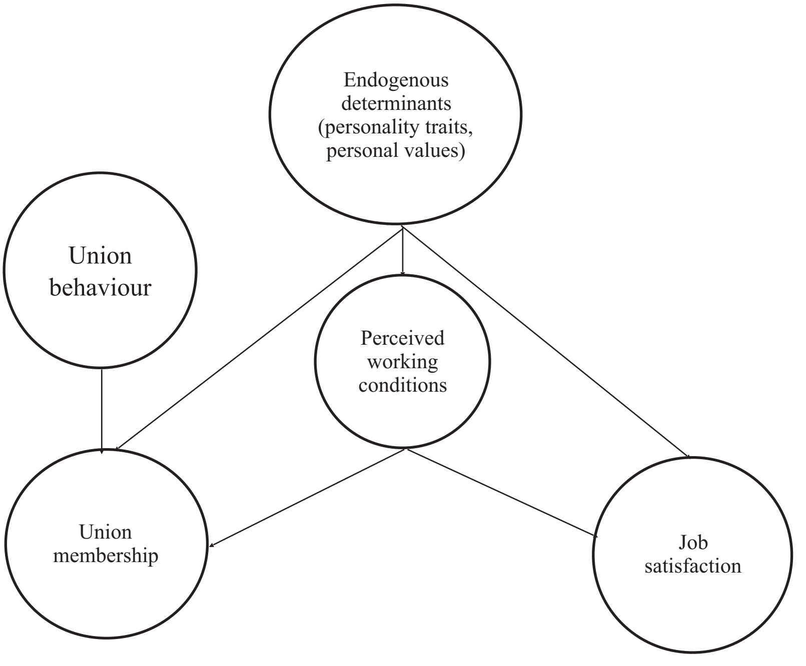
These differing theoretical perspectives are represented graphically in Figures 1 and 2. Following the logic of Pearl *et al*. (2016: 35–39; Cunningham 2018) these directed acyclic graphs (DAGs) help to clarify

FIGURE 1
A 'Chain' Relationship Between Working Conditions, Union Membership and Job Dissatisfaction



the proposed theoretical relationships and to determine the type of empirical model that we need to test the theories against one another. Perceived working conditions, union membership and job satisfaction are nodes in the graphs, with the edges connecting the nodes indicating the hypothesised causal relationships drawn from theory. Figure 1 shows the 'chain' relationship between perceived working conditions, union membership and job satisfaction posited by Freeman and Medoff (1984) and Borjas (1979). Union behaviour (campaigning activity) affects the perception of working conditions and influences whether or not a worker is satisfied or dissatisfied with a given set of working conditions. Perceived working conditions affect job satisfaction at a point in time, which, in combination with union behaviour, then affects the probability that a worker will join a union. Union membership then affects the probability that a worker will be satisfied with their job at a later point in time (because union members receive union campaigning materials and communications). In other words, union membership moderates the relationship between working conditions and job satisfaction. Union members tend to experience the same working conditions differently to their non-union counterparts as a result of union activities so that they tend to be less satisfied with their jobs.

FIGURE 2
A 'Forked' Relationship Between Working Conditions, Union Membership and Job Dissatisfaction



The graph in Figure 2 shows a 'fork' relationship where perceived working conditions have a causal impact on both union membership and job satisfaction, but there is no causal relationship between working conditions and job satisfaction (Gordon and Denisi 1995; Pfeffer and Davis-Blake 1990). Endogenous worker personality traits and values (Green and Heywood 2015; Heywood *et al*. 2002) influence choice and perception of working conditions, union joining and job satisfaction in both models. The testable prediction that follows from these graphs is that if the endogenous worker characteristics and exogenous differences in working conditions between union members and non-members are fully accounted for, there will be no statistical relationship between union membership and job satisfaction. To what extent does the existing empirical evidence support or contradict this prediction?

A key challenge in studying the union membership — job satisfaction relationship is to account for the endogenous variables of personality traits and personal values. Most empirical studies have not done this adequately (Laroche 2016: 712). Studies that have made a serious attempt to account for endogeneity fall into two categories, those that utilize longitudinal data and conditional fixed effects estimators to difference out the time invariant endogenous variables and those that adopt an instrumental variable approach.

Studies based on longitudinal data have found that the negative relationship between union membership and job satisfaction remains once unobserved endogeneity has been accounted for (Artz 2010, 2012; Bryson and White 2016a, 2016b; Green and Heywood 2015; Heywood *et al*. 2002; Powdthavee 2011). Two of these studies have also accounted for job-specific working conditions that do not change over time (Green and Heywood 2015; Powdthavee 2011). They found that union membership was associated with lower job satisfaction even after controlling for time-invariant working conditions. However, as Green and Heywood (2015: 588) note, union membership and declining job satisfaction could still be jointly determined by changes in working conditions that their study was not able to observe. Therefore, these studies leave open the possibility that either of the causal stories set out above are true.

Laroche (2016) identifies six studies that use an instrumental variable approach. Five of these find no statistical relationship between union membership and job satisfaction once instruments are used to account for unobserved worker values and attitudes (although in one of these studies, union members are more dissatisfied if they are not covered by union bargaining arrangements). The one study that does find a negative relationship (Borjas 1979) has limited measures of working conditions, so may be subject to omitted variable bias (Pfeffer and Davie-Blake, 1990: 265). While the preponderance of evidence from these five studies (and a further study: Laroche 2017) suggests that unions do not have a causal effect on their members job satisfaction, most of these studies has at least one limitation which makes us cautious in accepting their conclusions as being definitive. Gius (2012) and Lillydahl and Singell (1993) are based on studies of a single occupation, so results may not generalize to other contexts. Pfeffer and Davis-Blake (1990) measure working conditions through worker self-reports using similar question scales to worker self-reports of job satisfaction, so may be subject to common method bias.

Before considering the remaining studies, let us remind ourselves of the conditions an instrumental variable must meet to be considered a valid instrument. First, it must not exert its own independent causal impact on the independent variable. Second, it must be correlated with the endogenous explanatory variables. Third, it must not be correlated with the error term, that is, with other unobserved influences on job satisfaction (Pearl 2000). How well do the instruments used in previous studies meet these criteria? Bender and Sloane (1998), Bryson *et al*. (2004) and Laroche (2017) take measures of the climate of industrial relations as their instrumental variables. However, unobserved worker values and attitudes that cause membership and job dissatisfaction could also cause a poorer industrial relations climate (e.g. if they make workers more hostile to managers). In other words, it is not implausible that the supposed instrumental variable could be correlated with the error term (i.e. unobserved worker values and attitudes), so it is not necessarily a valid instrument.

Bryson *et al*. (2010: 369) take age of establishment and whether a workplace is part of a larger organization as instruments, assuming that while these variables are related to union coverage and membership, they have no independent causal impact on job satisfaction once other workplace characteristics are controlled for. Theoretically, this choice of instruments is not completely convincing because the supposed instruments could be correlated with the error term (i.e. worker values and attitudes). Older workers tend to work in older workplaces, and age is linked to values and attitudes (Inglehart 1977). Working with the like-minded may then reinforce these values and attitudes in a way that simply controlling for worker age fails to capture. Unobserved workplace characteristics correlated with age may also have an independent effect on job satisfaction. Specifically, older workplaces often have poorer physical working conditions than newer establishments. Similarly, multi-establishment organizations may be able to attract higher-quality managers as a result of better human resource management capabilities and promotion prospects, so this may also have an independent impact on job satisfaction. Manager quality may also influence worker attitudes, meaning that this variable may potentially be correlated with the error term too.

The overall point is that the weight of evidence from instrumental variable studies suggests that there is typically no causal relationship between union membership and job satisfaction once instruments are used to account for the unobserved endogenous determinants of union membership and job satisfaction. This contradicts the findings of longitudinal studies. One plausible reason for this discrepancy could be that there are problems with the instruments used in these studies.

Overall then, previous research is divided as to whether there is a relationship between union membership and job satisfaction once endogenous worker characteristics are taken into account. The question of whether or not there is a causal relationship between union membership and job satisfaction is yet to be settled. To answer this question, it is necessary to have longitudinal data so that the effects of time-invariant worker values and attitudes can be controlled for through a fixed effects model. These data must also contain measures of exogenous change in working conditions, so that these exogenous changes can be used in quasi-experimental analysis that can allow strong inferences about causality to be drawn (Green and Heywood 2015: 597). The key contribution of this article is base analysis of the relationship between unions and job satisfaction around such an exogenous change in working conditions. The next section outlines the change.

2. UK Public Sector Pension Reform and the 2011 Industrial Dispute

In March 2011, the results of an independent review of UK public sector pensions were announced and quickly accepted by the government. The new policy had four elements which would be applied to a range of different public sector pension schemes, covering five million public sector workers. First, pensions would no longer be linked to salary on retirement (final salary) but to the average salary while retirees were paying into the pension scheme. As most public sector jobs have seniority-based pay scales and internal labour markets which see workers move up through the grading system over time, such a change would cut pension entitlements for many workers with those on a career track to senior and highly paid positions most affected. Second, the annual uprating of pension payments would no longer be linked to the retail price index of inflation but to the consumer price index, which typically increases at a slower rate than the retail price index so cutting long-term retirement income. Third, retirement age would be linked to the state retirement age, so that instead of being able to claim pension benefits at 65, by 2028 retirees would have to wait until 67 before claiming their pensions. Finally, worker contributions to the pension scheme would rise, by an average of 3 per cent, with level of contribution depending on income. For example, a worker earning £30,000 a year would pay an extra £75 a month in pension contributions (a 4 per cent cut in net income), while low-paid workers earning less than £15,000 would not have to make any additional contributions (Holden 2011). Therefore, the proposed changes represented a substantial exogenous worsening of employment terms and conditions.

Workers affected are represented by around 30 separate unions. UK unions are legally required to hold postal ballots of their members before taking strike action. Unions balloted their members over the summer and autumn of 2011; 2,164,775 strike ballots were distributed, implying union membership density among affected workers of around 43 per cent under open shop membership arrangements. Thirty-nine per cent of eligible members returned their ballots, with 77 per cent of those voting casting their ballot in favour of striking (Guardian 2011). A co-ordinated one-day strike was held by all unions involved on 30 November 2011 (some individual unions held additional one-day strikes). Union members then voted to accept a revised set of proposals in December 2011, bringing the dispute to a close (BBC 2011). The revised proposals deferred the start date of the reduction in pension benefits until April 2022 and mitigated some of the long-term losses, but still included key elements of the original package, including increased employee pension contributions, which were then phased in from April 2015.

What do the theoretical models set out in Figures 1 and 2 suggest will happen to union membership, job satisfaction and the relationship between them as a consequence of the pensions changes and associated industrial dispute? The chain model in Figure 1 suggests that the pensions changes will raise job dissatisfaction which will cause some workers to unionize. Worker who have unionized will then have higher job dissatisfaction than those who did not unionize because union behaviour has a causal impact on their job satisfaction. In contrast, the fork model in Figure 2 suggests that the pensions changes will cause unionization and job dissatisfaction, but these two things will be independent of each other. Both union members/joiners and non-members will experience similar increases in job dissatisfaction. In the next section, we explain the data we will use to test these predictions empirically.

3. Data

Data come from the harmonized sample of the BHPS and UKHLS, spanning a 12-year period from 2004 to 2015. BHPS–UKHLS is a large longitudinal study of people's lives in the United Kingdom. These surveys are characterized by high response rates and low levels of panel attrition and are considered to be representative of the wider UK population as a result of stratified random sampling methods (Buck and McFall 2012). We used the last eight waves of BHPS and five waves of UKHLS. Our decision to restrict the BHPS part of the sample to eight waves was shaped by the need to balance the desirability of studying a longer time period pre- and post-dispute with limiting the impact of panel attrition on the sample (we had to keep a meaningful number of observations per group to permit robust econometric analysis).

The period prior to pensions dispute is covered by waves 11 and 18 of BHPS and waves 'a' to 'b' of Understanding Society while waves d to f of Understanding Society span the post-dispute period (we have omitted wave 'c' as it is comprised of interviews that took place in the midst of the dispute). From an initial unbalanced sample of 21,380 individual respondents (including non-workers), we employed an unbalanced panel of 15,297 worker observations (79,763 observations in total across all waves) obtained from the linked BHPS–UKHLS. Data from waves 11 and 18 of BHPS and waves 'a' and 'b' of UKHLS were collapsed into a pre-treatment period of 55,967 observations in total while remaining waves of UKHLS ('d' to 'f') were merged to form a post-treatment period of 23,796 observations. In sensitivity analysis described below, we also used a merged sample of BHPS and UKHLS balanced panels of 9,646 workers in total (4,321 workers from BHPS and 5,325 workers from UKHLS), with no significant deviation from the relationships estimated on the basis of the unbalanced panel. We then restricted the sample to understanding society only using a balanced panel of 5,325 workers. Likewise, it has not significantly affected regression estimates.

Our measure of job satisfaction is ordinal derived from a single 7-point Likert-type variable. The question is worded as follows: 'On a scale of 1 to 7 where 1 means "Completely dissatisfied" and 7 means "Completely satisfied", how dissatisfied or satisfied are you with your present job overall?' Such single-item measures of job satisfaction have adequate convergent validity with multi-item measures (Wanous *et al.* 1997). To identify those affected by the pensions changes we used occupation and public/private sector variables, which allowed us to single out public sector employees in the occupational groups affected by the pensions dispute. While there may be some measurement error in this variable if workers mistakenly classify themselves as working in the private sector (e.g. a cleaner working in a public hospital while employed by a private cleaning contractor may think of themselves as working in the public sector even though national account definitions would classify them as working in the private sector and they would be unlikely to be covered by public sector pension arrangements) the use of detailed (four-digit) occupation codes allows us to reduce measurement error, because many of the occupations are specific enough that there are no or very few members of the occupation employed in the private sector. For the sake of brevity, we provide a list of amalgamated occupations: teachers and all employees in schools and colleges; municipal government workers (including, for example, social workers, gardeners and ground staff, clerical officers, refuse collectors), probation officers, clerical and administrative grades working in job centres, law courts, police, passport office and other government agencies; the National Health Service including healthcare assistants, clerical staff, porters, paramedics, cleaners, podiatrists and chiropodists, nurses and hospital doctors; HM Revenue and Customs (HMRC). We bundled the foregoing occupational categories to form a group affected by the pensions which was coded '1'. All other participants in the sample were coded '0', assuming they were unaffected by the course and outcomes of the pensions dispute.

Descriptive statistics for main study variables suggest that the proportion of workers affected by industrial action and union membership density in our sample are broadly similar to the population. Average union membership density in the sample stands at 30.8 per cent, which is slightly higher than the figure of 23.5 per cent from the official statistics. The same applies to the proportion of workers affected by pensions dispute (20.4 per cent in our sample against 16 per cent; ONS 2015).

First we investigate, in this dataset, if job satisfaction is lower among union members and whether this holds even after controlling for worker and job fixed effects (we use worker-in-job fixed effects as per Green and Heywood 2015). Table 1 reports mean job satisfaction (panel A) and regression estimates (Panel B) for an unbalanced panel of 15,297 workers and for the balanced panels of 9,646 workers (using pooled OLS and probit adapted ordered least squares, respectively). As with the bulk of previous studies, the negative effect of union membership on job satisfaction holds, implying that union members are more dissatisfied with their jobs than non-members even after controlling for time invariant aspects of jobs, personality and values.

Next, we are interested in understanding how job satisfaction and union membership changed before and after the industrial dispute. Figure 3 tracks job satisfaction for workers affected by the pensions changes compared to unaffected workers. It suggests job satisfaction declined for workers affected by the pensions dispute. Figure 4 further decomposes changes in job satisfaction by whether a worker was affected by the pensions changes and by union membership status. It suggests that both unionized and non-unionized workers affected by the pensions change experienced a similar decline in job satisfaction at the time of the dispute.

Finally in this section, we investigate whether non-union members affected by the pensions changes and dispute were more likely to unionize than non-members who were not affected by the pensions changes. We have truncated the original sample by leaving out those who were union members prior to the pensions dispute. We then cross-tabulated union membership status with the treatment variable to identify the percentage of those who had joined

TABLE 1
The Relationship Between Union Membership and Job Satisfaction

Panel A

	Union members	*Non-members*	*t-value [95 % CI]*
Job satisfaction	5.30	5.37	6.4303*** [0.045;0.084]

Panel B

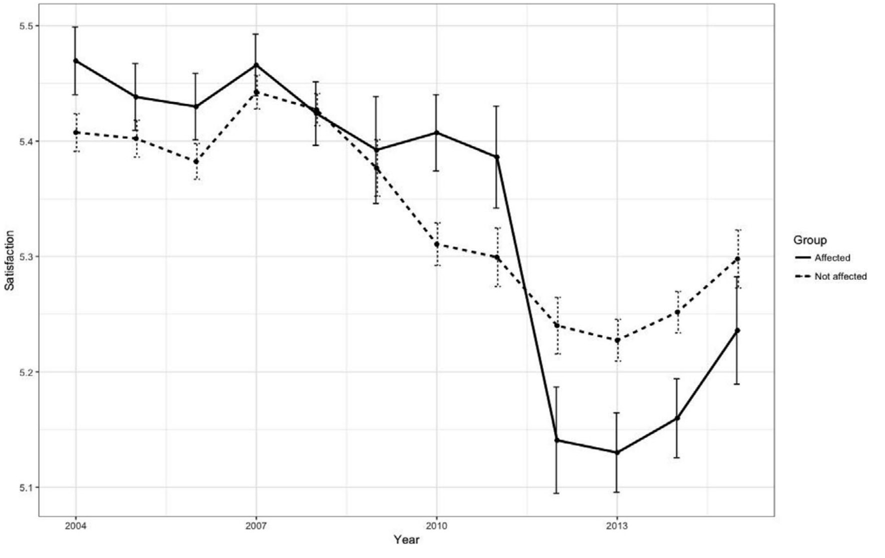
	Estimate (standard error)	Estimate (standard error)	Estimate (standard error)
	(1) unbalanced panel	*(2) unbalanced panel*	*(2) balanced panel*
Union membership	−0.100*** (0.011)	−0.114*** (0.021)	−0.078*** (0.023)
Pooled OLS	√		
POLS (probit-adapted ordinary least squares)		√	√
Match fixed effects (worker-in-job)		√	√
Occupation dummies	√	√	√
Industry dummies	√	√	√
Year dummies	√	√	√
Control variables	√	√	√
R-squared	0.020	0.008	0.010
Sample size (workers)	15,297	15,297	9,646

Panel characteristics: models (1) and (2) — unbalanced BHPS-UKHLS panel (79,763 observations, 2004–2015); model (3) — merged balanced panels of BHPS and UKHLS (55,707 observations, 2004–2015).
Significance codes: '***' 0.001 '**' 0.01 '*' 0.05.
Control variables (as per Table 3): gender, age, qualification, average weekly working hours, type of employment contract, average weekly income, position in the organization, firm size.
Weighted base: individual level household grid.

trade unions in the aftermath of the pensions dispute. The unionization effect was four times higher among affected non-members compared to their not affected counterparts (the chi-square test was statistically significant with $\chi^2 = 5238.8$ at $\rho < 0.001$).

In this section, we have established that job satisfaction is lower for union members than for non-members, even after accounting for time-invariant aspects of worker personality, values and attitudes and time-invariant job characteristics. We have also shown that those exposed to an exogenous change in working conditions (the pensions dispute) experienced a decline in job satisfaction, and that this decline was experienced similarly by union and non-union members. This finding is in line with the predictions of the fork model set out in Figure 2, and is therefore suggestive of there being no relationship between union membership and job satisfaction once job characteristics are accounted for. However, this simple descriptive analysis does not account for potentially confounding factors. In the next two sections,

FIGURE 3
Job Satisfaction by Year and Group



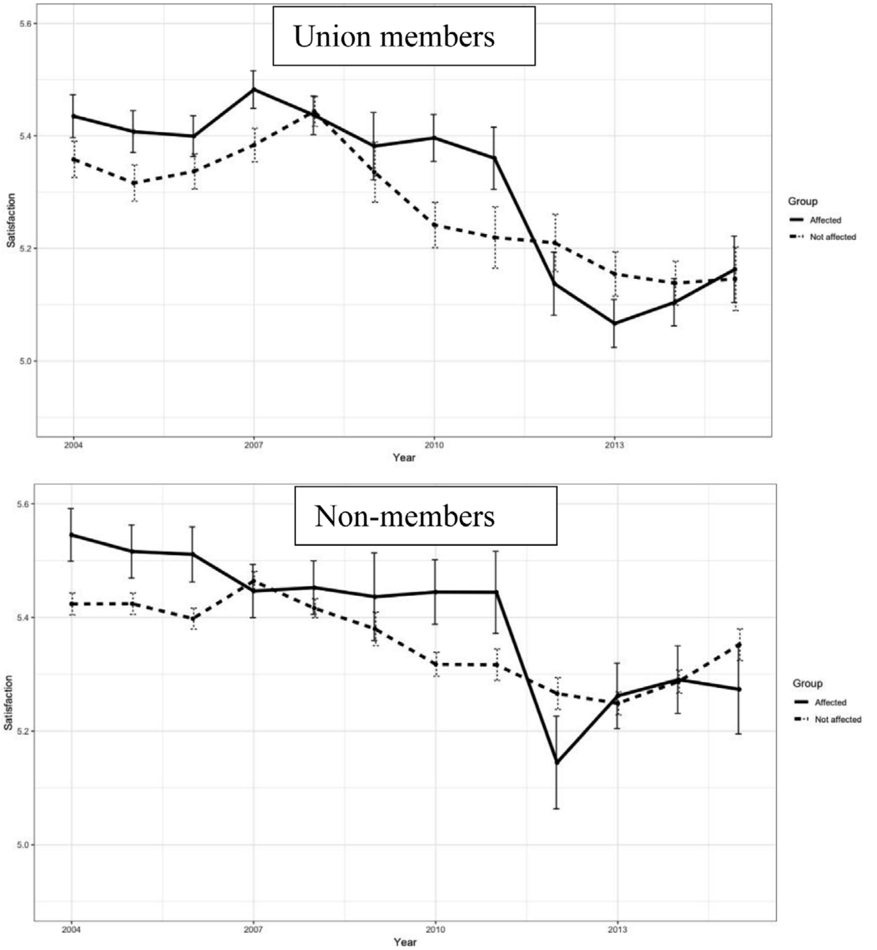
we explain the two studies we undertook to test the competing theories more rigorously.

4. Study 1: Difference in Differences Analysis

Methodology

In order to try to draw a causal link between the pensions dispute and job satisfaction, we turn to difference in differences (DiD), a quasi-experimental econometric method used to evaluate the effect of social programmes and interventions (Angrist and Pischke 2014; Bertrand *et al.* 2004). Difference in differences design begins with identification of a *treatment* in a form of an exogenous effect which concerns only some groups in the population (Donald and Lang 2007). The treatment group is separated from those not affected by the intervention (the control group) and measurements are taken at two points in time, before and after the intervention (Bertrand *et al.* 2004). In this study, the treatment group includes public sector workers affected by the pensions dispute, both union members and non-members. We further separate affected union members from affected non-members to test whether there was an additional treatment effect for trade union membership as suggested by the model represented by Figure 1. Control groups are used to construct an assumed counterfactual trajectory, with which the change in the treatment group is compared. Sequential differences between the effect of treatment on the treated and the assumed counterfactual if no treatment were in place return difference in differences estimate.

FIGURE 4
Job Satisfaction by Year, Group and Union Membership



Note that DiD analysis typically rests on two assumptions. First that allocation to control and treatment groups is as good as randomly assigned. This is deemed necessary because the statistical properties of random (or as good as random) assignment mean that estimates will be unbiased even if variables that have a causal impact on job satisfaction are missing from the model because there are no data on them (Pearl *et al*. 2016: 105). However, assignment into the treatment groups of a public sector jobs and union membership within a public sector job is not as good as random. It is influenced by worker values and personality and by job-specific social norms that cause workers to prefer public sector jobs and/or union membership (as per Figures 1 and 2). However, in this case we argue that pseudo-random properties are not necessary, because following the logic set out by Pearl

TABLE 2
Parallel Trends Assumption

	Estimate (standard error) (1)	*Estimate (standard error) (2)*	*Estimate (standard error) (3)*
Affected X Year (2005)	−0.021	−0.033	0.039
	(0.037)	(0.041)	(0.081)
Affected X Year (2006)	−0.006	−0.042	0.155
	(0.037)	(0.041)	(0.081)
Affected X Year (2007)	−0.021	−0.016	−0.025
	(0.036)	(0.041)	(0.080)
Affected X Year (2008)	−0.039	−0.058	0.047
	(0.036)	(0.040)	(0.079)
Affected X Year (2009)	−0.011	0.007	−0.061
	(0.043)	(0.048)	(0.095)
Affected X Year (2010)	0.057	0.069	0.039
	(0.037)	(0.042)	(0.082)
Clustered (robust) standard errors	√	√	√
Match fixed effects (worker-in-job)	√	√	√
Occupation dummies	√	√	√
Industry dummies	√	√	√
Control variables	√	√	√
Sample size (workers)	14,102	10,536	3,566

Unbalanced BHPS-UKHLS panel of 79,763 observations: model (1) — truncated sample before 2011; model (2) — truncated sample of non-members; model (3) — truncated sample of union members.
Significance codes: '***' 0.001 '**' 0.01 '*' 0.05.
Control variables (as per Table 3): gender, age, qualification, average weekly working hours, type of employment contract, average weekly income, position in the organization, firm size.
Weighted base: individual level household grid.

and Mackenzie (2018: 143–50) we are able to control for these confounding variables using a conditional fixed effects estimator akin to that developed by Green and Heywood (2015). The longitudinal nature of the data allows us to difference out time-invariant aspects of personality, personal values and attitudes, and job characteristics that cause workers to choose union membership and public sector jobs. It is perhaps necessary to add the caveat here that personality and values are not wholly time invariant and are indeed influenced by what we do at work (Woods *et al*. 2020). Nevertheless, it seems likely that any such changes happen over a long-run period so it is reasonable to assume that any changes that occur over the 12-month period between waves in our data are slight and so will not bias results (Green 2006).

The second assumption is that there are parallel trends in job satisfaction among the treatment and control groups in the years prior to the treatment. Figure 3 suggests that with the exception of a single year (2010) there were indeed parallel trends. Table 2 reports regression coefficients that test this more formally. We have truncated the dataset leaving only observations before the industrial action and included a time-group interaction effect. Regression coefficients for the interaction effect were small in size and statistically insignificant. Since statistical insignificance of interaction effects may be caused by sampling issues, we have arbitrarily selected different points in time for a counterfactual treatment effect and collapsed the data into pre- and post-treatment periods. Such a manipulation has returned statistically insignificant interaction effects. Hence, it is highly likely the trends between control and treatment groups were parallel prior to pensions dispute and that this holds for both union and non-union members.

We now formally specify the empirical model. Following Green and Heywood (2015) we estimate Equation 1 with match (worker-in-job) fixed-effects. This strategy matches each job for each worker in the sample and therefore is more effective than simple worker-specific fixed effects in accounting for the effect of changes in job status on workers' job satisfaction.

$$Y_{itj} = \alpha + \mu\Lambda_i + \psi\ \Phi_t + \theta\ \Lambda_i * \ \Phi_t + \lambda Z_{it} + \varphi X_{itj} + \upsilon_{ij} + e_{itj} \qquad (1)$$

where:

Y_{itj} is job satisfaction of worker i in period t occupying a specific job j;

α — intercept;

Λ_i — treatment dummy, (Λ =0 signifies control group);

Φ_t — pre- and post-intervention dummy variable (coded 0 and 1, respectively);

θ – difference in differences estimate — the average effect of pensions dispute on the job satisfaction of the treated. θ was estimated first using the complete sample and then comparing affected union members and affected non-members as the treatment and control group, respectively;

Z_{it} — vector of person-specific control variables;

X_{itj} — vector of job-specific control variables;

υ_{ij} — worker-in-job fixed effects (of worker i in job j);

e_{itj} — heteroskedastic (robust) standard error clustered at the individual level.

In a standard difference in differences model, θ is the outcome of sequential differences derived by Equation 2, where Δ is the difference operator signifying differencing out in time.

$$\begin{aligned}\Delta t y_{it} &= (E(y_{it}|\Lambda = 1, \Phi = 1) - E(y_{it}|\Lambda = 0, \Phi = 1)) \\ &\quad - (E(y_{it}|\Lambda = 1, \Phi = 1) - E(y_{it}|\Lambda = 0, \Phi = 0))\end{aligned} \qquad (2)$$

Owing to the ordinal nature of the dependent variable (job satisfaction), we employed probit-adapted ordinary least squares (POLS) regression. POLS rests on the transformation of an ordinal-dependent variable into a cardinal one (Ferrer-i-Carbonell and Frijters 2004; Green and Heywood 2015). First, relative frequencies of existing outcome categories are used to derive Z-scores and conditional expectations for each category. These are then utilized to construct an unbounded normally distributed dependent variable. In line with POLS procedure, an ordinal dependent variable is transformed by Equation 3 (see Ferrer-i-Carbonell and Frijters (2004: 16-19) for further details of the procedure).

$$Y_{it} = k \Leftrightarrow \lambda_k^i \leq Y_{it}^* \leq \lambda_{k+1}^i \tag{3}$$

The above equation permits the use of fixed effects. To check the robustness of the outcomes of regression modelling we have also estimated Equation 1 using the 'blow-up and cluster' method (Baetschmann *et al.* 2015). The size and direction of the main DiD effect were similar.

Our main regression equation is prone to a common random effect at the group-year level (Donald and Lang 2007). To tackle this problem, we have estimated robust (heteroskedastic) standard errors (Cameron and Miller 2015). Regression models also control for potentially confounding time-invariant variables (personality and values) associated with the worker sorting hypothesis. These include basic demographic characteristics like gender and age, social status variables including the highest qualification obtained, terms and conditions of employment (average weekly working hours, permanent/temporary employment contract, direct/indirect working arrangements, average weekly income adjusted to inflation, position in the organization, firm size, industry (at the two-digit standard industrial classification level)). Controlling for income is particularly important because changes to tax credits paid to working parents and a pay freeze for most workers in the treatment group were occurring at the same time as the dispute. To account for other possible confounding effects of income (lagged and nonlinear effects), we included a lagged income variable and the quadratic term as control variables in Equation 1. Descriptive statistics for dependent, independent and control variables are reported in Table 3. The table also contains descriptive statistics for the control and treatment group. There were some differences between the groups most notably in relation to the qualification levels, gender composition and unionization. It is important to note that these disparities are sector driven, accounted for by worker-in-job fixed effects without further implications for our quasi-experimental design.

Results

We first report average levels of job satisfaction among those affected by the pensions dispute and those not affected (treatment and control groups, respectively), distinguishing further between union and non-union members (the analysis includes those whose union membership status changed between the waves). Table 4 contains the respective means and the size of raw DiD effect, suggesting that the effect of the pensions issue on job satisfaction of affected workers is negative. The effect diminishes if we compare affected union members with affected non-members.

Table 5 corresponds to Equation 1 and reports the main effect among the control and treatment group including both union members and non-members. The output contains three models: model one corresponds to the

TABLE 3
Descriptive statistics

Variable	*Measurement*	*Mean/SD (% for categorical variables)*	*Mean/SD (% for categorical variables) — treatment group*	*Mean/SD (% for categorical variables — control group*
Main study variables				
Job satisfaction	7-point Likert scale from '1 — very dissatisfied' to '7 — very satisfied'	5.35/1.31	5.36/1.31	5.34/1.32
Treatment group	Categorical dichotomous	20.4%	-	
Control group		79.6%		
Affected union members	Categorical nominal	12.0%		
Affected non-members		8.4%		
Not affected union		18.8%		
members		60.8%		
Not affected non-members		1.6%		
Affected public sector workers in retirement age				
Trade union membership density	Categorical dichotomous	30.8%	63.1%	24.0%
Control variables				
Gender (female)	Categorical dichotomous	52.3%	71.6%	45.2%
Age	Continuous (years)	35.94 / 19.70	36.45/17.14	35.05/19.85
Average weekly working hours	Continuous (hours)	32.69/10.83	34.52/9.20	31.27/11.36
Employment contract (permanent as opposed to non-permanent)	Categorical dichotomous	94.3%	95.0%	93.9%
Qualification (degree)	Categorical nominal	21.9%	33.1%	17.7%
Qualification (other higher degree)		11.1%	18.6%	8.7%
Qualification (A-level)		20.9%	15.8%	22.9%
Qualification (GCSE)		21.2%	15.6%	23.6%
Qualification (other qualification)		9.9%	6.0%	10.9%
Qualification (no qualification)		15.0%	10.9%	16.2%

(*Continued*)

TABLE 3
Continued

Variable	*Measurement*	*Mean/SD (% for categorical variables)*	*Mean/SD (% for categorical variables) — treatment group*	*Mean/SD (% for categorical variables — control group*
Average monthly income	Continuous (pounds per month)	1571.30 / 1102.82	1594.30/993.12	1557.06/1130.97
Position (self-employed large establishment)	Categorical nominal	0.2%	0.1%	0.2%
Position (self-employed small establishment)		2.4%	0.3%	2.8%
Position (self-employed no employees)		10.9%	3.8%	12.6%
Position (manager large establishment)		13.7%	18.0%	12.7%
Position (manager small establishment)		5.8%	3.9%	6.3%
Position (foreman or supervisor)		11.7%	17.1%	10.5%
Position (employee)		55.3%	56.8%	54.9%
Firm size (1-2 employees)	Categorical ordinal	3.8%	2.4%	4.2%
Firm size (3-9 employees)		13.3%	6.1%	15.3%
Firm size (10-24 employees)		16.0%	13.5%	16.7%
Firm size (25-49 employees)		14.0%	16.3%	13.4%
Firm size (50-99 employees)		11.5%	14.2%	10.8%
Firm size (100-199 employees)		9.8%	12.0%	9.3%
Firm size (200-499 employees)		11.6%	10.2%	12.0%
Firm size (500-999 employees)		6.3%	6.0%	6.4%
Firm size (1000+ employees)		12.5%	17.5%	10.9%
Firm size (don't know but less than 25)		0.2%	0.2%	0.2%
Firm size (don't know but more than 25)		1.0%	1.6%	0.8%

Sample: unbalanced BHPS-UKHLS panel of 15,297 workers (79,763 observations, 2004–2015).

TABLE 4
Average Job Satisfaction Among Control and Treatment Groups

		Pre-dispute		*Post-dispute*		*DID effect*
Wave/group	*Sample size (workers)*	*Mean*	*SD*	*Mean*	*SD*	$(treatment_{after} - treatment_{before}) - (control_{after} - control_{before})$
Treatment (all affected)	3,976	5.44	1.26	5.16	1.44	−0.15
Control (all not affected)	11,321	5.38	1.26	5.25	1.40	
Treatment (union members affected)	2,299	5.42	1.23	5.10	1.41	
Control (non-members affected)	1,677	5.48	1.27	5.25	1.44	−0.09
Observations (total)	79,763		55,967			23,796

Sample: unbalanced BHPS-UKHLS panel of 15,297 workers (79,763 observations, 2004–2015).

unbalanced panel derived from the BHPS–UKHLS sample with control variables and worker-in-job fixed effects; model 2 adds lagged and squared effects of income while model 3 is identical to model two but estimated on the balanced BHPS and UKHLS panels of 9,646 workers. The DiD coefficient was negative and statistically significant in all three models. This corroborates our assumption that an exogenous change to the terms and conditions of employment caused a decrease in employees' job satisfaction. Did the trade union campaign of opposition cause job satisfaction to fall further for union members? We can investigate this question by comparing affected union members and affected non-members.

Table 6 reports the outcomes of econometric analysis when we examine if the treatment effects differ depending on whether the treatment was experienced by members or non-members. It compares affected union members and affected non-members using the unbalanced panel of 3,976 public sector workers and the balanced panel of 2,315 workers affected by the dispute. The regression coefficient is negative but statistically insignificant, which indicates that union members were not disproportionately affected by pensions dispute.

As our findings may suffer from attrition bias stemming from the use of the linked BHPS-UKHLS sample, we re-estimated all regression models on a shorter unbalanced panel of 9,447 workers and a balanced panel of 5,325 workers across five waves of the UKHLS. This did not materially affect statistical relationships reported in this study. Finally, to rule out the possibility of a false positive we conducted a permutation test, randomly assigning respondents into treatment to mimic simulation of a placebo intervention (Betrand *et al*. 2004). The exercise was repeated 200 times, with equation one employed to estimate the likelihood of a false positive results.

TABLE 5
Results of Difference in Differences Analysis

	Estimate (standard error) (1)	*Estimate (standard error) (2)*	*Estimate (standard error) (3)*
Main effect (time X treatment)	−0.166***	−0.166***	−0.177***
	(0.026)	(0.033)	(0.036)
Time (pre-dispute — reference category)	−0.109***	−0.083***	−0.061**
	(0.013)	(0.017)	(0.019)
Treatment (control group — reference category)	−0.001	−0.012	−0.019
	(0.068)	(0.069)	(0.074)
Age (log)	−0.017***	−0.016***	−0.010*
	(0.004)	(0.004)	(0.005)
Working hours	−0.041	−0.039	−0.040
	(0.183)	(0.192)	(0.197)
Income (log)	0.093***	0.090***	0.113***
	(0.014)	(0.016)	(0.019)
Income (t − 1, log)		−0.001	−0.003
		(0.009)	(0.010)
Incomeˆ2 (log)		0.018	−0.006
		(0.017)	(0.010)
R-squared	0.011	0.012	0.011
Clustered (robust) standard errors	√	√	√
POLS (probit-adapted ordinary least squares)	√	√	√
Match fixed effects (worker-in-job)	√	√	√
Occupation dummies	√	√	√
Industry dummies	√	√	√
Control variables	√	√	√
Sample size (workers)	15,297	15,297	9,646

Panel characteristics: models (1) and (2) — unbalanced BHPS-UKHLS panel (79,763 observations, 2004–2015); model (3) — merged balanced panels of BHPS and UKHLS (55,707 observations, 2004–2015).
Significance codes: '***' 0.001 '**' 0.01 '*' 0.05. Control variables (as per Table 3): gender, qualification, type of employment contract, position in the organization, firm size.
Weighted base: individual level household grid.

On average, this fictitious scenario returned statistically significant results for the treatment effect less than 5 per cent of the time, an outcome that we take as evidence of the robustness of the statistical outcomes reported in this study.

Overall then, the results of this analysis suggest that there is no causal relationship between union membership and job satisfaction. Rather, union membership and job satisfaction are jointly determined by working conditions. Once an exogenous change in working conditions is taken into account, the statistical relationship between union membership and job satisfaction disappears. However, there may be reason to doubt these results because the treatments of working in the public sector and union membership are not 'as good as randomly assigned'. Our second study, using discontinuity analysis is designed to address these doubts.

TABLE 6
Results of Difference in Differences Analysis

	Estimate (standard error) (1)	*Estimate (standard error) (2)*	*Estimate (standard error) (3)*
(treated members/treated non-members)			
Main effect (time X treatment)	−0.103	−0.101	−0.074
	(0.055)	(0.060)	(0.061)
Clustered (robust) standard errors	√	√	√
POLS (probit-adapted ordinary least squares)	√	√	√
Match fixed effects (worker-in-job)	√	√	√
Occupation dummies		√	√
Industry dummies		√	√
Lagged and squared income		√	√
Control variables		√	√
Sample size (workers)	3,976	3,976	2,315

Panel characteristics: models (1) and (2) — unbalanced BHPS-UKHLS panel of public sector workers affected by the dispute (17,675 observations, 2004–2015); model (3) — merged balanced BHPS -UKHLS panels of affected public sector workers (13,310 observations, 2004–2015). Significance codes: '***' 0.001 '**' 0.01 '*' 0.05. Control variables (as per Table 3): gender, age, qualification, type of employment contract, position in the organization, firm size, weekly working hours, pay. Weighted base: individual level household grid.

5. Study 2: Discontinuity Analysis

Methodology

The intuition behind a discontinuity design is that a causal relationship can be identified by comparing changes in job satisfaction for those affected by the treatment with individuals who just miss out on being assigned to the treatment group. This way, control and treatment groups are as good as randomly assigned. If the cut-off point generates a sharp discontinuity in the treatment the causal effect is thought to be established. Normal retirement age is a variable pertinent for the research design in question because although there is likely to be some differences in workers' personality and values according to age (Inglehart 1977), among workers of the same generation who retire shortly before or just after the pensions changes were introduced such age-related differences are likely to be trivial or non-existent. Further, some workers affected by the dispute were not affected by the treatment of the pensions changes, because they reached normal retirement age prior to the increase in pensions contributions enacted on the 5th of April 2015. Therefore, we created a 'treatment group' of those who reached normal retirement age before this date (note that we considered using proximity to retirement in DiD analysis, but trends in job satisfaction prior to the treatment were significantly different for those approaching retirement compared to younger workers, violating the parallel trends assumption of DiD analysis). Equation 4 corresponds to discontinuity regression design.

$$Y_{iput} = f\left(a_{iput}\right) + \Delta X_{iput} \quad + e_{iput} \tag{4}$$

where:

Y_{iput} — job satisfaction of worker i affected by pensions dispute p in time period t, differentiated further by union membership u;

a_{iput} — worker i's age relative to normal retirement age;

X_{iput} — a dichotomous variable that is equal to 1 if $a_{iput} \geq 0$ — that is, if worker i is assigned to the treatment group based on cut-off (reaching normal retirement age just before changes to the pensions system were introduced);

Δ — the parameter of interest that captures the effect of pensions dispute on job satisfaction relative to the retirement age.

Exposure to the pensions change 'treatment' is partially under workers' control because they can choose to work past normal retirement age. Workers who choose to work longer may do so because they have higher job satisfaction. If this is the case it would confound our estimates. Therefore, we restricted the sample to workers below normal state retirement age at the time of their interview (as a robustness check, we also looked at what would happen without this restriction, it did not significantly change our results).

In discontinuity design, determinants of an outcome variable ought to be balanced across the cut-off. We checked the distribution of union membership density, gender, position in the organization across the threshold and found no evidence that these variables change sharply across normal retirement age. We have also ensured that our results are robust to different bandwidth specifications and to counterfactual cut-off points. We estimated local linear regression with the Imbens-Kalyanaraman data-driven algorithm for bandwidth selection (Imbens and Kalyanaraman 2012)

Results

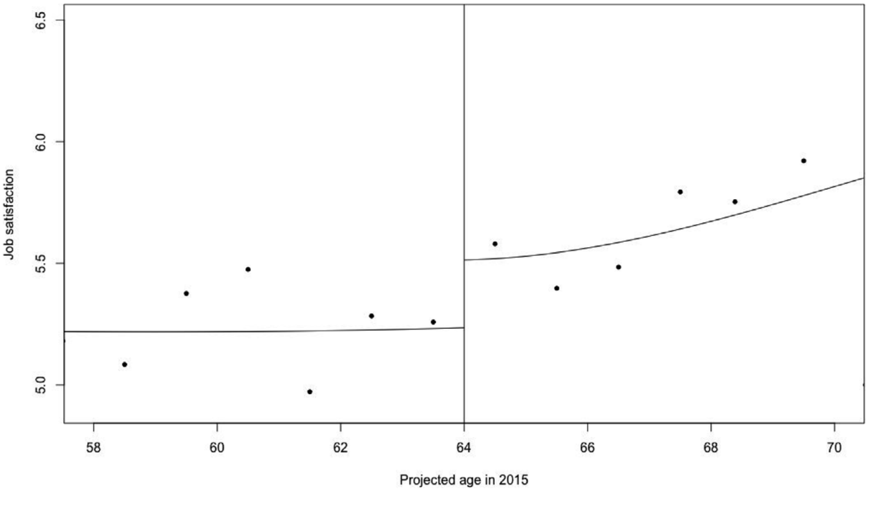
Table 7 corresponds to the discontinuity design and reports regression coefficients for control and treatments groups based on proximity to retirement by the time changes to the pension system were introduced. The sample was obtained from the unbalanced UKHLS panel of 7,970 observations (2,129 workers) affected by the pensions dispute, using UKHLS only as the linked BHPS–UKHLS sample does not contain sufficient observations of those workers who are about to retire. Figure 5 shows the discontinuity effect where the vertical line goes through the cut-off point (i.e. retirement age), demonstrating an increase in job satisfaction at the cut-off. That is, workers who had retired before higher pensions contributions were introduced experience higher levels of satisfaction than their counterparts whose pre-retirement income fell as a result of the pensions changes. Note that the graph shows that those who were nearest to retirement at the time of the dispute tended to have the highest job satisfaction. This is compatible with the idea that the industrial dispute and pensions changes did not affect their

TABLE 7
Discontinuity Analysis by Proximity to Retirement

	(1) (complete sample)	*(2) (union members)*	*(3) (non-members)*
Main effect (Age>retirement)	0.271***	0.313**	0.348**
	(0.049)	(0.096)	(0.111)
Clustered (robust) standard errors	√	√	√
Match fixed effects (worker-in-job)	√	√	√
Occupation dummies	√	√	√
Industry dummies	√	√	√
Control variables	√	√	√
Sample size (workers)	2,129	1,399	877

Data: UKHLS unbalanced panel of public sector workers affected by the dispute (7,970 observations, 2009-2015).
Significance codes: '***' 0.001 '**' 0.01 '*' 0.05.
Control variables (as per Table 3): gender, qualification, type of employment contract, position in the organization, firm size, weekly working hours, pay.

FIGURE 5
Discontinuity Analysis — Are Workers Due to Retire Before the Introduction of by Higher Pensions Contributions More Satisfied with Their Jobs?



job satisfaction because they were unaffected by them but it may also reflect psychological processes unrelated to the strike that result in job satisfaction rising as retirement approaches.

Model 1 (Table 7) reports the outcomes of discontinuity analysis using the complete sample while models 2 and 3 correspond to the regression estimates among union members and non-members, respectively. The results are broadly comparable, supporting the idea that the pensions dispute had a causal impact on job satisfaction which affected union members and non-members in a similar way.

6. Discussion and conclusions

One of the great empirical regularities of industrial relations research is that union members tend to be less satisfied with their jobs than their non-union counterparts. Three theoretical explanations have been put forward to explain this regularity. First, that unions cause job dissatisfaction through their campaigning activity an idea expressed in the chain model set out in Figure 1 (Borjas 1979; Freeman and Medoff 1984). Second, that union workers are exposed to poorer working conditions than their non-union counterparts, and this causes job dissatisfaction which triggers unionization, although union membership and job satisfaction are independent of each other, that is the for model set out in Figure 2 (Gordon and Denisi 1995; Pfeffer and Davis-Blake 1990). Third, that workers who are predisposed to job dissatisfaction because of their values, attitudes and personality traits tend to sort themselves into union jobs and union membership because of those values, attitudes and personality traits. Which of these theories is correct matters because a sense of job satisfaction is inherently valuable to workers while employers make considerable investments in actively managing the work attitudes of their employees, including job satisfaction, because it is predictive of a range of pro-employer worker behaviours. Do unions undermine employers' considerable investments in managing worker satisfaction?

Previous research has established that while values, attitudes and personality traits may explain some of the cross-sectional association between unions and job satisfaction it cannot explain all of it (Green and Heywood 2015). However, previous studies have not been able to convincingly differentiate between the first two theories. In this context, the novel contribution of this article is to use quasi-experimental methods to try to do this. Our results suggest that an exogenous change to the terms and conditions of employment caused job satisfaction to fall among those workers affected by it. There was little difference in the size of this change between union and non-union members. Further, those unaffected by the change because they were nearing retirement were less likely to experience falls in job satisfaction than those also nearing retirement who were going to be affected by the change regardless of union membership status. These results were robust to a range of controls and model specifications, including fixed effects models that controlled for time-invariant aspects of jobs, and worker personality and personal values. Of course, one could argue that the union campaign of opposition impacted on the job satisfaction of members and non-members alike. However, this argument does not explain why given the ubiquity of open-shop unionism in the UK, it is union members rather than all of those in jobs covered by union representation who are typically less satisfied in their jobs. If it is union campaigns that affect job satisfaction, we might also expect the job satisfaction of those approaching retirement to be affected in a similar way to younger workers even though they were not affected by the pensions changes. Our discontinuity analysis suggests that this was not the case.

Therefore, in the light of these results, our overall judgement is that union membership does not have a causal impact on job satisfaction. If the voice aspects of unions were a cause of dissatisfaction as Freeman and Medoff (1984) and Borjas (1979) posited, the impact of the change would be greatest on union members who are most exposed to the union campaign of opposition. Instead, results are broadly supportive of the theory, first put forward by Pfeffer and Davis-Blake (1990) that it is exogenous changes in working conditions rather than the voice effects of unions that cause job dissatisfaction. Union members' job dissatisfaction is the result of the accumulation of exogenous changes to working conditions that cause job dissatisfaction at a slightly greater rate in workplaces and sectors that employ larger numbers of union members.

Nevertheless, there are a number of limitations to our study which mean that the results should be considered provisional until supported by corroborating research. First, results represent an 'average treatment on the treated' so may not generalize to other groups of workers in other sectors and countries. Given that Green and Heywood (2015) observed that union worker job satisfaction is greater in the private sector than in the public sector, further research that looks at the relationship between exogenous changes in working conditions, union membership and job satisfaction in the private sector and in countries other than the UK would be desirable.

Second, given that the theoretical mechanism through which unions are hypothesized to influence job satisfaction is 'voice-induced complaining' (Freeman and Medoff 1984), unions may only affect job satisfaction if they engage members in campaigning against the source of the complaint. Rational union members will only become so engaged if they perceive that the union can be instrumental in bringing about change. Union members who do not perceive union instrumentality will not engage with the 'voice-induced complaining' so their job satisfaction will be no different to any other worker affected by the exogenous change. Given the exogenous change we have studied was decided at a national political level and British unions have historically had only limited success in resisting such nationally initiated changes, many union members may not have believed that the union campaign would be successful, so would not have engaged with voice-induced campaigning. This implies that a different form of exogenous change, where union members perceive greater union instrumentality, might lead us to different results and conclusions. The lesson for future researchers is that it would be desirable to capture data on worker engagement with union campaigns and perceived union instrumentality when conducting studies in this vein.

Third, a number of studies have suggested that once working conditions that unions are unable to influence are taken into account, unions may have a positive impact on job satisfaction (Bryson and White 2016a; 2016b; Bryson *et al*. 2010; Pfeffer and Davis-Blake 1990). The analysis in this article was not designed to test this theory. Studies designed to explicitly test for positive effects of unions on job satisfaction would therefore be desirable in future.

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