Pay Growth, Fairness and Job Satisfaction:
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# Pay Growth, Fairness and Job Satisfaction:
## Implications for Nominal and Real Wage Rigidity

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**Abstract**

Theories of wage rigidity often rely on a positive relationship between pay changes and utility, arising from concern for fairness or gift exchange. Supportive evidence has emerged from laboratory experiments, but the link has not yet been established with field data. This paper contributes a first step, using representative British data. Workers care about the level and the growth of earnings. Below-median wage increases lead to an insult effect except when similar workers have real wage reductions or firm production is falling. Nominal pay cuts appear insulting even when the firm is doing badly.

*Keywords*: Pay cuts, Social comparisons, Gift exchange

*JEL classification*: J33, M52, J28, E24

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# I. Introduction

Wage dynamics are complex: micro data reveal notable heterogeneity, with significant nominal or real rigidity for some coexisting with apparent flexibility for others (e.g. Dickens et al. 2007; Smith 2000). At present the foundations for wage rigidity are poorly understood. Here I examine the extent to which wage dynamics are consistent with a model of wage-setting based on fairness, where workers' utility depends on how their own income compares with some 'reference income'.

There is strong evidence from laboratory experiments that fairness considerations drive participants' reactions to wages and wage changes, and surveys of managers consistently report concern for workers' morale when setting wages (Fehr et al. 1993, 2009; Blinder and Choi 1990; Agell and Lundborg 1995, 2003; Campbell and Kamlani 1997; Bewley 1999). However, the link between wage rigidity and potential fairness foundations has not yet been established with field data. Using British field data from 1991 to 2007, I examine the impact of compensation and, in particular, changes in compensation, on job satisfaction. Three questions are addressed: Does job satisfaction depend on the change in pay in addition to the level of pay? If so, does the effect of a pay change differ between increases and decreases (either real or nominal)? Does satisfaction with pay changes depend on what is happening elsewhere?

Few researchers have investigated the impact on job satisfaction of pay changes. British, German and Japanese field data have been analysed by Clark (1999), Grund and Sliwka (2007) and Kawaguchi and Ohtake (2007) respectively, but these studies were limited in scope and do not allow robust detailed conclusions to be drawn about the effect of pay cuts. Performance declines were found after a pay change 'loss' by Mas (2006) and to some extent by Lee and Rupp (2007), using data on specific employee groups (police and pilots respectively).

A small but growing number of field experiments have also examined the relationship between wage, fairness and effort. The detrimental effect of downward deviation from a reference point is a reasonably consistent result. Findings are more mixed concerning the effect of a rise above the reference point (e.g. Kube et al. 2010; Cohn et al. 2013).

Recent experimental investigations have also unearthed sources of heterogeneity in reactions to pay changes stemming from comparisons with other workers (Cohn et al. 2011), firm profitability (Hennig-Schmidt et al. 2011), perceived pay fairness (Cohn et al. 2013) and expectations (Abeler et al. 2011). I examine whether these findings carry over into representative 'natural' field data.

This study shows that pay growth has a positive impact on job satisfaction, consistent with explanations for downward wage rigidity based on fairness, such as Akerlof's (1982) gift exchange model. If earnings are cut, job satisfaction suffers even more than would be expected given the general pay growth impact; indeed, there is a step reduction in job satisfaction – Bewley's (1999) 'insult effect'. This reduction in satisfaction occurs once earnings growth falls below the median, implying that employers need to keep earnings growth at or above average to avoid damaging workforce morale.

Comparisons with the firm and comparisons with salient others affect reaction to a given pay change by determining whether an 'insult' is felt. If comparison pay falls, a firm could get away with a real – but not nominal – pay cut without a step reduction in morale, even if its output is rising. If the pay of similar other workers increases, such a pay cut would have an 'insult effect' if the firm's output is rising, but not if it is falling. No matter how badly the firm is doing, nominal cuts cause a step decline in morale, unless other workers are also experiencing pay cuts. Thus, results provide particular motivation for downward nominal rigidity.

# II. Data and method

Data are drawn from the first 17 waves of the British Household Panel Survey. The BHPS contains quite rich data on job satisfaction, individual and job characteristics, earnings, and from 1999 also basic hourly wage. These data are supplemented by industry performance information from the UK Office for National Statistics, regional unemployment and aggregate prices (see the Data Appendix for details).

Overall job satisfaction is chosen as the best measure of worker morale (which, according to Akerlof's (1982) gift exchange theory, will be affected by pay level and changes). Bewley (1999) identified three components of morale: identification with the firm's ob-

jectives; belief in positive reciprocation (gift exchange); and a mood conducive to good work. Job satisfaction is likely related to all of these. An individual reports their job satisfaction in the BHPS by choosing one of seven categories, ranging from "not at all" to "completely" satisfied.

Results are reported for wage rate and earnings. Both might be relevant in terms of fairness. For hourly-paid workers, the basic hourly wage rate (excluding variable components such as bonuses) is generally thought most relevant: it comes close to the ideal of capturing the wage that is the focus of the employment contract (Bewley 1999, Dickens et al. 2007). For salaried workers, an hourly earnings measure can be calculated by dividing usual salary by usual hours, but this has the disadvantage that it is particularly subject to measurement error stemming from incorrectly-measured hours of work (see Bound et al. 2001). This suggests that total earnings might be the best available measure for salaried workers – and indeed this is often used in job satisfaction studies (Clark et al. 2008). Here, current and past weekly hours of work are included separately as control variables, so coefficients on weekly earnings and its growth could be reformulated into those that would obtain if hourly earnings were used.

Following the precedent of the International Wage Flexibility Network (Dickens et al. 2007), I trim pay data to remove large changes likely due to error. Nominal earnings growth below -85% and above 100% and hourly wage rate growth below -35% and above 60% are discarded. As is commonly found, such trimming actually makes no difference to results. On average, 17% of basic hourly wage changes involve nominal freezes and 14% involve nominal cuts. 13% of hourly wage changes feature nominal raises that do not raise hourly wages in real terms. The remaining 56% enjoy real raises. Figures for earnings are 7% freezes, 29% nominal cuts, 8% between nominal and real zero percentage change, and 56% real raises. The extra volatility in earnings growth reflects the fact it includes more flexible pay components (overtime and bonuses).[1]

[1]The extent of pay cuts may appear surprising, not least because under UK employment law, employers cannot unilaterally cut an employee's pay. An employer who forces a pay cut on an unwilling employee could be subject to a claim against them for breach of contract. However, in their defence, the employer could legally use evidence that there are genuine business or economic reasons why the cut has to be forced upon the employee, meaning that in practice the law does not prevent 'warranted' cuts.

Because the focus of this study is the reaction of workers' job satisfaction to changes in pay, the sample is restricted to 'job stayers' who had no job change of any sort since their last interview. Unusually, and beneficially for this study, the BHPS allows job change to be defined so as to encompass promotions and grade changes, as well as changes of employer. The sample is restricted further to ensure that, as far as possible, there are no changes in job conditions. It is important to control for or exclude changes in work conditions because pay changes may 'compensate' for these, with the result that overall job satisfaction appears unresponsive to pay variation. The aim here is to isolate the satisfaction response to uncompensated changes in pay.

Controls include variables that are standard in satisfaction equations and others less commonly included. These variables are included to capture systematic factors rendering an individual happier or otherwise. Controls include a quadratic in age, gender dummy, three ethnic status dummies, four marital status dummies, number of children aged 16 or below, a dummy for health problems, three education dummies and the log of real non-labour income. Macroeconomic and local labour market conditions are controlled for by the use of year dummies (fifteen for earnings and seven for wage rate) and eleven region dummies. I investigate quadratics in tenure and actual work experience (with dummies for missing values). Other job-related controls include nine occupation dummies and seventeen industry dummies, travel-to-work time in hours, nine dummies for workplace employment size, and a dummy for the presence of an employer pension scheme. 'Framing effects' are known to be important in subjective data such as satisfaction. Conti and Pudney (2008) demonstrated that reported job satisfaction is sensitive to interview-related factors, so I control for presence at interview of the respondent's children or partner and whether the respondent's cooperation during the interview was judged not good. A general-to-specific method is used to determine final specifications for each pay measure.

Turning to the econometric method, most of the empirical work in this paper uses a fixed effects model, and I calculate robust standard errors allowing for clustering at the individual worker level. There are at least two good reasons for using a fixed effects

model in the present context. First, I am interested in the effects of income and income changes on individual wellbeing, rather than comparisons across individuals. Second, using the fixed effects model goes some way to control for possible endogeneity of pay. By controlling for fixed effects I control for individuals' inherent tendency to be happy (have a positive attitude or motivation). Therefore, to the extent that it is this fixed inherent attitude that is correlated with pay and pay growth, controlling for fixed effects removes this possible source of endogeneity. I also estimate selection models that control for endogenous job changes. To estimate these relatively complex models I make use of the POLS (probit-adjusted OLS) estimator for ordered variables developed by van Praag and Ferrer-i-Carbonell (2008). POLS uses the typical assumption that underlying the observed categorical, ordered, job satisfaction variable is an unobserved continuous and normally-distributed latent job satisfaction variable. Under POLS the sample probability of satisfaction falling in each category is calculated. Then, by assuming that the natural log of the latent tendency to be satisfied follows a normal distribution, these probabilities are used to 'back out' the underlying latent tendency to be satisfied, which is the variable of interest. For each sample proportion, the POLS dependent variable – the approximated latent tendency – takes the value of the mean over the interval into which the sample proportion falls. POLS and ordered probit give very similar results, subject to a scaling factor (van Praag and Ferrer-i-Carbonell 2008). POLS is far less time-consuming to implement, and enables richer models – such as those allowing for fixed effects and selection – to be estimated easily.

# III. Results

*Job satisfaction, pay, pay growth, and hours*

Table 1 presents coefficients on pay variables resulting from estimation of a fixed effects model of job satisfaction for job stayers whose job conditions do not change. These equations include controls for a wide range of demographic, macroeconomic, interview- and job-related variables (see Section II). As discussed above, two key pay variables are used: log real usual gross weekly earnings (available for hourly-paid and salaried workers from 1991) and log real basic hourly wage rate (applicable for hourly-paid workers and

collected from 1999).[2]

Columns (1) and (3) include only the level of pay (plus controls), and confirm that pay is positively related to overall job satisfaction. Real pay has a positive impact even controlling for current real pay and lagged hours (columns (2) and (4); the dynamic effect of real basic wage rate growth is significant at the 7% level).

Real raises make people happier, and the larger the raise the greater the happiness improvement, consistent with past income levels acting as a referent. Rearranging, the coefficients indicate that current earnings increase job satisfaction faster than lagged earnings reduce it (effects 0.227 and $-0.084$), and a similar 'adaptation' rate is found for hourly wages (a current wage effect of 0.625 compares to a lagged coefficient of $-0.212$).

The significant effects of lagged wage and earnings have important implications for theories of wage rigidity. These results provide evidence that workers' job satisfaction suffers if pay falls below last period's level, and are consistent with a version of Akerlof's (1982) gift exchange hypothesis in which the relevant reference income is own past pay. This could explain why employers are reluctant to cut wages, since the resulting loss in satisfaction may lead to a demotivated and unproductive workforce.

In the remainder of this paper I examine the robustness of this result, including testing whether the result is uniform over the whole range of pay growth rates and over all individuals at all times. It would not be surprising, intuitively and theoretically, if workers' reference income – and the importance they place on it – varied over different situations. If this is true, and if we could identify how reference income changed, then it would be possible to discern the conditions under which wage cuts are more likely.

Total weekly hours of work (the sum of normal standard and normal overtime hours) and change in weekly hours have no effect on job satisfaction for hourly-paid workers, once other factors are controlled for (see columns 3 and 4 of Table 1). In contrast, total weekly hours do have an impact when weekly earnings are the pay variable: in this case, both current and lagged log weekly hours negatively affect job satisfaction. The negative

[2]The use of time dummies means most results are identical whether real or nominal values are used. When analysing nonlinearities, the analysis distinguishes between real and nominal phenomena, providing evidence on whether workers' morale relates to real or nominal wage rigidity.

signs are consistent with hourly pay mattering, but the negative hours effects appear bigger than would be warranted if this were the only role of hours.

Relationships between job satisfaction and control variables are similar to those found previously, subject to the caveats that most of the literature has estimated ordered probit rather than fixed effects models and that significance varies across pay measures. Job satisfaction tends to be lower among those with poor health (Clark and Oswald 1996). A negative coefficient on squared job tenure implies an inverse-U-shaped relationship between tenure and satisfaction (Theodossiou and Zangelidis 2009). (However, once fixed effects are included, the standard ordered probit or linear regression result of a U-shaped satisfaction-age relationship seems to vanish.) Presence of partner at the interview ("don't show your partner how satisfied you are") and poor interview cooperation are associated with lower reported job satisfaction (as in Conti and Pudney 2008). Job satisfaction rises with the number of children. Given that the worker is in the same job, they are happier if their establishment is in the smallest employment size category (1-2 workers). Year dummies are significant and, particularly for hourly-paid workers, show a declining trend in job satisfaction over time.

*Is there nonlinearity in the job satisfaction–pay growth relationship?*

Bewley (1999) suggested that workers feel 'insulted' if a pay award falls below an expected or reference level that embodies workers' notion of fairness. The 'insult' causes a step change, or jump, in satisfaction once the reference level is reached. Loss aversion and diminishing marginal utility, however, would suggest looking for breaks in the slope, rather than the intercept, of the satisfaction–pay growth relationship. If past pay constitutes a reference point, loss aversion would be reflected in a steeper job satisfaction-wage growth relationship for wage growth below zero than above, so 0% would form a slope break point. A 0% break point is supported by early experiments on monetary gambles and on emotional responses to nominal cuts (e.g. Goette and Huffman 2007). But, outside the laboratory, people may pay attention to the rise in prices, so the 'kink' might occur at real zero. Loss aversion might also be felt in relation to other reference points, such as the pay level of other workers, or expected pay (Abeler et al., 2011).

Because theories suggest a number of alternative nonlinearities in the relationship between satisfaction and pay growth, it is desirable to adopt a flexible method of determining whether and where nonlinearities arise. In cases where the location of breaks are not known for certain *a priori*, Bai and Perron (1998) and Andrews (1993) suggest a simple 'grid search' over a range of break point candidates. At each possible break point, a Chow-type structural change test is conducted. Here, the test is for partial structural change involving a threshold model (Bai and Perron 2003). The test investigates whether either, or both, the intercept or the slope of the satisfaction–pay growth relationship varies with the level of pay growth. $F$ statistics testing the null hypothesis of no break at each value of pay growth were calculated, in steps of 0.1 percentage point.[3] Andrews (1993) and Bai and Perron (1998) provide critical values to judge whether the relationship is significantly non-linear. In the case of significant nonlinearity, the breakpoint is located at pay growth value corresponding to the maximal test statistic (termed the $SupF$ statistic, after Andrews 1993). The test can be extended to the case of multiple breaks (Bai and Perron 1998).

Testing separately and jointly for breaks in the level and slope of the satisfaction–earnings growth relationship indicates that level and slope breaks do occur, at different values of earnings growth. For earnings, the supremum of the $F$-statistics testing intercept constancy locates an intercept break at 1.5%, which happens to be median earnings growth. This break is a significant one ($SupF$ statistic 27.30, 1% critical value 13.58; see Table 2 and Figure 1). A break in slope is not identified in the region of nominal or real zero pay growth,[4] thus giving little support to theories of loss aversion related to nominal or real pay cuts. A change in the behaviour of job satisfaction is found

[3] The tests use the fixed effects model and assume all other coefficients to be constant. Trimming (exclusion of sample endpoints) must be conducted as test statistics tend to infinity towards the end of the sample. The $SupF$ critical values differ according to the proportion of the sample over which the test is conducted, and are obtained from Andrews (1993) or Bai and Perron (1998). 5% trimming is applied (as in Bai and Perron 1998). Andrews (1993) shows that it is in fact possible to use the same critical values to detect breaks within the first 5% of the earnings sample, because the critical values depend on the ratio of the proportions trimmed at begining and end, $\lambda = \pi_2 (1 - \pi_1) / [\pi_1 (1 - \pi_2)]$, so a critical value for $\pi_1 = \pi_2 = 0.05$ also applies to $\pi_1 = 0.01, \pi_2 = 0.785$, for example. The sample size of nearly $45,000$ means that tests will have reasonable size and power that close to the beginning of the sample (ordered in terms of earnings growth).

[4] $F$ statistics lie between 8 and 9 in this region, compared to a 5% critical value of 9.63, and do not represent global or local maxima.

close to where pay growth falls by a quarter. Specifically, at pay growth of −24.1%, $F$ statistics for a break in slope peak (see Table 2). Just under 5% of the sample have pay growth lower than −24.1%, and this is not a result of economic consequence, but it proves statistically important to control for the change in satisfaction with pay growth below this point. For lower pay growth, unexpectedly, the slope of the relationship is less steep; indeed, satisfaction apparently rises as earnings cuts get larger (recall that this is controlling for hours changes and observable changes in job characteristics). This is, of course, surprising – but was actually evident in Clark's (1999) results: he reported that average job satisfaction dropped as pay rose from the lowest to the second-lowest quintile, and the percentage "highly satisfied" was also bigger in the lowest quintile than in the middle quintile.

The very different locations of the $SupF$ statistics in the separate tests for breaks in intercept and slope indicate that it would be incorrect to test for a simultaneous change in both.[5] To ensure robustness of detected breaks, bi-directional sequential tests were undertaken, using the $F(l+1|\,l)$ test of Bai and Perron (1998). Imposing the intercept break at 1.5%, a break in slope was detected at −24.1%, and when a slope break was imposed at −24.1%, an intercept break was detected at 1.5% (see Table 2). The break at −24.1% pay growth could equally well be characterised as a break in slope or a break in intercept: a sequential test for a second intercept break, imposing a first intercept break at 1.5%, confirmed a second intercept break at −24.1%, with a very similar $SupF$ statistic to the sequential test for a second break in the form of slope change. Investigation revealed no evidence whatsoever of further breaks, with $F$ statistics not rising above 5 thoughout the sample, compared with a 10% critical value of 10.45 for a sequentially detected third break (Table 2 does not report these insignificant results of tests for a third break).

[5] If the restriction that both intercept and slope break together is imposed, a break significant at the 1% level ($F$ statistic 18.45, 1% critical value 16.64) is detected at 1.0% pay growth – close to the detected intercept break, which reflects the fact that this is the dominant break. (Bai and Perron 1998 show that the estimate of the break location in a single structural change regression applied to data that contain two breaks converges to one of the two true break locations.) Figures plotting this and the other nonlinearity test results reported in this section are available in a web Appendix at http://go.warwick.ac.uk/jennifersmith.

Results for hourly basic wage rate differ from those for total earnings. No significant nonlinearity is detectable in the satisfaction–basic wage rate growth relationship (see Table 2). Thus the relationship is simply the linear one described in Table 1, in which the positive influence of hourly wage growth on satisfaction is consistent with a gift exchange model with past pay acting as a reference point.

The $SupF$ test methodology is atheoretic but, by estimating a model of the satisfaction–earnings growth relationship in which the detected nonlinearities are imposed, theoretical conclusions can be drawn. Below-median earnings growth induces a 'step change' in job satisfaction: satisfaction is lower by an average of 4.2% (see column 1 of Table 3).[6] This strong result accords with the importance Bewley (1999) attributed to the 'insult effect'. It also mirrors the 'Vince Lombardi effect' described by Mas (2006), named after a professional football coach who said that "winning isn't everything, its the only thing".

The 'insult' effect is statistically strong, and it is also not small, relative to other effects. The reduction in job satisfaction when pay growth is below median is more than one and a half times greater than the detrimental effect from a health problem, for example.[7]

The insult effect could be interpreted in terms of a gift exchange model: the nonlinear effect could reflect a reference point at median pay growth, such that workers feel particularly aggrieved if they get anything less than this average rate. This median pay growth reference point acts in addition to the reference point effect of lagged own pay (captured in the linear pay growth term). The step change at the median accounts for a good deal of the impact of pay growth on satisfaction. Once it is explicitly incorporated, the economic size and statistical significance of the continuous increase in satisfaction with higher pay growth is reduced. A one percentage point rise in earnings growth (not entailing a rise from below- to above-median) is now estimated to increase satisfaction

[6] Although this represents an asymmetry, it is not strictly speaking a loss aversion effect since it does not reflect a steeper relationship slope for losses. The fact that in these field data the form of the utility loss can be identified illuminates experimental findings, which are unable to make a slope- versus step-change distinction as they are typically based on very few (and often one) pay cut.

[7] The coefficient of $-0.027$ on a dummy taking value 1 when the individual reports health problems is significant at the 3% level. Health problems covered include problems such as arthritis with arms, legs, hands, feet, back or neck, sight, hearing, skin, allergies, chest/breathing, heart/blood pressure, stomach/digestion, diabetes.

by around 6%, an effect just significant at the 10% level.

Theory and intuition suggest further points of the pay growth distribution worthy of more detailed investigation – points that relate to pay rigidity and pay cuts. Is it really the case that nominal cuts are no worse than any other below-median raise? Bewley's (1999) evidence, among others, highlighted the particular morale-reducing impact of nominal cuts. This implies that (especially nominal) rigidity and raises should lead to a smaller reduction in satisfaction than nominal cuts. However, results in column 2 of Table 3 suggest that there is no significant difference in satisfaction over the range of pay changes below the median: satisfaction is the same among those who have nominal cuts, nominal freezes, real cuts involving nominal raises, and below-median real raises ($p = 0.71$ in a Wald test of identical dummy coefficients, the largest negative coefficient being on nominal freezes). Any reduction in earnings growth below the median reduces satisfaction, relative to those with above-median earnings growth.[8]

Why is the median important? The importance of the median could reflect the influence of social comparisons, since it captures average pay growth of other workers. Further investigation into external comparisons is undertaken in the following section.

*Social preferences*

I examine the effect of social comparisons with similar other workers, and whether and how social concerns interact with the morale effects of wage dynamics. Salient others are defined in a similar way to Layard et al. (2009) and others: in terms of mean by gender, three education groups and four age groups, resulting in 24 different comparison-pay values.[9] Two hypotheses are investigated. First, does comparison pay form a reference

[8]Table 3 documents only possible nonlinearities below the median. Changes above the median were investigated, but no deviation from a linear satisfaction–earnings growth relationship was found, consistent with *SupF* results. The existence of a below-median insult effect was also explored for hourly wages. Although there was a similar pattern of negative coefficients on below-median wage growth dummies, effects were insignificant, consistent with the lack of nonlinearity in the satisfaction–hourly wage growth relationship detectable via *SupF*-type tests. Further research into the difference in satisfaction responses between earnings and hourly wage – for example, with a larger dataset on hourly wages, or one that enabled a full examination of non-basic wage components of earnings – would undoubtedly be useful.

[9]Results using this definition of comparison pay are very similar to those obtained if, instead of gender, (eleven) regions are used to define comparison groups (along the lines of Ferrer-i-Carbonell 2005).

point, thus directly and negatively affecting workers' satisfaction? Second, are pay cuts – and below-median pay growth generally – (more) acceptable if similar others are also experiencing real cuts? Cohn et al. (2011) found just this in a field experiment.[10]

Bewley (1999), among others, emphasised comparisons with the firm: his interview evidence indicated that workers might accept pay cuts if their firm or industry is doing particularly badly (see also Kahneman et al. 1986). Workers might believe that by taking a pay cut they can preserve their jobs (or their firm). An alternative rationale relies on workers essentially making comparisons with firm performance when assessing satisfaction with pay, and indulging in gift exchange (Akerlof 1982). Workers would be willing to sacrifice pay if the firm's performance is poor, but would be disgruntled to have the same cut forced on them if the firm is doing well. To test these hypotheses, I differentiate according to firm performance using a dummy distinguishing whether the individual's industry output rises or falls in real terms (the data are disaggregated into 33 industries for this purpose). Just over one fifth of observations relate to industries where output falls.

For clarity, I first look separately at each comparison – with the firm and with other workers – and then allow them to interact, to investigate how workers react to the existence of multiple referents.

Does others' pay act as a reference point? When fixed effects are used, as preferred, it appears not (see columns 2 and 6 of Table 4). The estimation method matters: in an ordered probit, the pay of similar others is estimated to have a substantial negative impact on job satisfaction, consistent with it playing a reference point role (columns 1 and 5). However, this comparison income effect is wiped out by the inclusion of time-invariant individual characteristics (fixed effects, gender, education and age). Several interpretations are possible, but the most likely is that there is insufficient within-individual variation in the level of comparison pay for a significant effect to be estimated. For

[10] Intuition and theory provide strong grounds for dividing others' pay growth into real raises and real cuts, and the importance of real zero was confirmed by an atheoretic grid-search over possible break points (similar to the $SupF$ tests above) which confirmed that 0% was the most likely candidate. Comparison earnings fall in real terms in around 5% of cases. No 'reference point' effect was found from the growth rate of others' pay and the change in real industry output: they have no significant *direct* impact.

earnings, for example, within-individual variance of comparison pay accounts for less than 9% of total variance. Because of the advantages of fixed effects in substantially removing endogeneity problems, I will proceed using this estimation method (and therefore exclude the level of comparison pay) – but it is useful to bear in mind that the fixed effects could be interpreted as capturing the substantial influence of social comparison with others' pay level.

Do workers always look to past pay as a reference point, or only under some circumstances? Interacting lagged own pay (pay growth) with dummies capturing firm performance and rate of growth of others' pay reveals that the reference point of past pay is robust: its negative impact on satisfaction does not vary much in relation to what is happening elsewhere (columns 3, 4 and 7 of Table 4).[11] However, the effect of own lagged pay is insignificant at the 10% level when others' pay is falling (column 4), which could be taken to imply that workers tend to focus more on their own pay growth when others' pay is also growing (indicating a desire to "keep up with the Joneses").

Results show that whether an 'insult' is felt upon earnings growth falling below the median depends on social comparisons. A step reduction in satisfaction is only felt if comparison earnings rise (see column 4 of Table 4). If others are also experiencing cuts, then low pay growth is more acceptable. But an insult effect is apparent whether the industry is expanding or contracting (column 3) – a result not supportive of Bewley's (1999) contention that poor firm performance excuses cuts.

Table 5 investigates the insult effect of below-median earnings growth further, with two aims: to investigate the interaction between the two social comparisons (with firm and other workers), and to see whether nominal cuts are treated just like any other low pay growth. Distinguishing categories of low pay growth (column 2 of Table 5) shows that interactions between referents matter: it is only when others are enjoying real raises *and* industry output is rising that workers exhibit 'insult' unhappiness with

[11] In order to constrain the effect of workers' current pay to be constant while examining interactions of the their past pay with social comparisons, it is necessary to substitute own pay growth with lagged own pay. The two are interchangeable: the negative of the coefficient on lagged pay gives the effect of pay growth. Falls in comparison pay can only be investigated for earnings because there are insufficient cuts in comparison hourly wage (the average hourly wage over age-education-gender cells).

freezes, nominal raises and other below-median pay growth involving small real cuts (but not nominal cuts). This is a fairly uniform impact,[12] taking the form of a step reduction in satisfaction of around 5% if own pay growth is low while similar others' pay and firm performance are both rising. It is noteworthy that if industry output declines, or if others' pay is falling, no insult effect accompanies low pay growth (above a nominal cut).

Nominal cuts are a special case. The reaction to nominal cuts does not seem to be influenced by fairness in the form of reference to firm performance. Workers are unhappy with nominal cuts if others are enjoying raises, whether their industry is expanding or declining. These findings therefore provide particular motivation for downward nominal wage rigidity. Nominal cuts are the most numerous of the four below-median categories, so using a below-median pay growth dummy reflects the pattern of results for nominal cuts (see column 1 of Table 5). Overall, these disaggregated results confirm the importance of the median, first uncovered in the *SupF* tests. What the *SupF* tests could not reveal, of course, was that the below-median difference only applied under certain conditions.

In summary, the fairness of own pay growth appears to be judged against others' pay growth. Furthermore, results imply that it is not just horizontal equity – equity across similar workers – that matters, but also 'vertical' equity with the firm. The behaviour of job satisfaction in relation to whether pay changes meet this multifaceted reference point is consistent with a gift exchange model embodying an insult effect. Comparisons with other workers and with the firm both appear to matter equally to workers as they react to low pay changes, apart from the special case of nominal cuts, where comparisons with other workers are dominant in determining morale effects.

Results have clear implications for compensation policy. Firms whose performance is improving need to avoid any cut in real earnings in order to maintain morale – unless other similar workers are experiencing pay cuts. A firm whose performance is declining

[12] In a Wald test, equal coefficients cannot be rejected, with a $p$-value of 0.56. All interaction effects are relative to the base of above-median pay growth, average industry output growth and average pay growth for similar other workers.

could impose a small real earnings cut – whatever is happening to other workers' pay – without any extra adverse morale impact above and beyond the 'normal' response of satisfaction to pay growth, as long as nominal cuts were avoided. Nominal cuts are a special case, in that even if the firm is in dire straits, they would adversely affect morale through an insult effect, unless other workers are experiencing cuts.

*Robustness checks*

Robustness checks were carried out to ensure the results were not driven by sample selection or measurement error.[13]

Perhaps the sample of workers continuing in their jobs is selected, because the prospect of pay cuts leads to turnover concentrated among most able workers. Such endogenous turnover is certainly feared by employers (Bewley, 1999). If remaining lower-ability workers are relatively satisfied with the reduced income (as they have lower expectations or reference points), results would underestimate the average adverse impact of cuts. However, there is some evidence that relative losses do not induce quits: Mas (2006) finds no change in police employment after arbitration rulings against the union – although all his arbitrated settlements involved nominal raises, and most real raises. Other forms of sample selection are possible, though. For example, if the wage bill is held 'too high' through downward rigidity, the firm might need to reduce employment, implying the sample of continuing workers would have above-average ability.

To find out if selection is affecting results, sample selection models were estimated. Selection into the estimating sample was captured by a probit model, with the dichotomous dependent variable taking value 1 if the worker was employed last period and this, did not change job (so excluding promotions and grade changes), job characteristics did not change (as defined in Section II), and data to estimate the job satisfaction equation were available. The selection variable took value 0 if the worker was employed last period but was in a different job with the same employer (including promotions and grade changes), or was in the same job but job characteristics or work conditions

[13]Recall that I have already removed large pay changes likely due to error by trimming pay growth (see section II). Full results for this section are not reported here but are available in a web Appendix at http://go.warwick.ac.uk/jennifersmith.

changed, or was with a different employer, or was not employed this period. The models were estimated using a two-step method whereby a quadratic in the inverse Mills' ratio derived from a first stage selection probit was included in a fixed effects satisfaction equation (including pay and pay growth, and dummies for nonlinear earnings growth effects). Identification was ensured by the inclusion of additional variables in the selection probits, where the identifying regressors were jointly significant in the selection equation (and, in a separate test, were found jointly insignificant when included in the job satisfaction equation).[14] Using a Wald test for the joint significance of a quadratic in the inverse Mills' ratio in the satisfaction equation, independence of selection and job satisfaction equations could not be rejected ($p$-values 0.61 for earnings and 0.22 for hourly wage), although the inverse Mills' ratio was individually significant at 10% for the hourly wage sample.[15] The 'selection-corrected' estimates continued to show positive impacts on job satisfaction from pay and pay growth (the latter dominated by a step jump at the median in the case of earnings). Thus sample selection does not appear to influence results.

I now turn to the question of whether results are driven by measurement error in pay. Does the relationship between satisfaction, pay and pay growth reflect the reality, as suggested by Akerlof et al. (1996), that many nominal cuts are spurious, being due to measurement error in earnings levels?

Measurement error was examined by looking at two specific types of subsample that

[14] Identifying variables were chosen on intuitive and theoretical grounds as those which might influence the probability of remaining in the same job but not affect job satisfaction directly. The set of variables differed slightly between basic wage and earnings to ensure identifying conditions were met, and included the regional (Claimant Count) unemployment rate and percentage changes in unemployment rate, log regional gross value added per capita and its first difference, dummies for whether the worker's spouse was employed, house tenure (private renting and renting from local authority – council housing – with base case home ownership) and log non-labour income. In all cases, a test of joint exclusion of the identifying regressors in the job satisfaction equation could not be rejected (with significance level 40% or larger) and their joint insignificance in the selection probit could be rejected at <0.001% level. Observations on non-stayers numbered 27,402. Because the identifying variables were found independent of job satisfaction, in principle they might be used to instrument pay and pay growth to control for endogeneity. However, although strongly correlated with the event of staying in a job, they proved less related to pay; and using them in fixed effects instrumental regressions rendered pay variables insignificant.

[15] It is possible that, on average, non-stayers might effectively not differ much from stayers, due to heterogeneity among non-stayers: non-stayers include a substantial proportion of voluntary movers and those who were promoted, as well as involuntary or 'disadvantaged' job leavers.

are very likely free of measurement error. The first type of subsample consists of those cases where the interviewer has documentary evidence on pay, or where the basic rate is known exactly. Within the BHPS there is information about whether the pay slip – given to the worker by the employer as a record of pay – was checked when the interview response on pay was given. The pay slip was examined in both years relevant to the pay change by 22% of the earnings sample. Many hourly-paid workers will not receive a pay slip; only 16% inspect it in both years. For hourly-paid workers, there is a second check: there is information about whether the wage rate stated is "known exactly" or "estimated". 60% state an exact amount in both years relevant to the wage change. The relatively low proportions whose pay slip is checked clearly give rise to concerns about selection, and indeed comparisons of means show that those who check pay slip are older and so have higher pay but slower pay growth; other features also differ significantly between checkers and non-checkers. So, although the checker subsample gives information about the satisfaction–pay growth relationship that is relatively free of the influence of measurement error, inference needs to be confined to this subsample.

The second type of subsample consists of data since the introduction of computer-assisted 'Dependent Interviewing' (DI) in 2006 (Wave 16), which should have helped clean data of earnings change errors. An earnings check question is now triggered when the computer-aided comparison of last year's hourly earnings with this year's indicates a nominal cut bigger than 30% or nominal raise bigger than 40% (for stayers, or 60% for movers), which means that waves 16 and 17 should be free of large earnings change errors.

Results from error-free subsamples give no real grounds to doubt full-sample findings, and are rather disparate. In the payslip-check subsamples, both basic wage and earnings effects are purely dynamic (for earnings, this dynamic effect being dominated by a step jump), and the pay growth effect is substantially bigger in these error-free subsamples. However, when the basic wage is stated exactly it is only the basic wage level that is significant, and no pay growth effect is significant after DI was introduced. Rather surprisingly, the excess happiness of those with large earnings cuts remains in all error-

free subsamples.

Overall, therefore, robustness checks suggest that measurement error does not appear to systematically influence the results.

# IV. Conclusion

If fairness considerations are important in wage-setting, there will be heterogeneity in the response of workers to pay cuts. Building up a picture of the empirical regularities underlying this heterogeneity will allow us to predict when pay cuts will occur (without adverse morale consequences).

There are several key findings. The first is a basic one: the significant impact of pay growth on job satisfaction is consistent with workers using past pay as a referent.

Second, there is what has been termed an 'insult' effect by Bewley (1999) or a 'winning is the only thing' effect by Mas (2006): there is a step-reduction in satisfaction for earnings cuts and below-median earnings growth generally. Beyond this, though, there is no evidence of loss aversion.

Third, reaction to pay cuts depends on the perceived fairness of the pay change. Real cuts (and below-median increases generally) that do not involve a nominal cut only significantly adversely affect morale if two conditions apply: if similar other workers are enjoying raises and the workers' industry is expanding. Dissatisfaction when pay growth does not reflect good industry performance could reflect workers' disgruntlement at apparently unfair treatment by the firm, in accordance with the gift exchange model.

One interpretation of the insult effect is that there is an implicit or explicit contract that the wage should increase with the general wage trend, and that the nominal wage should not fall, as suggested by Holden and Wulfsberg (2008, 2009). The insult effect might be a perceived break, or change, of that contract.

A related explanation for the insult effect, and its heterogeneity, emphasises expectations as a reference point (Abeler et al. 2011). Looked at in this way, what this paper begins to do is identify forces driving expectation formation. Results show formally what many would intuitively suspect: that expectations of pay depend in part on one's own past pay, and that expectations of pay change are influenced by how similar other

workers are faring in the labour market and how one's company is doing, capturing shocks to labour supply and productivity. Satisfaction responds adversely to a fall in pay and pay growth relative to expectations. Crawford and Meng (2011) find effects similar to the influence of median pay growth found here: they found sample average pay, proxying expectations or the general trend in wages, influential in determining the point at which New York cab drivers choose to stop work – and, as here, much of the effect was in the form of a 'jump' once the expectation (or reference point) was reached. Further work in laboratory or field to distinguish between fairness, contract, and expectations effects in reference point models could usefully illuminate empirical results and help unite sometimes disconnected literatures.

A further key finding of this paper is that nominal cuts are disliked unless cuts are universal (even when industry output is declining), providing further morale-based motivation for downward nominal rigidity. These results are similar to those of Cohn et al. (2011), whose field experiment showed that effort falls when a worker suffers a nominal pay cut not experienced by others.

The general support found for the importance of gift exchange and social preferences might surprise some familiar with sceptical views (e.g. Levitt and List 2007). Results imply that we can get closer to understanding wage rigidity and how it varies by acknowledging the importance of fairness and comparisons in the labour market.

There is, of course, still a great deal of work to be done. Data covering many workers in the same firm, and including firm performance information and job satisfaction, would allow a more accurate assessment of the importance of fairness concerns. Such data are rare. Simple confirmation of this paper's findings from British data in other typical survey datasets would be useful. Results also suggest there is scope for finer tests in field and laboratory; at present, the number of studies investigating interactions between changes in own pay and either other workers' pay changes or firm surplus is very small.

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# Data Appendix

The BHPS data used in this paper were made available through the UK Data Archive. The data were originally collected by the ESRC Research Centre on Micro-social Change at the University of Essex, now incorporated within the Institute for Social and Economic Research. Neither the original collectors of the data nor the Archive bear any responsibility for the analyses or interpretations presented here.

Industry performance is measured by real output per capita (volume measure, at 2005 prices) for 33 industries, according to their 2-digit 1992 Standard Industrial Classification. During the sample period the BHPS classification system changed, and coding frames from the Office of National Statistics (ONS) are used to recode SIC80 to SIC92 (see "Standard Industrial Classification of Economic Activities: Mapping of SIC (80) to SIC (92) and Mapping of SIC (92) to SIC (80)", OOSS User Guide 1990: 09, May 2003, Occupational Information Unit at the ONS). For manufacturing industries, industry performance data are matched to BHPS data according to month of interview. For other industries, data are matched according to quarter of interview.

The 'overall' job satisfaction question is asked during the interview. It follows four questions relating to satisfaction with particular aspects of the job: "I'm going to read out a list of various aspects of jobs, and for each one I'd like you to tell me from this card which number best describes how satisfied or dissatisfied you are with that particular aspect of your own present job... 1 The total pay, including any overtime and bonuses; 2 Your job security; 3 The actual work itself; 4 The hours you work." The interviewer shows the respondent a card with the following verbal labels attached to the seven possible numerical responses: 7=Completely satisfied, 6=Mostly satisfied, 5=Somewhat satisfied, 4=Neither satisfied nor dissatisfied, 3=Somewhat dissatisfied, 2=Mostly dissatisfied, 1=Completely dissatisfied. The overall job satisfaction question follows: "All things considered, how satisfied or dissatisfied are you with your present job overall using the same 1-7 scale?". (Waves A to G, 1991-1997, included three additional job satisfaction domains: promotion prospects, relations with superiors and initiative.) The job satisfaction questions are asked in the 'Employment' section of the interview and follow

simple factual questions on employment status, industry, occupation, employer, duties, hours of work and travel to work. The satisfaction questions immediately precede questions on pay, so reports and any interaction concerning pay will not influence reported job satisfaction – although respondents familiar with the interview structure might have in mind, or have specifically recalled, their pay and pay history in preparation for those questions.

I exclude any 'job stayer' who, despite reporting no job change, also reports a change between full- and part-time status, between temporary/contract/fixed-term/seasonal and permanent status, between public and private sectors, and between either of managerial or supervisory positions and other. I also omit those whose location of work alters (between employer's premises and other locations). Excluding these observations is numerically important: 28% of the sample is affected (for both hourly-paid and salaried workers).

In Section III I check robustness using pay-slip and additional basic hourly wage checks. The BHPS questionnaire emphasises "RESPONDENT TO CHECK PAY SLIP IF POSSIBLE", and whether the respondent checked the latest or an earlier pay slip is recorded. The precise wording for the additional basic wage check is "What is your hourly rate of pay for your basic hours of work? WRITE IN AMOUNT PER HOUR. IF EXACT AMOUNT NOT KNOWN ENTER APPROXIMATE AMOUNT AND CODE 'Estimated amount' BELOW". Almost all (96%) of pay slip checkers state an exact amount for basic wage.

I also check robustness using the introduction of Dependent Interviewing during the sample period, which entails a computer-prompted earnings check of large earnings changes. The post-DI sample comprises around one fifth of the total. On the basis of previous waves, Jäckle et al. (2007) report that the earnings check question would have been posed to around 10% of respondents. The earnings check question, which is asked in relation to net pay if possible or gross pay if not, is: "So your [net] pay has gone <UP/DOWN> since last time we interviewed you, from <CONVERTED AMOUNT> per <PERIOD> for a <TOTALHOURS> hour work week (including overtime), to

<AMOUNT STATED THIS YEAR> per <PERIOD>, is that correct?" (Yes / No / Don't Know or Other)." Respondents not confirming that the pay change was correct are asked for a verbatim explanation of the recorded pay change. Jäckle, Lynn and Uhrig (2007) mention big promotions as a possible explanatory factor, but state that they expect the reason to be data entry error in either wave, such as mis-recording pay period as month rather than year. "Ultimately, this DI application is designed to enhance data quality by reducing the number of outliers that can be difficult to deal with during analysis" (p.12). Unfortunately neither an indicator for when the check was applied nor a record of the verbatim response are included in the released BHPS data.

Table 1: *Impact of pay and pay changes on overall job satisfaction*

| Pay measure | Earnings | | Hourly wage | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ln Real pay | 0.129 | 0.143 | 0.210 | 0.413 |
| | (0.022)*** | (0.028)*** | (0.095)** | (0.153)*** |
| Real pay growth | | 0.084 | | 0.212 |
| | | (0.025)*** | | (0.117)* |
| ln Hours | -0.262 | -0.185 | -0.043 | -0.026 |
| | (0.030)*** | (0.033)*** | (0.053) | (0.069) |
| ln Lagged hours | | -0.129 | | -0.043 |
| | | (0.031)*** | | (0.070) |
| Observations | 46,263 | 44,931 | 10,374 | 8,601 |
| Individuals | 11,837 | 11,678 | 4,332 | 3,611 |

*Notes*: The dependent variable is the natural log of overall job satisfaction (continuous variable created by probit adjustment). Coefficients are from fixed effects models. Asymptotic standard errors (in parentheses) are adjusted for clustering on individuals. Significance is indicated at the following levels: *** 1%, ** 5%, * 10%. The pay measure in columns (1) and (2) is real gross usual weekly earnings (observed for all workers from 1991 to 2007). Hours are total normal weekly hours (normal standard hours plus normal overtime hours). The pay measure in columns (3) and (4) is the real hourly basic wage rate (observed for hourly-paid workers, and available from 1999 to 2007). Controls include a dummy for health problems, age squared, number of children aged 16 or below; tenure squared and a dummy for missing values of tenure, eight employment size dummies; dummies for the presence of the respondent's partner and for poor cooperation by the respondent during the interview; year dummies (fifteen for earnings and seven for hourly wage rate).

Table 2: *SupF-type statistics for breaks in the job satisfaction–pay growth relationship*

| Test | Earnings | | Hourly wage | Critical values |
|---|---|---|---|---|
| | *SupF* | Location | *SupF* | (1%, 5%, 10%) |
| Single intercept break | 27.30 | 1.5% | 3.74 | (13.58, 9.63, 8.02) |
| Single slope break | 23.33 | -24.1% | 1.35 | (13.58, 9.63, 8.02) |
| Slope break given intercept break at 1.5% | 14.38 | -24.1% | | (15.03, 11.14, 9.56) |
| Intercept break given slope break at −24.1% | 16.50 | 1.5% | | (15.03, 11.14, 9.56) |
| Intercept break given intercept break at 1.5% | 15.37 | -24.1% | | (15.03, 11.14, 9.56) |
| Slope break given slope break at −24.1% | 7.19 | | | (15.03, 11.14, 9.56) |
| Joint intercept and slope break | 18.45 | 1.0% | 1.87 | (16.64, 12.89, 11.02) |

*Notes*: Tests are the Multiple Break and Sequential Break tests of Bai and Perron (1998) (essentially identical to the *SupF* tests of Andrews 1993 when there is a single break). Critical values are taken from Andrews (1993) Table I, p.840, Bai and Perron (1998) Tables I and II, pp.58 and 61, with 5% trimming.

Table 3: *Nonlinearities in the relationship between satisfaction and earnings growth*

| | (1) | (2) |
|---|---|---|
| ln Real pay level | 0.154 | 0.154 |
| | (0.028)*** | (0.028)*** |
| Real pay growth | 0.058 | 0.061 |
| | (0.036)* | (0.037)* |
| *Pay growth dummies*: | | |
| (Base) Above-median pay growth | 0.000 | 0.000 |
| | (N/A) | (N/A) |
| Below-median pay growth | -0.042 | |
| | (0.010)*** | |
| Below-median real raise | | -0.033 |
| | | (0.016)** |
| Real cut but nominal raise | | -0.047 |
| | | (0.015)*** |
| Nominal freeze | | -0.053 |
| | | (0.016)*** |
| Nominal cut | | -0.039 |
| | | (0.013)*** |
| Real earnings growth × Below −24.1% | -0.246 | -0.241 |
| | (0.065)*** | (0.065)*** |

*Notes*: The dependent variable is the natural log of overall job satisfaction. The pay measure is real gross weekly earnings. Coefficients are from fixed effects models, with asymptotic standard errors (in parentheses) adjusted for clustering on individuals. Both regressions use 44,931 observations on 11,678 individuals. Significance: *** 1%, ** 5%, * 10%. For details of controls, see notes to Table 1.

Table 4: *The influence of reference points*

| Pay measure | Earnings | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | | (2) | | (3) | | (4) | |
| | Coeff. | S.E. | Coeff. | S.E. | Coeff. | S.E. | Coeff. | S.E. |
| ln Real pay level | 0.315 | (0.050)*** | 0.212 | (0.040)*** | 0.215 | (0.040)*** | 0.212 | (0.040)*** |
| ln Real comparison pay level | -0.259 | (0.026)*** | -0.021 | (0.039) | | | | |
| ln Lagged own real pay level | -0.211 | (0.047)*** | -0.059 | (0.036)* | | | | |
| × Real industry output rises | | | | | -0.060 | (0.037)* | | |
| × Real industry output falls | | | | | -0.062 | (0.037)* | | |
| × Real comparison pay rises | | | | | | | -0.059 | (0.036)* |
| × Real comparison pay falls | | | | | | | -0.054 | (0.037) |
| *Pay growth dummy and interactions*: | | | | | | | | |
| (Base) Above-median pay growth | 0.000 | (N/A) | 0.000 | (N/A) | 0.000 | (N/A) | 0.000 | (N/A) |
| Below-median pay growth | -0.061 | (0.014)*** | -0.042 | (0.010)*** | | | | |
| × Real industry output rises | | | | | -0.041 | (0.011)*** | | |
| × Real industry output falls | | | | | -0.047 | (0.018)*** | | |
| × Real comparison pay rises | | | | | | | -0.044 | (0.011)*** |
| × Real comparison pay falls | | | | | | | -0.001 | (0.034) |
| Real earnings growth × Below −24.1% | -0.259 | (0.083)*** | -0.245 | (0.065)*** | -0.245 | (0.066)*** | -0.244 | (0.065)*** |
| Observations | 41,524 | | 44,931 | | 44,236 | | 44,931 | |
| Individuals | 10,305 | | 11,678 | | 11,612 | | 11,678 | |
| Estimation method | Ordered Probit | | Fixed Effects | | Fixed Effects | | Fixed Effects | |

*Notes*: The dependent variable is the natural log of job satisfaction. Comparison pay is the mean over cells defined by gender, three education groups and four age groups. Industry is measured at the 2-digit level. Standard errors (in parentheses) allow for clustering at the individual level. Additional regressors for ordered probit include marital status and ethnicity dummies, tenure, a quadratic in actual experience, occupation and region dummies. (If age, education and gender are included as additional regressors in ordered probits, comparison income becomes insignificant, since these variables identify the cells over which means are taken to define comparison income.) Other notes are as for Table 1.

Table 4, continued: *The influence of reference points*

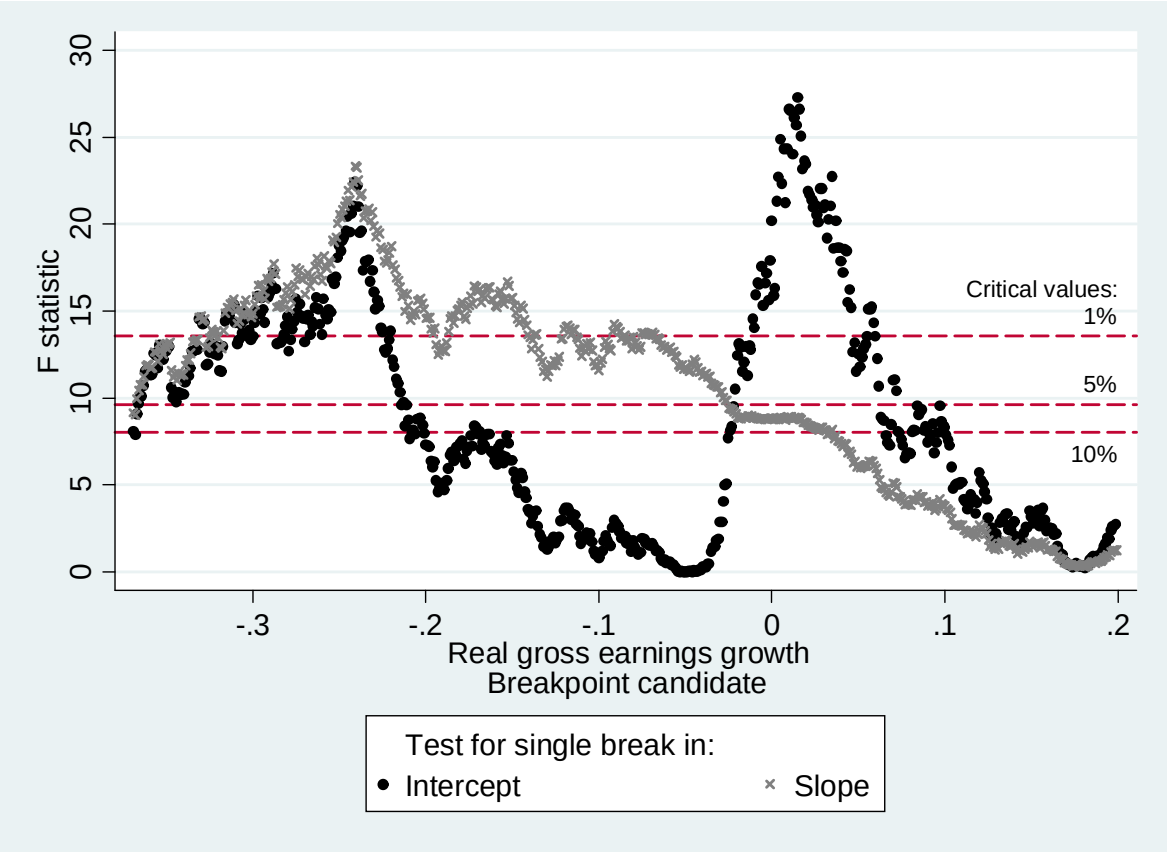
| Pay measure | Hourly wage | | | | | |
|---|---|---|---|---|---|---|
| | (5) | | (6) | | (7) | |
| | Coeff. | S.E. | Coeff. | S.E. | Coeff. | S.E. |
| ln Real pay level | 0.783 | (0.136)*** | 0.599 | (0.135)*** | 0.599 | (0.137)*** |
| ln Real comparison pay level | -0.729 | (0.139)*** | 0.034 | (0.224) | | |
| ln Lagged own real pay level | -0.674 | (0.130)*** | -0.218 | (0.115)* | | |
| × Real industry output rises | | | | | -0.221 | (0.117)* |
| × Real industry output falls | | | | | -0.206 | (0.118)* |
| × Real comparison pay rises | | | | | | |
| × Real comparison pay falls | | | | | | |
| *Pay growth dummy and interactions:* | | | | | | |
| (Base) Above-median pay growth | | | | | | |
| Below-median pay growth | | | | | | |
| × Real industry output rises | | | | | | |
| × Real industry output falls | | | | | | |
| × Real comparison pay rises | | | | | | |
| × Real comparison pay falls | | | | | | |
| Real earnings growth × Below −24.1% | | | | | | |
| Observations | 7.718 | | 8,864 | | 8,643 | |
| Individuals | 3,121 | | 3,701 | | 3,668 | |
| Estimation method | Ordered Probit | | Fixed Effects | | Fixed Effects | |

Table 5: *Interactions with industry perfomance and other workers' pay*

| | (1) | | (2) | |
|---|---|---|---|---|
| | Coeff. | S.E. | Coeff. | S.E. |
| ln Real pay level | 0.214 | (0.040)*** | 0.216 | (0.043)*** |
| ln Lagged own real pay level | -0.060 | (0.037)* | -0.063 | (0.038)* |
| *Pay growth dummies and interactions:* | | | | |
| (Base) Real pay growth above median | 0.000 | (N/A) | 0.000 | (N/A) |
| Below-median pay growth | | | | |
| × Real industry output rises × Real comparison pay rises | -0.042 | (0.011)*** | | |
| × Real industry output rises × Real comparison pay falls | 0.023 | (0.033) | | |
| × Real industry output falls × Real comparison pay rises | -0.053 | (0.015)*** | | |
| × Real industry output falls × Real comparison pay falls | -0.032 | (0.057) | | |
| Below-median real raise | | | | |
| × Real industry output rises × Real comparison pay rises | | | -0.039 | (0.018)** |
| × Real industry output rises × Real comparison pay falls | | | 0.094 | (0.062) |
| × Real industry output falls × Real comparison pay rises | | | -0.044 | (0.034) |
| × Real industry output falls × Real comparison pay falls | | | 0.137 | (0.118) |
| Real cut but nominal raise | | | | |
| × Real industry output rises × Real comparison pay rises | | | -0.055 | (0.017)*** |
| × Real industry output rises × Real comparison pay falls | | | 0.027 | (0.053) |
| × Real industry output falls × Real comparison pay rises | | | -0.037 | (0.028) |
| × Real industry output falls × Real comparison pay falls | | | 0.040 | (0.088) |
| Nominal freeze | | | | |
| × Real industry output rises × Real comparison pay rises | | | -0.056 | (0.019)*** |
| × Real industry output rises × Real comparison pay falls | | | -0.063 | (0.075) |
| × Real industry output falls × Real comparison pay rises | | | -0.033 | (0.030) |
| × Real industry output falls × Real comparison pay falls | | | -0.107 | (0.138) |
| Nominal cut | | | | |
| × Real industry output rises × Real comparison pay rises | | | -0.034 | (0.014)** |
| × Real industry output rises × Real comparison pay falls | | | 0.023 | (0.046) |
| × Real industry output falls × Real comparison pay rises | | | -0.066 | (0.021)*** |
| × Real industry output falls × Real comparison pay falls | | | -0.095 | (0.083) |
| Real earnings growth × Below −24.1% | -0.243 | (0.066)*** | -0.239 | (0.066)*** |

*Notes*: The dependent variable is the natural log of job satisfaction. The pay measure is real gross weekly earnings. The regressions use 44,236 observations covering 11,612 individuals. Coefficients are from fixed effects models, with asymptotic standard errors (in parentheses) adjusted for clustering on individuals. Significance: *** 1%, ** 5%, * 10%. See also notes to Table 1.

Figure 1: *Tests of single change in (a) the intercept and (b) the slope of the job satisfaction–earnings growth relationship*




*Notes*: $F$-statistics test the null of insignificant change in intercept or slope coefficient of the job satisfaction–pay growth relationship at the relevant breakpoint. Nonlinearity is detected if the maximal $F$-statistic exceeds the $SupF$ critical values (obtained from Andrews, 1993, or Bai and Perron 1998), and in that case nonlinearity is located at the maximum.