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

Jennifer C Smith*

Department of Economics, University of Warwick, CV4 7AL, UK.

jennifer.smith@warwick.ac.uk

August 2009

Abstract

Several important theories of wage rigidity rely on a positive relationship between pay growth and utility, so workers become unhappy if they suffer pay cuts. This paper investigates this relationship using job satisfaction as proxy for utility. Non-linearities are investigated, includingg the presence of loss aversion, and 'excess' satisfaction among those who experience pay freezes.

Results show a significant concave relationship between job satisfaction and real raises: workers prefer large raises but there are diminishing marginal benefits. Nominal freezes are better (for the workers receiving them) than nominal raises leaving pay the same in real terms. This is explained in part by external referents: workers consider their firm's performance when assessing the merits of a particular pay change, and appear particularly happy with freezes when real output of the firm's industry falls, which is consistent with relief at avoiding warranted nominal cuts. Evidence is also found that workers also compare themselves to others like themselves. No evidence of loss aversion is found. Overall, the relationship between pay growth and job satisfaction is less steep for cuts than for raises.

Keywords: Wage rigidity, Pay growth, Pay cuts, Job satisfaction

JEL classification: J30, E24, J28

*I would like to thank Truman Bewley and Peter Howlett for helpful discussions on related issues. The 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.

1 Introduction

'Micro' data reveal a great deal of flexibility in both prices and wages, in terms of both frequency and size of changes.[1] On the other hand, the same micro data also show clear evidence of both real and nominal downward rigidity. This heterogeneity presents challenges. The challenges are theoretical and empirical, and also face policymakers wishing to predict macroeconomic responses to policy changes. Can a single, simple theoretical framework be applied? How can commonly-used models of rigidity, such as those based on Calvo (1983) staggered price-setting, be adapted to fit the data? Empirical work is needed to aid theoretical development - and policy - by providing a better understanding of the reasons for heterogeneity. This paper aims to contribute to the understanding of wage dynamics by means of an investigation of the utility implications of pay changes. There are two major motivations. The first motivates the study of pay changes: the observation that inflation is persistent is best captured in models in which a substantial proportion of price-setting is backward-looking. The Gali and Gertler (1998) model embodying this assumption has been widely influential in macro modelling (Smets and Wouters 2003). Gali (2009) translates this inflation persistence into wage growth persistence, embodied in a substantial proportion with 'nominal wage rigidity' (estimated at eighty percent).[2] My second motivation stems from a desire to investigate pay growth in the context of models of wage-setting based on fairness, in which workers' utility depends on how their own income compares with 'reference income' (Akerlof and Yellen 1990). Conceptualising reference income is not easy. It is certainly reasonable to assume that comparisons are made with similar ('salient') others, but it is also reasonable to assume that an individual's own past experience will have an impact on what that individual currently expects. Much work has investigated 'external' referents, and this literature is reviewed below. However, backward-looking referents have more immediate relevance to macro models, where as noted the continuing struggle is to explain inflation persistence.[3]

There has been a substantial amount of effort devoted to discovering the facts about wage

[1] See for example, on prices: Bils and Klenow (2004) and Klenow and Kryvtsov (2008) for the US and Alvarez *et al* (2006) for the EU; and on wages: international evidence in Dickens *et al* (2007) and Holden and Wulfsberg (2008; 2009).

[2] Eighty percent of wage setters are estimated to be non-optimising. In common with the literature, Gali (2009) allows these individuals' wages to change: sixty percent of non-optimising wage-setters are backward-looking and base their wage growth on last period's price inflation, the remainder on this period's price inflation. It is easy to see that in a mark-up model this entails influence from last period's wages on current wages.

[3] Holden and Driscoll (2003) show that if workers care about other workers' *current* wages there is no inflation persistence (at least in the context of the commonly-used Fuhrer-Moore 1995 model), but in Driscoll and Holden's (2004) 'loss aversion' fairness model, inflation persistence arises if workers' expectations are based on the past behavior of wage growth.

rigidity and flexibility, and some work on the macro consequences, but only limited success in finding what drives these features. Early work devoted to investigating nominal rigidity found a fairly large proportion of wage freezes in annual data - around 8-10% in the UK and US - but also substantial downward nominal flexibility (around one fifth of pay changes featuring nominal cuts is typical) (Smith 2000 and Nickell and Quintini 2003 for the UK; Kahn 1997 and Card and Hyslop 1997 for the US; and Beissinger and Knoppik 2003 for Germany). Recent work (summarised in Dickens *et al* 2007 and Goette, Sunde and Bauer 2007) emphasises the need to examine real as well as nominal rigidity: both will have macroeconomic consequences, and these papers show that real rigidity is quantitatively more important than nominal rigidity.

The macro consequences of wage rigidity have been studied by among others Akerlof, Dickens and Perry (1996), Card and Hyslop (1997), Dickens *et al* (2007) and Elsby (2009). Elsby (2009) has summarised the situation regarding downward nominal wage rigidity (DNWR) as embodying a 'macro-micro puzzle': although there is clear micro evidence of DNWR, it is difficult to find evidence of adverse macroeconomic consequences. However, Goette, Sunde and Bauer (2007) summarise evidence from three countries that downward real wage rigidity (DRWR) does have a detrimental impact on employment. Holden and Wulfsberg's (2008, 2009) findings that the impacts of DNWR and DRWR extend to the industry level confirms that wage rigidity at individual level is not undone by compositional changes that might be expected to result from wage rigidity.

At present the correlates of nominal and real rigidity are not very well understood. Dickens *et al* (2007) investigated many likely institutional candidates, including employment protection legislation, level and coordination of bargaining, corporatism, taxation, replacement ratio, active labour market policies and product market regulation - but the only significant finding was that unions (through density) increased DRWR. Holden and Wulfsberg (2008) find that unions raise DNWR and also that strict employment protection legislation prevents nominal cuts, and Holden and Wulfsberg (2009) show that the same features also reduce the prevalence of real wage cuts.

In contrast to the relative difficulty of identifying institutional or structural correlates of wage rigidity, there has been more success in 'explaining' price rigidity (see Alvarez *et al*, 2006, for a review). The contrast suggests that other factors might be complicating the picture for wage rigidity. It raises the possibility that wage rigidity might have 'behavioural' or 'fairness' foundations which interact with institutional characteristics in a complex way. Of course, this

possibility as long been recognised (for example in the 'equity theory' of Adams 1963). A key feature of theories involving 'behavioural' features is that the utility function is not simply based on absolute income, but also includes relative terms. Workers will experience a reduction in utility if the wage falls below a certain level (the 'reference point'). Loss aversion is an important corollary of relative wage theories: the utility loss arising from downward deviation from the reference point exceeds the utility gain from achieving in excess of the reference point.[4] These theories can explain DNWR if the referent - the comparator when assessing the utility of own current income - is own past nominal income.[5] Real rigidity can also follow, if the worker compares current real income with past real income. The implication that utility depends on nominal or real pay changes has been the subject of very interesting experimental and survey empirical studies (Kahneman and Tversky 1979; Abeler *et al* 2009, Blinder and Choi 1990, Agell and Lundborg 1995 and 2003, Campbell and Kamlani 1997, Bewley 1999), but the number of papers attempting to assess the implications using 'field data' is small (Mas 2006).

This paper contributes by using field data representative of the British population during 1991 to 2007, and using job satisfaction data as a proxy for utility.[6] Despite some initial reservations amongst economists about the use of subjective measures, the use of job satisfaction and other 'happiness' data to investigate economic issues has grown rapidly in recent years. Surveys of this literature can be found in Frey and Stutzer (2002), Layard (2005), and Clark, Frijters and Shields (2008).

The basic idea of this paper is simple: to examine the implications of fairness-based theories of wage rigidity that workers are relatively satisfied with nominal and real rigidity relative to nominal and real cuts. Clark (1999) is one of very few papers to investigate the impact on job satisfaction of pay changes. Using British Household Panel Survey data from 1991 and 1992, Clark finds the effect of pay to be totally dynamic: the negative ('reference') effect of lagged pay is equal to the positive effect of current pay. Clark (1999) uses dummies for nominal and real cuts but does not detect significant non-linearity in the effect of pay growth on happiness, thus finding

[4] Loss aversion refers to people's tendency to strongly prefer avoiding losses to acquiring gains. Under loss aversion, the value function is concave in gains, but convex in losses - and in particular the value function is steeper for losses than for gains (Kahneman and Tversky 1979, Tversky and Kahneman 1991).

[5] It is more common in the literature to use 'external' rather than 'past internal' referents (see Clark, Frijters and Shields 2008 for a review). I investigate the impact of external referents empirically below.

[6] Related work has bypassed utility and instead investigates its consquences, assessing the impact on measures of worker performance and labour supply. For example, Camerer *et al* (1997) and Fehr and Goette (2007) find evidence consistent with a 'target income' model, at least among some workers. Fehr and Goette's work on labour supply of Zurich bicycle couriers found that those expressing significant loss aversion in an experimental context also supplied their labour according to a 'target income' model, while others did not deviate from classical behaviour.

no evidence of loss aversion. Grund and Sliwka (2007) find that job satisfaction is positively related to pay change using German Socio-Economic Panel data from 1994 to 2002. Grund and Sliwka (2007) do not distinguish between nominal and real changes (nominal pay is used but year dummies control for price changes) and nor to they investigate loss aversion. Kawaguchi and Ohtake (2007), using Japanese survey data from 2000, find that workers' satisfaction with annual compensation and their change in morale over the past three years are both reduced by nominal pay freezes (relative to raises), and reduced further by nominal cuts. (The Japanese survey was undertaken during deflation and there is no information about whether the nominal cuts involved real cuts or raises.) Kawaguchi and Ohtake (2007) are not able to investigate the impact of the size of pay change on job satisfaction, which is an important part of the present study. One of the few papers to investigate the impact of the size of pay change 'loss' using field data is Mas (2006). Mas finds that New Jersey police performance declines after an arbitration decision against the union claim and in favour of the (lower) employer offer, and that the performance decline is increasing in the size of the loss.[7]

A further aim of this paper is to improve understanding of pay flexibility. Are there some individuals who are happy to take a pay cut - and, if so, what characteristics or events are responsible? Understanding heterogeneity is an important part of this study: the utility of a given nominal or real pay growth may depend on what is happening to similar others or on the performance of the worker's firm. Furthermore, the worker might well assess pay in relation to other characteristics of the job: disamenities and fringe benefits might be important determinants of the worker's attitude to pay and pay changes.

The paper proceeds as follows. Section 2 sets out a basic modelling framework. Section 3 briefly discusses data and method. In Section 4 results are presented. Flexibility is addressed prior to investigating rigidity. Section 5 concludes.

2 The relationship between job satisfaction and pay growth

In this section I set out a simple model of the effect of pay growth on job satisfaction, using the general framework typically used to capture reference-dependent utility (Clark and Oswald 1996; Clark 1999; Clark, Frijters and Shields 2008). Utility is assumed to be increasing in consumption, which is a function of real labour income. If comparisons matter, utility also

[7] The average difference between offer and claim was about 1.5% (p.808). It is worth emphasising that in Mas (2006) the relativity that is inducing changes in effort is deviation of pay raise from expectations (embodied in the union claim).

depends on reference income. In common with the satisfaction literature, job satisfaction is assumed to reflect utility from working, which forms a sub-utility function:

$$U_{it} = u\left(y_{it}, y^*_{it}, h_{it}, \mathbf{X}_{it}\right) \qquad u_1 > 0,\ u_2 < 0,\ u_3 < 0$$

where y_{it} is real pay for individual i at time t, y^*_{it} is the individual's reference point for real pay, h_{it} are the individual's working hours at t, and $\mathbf{X}_{it}$ is a set of individual-, job-, region-, industry-, occupation- and time-varying characteristics.

If an individual's past labour income is a salient reference point, their utility will also be increasing with changes in real labour income:

$$U_{it} = u\left(y_{it}, \Delta y_{it}, h_{it}, h_{it-1}, \mathbf{X}_{it}\right)$$

where Δy_{it} is the individual's real pay growth between $t-1$ and t, and last period's working hours h_{it-1} may also influence current utility. The phenomenon known variously in the literature as 'habituation' or 'adaptation' to a given level of income would suggest that changes in income should matter (this has also been termed the 'hedonic treadmill' - see Clark, Frijters and Shields 2008 for a review).

If there is loss aversion, there will be a kink in the relationship between utility and changes in real labour income. *A priori* it would be difficult to ascertain whether such a kink should occur at real or nominal zero. If there is money illusion, of course, one might expect the kink to occur at nominal zero. Although Shafir, Diamond and Tversky (1997) famously present a variety of survey and experimental evidence that money illusion exists under certain circumstances, other research has found less or no evidence. Fehr and Tyran's (2001) experiments unearthed only a "small amount" of money illusion, and Boes, Lipp and Winkelmann (2007) use German Socio-Economic Panel data to show that income satisfaction responds equally, but in opposite directions, to changes in nominal income and prices, illustrating an absence of money illusion.

The response of happiness to pay can be used to distinguish between theories explaining why pay might be rigid. The most relevant are probably efficiency wage theories. Akerlof and Yellen (1990) note that their fairness model might apply to either real or nominal pay. Bewley's (1999) 'morale' theory emphasised that nominal cuts might 'insult' workers, leading to a direct impact on utility. The counterpart to dissatisfaction with nominal cuts is that people with nominally rigid pay might be unusually happy as they have avoided a nominal cut.

The definition of morale is not straightforward. According to Bewley (1999, 2002) it has three components: identification with the firm's objectives; belief in positive reciprocation (gift exchange); and "a mood conducive to good work" (2002, p.5). Although morale is not identical

to job satisfaction, job satisfaction will undoubtedly reflect morale, and is likely to be related to all three of Bewley's components. (Satisfaction with pay is a sub-domain of overall job satisfaction and is likely to reflect the perceived fairness of pay but might be less correlated with morale as it ignores other aspects of the job.) "Managers are concerned about morale because of its impact on labor turnover, on recruitment of new employees, and on productivity" (Bewley, 2002, p.5). Unhappy workers will quit if they can. Unhappy workers will not be a good advertisement or good recruiters for the firm. In terms of productivity, Bewley suggests that morale has little impact on the speed of production, but instead affects workers willingness to make sacrifices, work well without supervision, communicate, and so on.

What about the relationship between pay raises and morale? Bewley (2002) records that "employers say they do not see much connection between effort or morale and wage levels; productivity and morale do not increase with pay levels, though they can be hurt by pay reductions or disappointingly small raises. Even generous pay increases do not increase morale or productivity, because workers quickly get used to increases and grow to believe they have a right to them" (p.8). In this, Bewley's morale theory differs from the fairness theory of Akerlof and Yellen (1990) and Solow's (1979) efficiency wage theory, which do propose a link between the wage level and morale.

In the following sections I hope to shed light on the following questions: Do workers really resist cuts, or do they get something else to compensate? Are (some) workers unhappy when their pay is cut (and, if so, which workers)? Are workers unusually happy receiving rigid pay, knowing that they have avoided a 'warranted' pay cut? Are patterns of happiness with pay change correlated with observable features of jobs and the wider economy?

3 Data and method

Data are drawn from the first 17 waves of the British Household Panel Survey. Weighting ensures that the sample remains representative of Britain (below the Caledonian Canal in Scotland). The BHPS contains quite rich data on job satisfaction, pay, individual and job characteristics that are described further below. These data are supplemented by measures of regional and industry performance. Data are taken from the UK Office for National Statistics on regional unemployment, regional gross value added per head, industrial output at the 2-digit Standard Industrial Classification level and aggregate prices. The Data Appendix gives further

details.

As suggested above, job satisfaction should reflect morale, and this correlation is likely to be highest with overall job satisfaction. An individual reports their job satisfaction by choosing one of seven categories, ranging from "not at all" to "completely" satisfied. As is well known, most people regard themselves as reasonably 'happy'. For the waves examined here (1992 on, since the first wave is lost through calculation of pay growth) nearly sixty percent of respondents rate themselves "completely" or "mostly satisfied" with their jobs overall.[8]

Bewley (2002) has a careful discussion of the important issue of what measure of pay is relevant to workers and firms in terms of fairness. He notes that for the firm what matters is average hourly nominal labour cost per job, while employee welfare relates to total nominal compensation per worker. But when it comes to the wage measure that is the focus of judgements of fairness, Bewley's extensive field experience leads him to conclude that it is "nominal compensation for an employee with a given job tenure and continuing in the same position with the same employer under fixed working conditions" (hourly rate and benefits for hourly-paid and total compensation for salaried employees) (Bewley, 2002, p.3). Measures of total monthly pay and basic hourly wage rate for stayers go some way to embodying this definition. Bewley goes on to say that "in order to adhere even more closely to the sense of fairness prevailing in business, it might be advisable to include only base pay and exclude variable components, such as bonuses" (p.3). The basic hourly pay rate captures this notion of the wage, for hourly-paid workers. Following the precedent of the International Wage Flexibility Network (Dickens *et al* 2007) I trim the sample to remove large pay changes likely due to error. Earnings growth below -85% and above 100% and hourly wage rate growth below -35% and above 65% are trimmed. As is commonly found, such trimming actually makes no difference to results.

BHPS data show that, on average over 1992 to 2007, 8.4% of pay changes involve nominal freezes and 24.5% involve nominal cuts (Figure A1 in Appendix 2 shows the evolution of rigidity and cuts over the sample period). 9.6% of pay changes feature nominal raises that do not raise pay in real terms. The remaining 57.5% enjoy real raises. The extent of pay cuts - and cuts in basic hourly wage rate - may appear surprising. 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

[8] A histogram of job satisfaction is shown in Figure A2 in Appendix 2. Satisfaction with pay tends to be lower than overall job satisfaction, which may indicate compensating differentials for non-pecuniary aspects of the job, which are examined below.

employer could legally use evidence that there are genuine business or economic reasons why the cut has to be forced upon the employer.

Controls include standard variables in satisfaction equations and others less commonly included. These variables are included to capture systematic and non-job-related 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 experiment with quadratics in tenure and experience (with dummies for missing values), Job-related controls include nine occupation dummies and seventeen industry dummies. Other job characteristics include trade union membership, part-time job dummy, temporary/casual/seasonal job dummy, manager and supervisor dummies, travel-to-work time in hours, dummy for evening and night working, dummy for working elsewhere than employer's premises, nine dummies for workplace employment size, and dummies for employer bonus and pension schemes and being on an incremental pay scale.

Most of the empirical work in this paper uses the ordered probit model often used in the life- and job-satisfaction literature to allow for the categorical nature of the dependent variable. The literature typically simply reports the coefficients of such models. The estimated coefficient should reflect the impact on the latent continuous propensity to be satisfied. In most of this paper I prefer to report marginal effects, and I do so for the top two categories: mostly or completely satisfied. For dummy variables (which will be the major focus), marginal effects are interpreted as the effect on the average individual's job satisfaction of moving into the relevant satisfaction category. The size and sign of marginal effects depend on estimated cut-points differentiating each of the (seven) categories of the observed ordinal variable, as well as coefficients (the cut-points are not shown as by themselves they are difficult to interpret). The relative size of marginal effects for categories of job satisfaction can be inferred from the size and sign of the coefficient, but the cut-point estimate is also needed to infer precisely where in the range of job satisfaction categories the marginal effect switches sign. The advantage of marginal effects, in addition to their interpretability, is their comparability across sub-samples, which will be important in what follows. In contrast, because cut-points can vary across samples, an ordered probit coefficient is not necessarily comparable across subsamples.

Due to the complexity of many of the ordered models, it is rare for allowance to be made for selection. I do so here using the Probit-augmented OLS method devised by van Praag and Ferrer-i-Carbonell (2008). POLS transforms the categorical satisfaction variable into a continuous variable lying anywhere on the real line. POLS is therefore an alternative approximation of (the natural log of) the latent tendency to be satisfied with the job. Under POLS the sample probability of 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. For each sample proportion, the POLS dependent variable - the approximated latent tendency - takes the value of the conditional mean of the interval into which the sample proportion falls. As discussed in van Praag and Ferrer-i-Carbonnell (2008, especially Chapter 2), POLS and ordered probit give very similar results (subject to a scaling factor - see also Stewart 1983). POLS is far less time-consuming to implement, and enables richer models - such as those allowing for selection - to be easily estimated.

4 Results

I start by presenting results showing that pay growth impacts on job satisfaction. In subsequent sections I turn to the main focus of the paper: non-linearity in that relationship.

4.1 Job satisfaction, pay, pay growth, and hours

Ordered probit coefficients on pay variables are shown in Table 1. These result from estimation of a job satisfaction regression typical in the literature: controls include usual weekly hours of work (including overtime), age, gender, ethnicity, children, health problems, non-labour income, region and year. I deliberately exclude job-related variables from the regression at this stage in order to obtain estimates of the impact of pay that may include compensation for non-pecuniary features of the job. These and other factors will be investigated in detail below.

The effect of log real weekly usual earnings, y, is positively related to overall job satisfaction. When real pay growth, Δy, is included it appears to have a positive impact even controlling for current real pay and lagged hours (column (2)). Real raises make people happier, and the larger the raise the greater the happiness improvement - results consistent with 'adaptation' and past income levels acting as a referent. Column (3), where current and lagged real pay levels are

included rather than current level and pay growth, shows this clearly. Previously results for life satisfaction suggest that adaptation removes around two thirds of the benefit of any income rise (Frey and Stutzer, 2002; Layard, 2005). Results in Table 1 indicate that adaptation to earnings may be substantially faster (although not as fast as found by Clark 1999, where job satisfaction was found to depend only on pay growth and not on pay level). Both current and lagged log hours negatively affect job satisfaction. Interpreting the coefficients differently, these results show a positive impact from hours growth (coefficient +0.161) and a negative impact from current hours (-0.362).

The focus of this paper is on pay growth, so it is worthwhile taking a little time to consider the best measure - although it turns out that conclusions are not altered by the precise measure chosen. The pay variable in columns (1) to (3) is usual gross weekly earnings. The use of weekly, rather than hourly, earnings is motivated by the facts that hours data are measured with substantial error (Bound, Brown and Mathiowetz 2001), and results are typically qualitatively similar but less well-determined when pay divided by hours is used instead. To ensure validity of total earnings, column (4) replaces total real pay growth with hourly real pay growth, but retains the log of total real pay as the 'income' variable in the regression and continues to control for both current and lagged log hours. Given I control for the change in hours, it is not surprising that real hourly pay growth is found to impact positively on job satisfaction, nor that results are not changed if the log of real hourly pay is used instead of log real total pay.

From 1999 there is information on basic hourly wage rate for hourly-paid workers. For these workers, this variable comes close to the ideal of capturing the wage that is the focus of the employment contract (Dickens *et al* 2007). Column (5) focuses on the impact of wage rate growth, which can be seen to have a positive relationship with job satisfaction. Interestingly, for hourly-paid workers, the effect of pay appears dynamic - there is no impact from either the (log) level of real earnings (as shown) or log real hourly wage rate when this is used instead. Thus the effect disappears after a year (consistent with very rapid adaptation): the negative impact of lagged wage rate exactly counterbalances the positive effect of current wage rate. I note, though, that the size of the coefficient on the log of real earnings is similar to that when other pay variables are used; and if the full unweighted sample is used, the log level of earnings reaches significance at the 6% level (and other coefficients are unchanged).

Relationships between job satisfaction and control variables are very similar to those found previously. There is a U-shaped relationship with age (Clark and Oswald 1996), higher satisfac-

tion among women (Clark 1997), lower satisfaction among better-educated and lower satisfaction among those with poor health (Clark and Oswald 1996). There is a U-shaped relationship with job tenure (Theodossiou and Zangelidis 2006) - but the inclusion of tenure does not alter the impact of pay or pay growth. I experimented with including a quadratic in actual job experience, but its effect was no different to that of age (potential experience).

4.2 Nonlinearity in the job satisfaction - pay growth relationship

Results so far indicate that as long as current and lagged hours are controlled for, it does not matter whether total or hourly pay is used in the regression. The main point of interest for this paper is that pay growth impacts positively on job satisfaction. Of course, these initial results are based on a pooled sample including both men and women, and full-time and part-time workers. There may be important differences across these subsamples that I investigate below. In this section I turn to the main focus of this paper: whether the effect of pay growth is nonlinear. Since Table 1 confirmed that there are no differences between results for hourly earnings and total earnings if I control for hours, I focus on total pay and hourly basic wage rate. (The results for hours are not shown as they do not differ from those reported in Table 1.)

To investigate nonlinearity in the effect of pay growth, Table 2 replaces real pay growth with dummy variables covering pay changes of various sizes. To summarise the three key hypotheses investigated: Loss aversion should be reflected in a more positive impact of pay growth below nominal (or real) zero (a steeper relationship for pay cuts). An 'insult' effect from nominal cuts might show up as a lower level of job satisfaction below nominal zero. Nominal rigidity might induce greater job satisfaction as workers are relieved to have avoided warranted cuts.

In the first column of Table 2 the effects are relative to a rough approximation of unchanged real pay - this is measured as a band covering the inflation rate plus or minus 0.5 percent inclusive (1,740 observations in the sample). This is the base case for a series of dummies covering (real) pay changes of various sizes, with an extra dummy for nominal pay rigidity (real growth equal to minus inflation, which is excluded from both other relevant dummies; inflation averages 2.7% in the sample). As expected, real raises make people happier, and the larger the raise the greater the happiness improvement - although the positive impact is only significant for real pay raises of around 5 percent and above. But there are two most striking findings. Contrary to expectations, job satisfaction is not generally significantly worsened by

either nominal or real cuts. The other striking finding is the exception to this general result: those who have nominally rigid pay actually appear more satisfied with their job than those whose nominal pay raise only deviates from inflation by 0.5 percent - and so do those with small nominal cuts (equivalent to real cuts of between 2 and 5 percent).

Column (2) elucidates these results by including a set of dummies defined in terms of nominal pay growth, with a base case of nominal freezes (2,659 observations in the sample). Consistent with freezes being relatively good compared to nominal cuts, nominal cuts greater than 2 percent have negative coefficients - but only cuts between 10 and 20 percent are significant (and only at the 10% level). Raises improve job satisfaction relative to nominal rigidity if they are bigger than 20 percent in nominal terms, and small raises between 2 and 5 percent attract negative coefficients - significant in the case of +3% and +4%. Sample inflation varied between 0.7% and 4.8%, so this is consistent with the picture from Column (1) that individuals whose pay is (close to) rigid in real terms are relatively dissatisfied with their job.

Results for earnings are consistent with concavity in the pay growth - job satisfaction relationship for real raises, but are clearly inconsistent with convexity for cuts. Indeed, the relationship for nominal cuts apparent from these initial regressions can best be described as surprisingly 'flat' over much of the range. The question why pay cuts are not more painful (in a job satisfaction sense) is addressed below. Proponents of nominal rigidity should not despair, however, as results overall show high job satisfaction from freezes: a real raise of around 5% is needed before people are happier than they are with freezes. This is in part related to the second surprising finding: that individuals who maintain the real value of their earnings are relatively dissatisfied. I also investigate this further below.

The lack of evidence of loss aversion is completely contrary to Mas (2006), who demonstrated that police performance was decreasing in size of 'losses' represented by deviation of arbitration award below claim. However, there is a suggestion here of what Mas terms the 'Vince Lombardi effect' (after a professional football coach who said that "winning isn't everything, its the only thing" - but might more accurately be called the 'insult effect' (following Bewley, 1999). This takes the form of a 'category effect': nominal cuts are worse, no matter how trivially small they are. This discontinuous drop in satisfaction with nominal cuts holds only for small and medium cuts. Higher satisfaction with larger losses is surprising - but not unprecedented: an increase in satisfaction at the lowest quintile of pay growth was also noted by Clark (1999). Possible explanations are discussed below.

The lack of a steeper relationship below zero was also uncovered by Vendrik and Woltjer (2006) using German Socio-Economic Panel data. Vendrik and Woltjer (2006) model the relationship between job satisfaction and pay change using two separate power functions above and below zero. The use of power functions, rather than the usual logarithmic form, allows them to test convexity below zero (a log form implies concavity below as well as above zero). They reject convexity and indeed find stronger concavity below than above zero, which is at least qualitatively similar to the picture from British data.

The fine print of the hourly basic wage rate growth - job satisfaction relationship is investigated in columns (3) and (4). As for earnings, column (3) shows dummies with a base case of approximate real rigidity and defined in terms of real growth rates, whereas column (4) has a base of nominal freezes and dummies defined in terms of nominal growth rates. As for earnings, there is greater, but diminishing, satisfaction with real raises. Unlike for earnings, the size of the coefficients is consistent with lower satisfaction as wage cuts become more negative, but nothing is significant. And there is no indication that wage freezes make hourly-paid workers unusually satisfied with their job.

A very important caveat to bear in mind is that Table 2 shows pay effects 'uncompensated' by any job-related factors, or changes in such, that might counteract or compensate for (i.e. be inversely correlated with) pay changes. The inclusion of controls for potential compensating factors is very likely to lead to a different picture. A flat relationship between job satisfaction and pay cuts might be consistent with compensating changes taking place elsewhere in the job - but raises clearly are not solely compensating for adverse non-pecuniary changes.

It is clear that the relationship between pay growth and job satisfaction is not easily summarised, due to intricate variation around key points in the pay growth distribution. From now on I will adopt a largely graphical presentation which will, I hope, be relatively clear and digestible. In the graphs I will present marginal effects from regressions involving the dummies in column (2) of Table 2 - that is, with base case nominal zero.

I use marginal effects because I wish to compare results across different samples and different control variables. Seven marginal effects are obtained from the seven-category job satisfaction variable, and I will present the sum of the marginal effects for the top two categories - thus capturing the impact of a particular pay change on the probability that the average individual is mostly or completely satisfied with their job. (In practice marginal effects are very similar, subject to a scaling factor, compared to coefficients.)

Figure 1 disaggregates by gender and part-time status for earnings and hourly wage rate (the sample of part-time men is too small to generate reliable results and is omitted). Full-time women and full-time men both feature the whole-sample concave relationship between real earnings raises and job satisfaction. For hourly-paid workers the pay growth - satisfaction relationship shows no sign of concavity, being linear or even convex. The dip in satisfaction around pay rigid in real terms seems to apply to all workers. For all except hourly-paid men there is a rise in satisfaction around nominal freezes and lower satisfaction with smaller nominal cuts. Satisfaction for these workers is relatively high for nominal raises of between 0% and 1% (exclusive); some might find this suggestive of these being workers with a warranted nominal cut whose firms thought it in their interest to award an (epsilon-)small raise rather than simply freeze pay. Hourly paid men contrast with other groups, featuring lower satisfaction with nominal freezes than with nominal cuts. For many groups there is an insignificant or flat relationship with larger cuts; the exceptions are part-time women's earnings and full-time women's hourly wages, both of which show some strong dissatisfaction with nominal cuts.

What about loss aversion? Two very different readings of these data are possible. If one takes, piecewise, the relationships above real zero and for small cuts below nominal zero (to -20% for earnings and -10% for wages), the relationship below nominal zero is substantially steeper than that above real zero. This contrast is fundamental to loss aversion - but the picture described ignores satisfaction shifts between real zero and nominal zero, and perhaps more crucially ignores large cuts. An alternative reading would look at the whole relationship and conclude, because typically the picture is flatter below zero than above, that there is no evidence of loss aversion. To decide between these it would be useful to know how reliable are the data on large cuts - an issue investigated further below.

There are other possible explanations for the relationship between satisfaction and nominal pay cuts. I have suggested that last year's pay will only represent a reference point under certain circumstances. Perhaps, for everyone receiving pay cuts, it is simply not a reference point. Perhaps 'similar others' are receiving nominal cuts, and perhaps the individual's firm's performance is very poor. I investigate both of these possibilities below.

Perhaps nominal cuts lead to turnover concentrated among most able workers. This is certainly feared by employers (Bewley, 1999). If remaining lower-ability workers are relatively satisfied with the reduced nominal income (as they have lower expectations or reference points) this could explain the findings. 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. Below I investigate the relationship between rigidity and turnover further by splitting the sample between union and non-union workers. Holden and Wulfsberg (2008) argue that it would be difficult for a firm facing a union contract to replace workers whose wages were held up by rigidity. As noted by Holden (1994), this is a key area where 'institutional' and 'fairness' effects might reinforce each other.

Can further investigation:

- confirm or refute that the relatively high satisfaction with nominal freezes (and small nominal raises lower than 1%) is due to these being experienced by workers warranting nominal cuts?
- help explain why real rigidity is associated with dissatisfaction with the job?
- validate loss aversion?

To answer these questions I:

1. Control for potential compensating changes within the job. These could affect the pay growth - satisfaction relationship at all levels.
2. Try to eliminate measurement error in pay growth. If spurious cuts are eliminated, is the pay growth - satisfaction relationship more significantly and steeply sloped below zero?
3. Investigate the impact of unions. Union membership has previously been found to have a negative relationship with job satisfaction (Freeman 1978). Does this effect work in part through the impact of unions on pay growth? Perhaps unionised workers expect higher pay raises than other workers.
4. Control for comparisons with external reference groups - including 'similar others' and the firm. Workers might accept cuts if 'everyone else like them' is taking cuts, or if their industry is doing badly.

In discussing issues 1 and 2 - compensating changes in the job and measurement error in pay - I will primarily focus on whether they change the surprising results concerning pay cuts. Unions and comparisons (issues 3 and 4) might impact on both pay cuts and rigidity.

4.3 Sample selection

Prior to dealing with these, the issue of selection needs to be discussed. Stayers might form a selected sample of those employed last period. As discussed above, one response to pay cuts might be quits - particularly by most able workers. Alternatively, the firm might need to reduce employment if the wage bill is held too high through downward rigidity. Furthermore, it is commonly found that workers who leave their job include a substantial subset who are very frequent job changers, and who alternate between employment and non-employment (see Stewart 2009, for example).

To find out if selection is affecting results I estimate sample selection models for the male and female subsamples.[9] To operationalise the sample selection model I use POLS - 'probit-augmented OLS' - to estimate job satisfaction equations (see Section 3).

Selection into the estimating sample is captured by a probit model, with the dichotomous dependent variable taking value 1 if the worker was employed last period and this, and did not change employer, and the job satisfaction equation could be estimated (specified as underlying Figure 1). The selection variable takes value 0 if the worker was employed last period but was either with a different employer or not employed this period, and the individual falls into the relevant subsample (male or female). The model is estimated by maximum likelihood. Identification is ensured with the inclusion of additional variables in the selection probits, where the identifying regressors are jointly significant in the selection equation (and, in a separate test, are found jointly insignificant when included in the job satisfaction equation).[10]

For both males and females, the hypothesis of sample selection as measured by the Wald test of independence of selection and job satisfaction equations cannot be rejected: the correlation between the error terms of the two equations is significantly different from zero only at the 50% level for males and the 48% level for females. Thus sample selection does not appear to influence job satisfaction equation results. This result is possibly due to heterogeneity among non-stayers: non-stayers include a substantial proportion of voluntary movers as well as involuntary or 'disadvantaged' job leavers, so effectively on average might not differ from stayers.

[9] A split was not made between part- and full-time here in part because samples for job satisfaction equations do not exclude part-time/full-time switchers. The inclusion of current and lagged hours is intended to control for such changes (recall that in the dataset part-time status is defined by working less than 30 hours per week).

[10] The identifying variables are the regional unemployment rate and its first difference, regional gross value added per capita, and the RPI inflation rate. For females, dummies for house tenure (private renting and renting from local authority, with base case home ownership) are additionally included. For each subsample a test of joint exclusion of these regressors in the job satisfaction equation cannot be rejected (with significance level 32% or larger) and their joint insignificance in the selection probit can be rejected at the 1% level or smaller.

4.4 Compensating differentials, disamenities and fringe benefits

Recall the sub-utility function introduced in Section 2. Among the features of the job and work environment that impact on an individual's utility, of particular interest will be features that I gather under the heading 'job amenities'. The literature on compensating differentials commonly investigates whether pay compensates for disamenities or whether people are prepared to pay for amenities. In a similar way I will investigate whether changes in job amenities are compensated by changes in pay. With compensation, there should be a trade-off between job amenities (and changes in them) (denoted $\mathbf{npb}_{it}$) and pay (and pay growth), such that utility is held constant (at $\overline{U}$). In the absence of barriers to worker mobility between jobs and informational imperfections, jobs should pay differentials according to their pleasant or unpleasant characteristics.

$$U_{it} = u\left(y_{it}, \Delta y_{it}, h_{it}, h_{it-1}, \mathbf{X}_{it}, \mathbf{npb}_{it}\right) = \overline{U}$$

The study of compensating differentials has generally focused on pay levels. If pay is higher where there is a disamenity, and if job satisfaction adequately captures utility, the pay effect on job satisfaction will be biased towards zero if the disamenity is excluded from the regression. In fact, under perfect compensation, anything (other than the disamenity) that enters the utility function could in principle be altered to compensate for the disamenity. It is entirely possible that, if pay growth does affect individuals' utility, it is used as a compensator. The 'industrial relations' literature would not find the use of variations in pay growth to compensate for other changes in the job unusual. Negotiated pay settlements sometimes involve an additional element to compensate for changes in work organisation, for example (Millward, Forth and Bryson 2000). However, if it is not pay growth itself but pay that compensates for disamenities, it will be *changes* in disamenity that will be compensated by pay growth. (If a disamenity were found to 'compensate' in job satisfaction terms for a particular pay growth rate, it would effectively mean that people paid more would be willing to pay a larger amount to reduce the disamenity.)

The set of job characteristics I include under the heading 'amenities' differs in some respects from that commonly studied. Travel-to-work time and evening or night work have previously been found important (Stutzer and Frey, 2008, and Hamermesh, 1999, respectively). I also include other factors that seem likely to be compensated by differences in earnings and earnings growth: working elsewhere than employer's premises and provision of fringe benefits in the form of employer pension scheme. I also allow for compensation if a job is redefined in terms of temporary or permanent status.

Do changes in conditions compensate workers in utility terms for pay growth differences? If such changes are negatively correlated with - compensate for - pay growth, the overall effect of pay growth on job satisfaction will tend to zero unless these (changes in) job characteristics are controlled for. Econometrically: there may be omitted variable bias. Thus the generally insignificant effect of nominal cuts evident in Figure 1 could simply be due to the absence of controls for changes in 'job amenities'.

The top panel of Table 3 presents the pairwise correlation of changes of the job feature and pay changes of various sizes, focusing on large pay cuts for obvious reasons.[11] (The sample is all stayers for whom job satisfaction and pay growth are observed.) The lower panel of Table 3 shows the proportion of those experiencing a particular pay change who also experience a potentially compensating change in job characteristic.

Both panels of Table 3 show that potentially-compensating changes in job characteristic are more common among those experiencing large pay cuts. A reduction in travel-to-work time is more common the larger the cut experienced, and although the average reduction in travel time is not large (only 1 minute for those with the largest cut) it contrasts with the (small) rise for those with raises and smallish cuts. (These averages are taken over a very wide range of changes in travel-to-work time.) A change to daytime working from evening or night work is more common among those with large cuts; workers experiencing raises or smaller cuts tend to change their working arrangements in the opposite direction. The introduction of a pension scheme occurs more often among those with large cuts and is positively correlated with large cuts but negatively so with smaller cuts and raises. A change in job definition from temporary (or casual) to permanent is positively correlated with the largest pay cuts, in contrast to the negative correlation with raises and smallish cuts. Finally, changes in work location appear to be possible compensating changes for those with the largest cuts, but not for other workers.

In Figure 2 the solid line shows the satisfaction effect of pay changes including the effects of 'job amenities' and changes in them. It should be compared to the dotted line, which shows satisfaction effects excluding 'job amenities'. The inclusion of 'job amenities' and changes in them does alter the magnitude of the impact of large cuts in the expected direction, but the effect of these remains insignificantly different from that of nominal freezes.

Large pay changes might well be related to other features of the job, such as the presence (or introduction or removal of) a bonus scheme, managerial or supervisory responsibility. There

[11] Results of pairwise correlations are very similar to those from simple probits in which the dummy variable for pay cut in a particular range is regressed against all potentially compensating job changes.

are also factors that make large cuts unlikely, such as being on an incremental pay scale - and changes in such payment systems might also be correlated with relatively large pay changes. The dashed line in Figure 2 shows the impact of including these other job characteristics, and also controls for job size, industry and occupation, which will capture other differences in job amenities (including injury and death risk). The inclusion of these other job-related characteristics does not lead to major changes in the job satisfaction-pay growth relationship.

It is worth noting that the literature commonly finds no evidence that job disamenities raise wages (see for example Altonji and Usui 2007; Abraham and Lluis 2008; Boeckerman and Ilmakunnas 2006; Lehrer and Sousa Pereira 2007). In the absence of other controls I find a significant relationship between pay *growth* and *change* in job (dis)amenity, but in the full job satisfaction regression the common finding in the literature of insignificance is generally replicated: most changes in amenity are insignificant (the exceptions being change to employer's workplace, which significantly raises job satisfaction for men at the 10% level, and change to daytime working and the introduction of an employer pension scheme, which both significantly raise female part-time workers' job satisfaction at the 5% level). However, I find most of the *levels* of job disamenity significantly related to job satisfaction in the expected direction: travel-to-work time, temporary job and evening/night work reduce job satisfaction (though the last is insignificant for female full-timers and the temporary/permanent distinction has no significant impact on male job satisfaction). Finally, although changes in workplace location significantly affect job satisfaction, the workplace location itself does not.

4.5 Measurement error in pay growth

Does the unexpected (lack of) relationship between pay cuts and job satisfaction reflect measurement error in pay? Most importantly, does the slope of the pay growth-satisfaction relationship below nominal zero reflect the reality, as suggested by Akerlof, Dickens and Perry (1996), that many nominal cuts are spurious, being due to measurement error in pay levels? Alternatively, if cuts remain when data are 'cleaned' of error, will the expected significantly detrimental effect of cuts on satisfaction be found?

Researchers into nominal wage rigidity have typically been very aware of measurement error in pay growth. It is well known that measurement error in pay translates into spurious pay changes. Akerlof, Dickens and Perry's (1996) claim that many wage cuts are indeed spurious is based on comparing PSID data with their own telephone survey, union settlements, and

employer-reported pay. A series of papers (Altonji and Devereux 2000; Fehr and Goette 2005) have estimated models of wage rigidity making some distributional assumption about measurement error in pay, all finding that cuts are overstated and rigidity understated in the data. Research within the International Wage Flexibility Network summarised in Goette, Sunde and Bauer (2007) finds, though, that the earlier work that does not allow for real wage rigidity overestimates the impact of measurement error on nominal wage rigidity: once real wage rigidity is allowed for, far less downward nominal rigidity is found. Statistical comparisons tend to find less impact of measurement error than econometric models (which typically need to make identifying distributional assumptions that may not always hold). Dickens *et al* (2007) concluded, by comparing several different types of data source involving 16 countries, that differences in rigidity and flexibility across countries were not due to measurement error. Smith (2000) reports no higher nominal rigidity and only slightly less downward flexibility apparent in a relatively measurement-error-free subsample of BHPS data where workers' pay slips were examined at the time pay was reported.

There are several ways to investigate the impact of measurement error. Recall that I have already removed large pay changes likely due to error by trimming pay growth (see Section 3). In what follows I examine measurement error by looking at specific subsamples: where the interviewer has confirmed documentary evidence on pay, where recent computer-assisted interviewing should have helped clean data of pay change errors, and a sample split according to time between interviews.

4.5.1 Documentary evidence on pay

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 response on pay was given. The questionnaire emphasises "RESPONDENT TO CHECK PAY SLIP IF POSSIBLE", and whether the respondent checked the latest or an earlier pay slip is recorded.[12] The pay slip was examined in both relevant years by just under 30% of the sample.

[12] For hourly-paid workers, there is also information about whether the wage rate stated is "exact" or "estimated". The precise wording 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".

4.5.2 Computer-assisted reduction in pay change errors

The introduction of computer-assisted 'Dependent Interviewing' (DI) into the BHPS survey process in 2006 presents another opportunity to investigate measurement error. An income check question is now triggered when the computer-aided comparison of last year's hourly pay with this year's indicates a nominal cut bigger than 30% or nominal raise bigger than 40% (for stayers, or 60% for movers). On the basis of previous waves, Jackle, Lynn and Uhrig (2007) report that the income check question would have been asked to around 10% of respondents. The income 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. Jackle, 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. Nevertheless, the process should have cleaned large pay changes relative to previous waves, so it is useful to compare the relatively error-free data in waves 16 and 17 with previous. Does the DI error detection change the relationship between job satisfaction and pay growth at large values?

4.5.3 Time between interviews

Further error is introduced by interviews taking place at varying time intervals not exactly coinciding with settlement dates. Many wages change at annual frequency, and on a set date each year. Interviews more than twelve months apart might capture two wage changes, while interviews at less than annual frequency might miss a pay settlement. Whether time between interviews makes a difference can be investigated by allowing the effect of wage change dummies to vary depending on whether the inter-interview period is more or less than one year.

Figure 3 investigates each of the above three aspects of potential measurement error in pay.

The graphs use the whole sample as there is little *a priori* reason to believe that measurement error is determined by gender or part-time status. In the top two graphs the subsample that should be (relatively) free of measurement error is indicated by the solid line. The only significant difference appears to be greater - and seemingly more concave - effect on satisfaction of large raises. Payslip-checked and DI-checked data both do not feature any significant relationship between nominal pay cuts and job satisfaction. Sample size might be a factor, as large pay cuts are less frequent among those whose pay slips are checked: 6.4% of the unchecked subsample experience nominal cuts bigger than 10% in absolute terms compared with 10.8% of the unchecked subsample. Nevertheless, the size of each pay cut category should be sufficient to generate reasonably accurate estimates.

The counterpart measurement-error-affected subsamples in the top two graphs both show significantly lower satisfaction with nominal cuts relative to nominal freezes, but increasing size of cut does not lower satisfaction. (This picture is consistent with the 'insult' effect of cuts discussed above.)

The bottom panel of Figure 3 splits the sample according to time between interviews. 55% of the sample have under 365 days between interviews. Their job satisfaction-pay growth relationship is shown by the solid line. The dashed line shows the relationship for those whose interviews were 365 days apart or more. The average time between interviews is 340 days for those with less than a year between interviews and 387 days for those with at least a year. Many individuals in the former (solid-line) sample will have had a pay change, but some will not. Most individuals in the latter (dashed-line) sample will have had a pay change, and some may have had two.

Very small nominal cuts do not appear to worry individuals with shorter time between interviews: the solid line shows a substantial 'spike' for pay cuts between -1% and -2% that is difficult to rationalise unless it this pay change captures actual nominal rigidity with a small reporting error. The contrast whereby those interviewed at less than annual frequency are dissatisfied with nominal cuts, but those interviewed after a longer interval are not, is difficult to explain. (There is no significant difference in the proportions taking large cuts for subsamples split by time between interviews.)

Perhaps the most relevant conclusion to draw from this investigation of measurement error is that it does not appear to be responsible for a lack of dissatisfaction with pay cuts. It also has little impact on the features around nominal and real rigidity noted above: the dissatisfaction

when nominal pay growth is approximately equal to the inflation rate, and satisfaction with pay growth just above nominal zero (both relative to nominal freezes) remain quite robust, as does relative dissatisfaction with smallish nominal cuts.

4.6 Unions

In a general reference-dependent model, utility from a given pay raise should depend on workers' reference points, which will determine their expectations of the warranted raise. Union members might well therefore have very different expectations, or referents, to non-union workers. As pointed out by Dickens *et al* (2007), unions provide 'public good' services to their members in terms of distributing the cost of gaining information about price changes - and possibly also pay changes of external comparators, be these other worker groups or industry performance measures. The utility derived from a given differential between current and lagged pay might therefore depend on whether pay is negotiated by a union or not.[13] Recent empirical work has confirmed that unions enhance real wage rigidity (Goette, Sunde and Bauer 2007). Dickens *et al* (2007) report that an increase in real rigidity due to union density was the only institutional effect among many examined that applied across all countries at all times. Other British data from the Workplace Employment Relations Survey (Millward, Forth and Bryson 2000) indicate that bargained settlements are more likely to feature nominal raises but also more likely to be lower than non-union settlements. (This last finding is consistent with the declining union wage premium that has been uncovered by recent research using British data - see Blanchflower and Bryson 2004 and Arulampalam, Manquilef and Smith 2009.)

It is commonly found that union members express lower satisfaction with a given pay *level* than non-union workers.[14] In Figure 4 I investigate differences in satisfaction with pay growth between union members and non-members (first column) and between those whose workplace is covered by a union bargaining agreement and uncovered workers (second column). In general, unions do appear to reduce the effect of a given pay change on job satisfaction. For full-time men, non-union workers are significantly happier with both pay raises and cuts (although the relationship with cuts is upward-sloping despite controls for job characteristics). Male union members and covered men demonstrate particular dissatisfaction with nominal pay cuts. For

[13] It is also possible, though, that differences between union and non-union workers refect endogeneity of union membership. For example, more (unobservedly) able workers might self-select into union membership to receive perceived benefits, so the union effect would not only reflect their public good and voice functions but also a selection effect. To allow for such a selection effect is beyond the scope of this paper.

[14] However, Clark (1999) found that union workers did not care about their pay growth - whereas their non-union counterparts did gain a significant benefit from raises.

women, there is relatively little effect of unionisation, apart from lower satisfaction with pay cuts among covered full-time women.

Perhaps the most surprising feature of Figure 4 is the apparent absence of significantly greater dissatisfaction with real cuts, or changes in the region spanning nominal rigidity and modal real pay growth (which tends to lie just above the inflation rate, the sample average of which is 2.7%).

However, it could be that unions have a slightly different effect than the simple direct effect on satisfaction investigated in Figure 4. Unions might alter the impact of external referents. If unions do have a 'voice' or informational role, it is likely that they would decrease satisfaction with 'unexpected' or 'unwarranted' pay changes - pay changes that appear unfair in relation to other comparable workers or firm performance.

4.7 Comparisons with external referents

Some pay cuts might be acceptable to workers. Bewley (1999), among others, has emphasised that this might be the case if the worker's firm or industry is doing particularly badly. 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. A Nash bargaining framework - similar to that set out by Oswald (1985), for example - can result in the 'size of the pie' (i.e. firm profitability) being positively related to pay; so if the pie is shrinking, pay cuts might be acceptable. This is consistent with data from the British WERS: "Where no increase in pay had been implemented, the most common specific reason given was that the company could not afford it in the light of recent performance." (Forth and Millward 2000 p.12). The influence of firm performance on bargained wages (and hence on what wage is acceptable) can also come from reduced outside options: workers bargaining power will be lower if job and wage opportunities elsewhere are reduced. Outside options will be reduced if the industry is doing badly.[15]

Figure 5 shows the pay growth - job satisfaction relationship for workers whose industry experienced a fall in real output growth (solid line) compared to those whose industry output rose in real terms (dashed line). Unlike previous figures, to enable a clear focus on the impact of nominal rigidity, pay growth bands are defined in real terms. The impact of nominal rigidity actually occurs at various inflation rates, since these vary over time. The figure is shows the

[15] Possible alternative measures of outside options would be regional output (Gross Value Added per head) and regional unemployment. Both were investigated but neither was found to have a substantial impact.

effect of nominal rigidity at real earnings growth equal to minus the sample average inflation rate, which is -2.7%. Nominal rigidity has a significantly more positive impact on job satisfaction when industry output falls; this is indicated by the spike in the solid line just below zero. The height of this spike rates worker's satisfaction with nominal rigidity roughly equal.to their satisfaction with a real raise of 15%. (The actual marginal effect indicates a 5.2% increase in the probability the average worker reports themselves mostly or completely satisfied.) In contrast, there is no difference between nominal and real rigidity (a 0% real raise) in terms of satisfaction among those whose industry output rose, and indeed their satisfaction with nominal rigidity is insignificantly different from the lowest level reached by these workers.

The impact of comparisons with similar others is investigated using a different method, for reasons that will become clear. Salient others are defined in a similar way to Ferrer-i-Carbonell (2005) and others: in terms of mean earnings by three education groups, four age groups and eleven regions, resulting in 132 different comparison-income values. Then I investigate differences in response to pay changes of various sizes for samples split according to growth in comparison income. In the BHPS sample, it is only those whose comparators' pay rose in real terms who seem to pay attention to comparator's pay growth. Table 4 reports results for this subsample. If the 1,026 additional observations where comparators' pay growth fell in real terms are added, neither comparators' pay level nor comparators' pay growth is at all significant; the small number of comparison groups whose income falls in real terms seems to result in poorly-determined estimates. But when comparators' pay growth rises, the level of comparison pay affects utility with the expected negative sign and is significant at the 5% level. Comparators' pay growth also enters negatively, with a large coefficient, but it does not have a significant impact. Own pay growth has the positive impact I have previously reported, and own pay level continues to separately raise job satisfaction. The 'cut' dummy takes value 1 for all nominal cuts, and indicates lower satisfaction significant at the 10% level. Neither pay changes involving real cuts but nominal raises nor real raises themselves have an impact significantly different from the base case of nominal freezes.

5 Conclusion

This paper has investigated the extent to which patterns in wage dynamics can be related to worker job satisfaction. The use of such data to study wage rigidity is quite unusual. Recent

important work on wage dynamics by the International Wage Flexibility Network (Dickens *et al*, 2007) highlighted the need for further information on why some wages are downwardly rigid while others are flexible, and why some wages are sticky in real terms and others in nominal terms. This paper has investigated whether the coexistence of notable rigidity and remarkable flexibility could be related to the impact of pay change on job satisfaction.

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. Evidence is found that more than one referent applies: comparisons with salient others also affected job satisfaction. Second, there seems to be little support for loss aversion. The relationship between pay cuts and job satisfaction is less steep, rather than steeper, than the corresponding relationship with pay raises. Third, there is some indication that relatively small nominal cuts have what has been variously termed an 'insult' effect by Bewley (1999) or a 'winning is everything' effect by Mas (2006). Unionisation was found to be correlated with substantial dissatisfaction with pay cuts among full-time male workers, and union coverage had a similar effect for full-time females. In contrast, non-unionised workers did not exhibit any lower satisfaction with cuts than with nominal freezes. Nominal rigidity was consistently found to lead to greater satisfaction with the job than both small cuts and small nominal raises. Substantially higher satisfaction with nominal rigidity was found when the worker's industry output was declining, consistent with workers using firm performance as an external reference point. Nominal raises that left pay approximately rigid in real terms were found consistently to lead to lower job satisfaction than nominal freezes.

6 References

Abeler, Johannes, Armin Falk, Lorenz Goette and David Huffman (2009), "Reference points and effort provision", *American Economic Review* (forthcoming).

Agell, Jonas and Per Lundborg (1995), "Theories of pay and unemployment: survey evidence from Swedish manufacturing firms", *Scandinavian Journal of Economics*, 97 295–307.

Agell, Jonas and Per Lundborg (2003), "Survey evidence on wage rigidity", *Scandinavian Journal of Economics*, 105 15-30.

Akerlof, George A, William T Dickens and George L Perry (1996), "The macroeconomics of low inflation", *Brookings Papers on Economic Activity* 1, 1-75.

Akerlof, George A and Janet L Yellen (1990), "The fair wage-effort hypothesis and unemployment", *Quarterly Journal of Economics*, 105 (2) 255-283.

Altonji, Joseph G and Paul J Devereux (2000), "The extent and consequences of downward nominal wage rigidity", 383-431 in Solomon W Polachek (ed), *Worker Well-Being*, *Research in Labor Economics*, 19, Amsterdam: Elsevier.

Alvarez, Luis J, Emmanuel Dhyne, Marco Hoeberichts, Claudia Kwapil, Herve Le Bihan, Patrick Lunnemann, Fernando Martins, Roberto Sabbatini, Harald Stahl, Philip Vermeulen and Jouko Vilmunen (2006), "Sticky prices in the Euro area: a summary of new micro-evidence" *Journal of the European Economic Association*, 4 (2-3) 575-584.

Arulampalam, Wiji, Alejandra Manquilef and Jennifer C Smith (2009), "Differences in decline: quantile regression analysis of union wage differentials in the United Kingdom, 1991-2003", IZA Discussion Paper 4138.

Beissinger, Thomas and Christian Knoppik (2003), "How rigid are nominal wages? Evidence and implications for Germany", *Scandinavian Journal of Economics*, 105 619-42.

Bewley, Truman F (1999), *Why Wages Don't Fall During a Recession*, Cambridge, MA: Harvard University Press.

Bewley, Truman F (2002), "Fairness, reciprocity, and wage rigidity", Cowles Foundation Discussion Paper 1383.

Bils, Mark and Peter Klenow (2004), "Some evidence on the importance of sticky prices", *Journal of Political Economy*, 112 (5) 947-985.

Blanchflower, David G and Alex Bryson (2004), "What effect do unions have on wages now and would Freeman and Medoff be surprised?", *Journal of Labor Research*, 25 (3) 383-414.

Blinder, Alan S and D H Choi (1990), "A shred of evidence on theories of wage stickiness",

Quarterly Journal of Economics, 105 1003–1015.

Boeckerman, Petri and Pekka Ilmakunnas (2006), "Do job disamenities raise wages or ruin job satisfaction?", *International Journal of Management*, 27 (3) 290-302.

Boes, Stephan, Markus Lipp and Rainer Winkelmann (2007), "Money illusion under test", *Economics Letters*, 94 332-337.

Bound, John, Charles Brown and Nancy Mathiowetz (2001), "Measurement error in survey data", in J Heckman and Edward Leamer, *Handbook of Econometrics*, vol. 5, Amsterdam: Elsevier.

Calvo, Guillermo A (1983), "Staggered prices in a utility-maximising framework", *Journal of Monetary Economics*, 12 383-398.

Camerer, Colin, Linda Babcock, George Loewenstein and Richard Thaler (1997), "Labor supply of New York City cab drivers: one day at a time", *Quarterly Journal of Economics*, 112 407-441.

Campbell, Carl M and K S Kamlani (1997), "The reasons for wage rigidity: evidence from a survey of firms", *Quarterly Journal of Economics* 112 759–789.

Card, David and Hyslop, Dean (1997), "Does inflation 'grease the wheels of the labour market'?", 71-114 in Romer, Christina D and Romer, David H (eds), *Reducing Inflation: Motivation and Strategy*, NBER Studies in Business Cycles, 30, Chicago, IL: University of Chicago Press.

Clark, Andrew E (1997), "Job satisfaction and gender: why are women so happy at work?", *Labour Economics*, 4 341-372.

Clark, Andrew E (1999), "Are wages habit-forming? Evidence from micro data", *Journal of Economic Behaviour and Organisation*, 39 179-200.

Clark, Andrew E and Andrew J Oswald (1986), "Satisfaction and comparison income", *Journal of Public Economics*, 61 359-381.

Clark, Andrew E, Paul Frijters and Michael A Shields (2008), "Relative income, happiness and utility: an explanation for the Easterlin Paradox and other puzzles", *Journal of Economic Literature*, 46 (1) 95-144.

Dickens, William T, Lorenz Goette, Erica L Groshen, Steinar Holden, Julian Messina, Mark E Schweitzer, J Turunen and Melanie E Ward (2007), "How wages change: micro evidence from the International Wage Flexibility Project", *Journal of Economic Perspectives*, 21 (2) 195–214.

Driscoll, John C and Steinar Holden (2004), "Fairness and inflation persistence", *Journal*

of the European Economic Association, 2 (2-3) 240-251.

Elsby, Michael W L (2009), "Evaluating the economic significance of downward nominal wage rigidity", *Journal of Monetary Economics*, 56 (2) 154-169.

Fehr, Ernst and Lorenz Goette (2005), "Robustness and real consequences of downward nominal wage rigidity", *Journal of Monetary Economics*, 52 (4) 779-804.

Fehr, Ernst and Lorenz Goette (2007), "Do workers work more if wages are high? Evidence from a randomized field experiment", *American Economic Review*, 97 (1) 298-317.

Fehr, Ernst and J R Tyran (2001), "Does money illusion matter?", *American Economic Review*, 91 1239-1262.

Ferrer-i-Carbonell, Ada (2005), "Income and well-being: an empirical analysis of the comparison income effect", *Journal of Public Economics*, 89 997-1019.

Ferrer-i-Carbonell, Ada and Paul Frijters (2004), "The effect of methodology on the determinants of happiness", *Economic Journal*, 114 641-659.

Freeman, Richard (1978), "Job satisfaction as an economic variable", *American Economic Review*, 68 135-142.

Frey, Bruno S and Alois Stutzer (2002), "What can economists learn from happiness research?", *Journal of Economic Literature*, 40 402-435.

Fuhrer, Jeffrey and Gerald Moore (1995) "Inflation persistence", *Quarterly Journal of Economics*, 110 127-160.

Gali, Jordi (2009), "The return of the wage Phillips curve", CREI Working Paper.

Gali, Jordi and Mark Gertler (1999), "Inflation dynamics: a structural econometric analysis", *Journal of Monetary Economics*, 44 (2) 195-222.

Goette, Lorenz, David Huffman and Ernst Fehr (2004), "Loss aversion and labor supply", *Journal of the European Economic Association*, 2 (2-3) 216-228.

Goette, Lorenz, Uwe Sunde and Thomas Bauer (2007), "Wage rigidity: measurement, causes and consequences", *Economic Journal*, 117 F469-F477.

Grund, Christian and Dirk Sliwka (2007), "Reference-dependent preferences and the impact of wage increases on job satisfaction: theory and evidence", *Journal of Institutional and Theoretical Economics*, 163 (2) 313-335.

Holden, Steinar and John C Driscoll (2003), "Inflation persistence and relative contracting", *American Economic Review*.

Holden, Steinar and Frederik Wulfsberg (2008), "Downward nominal rigidity in the OECD",

The B E Journal of Macroeconomics, 8 (1) (Advances), Article 15.

Holden, Steinar and Frederik Wulfsberg (2009), "How strong is the macroeconomic case for downward real wage rigidity?", *Journal of Monetary Economics*, 56 (4) 605-615.

Jackle, Annette, Heather Laurie and S C Noah Uhrig (2007), "The introduction of dependent interviewing on the British Household Panel Survey", ISER Working Paper 2007-07.

Kahn, Shulamit (1997), "Evidence of nominal wage stickiness from micro-data", *American Economic Review*, 87 993-1008.

Kahneman, Daniel and Amos Tversky (1979), "Prospect theory: an analysis of decision under risk", *Econometrica*, 47, 263-291.

Kawaguchi, Daiji and Fumio Ohtake (2007), "Testing the morale theory of nominal wage rigidity", *Industrial and Labor Relations Review*, 61 (1) 59-74.

Millward, Neil, John Forth and Alex Byson (2000), *All Change at Work? British Employee Relations 1980-98*, Oxford: Routledge,

Layard, Richard (2005), *Happiness: Lessons from a New Science*, London: Allen Lane.

Nickell, Stephen J and Glenda Quintini (2003), "Nominal wage rigidity and the rate of inflation", *Economic Journal*, 113 (490) 762-781.

Oswald, Andrew J (1985), "The economic theory of trade unions: an introductory survey", *Scandinavian Journal of Economics*, 87 (2) 160-93.

Paull, Gillian (2003), "Biases in the reporting of labour market dynamics", IFS Working Paper WP02/10.

Shafir, Eldar, Peter Diamond and Amos Tversky (1997), "Money illusion", *Quarterly Journal of Economics*, 112 (2) 341-374.

Smets, Frank and Raf Wouters (2003), "An estimated dynamic stochastic general equilibrium model of the euro area", *Journal of the European Economic Association*, 1 (5) 1123-1175.

Smith, Jennifer C (2000), "Nominal wage rigidity in the United Kingdom", *Economic Journal*, 110 (462) C276-C195.

Solow, Robert (1979), "Another possible source of wage stickiness", *Journal of Macroeconomics*, 1 79-82.

Stewart, Mark B (1983), "On least squares estimation when the dependent variable is grouped", *Review of Economic Studies*, 50 141-9.

Stutzer, Alois and Bruno S Frey (2008), "Stress that doesn't pay: the commuting paradox", *Scandinavian Journal of Economics*, 110 (2) 339-366.

Taylor, Marcia Freed (ed) with John Brice, Nick Buck and Elaine Prentice-Lane (2009), *British Household Panel Survey User Manual Volume A: Introduction, Technical Report and Appendices*, Colchester: University of Essex.

Theodossiou, Ioannis and Alexandros Zangelidis (2006), "Career prospects and tenure-job satisfaction profiles: evidence from panel data", University of Aberdeen CELMR Discussion Paper 2006-04.

Tversky, Amos and Daniel Kahneman (1991), "Loss aversion in riskless choice: a reference dependent model", *Quarterly Journal of Economics*, 106 1039-1061.

van Praag, Bernard M S and Ada Ferrer-i-Carbonell (2008), *Happiness Quantified: A Satisfaction Calculus Approach* (Second Edition), Oxford: Oxford University Press.

Vendrik, Maarten and Geert Woltjer (2006), "Happiness and loss aversion: when social participation dominates comparison", IZA Discussion Paper 2218.

Table 1: Dependent Variable: Overall Job Satisfaction, Great Britain, 1992-2007

Pay measure	Weekly earnings			Hourly earnings	Basic hourly wage rate
	(1)	(2)	(3)	(4)	(5)
y [a]	0.0497	0.0554	0.384	0.0563	0.0675
	(0.0227)**	(0.0272)**	(0.0392)***	(0.0273)**	(0.0600)
y_{-1}			-0.328		
			(0.0390)***		
Δy		0.378		0.314	0.711
		(0.0377)***		(0.0371)***	(0.155)***
$ln\,(hours)$	-0.328	-0.248	-0.244	0.0300	-0.0515
	(0.0360)***	(0.0402)***	(0.0403)***	(0.0521)	(0.0862)
$ln\,(hours)_{-1}$		-0.149	-0.157	-0.424	-0.303
		(0.0393)***	(0.0395)***	(0.0432)***	(0.0750)***
Observations	38,813	32,534	32,534	32,563	5,844
Individuals	7,381	6,819	6,819	6,821	1,990
Log likelihood	-56576	-47140	-47155	-47221	-8539

Notes: Table 1 shows ordered probit coefficients. Asymptotic standard errors (in parentheses) are adjusted for clustering on individuals. Significance is indicated at the following levels: *** 1%, ** 5%, * 10%. All regressions are weighted using cross-section weights. y is the natural log of the relevant pay measure. Controls included in all regressions include (demographics) 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; (macro and local labour market conditions) year dummies (sixteen for earnings and eight for basic wage rate), eleven region dummies. [a] The BHPS derived variable monthly usual gross pay is used as the 'income' variable when the focus is the impact of changes in hourly basic rate because it is observed (calculated from basic wage rate, hours and non-basic payments) in around 1,000 cases where gross usual weekly pay is not. Results, however, hardly differ if the latter is used.

Table 2: Dependent Variable: Overall Job Satisfaction, Great Britain, 1992-2007

Weekly earnings				Hourly basic wage rate			
	(1)		(2)		(3)		(4)
y	0.0642	y	0.0648	y [a]	-0.0307	y [a]	-0.0322
	(0.0273)**		(0.0274)**		(0.0550)		(0.0547)
Real pay growth		Nominal pay growth		Real wage growth		Nominal wage growth	
$[-85-\pi,-40)$	0.0374	$[-85,-40)$	-0.0336	$[-35-\pi,-20)$	-0.176	$[-35,-20)$	-0.187
	(0.0675)		(0.0676)		(0.149)		(0.146)
$[-40,-20)$	0.0356	$[-40,-20)$	-0.0385	$[-20,-10)$	-0.0823	$[-20,-10)$	-0.106
	(0.0452)		(0.0458)		(0.109)		(0.124)
$[-20,-10)$	0.0237	$[-20,-10)$	-0.0707	$[-10,-5)$	-0.0662	$[-10,-5)$	0.123
	(0.0387)		(0.0372)*		(0.0951)		(0.107)
$[-10,-5)$	0.0404	$[-10,-5)$	-0.0429	$[-5,-2)$	-0.0614	$[-5,-3)$	-0.0855
	(0.0376)		(0.0371)	excl $-\pi$	(0.0825)		(0.112)
$[-5,-2)$	0.0765	$[-5,-2)$	-0.0396	$-\pi$	-0.0697	$[-3,-1)$	-0.0604
excl $-\pi$	(0.0381)**		(0.0375)		(0.0701)		(0.107)
$-\pi$	0.0810	$[-2,-1)$	0.0131	$[-2,-0.5)$	-0.0105	$[-1,0)$	0.101
	(0.0375)**		(0.0569)	excl $-\pi$	(0.0731)		(0.118)
$[-2,-0.5)$	0.0398	$[-1,0)$	-0.0233	$(0.5,2]$	0.115	$(0,1]$	-0.0368
excl $-\pi$	(0.0375)		(0.0520)		(0.0686)*		(0.104)
$(0.5,2]$	0.0347	$(0,1]$	0.0304	$(0.5,2]$	0.0647	$(1,3]$	0.101
	(0.0357)		(0.0476)		(0.0675)		(0.0557)*
$(2,5]$	0.109	$(1,2]$	-0.0379	$(5,10]$	0.132	$(3,5]$	0.131
	(0.0328)***		(0.0422)		(0.0687)*		(0.0590)**

Table 2 continued: Dependent Variable: Overall Job Satisfaction, Great Britain, 1992-2007

Weekly earnings				Hourly basic wage rate			
	(1) cont.		(2) cont.		(3) cont.		(4) cont.
$(5, 10]$	0.129	$(2, 3]$	-0.0899	$(10, 20]$	0.0827	DbNp10	0.152
	(0.0335)***		(0.0377)**		(0.0743)		(0.0549)***
$(10, 20]$	0.172	$(3, 4]$	-0.0684	$(20, 40]$	0.140	$(10, 20]$	0.181
	(0.0346)***		(0.0371)*		(0.0934)		(0.0552)***
$(20, 40]$	0.256	$(4, 5]$	-0.0136	$(40, 65 - \pi]$	0.442	$(20, 40]$	0.195
	(0.0371)***		(0.0401)		(0.178)**		(0.0770)**
$(40, 100 - \pi]$	0.300	$(5, 10]$	0.0391			$(40, 65]$	0.448
	(0.0447)***		(0.0292)				(0.150)***
		$(10, 20]$	0.0748				
			(0.0297)**				
		$(20, 40]$	0.153				
			(0.0317)***				
		$(40, 100]$	0.206				
			(0.0404)***				
Log likelihood	-47115		-47115		-8705		-8704

Notes: Table 2 shows ordered probit coefficients. Asymptotic standard errors (in parentheses) are adjusted for clustering on individuals. Significance is indicated at the following levels: *** 1%, ** 5%, * 10%. All regressions are weighted using cross-section weights. π is annual RPI inflation, measured at the month of interview. Pay and wage changes are trimmed according to nominal values so π appears when sample end-points are expressed in real terms (columns (1) and (3)). The base for earnings or wage growth dummies expressed in real terms (columns (1) and (3) respectively) is nominal earnings growth equal to inflation plus or minus 0.5% inclusive. The base for earnings or wage growth dummies expressed in nominal terms (columns (2) and (4) respectively) is nominal freezes. Controls included in all regressions include log weekly hours and its lag, (demographics) 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; (macro and local labour market conditions) year dummies (sixteen for earnings and eight for basic wage rate), eleven region dummies. Columns (1) and (2): 32,534 observations on 6,819 individuals; Columns (3) and (4): 5,904 observations on 2,009 individuals. [a] The BHPS derived variable monthly usual gross pay is used as the 'income' variable when the focus is the impact of changes in hourly basic rate because it is observed (calculated from basic wage rate, hours and non-basic payments) in around 1,000 cases where gross usual weekly pay is not. Results, however, hardly differ if the latter is used.

Table 3: Compensation for Large Cuts

Potential compensating change	Pay change (%)			
	$[-85, -40)$	$[-40, -20)$	$[-20, -10)$	$[-10, 100]$
	Pairwise correlation			
Change in travel-to-work time (hours)	-0.011**	-0.006	-0.008*	0.007
Shift to daytime working	0.023***	0.038***	0.097*	-0.032***
Pension scheme introduced	0.007*	0.011**	-0.001	-0.018***
Job becomes permanent	0.028***	0.008	0.005	-0.024***
New work location: employer premises	0.018***	0.005	0.012**	-0.018***
New work location: elsewhere	0.014***	-0.002	-0.000	-0.010***
	Mean change (hours) or Proportion experiencing change (%)			
Change in travel-to-work time (hours)	-0.017 h (-1 min)	-0.003	-0.004	0.004
Shift to daytime working	9.7	9.6	6.1	4.9
Pension scheme introduced	5.0	4.9	3.7	3.7
Job becomes permanent	4.8	2.1	1.9	1.6
New work location: employer premises	6.3	3.8	4.2	3.2
New work location: elsewhere	5.5	3.1	3.3	3.4
Any potential compensating change	25.9	19.1	16.7	14.8
Number experiencing pay change	544	1,490	2,335	38,167

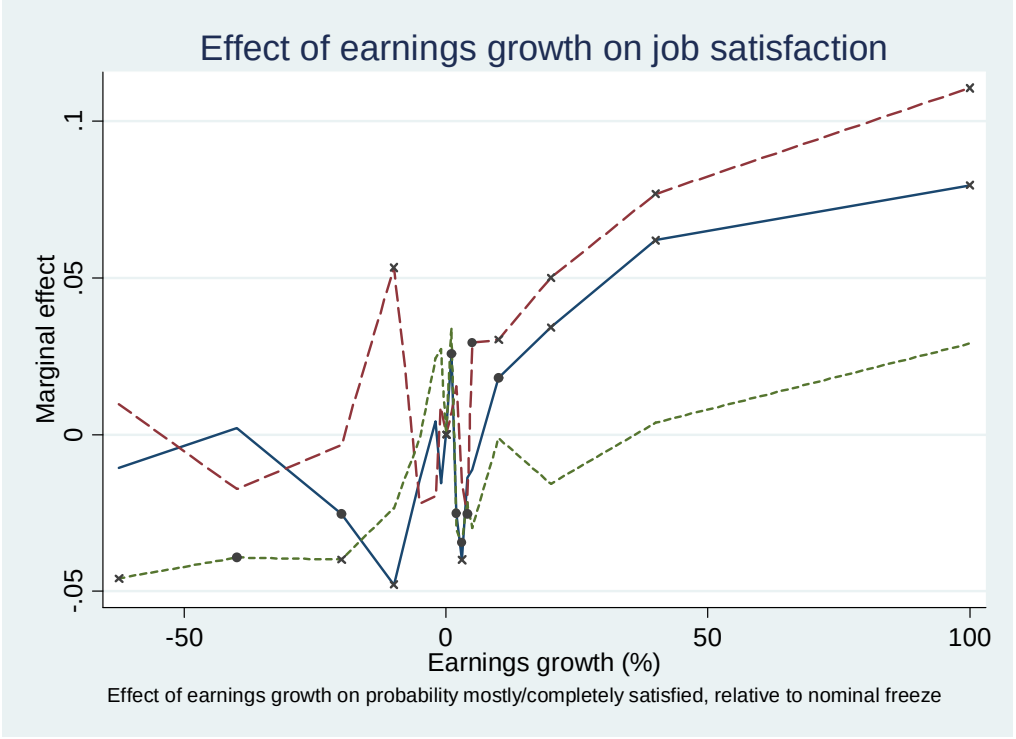
Notes: The sample includes all stayers for whom satisfaction is observed. All statistics are unweighted.

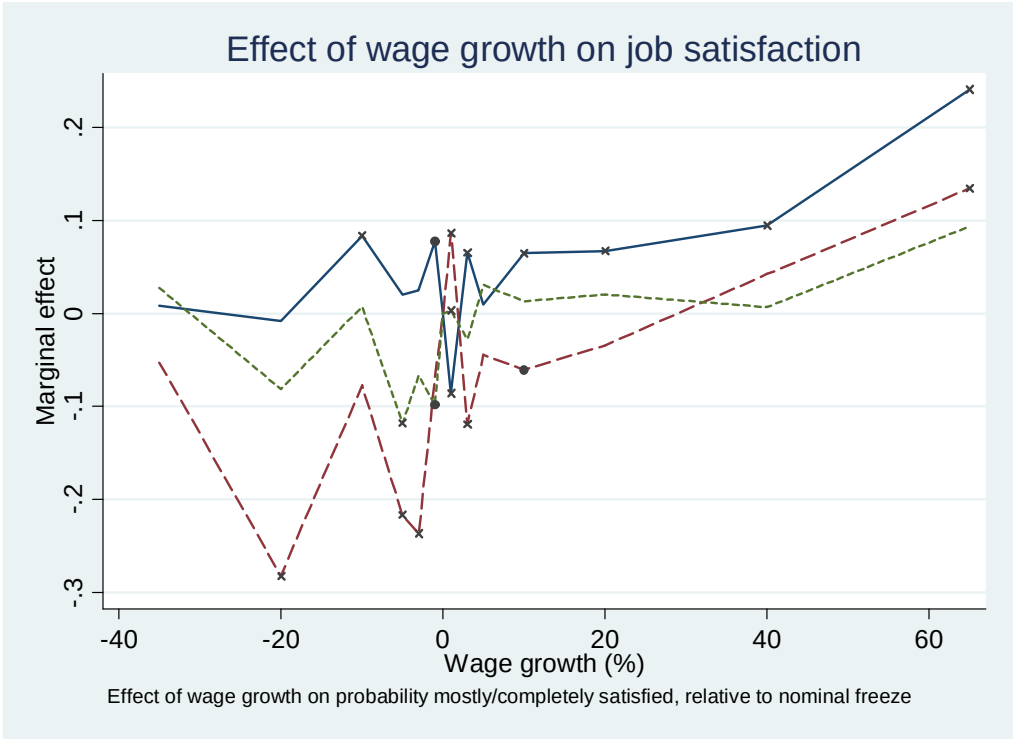
Table 4: Effect of Comparisons with Salient Others on Overall Job Satisfaction, Great Britain, 1992-2007

y	0.0917
	(0.0406)**
Δy	0.271
	(0.077)***
$y^{comparison}$	-0.177
	(0.0889)**
$\Delta y^{comparison}$	-0.746
	(0.710)
cut	-0.0670
	(0.0403)*
$nominal\ raise, real\ cut$	-0.0396
	(0.0410)
$real\ raise$	0.0061
	(0.0351)
Observations	20,682
Individuals	5,350
Log likelihood	-29298

Notes: Table 4 shows ordered probit coefficients. Asymptotic standard errors (in parentheses) are adjusted for clustering on individuals. Significance is indicated at the following levels: *** 1%, ** 5%, * 10%. All regressions are weighted using cross-section weights. The sample is all stayers whose comparators' real earnings rose. y is the natural log of real total weekly earnings. Comparators' earnings $y^{comparison}$ is the natural log of average real earnings in the relevant cell, by three education groups (high, medium and low), four age groups (below 25, 25 to 34, 35 to 54, and over 55), and eleven regions. Controls include current and lagged natural log of weekly total hours, 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, sixteen year dummies, ten occupation dummies, seventeen industry dummies, nine workplace employment dummies, travel-to-work time, dummies for presence of and 'positive' and 'negative' changes in the following (where significant): evening/night working, employer pension scheme, work location, non-permanent job, bonus scheme, incremental pay scale, managerial responsibility, supervisory responsibility.

FIGURE 1: EFFECT OF EARNINGS AND WAGE GROWTH ON JOB SATISFACTION, GREAT BRITAIN, 1992-2007





Notes: Part-time is defined as less than 30 hours worked per week.

FIGURE 2: EFFECT OF JOB CHARACTERISTICS ON THE PAY GROWTH-JOB SATISFACTION RELATIONSHIP, GREAT BRITAIN, 1992-2007

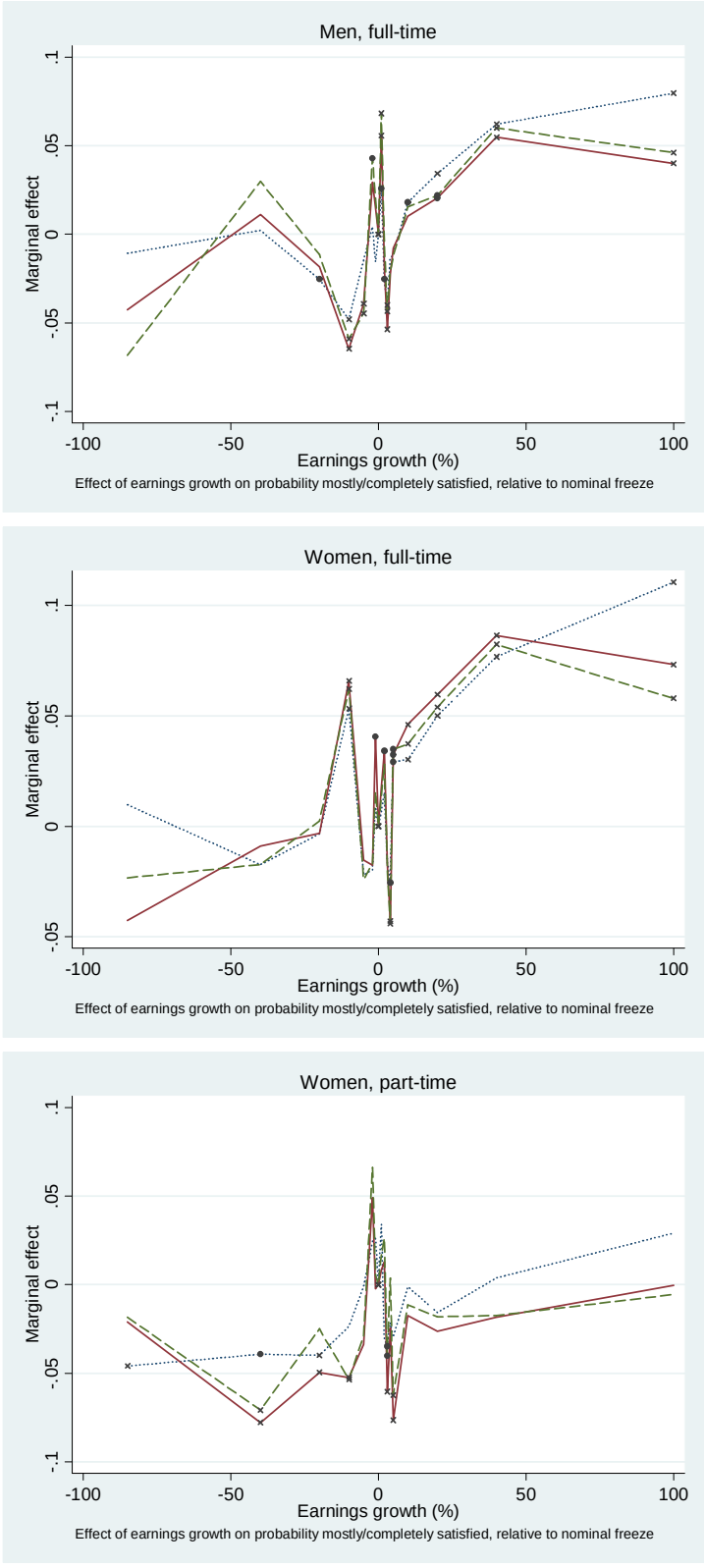


Figure 3: Effect of Measurement Error on the Pay Growth-Job Satisfaction Relationship, Great Britain, 1992-2007

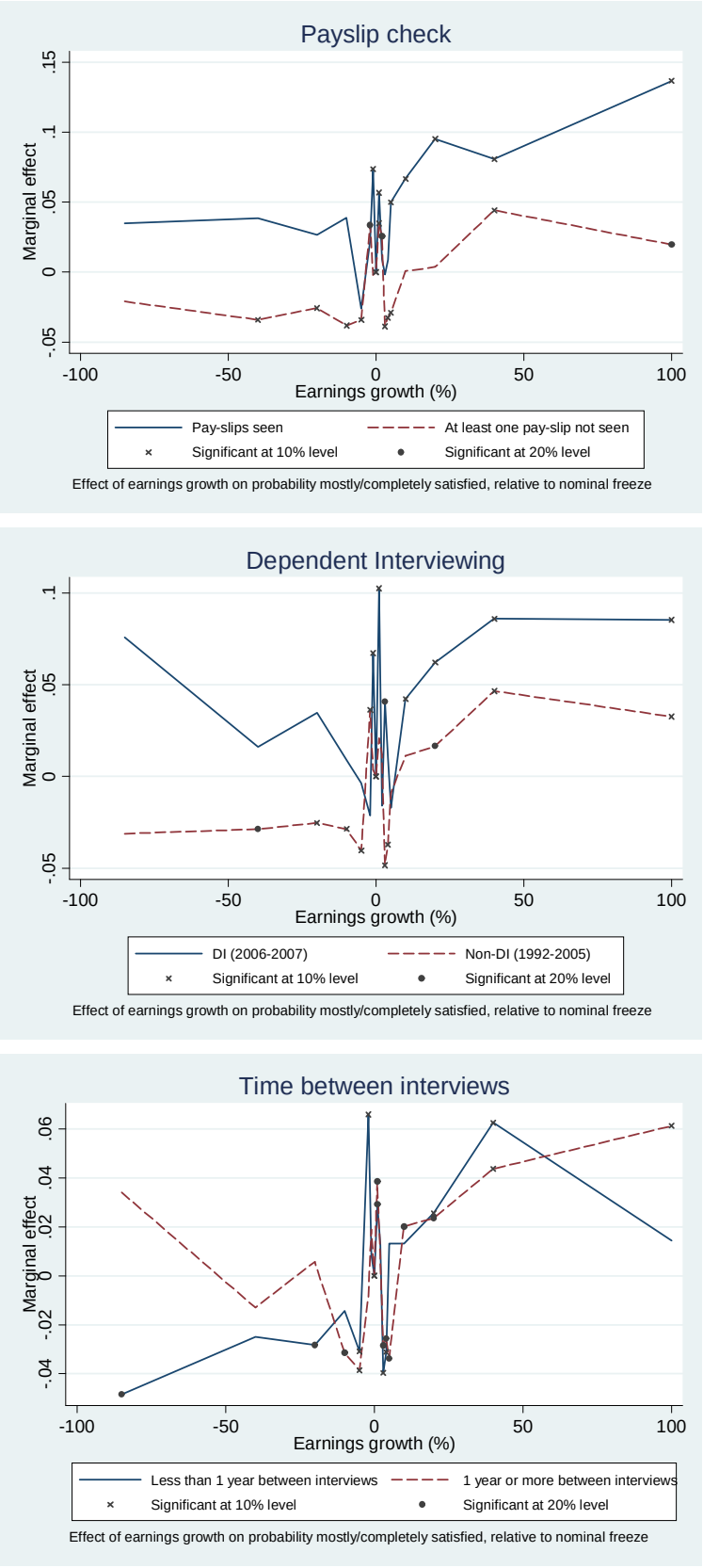
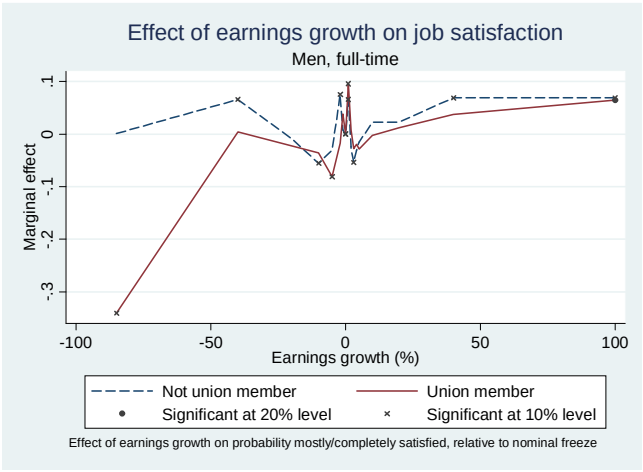
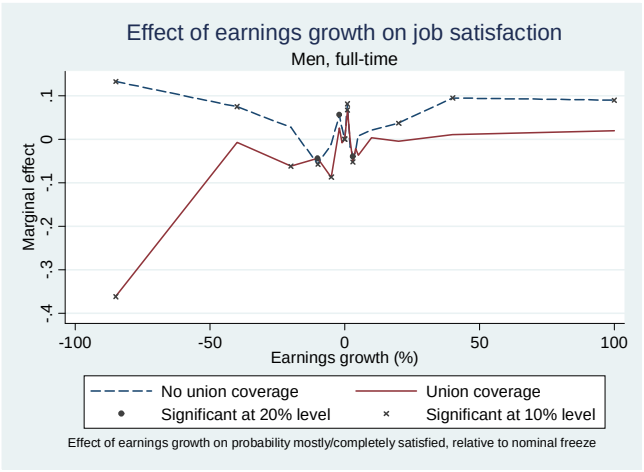
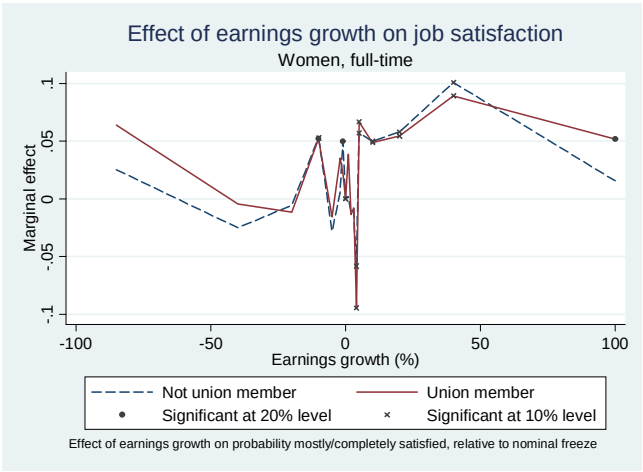
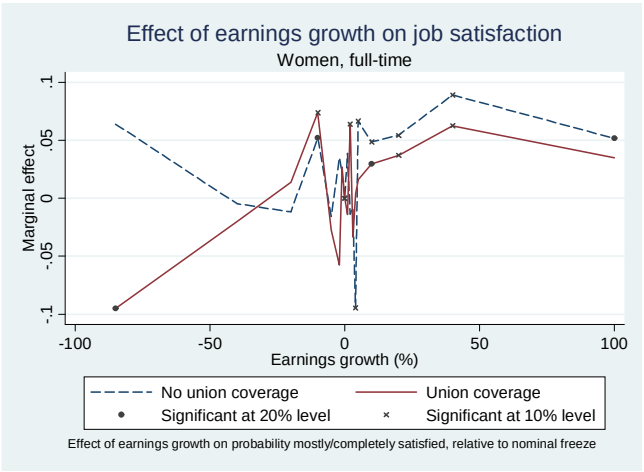


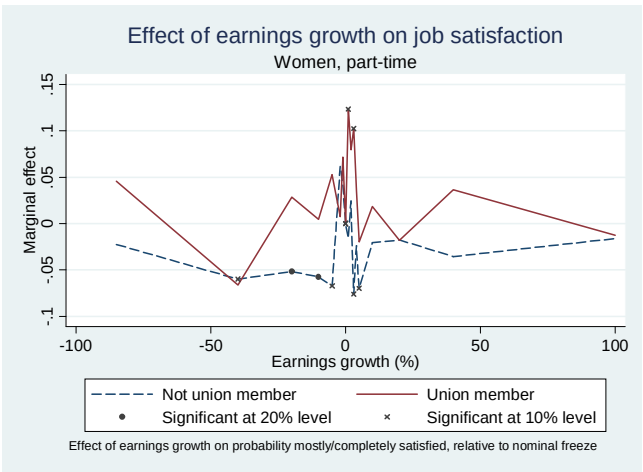
FIGURE 4: EFFECT OF UNIONISATION ON THE PAY GROWTH-JOB SATISFACTION RELATIONSHIP, GREAT BRITAIN, 1992-2007











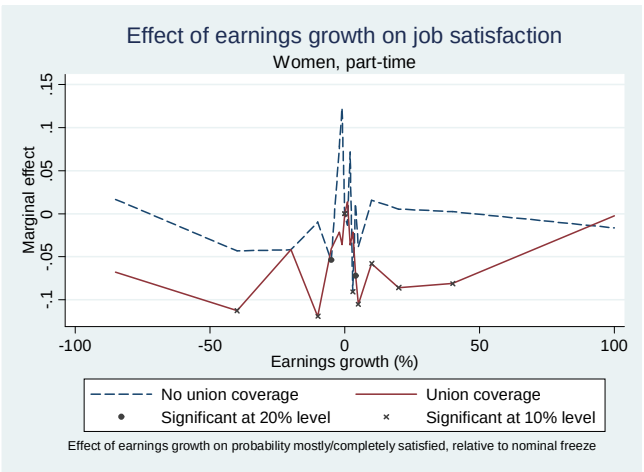
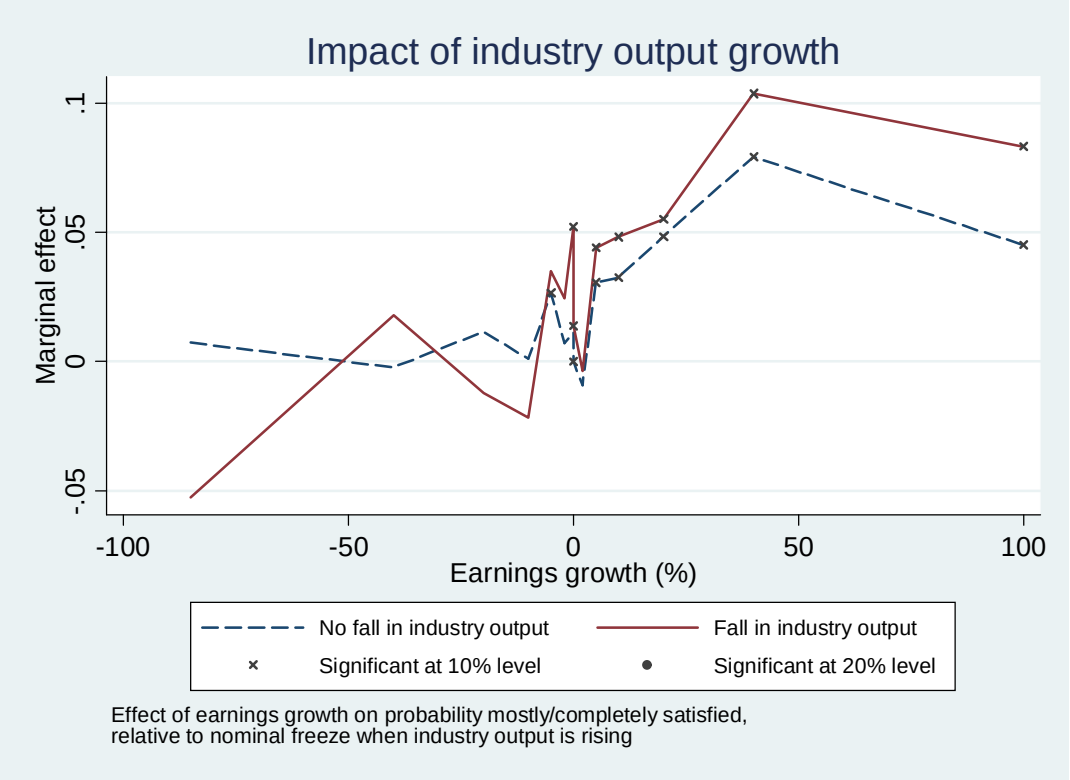


Figure 5: Effect of Industrial Output Growth on the Pay Growth-Job Satisfaction Relationship, Great Britain, 1992-2007



7 Appendix 1: Data Appendix

BHPS sample Cross-sectional respondent weights are used throughout this paper, which means that the sample excludes (unrepresentative) samples added since the BHPS started in 1991 to oversample Scotland, Wales and Northern Ireland and to incorporate the European Community Household Panel (ECHP), which oversampled low-income groups. The sample used in this paper incorporates Original Sample Members and their offspring and individuals who join their household (known as Temporary Sample Members). The original sample was representative of the British population (excluding Northern Ireland), according to the 1991 UK Census. The initial-year weights ensure that the sample is representative of the UK population and subsequent-year weights are adjusted for attrition and non-response. Children of OSMs are given a share of the weight of their household, which should, as the BHPS note, go some way to maintaining the representative nature of the BHPS (Taylor 2009).

Job satisfaction questions The 'usual' job satisfaction variable is obtained in the face-to-face interview. The 'overall' job satisfaction question follows four questions relating to 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 previously-interviewed respondents might have in mind, or have specifically recalled, their pay and pay history in preparation for those questions.

Pay and wage data Particularly when examining nominal rigidity, great care must be taken over the pay measure used. Usual gross weekly pay is calculated from 'raw' BHPS data on gross pay, pay period and whether reported pay is usual. The BHPS data include a derived usual gross monthly pay variable, but even if imputed values for this are excluded it will still be subject to error in cases where gross pay is calculated from net using estimated tax rates - error which will cover up true freezes. Over the 17 years I sacrifice 2,739 observations (2.9% of 93,020) where the BHPS gross pay variable exists (and is not calculated from net pay) but my raw variable does not, in order to be certain how the pay variable is calculated. Individuals citing a pay period less than 1 week are dropped, since these pay period data likely reflect coding errors (see BHPS documentation notes available at http://www.iser.essex.ac.uk/survey/bhps/documentation).

Job history data Tenure with employer (and actual work experience - which includes self-employment) are calculated using BHPS data on the entire job histories of respondents, which were collected at Waves 2 and 3 for Original Sample Members. The method of Paull (2002) is used to reconcile inconsistencies within different sources of labour market history to calculate tenure and experience.

Definition of job stayer A job stayer is defined as someone who does not change employer. In practical terms, this uses the Paull (2002) definition of a job spell (which defines spells relative to employers), and defines a job stayer as someone whose number of job spells does not increase. This is the definition available on most datasets worldwide (such as the PSID) and has the advantage of avoiding the difficulty of defining when a job has changed within employer. BHPS data do, unusually, include information on job changes within employer.

Price, industry and regional data Price, industry and regional data are drawn from the UK Office for National Statistics. The price index used is the RPI (Retail Prices Index, All Items). Annual inflation is matched into the BHPS data according to interview month. This means that the real pay increase of someone interviewed in February 2000 is measured by their growth in nominal pay since the last interview minus the annual inflation rate at February 2000. The idea behind this is that BHPS survey respondents may well have a reasonable idea of the current inflation rate, and may well use this as a reference in assessing the merits of their pay growth. Dickens *et al* (2007) note the measurement difficulty presented by the fact that surveys do not synchronise settlement dates. I hope to alleviate most concerns by investigating the impact of the time between interviews on results. Alternative measures of inflation were

used, including the average inflation rate between last interview and this (to capture the fact that settlements might have occurred at any time between interviews) and the actual growth rate of prices between last interview and this, with no change in results.

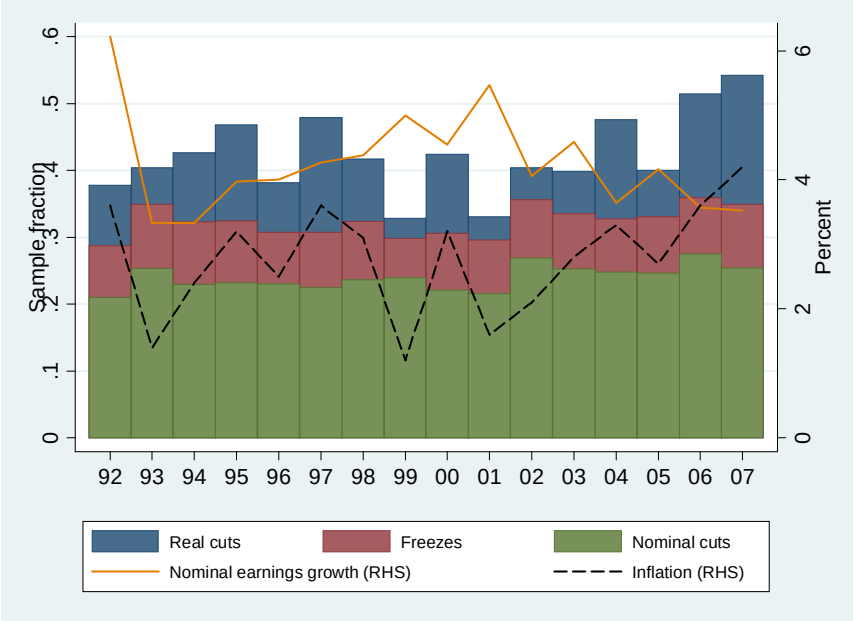
Industry performance is measured by real output per capita (volume measure, at 2005 prices) at for 33 industries, classified according to their 2-digit Standard Industrial Classification. For manufacturing industries this is matched to BHPS data according to month of interview. For other industries data are matched according to quarter of interview. When industry dummies are used these 33 industries are aggregated into 17 broader groups.

Regional prosperity is measured by regional gross value added (GVA) per capita , deflated by the RPI. GVA is a proxy for, and the best measure of, GDP at regional level in the UK, so regional GVA per capital measures regional productivity. Data are measured at the level of the 12 standard regions of the UK.

The regional claimant count unemployment rate is used in reported results as it covers all years of the sample. Results are very similar if the 'LFS' OECD definition of unemployment is used instead.

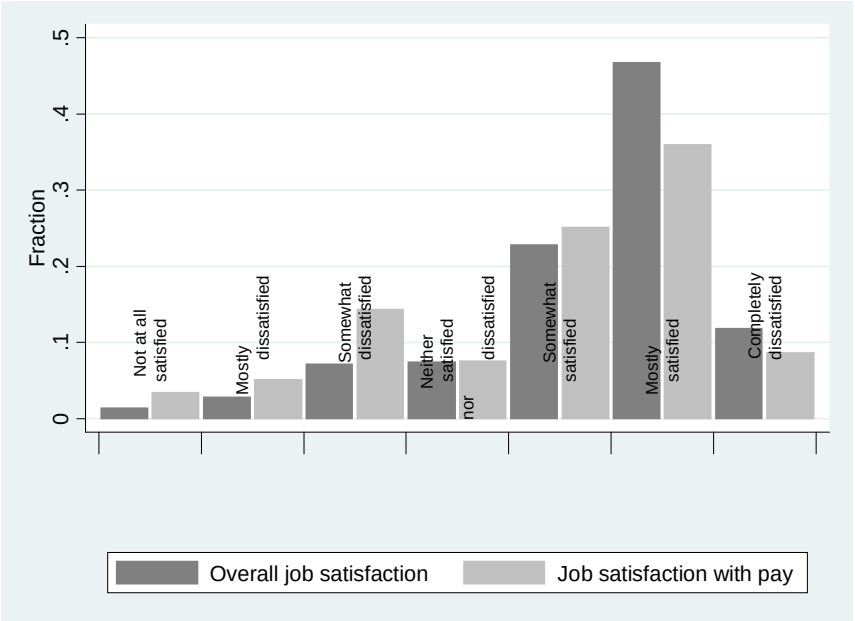
8 Appendix 2: Earnings growth over time and distribution of job satisfaction

FIGURE A1: EARNINGS GROWTH, GREAT BRITAIN, 1992-2007



Notes: Inflation is median RPI (All Items) inflation at the month of interview. Nominal earnings growth is median nominal earnings growth. Both relate to the Wave beginning in September of the relevant year. The sample is those employed last interview with no change in employer since then and observed earnings growth. The top graph is unweighted. In the top graph all statistics are weighted using cross-section weights.

FIGURE A2: JOB SATISFACTION, GREAT BRITAIN, 1992-2007



Notes: The sample is those employed last interview with no change in employer since then.