Are pay cuts involuntary?

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Preliminary - comments welcome

Abstract

Knowledge of what lies behind earnings changes is crucial to an understanding of the determination of pay and the distribution of income. A striking feature of the distribution of wage growth observable from individual panel surveys is the diversity of wage growth. Many workers enjoy pay rises, but some appear willing to take pay cuts – in nominal as well as real terms. This paper examines whether apparent cuts do actually occur, and, to the extent they occur, attempts to explain why workers might take pay cuts.

The notion of downward nominal ridigity in wages relies fundmentally on the hypothesis that workers dislike nominal cuts. Notably, Bewley (2000) has suggested that pay cuts are avoided because of their adverse effects on workers' morale. This paper uses panel data on the pay and satisfaction of a large number of individuals in Britain in the 1990s to test this assumption. The evidence for lower morale among those taking cuts is not as strong as might have been expected.

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1. Introduction

How flexible is pay? Do workers really accept pay cuts, and if so, why and when? It is important to know the extent of and explanations for pay cuts. In general, studying pay cuts contributes to our knowledge of what lies behind the dynamics of earnings, which is crucial in understanding the determination of pay and the distribution of income. Studying the explanations for pay cuts can help us decide whether they are believable – whether they really occur or not. The extent of valid pay cuts is a measure of labour market flexibility. Many commentators have claimed that the UK labour market of the 1990s is more flexible than in previous decades, but just how flexible is it? Knowing whose pay is flexible and why is useful for policy and welfare reasons. Does flexibility characterise the whole labour market, or are there some sectors which remain inflexible? Are there groups of workers who are powerless to resist (possibly repeated) cuts in their pay, exacerbating low pay and income inequality?

This paper first has the task of convincing readers that pay cuts do actually happen. Using survey data from British employees, we show that some pay cuts cannot be explained by measurement error, and that some of these pay cuts are not solely due to bonuses or overtime payments. Once we have established that some pay cuts do really occur, we then ask: are pay cuts involuntary or voluntary? If workers were willing to accept pay cuts, there would be no downward wage rigidity, at least in the Keynesian sense that could be alleviated by increased aggregate demand (or higher inflation). Adverse unemployment consequences of downward rigidity rely fundamentally on workers – facing the unpleasant alternatives of involuntary unemployment or involuntary pay cuts – not wanting to accept pay cuts. Involuntary unemployment is sometimes characterised as workers willing to work at wages less than currently being paid to other workers. Involuntary pay cuts are characterised here as pay cuts that workers are not happy with.

This paper uses data on workers' satisfaction to investigate whether pay cuts are involuntary. If cuts are involuntary, satisfaction should fall. The overlap is not perfect: satisfaction might fall even if pay cuts are willingly undertaken. But if satisfaction does not fall with pay cuts, it would be difficult to describe such cuts as involuntary in the sense that the Keynesian notion of downward rigidity

requires. For such cuts, there is no indication that workers did not want them. This paper will elaborate on many circumstances drawn from actual data where workers are happy with cuts. Near-retirement, child-bearing and rearing, changes in duties (even while retaining the 'same job') are examples of such circumstances. Pay flexibility might come through overtime, bonuses and even changes in shift arrangements. Are workers happier to accept cuts stemming from these components than cuts in basic pay?

In this paper we investigate the believability, extent and determinants of pay cuts using data from a panel survey following a large number of individuals in Britain between 1991 and 1997.[1] 'Raw' individual pay data generate pay growth distributions which are very similar across countries. Recent work using this type of data has generated some controversy over whether workers *really* take the pay cuts they appear to, or whether such observations are merely due to measurement error. This question has arisen in relation to the extent of nominal cuts versus nominal wage rigidity. Two recent papers present conflicting conclusions. Altonji and Devereux (1999) model measurement error in the US Panel Study of Income Dynamics econometrically and conclude that it can account for almost all reported nominal cuts in the hourly wage of those who remain in the same job. In contrast, Smith's (2000) statistical analysis of the UK British Household Panel Survey indicates that there are many nominal cuts (affecting 18% of job stayers) in monthly pay among the effectively measurement-error-free (those whose payslips are checked during the survey). Most of these cuts do not appear to be due to changes in hours, overtime or bonus pay. Earlier studies have also produced contradictory results. Akerlof, Dickens and Perry (1996) argued that virtually no worker really took nominal cuts, and that almost all cuts in individual panel surveys reflect measurement error. Previously, McLaughlin's (1994) work using the PSID had suggested that a large proportion (12%) of workers might take nominal wage cuts, even after measurement error is taken into account. Other papers on nominal rigidity which use individual panel survey data (Kahn (1997); Lebow, Stockton and Wascher (1995); Card and Hyslop (1997)) have not dealt

[1] In a companion paper we focus on the dynamics of pay cuts themselves, looking at whether certain types of individual are more prone to suffer repeated cuts than others.

with measurement error in such detail, generally assuming it to have a reasonably limited effect on the wage growth distribution.

To try to unravel these conflicting findings we adopt three lines of attack. After presenting the 'facts' as they appear from the raw survey data we first check whether apparent pay cuts are due to changes in hours, bonuses, overtime and shift pay – which would be consistent with stability in basic pay per hour – or measurement error. Second, we examine whether the apparent cuts exhibit features we would expect if they really occurred. We investigate whether cuts reduce satisfaction, and whether dissatisfaction is greater, the larger the cut. Third, we construct a full econometric model of pay cuts. If cuts really occur, they should have the expected relationships with explanatory variables derived from human capital theory, reflecting productivity shocks, and capturing firm characteristics, bargaining power and fall-back options and social exclusion. Our model of pay cuts has the ultimate aim of discovering *why* workers accept cuts. Are cuts distributed randomly or are they concentrated among certain groups of individuals? Does this reflect a disadvantage that should be of policy concern, such as exclusion or lack of training or education? Do cuts imply welfare losses? Are they part of a rational decision process whereby workers sacrifice some income in return for achieving other goals? Even the latter may be of policy concern. For example, if women are forced to take cuts in monthly pay as they go part-time to look after children, this might well be of concern to policy-makers (in that childcare provision appears inadequate).

We investigate real as well as nominal cuts, but nominal cuts are of particular interest in terms of believability. Real cuts that do not reduce nominal pay are not unlikely. In contrast, in the United Kingdom (as in the rest of Europe) a nominal cut is not a formality: the employer has no right to cut pay without the employee's consent (see Deakin and Morris (1988), pp. 247, 269 and 274–5).[2] Employment law is such that the employer cannot unilaterally alter the contract terms and conditions. The employer may dismiss the employees and offer them new terms, but this dismissal might be ruled as unfair, giving rise to damages or redundancy compensation claims, which could clearly be particularly expensive if

[2]I am indebted to Steinar Holden for pointing this out and providing details of the legislation.

large numbers of employees are involved. Even if the terms were previously agreed by a trade union from which recognition is subsequently withdrawn, the existing terms remain in force until the individual employee consents to vary them. The extent of nominal cuts is also of policy interest: downward nominal flexibility would relieve monetary policy-makers of a need to keep inflation above 0% (see Smith (2000), for example, for further discussion).

Section 2 describes the data. Section 3 presents the 'raw' statistics, compares these with other data and discusses the problem of measurement error. Section 4 checks whether cuts reduce satisfaction. The econometric and economic models of pay cuts are described in Section 5, and Section 6 discusses the econometric results. Section 7 concludes.

2. Data

We use data on individuals' pay and characteristics from the British Household Panel Survey covering the first seven waves 1991–1997. Survey interviews begin in September each year and most are completed by the end of the year. There are a total of 23,834 observations on individuals' pay growth during this period. The BHPS provides rich information on factors that might explain changes in individuals' pay. Because we are interested in the overall level of pay flexibility in the economy, our full sample is as broad as possible, including all employees – those who change jobs as well as stayers, and part-time as well as full-time workers – although we investigate sub-samples where relevant.[3] We do not, however, include the self-employed as their reported pay can be very unreliable.

Individuals' pay levels are recorded at each interview. The individual's latest pay and their usual pay are both recorded. We use usual pay as this is less likely to be affected by exceptional (volatility-inducing) bonus, overtime and shift pay and by unusual hours. Individuals can state pay for any pay period, so it is necessary to divide by pay period to standardise across individuals. Individuals can state either gross or net pay. We use gross pay as this is not affected by differences

[3]Movers may have had a spell of unemployment, but its duration would have been less than the between-interview period (typically around one year). Movers include those who change job within a firm, for example due to promotion, in addition to those who change employer.

in union dues, pension contributions, and so on. The BHPS provide a derived variable for gross monthly pay, although this includes values which are calculated from stated net pay using data on tax rates, marital status, partner's activity and pension scheme membership, and also includes imputed values.[4] We examine the impact of these procedures below. Using the BHPS derived variable as the basis for analysis aids replicability and hopefully the usefulness of this study.

We calculate monthly pay growth as the percentage change in the BHPS gross monthly pay variable. We also use hourly pay growth. Hourly pay is calculated as gross monthly pay divided by standard monthly hours. In addition to these nominal pay growth variables we investigate real pay cuts. Real pay growth is simply nominal growth minus the percentage change in the UK Retail Price Index (all items). We calculate annual growth rates from monthly RPI data and use average annual growth from the previous October to when the interviews start in September, since pay changes might have taken place at any time during the year between interviews.

One aim of this paper is to investigate whether pay cuts are believable or whether they reflect error. In addition to using data on whether gross pay is imputed, calculated from net and other information, we also make use of the BHPS pay-slip check. Each employee is asked to check their pay-slip when recording their latest pay level. If the latest pay-slip is checked the pay data will be correct (to the nearest £1). We assume that the usual pay of individuals whose pay-slips were checked is also correct (insofar as 'usual' pay is ever correct).[5] We may also want to discount cuts due to non-basic pay components.[6]

[4] A regression-based imputation procedure known as predictive mean matching is used. A model of pay is selected on an R-squared criterion and the closest actual pay matching the predicted pay for missing cases is selected (see Taylor (1998) and BHPS User Manual Volume A, Section V.3). Various models for pay are employed, depending on the other data available. Some models use cross-wave as well as intra-wave information.

[5] We assume error-free data result from the checking of either an earlier pay-slip or the latest one.

[6] Unfortunately BHPS data do not tell us how much bonus pay a worker received, nor how much overtime pay they received. They tell us each year whether the worker's pay "ever include[s] incentive bonuses or profit related pay", and they tell us how many hours overtime and how many hours *paid* overtime are worked (from which we can infer unpaid overtime hours). We focus on what we can examine using BHPS data. Cuts will occur if a bonus scheme is scrapped (assuming bonuses were actually paid). Cuts in *monthly* pay will arise if there is a decline in

As mentioned in Section 1, we use data on satisfaction to check the impact of cuts. The BHPS survey involves the following question: "I'm going to read out a list of various aspects of jobs, and after 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". The respondent is told that 1 = completely dissatisfied, 7 = completely satisfied and 4 = neither satisfied nor dissatisfied. The respondent is prompted if necessary: "How satisfied would you say you are with the ... in your present job?". The questions refer to 1. Promotion prospects; 2. The total pay, including any overtime or bonuses; 3. Relations with your supervisor or manager; 4. Your job security; 5. Being able to use your own initiative; 6. The actual work itself; 7. The hours you work. The respondent is finally asked, "All things considered, how satisfied or dissatisfied are you with your present job overall using the same 1–7 scale?".

3. The extent of pay cuts

Table 1 analyses the frequency of cuts in nominal and real usual gross monthly and hourly pay, and also reports nominal rigidity, nominal rises and real rises. The table relates to all workers – job changers as well as job stayers. Figures for stayers alone (70% of all workers) are similar. Considering monthly pay statistics, 41% experience falls in real monthly pay, the remaining 59% enjoying real rises. A substantial 17% suffer real falls of more than 10%, and 5% experience real cuts of 30% or more. 8% experience relatively small real cuts such that their pay rises in nominal terms. A further 5% have pay that is rigid in nominal terms; these workers experience a real cut equal to minus the inflation rate.[7] 28% suffer nominal cuts. Cuts in hourly pay are more frequent that those in monthly pay and rises are correspondingly less common, reflecting the general rise in working hours that has occurred during the sample period. Nominal rigidity is much lower

paid overtime hours. *Hourly* pay will fall if unpaid overtime hours rise, as our 'hours' measure includes standard and all overtime hours. Hourly pay will also decline if overtime hours fall as a proportion of total hours worked: this will reduce average hourly pay since overtime is normally paid at a premium. Shift pay could be responsible for earnings falls if workers worked unusual hours last year but not this, as work at unusual times of day is also often paid at a premium.

[7] This figure is lower than reported in Smith (2000) because in that paper observations where the pay level is imputed, calculated from net or subject to obvious error are dropped.

in hourly pay. This might reflect the rise in hours, but it might also reflect the well-documented measurement error in hours (see Bound and Krueger (1991) and Bound, Brown, Duncan and Rodgers (1989)). The tails of the monthly pay growth distribution are fatter than those of the hourly distribution, suggesting that some large errors in monthly pay growth stem from changes in hours.[8]

The proportion of workers who take pay cuts is surprisingly large. This degree of downward wage flexibility suggests a relatively flexible labour market: if workers are willing to take pay cuts, the unemployment effects of negative shocks would be minimal. Nevertheless a substantial fraction of workers have exactly zero nominal pay growth in their monthly pay. This "zero spike" could suggest – in contrast to the implication of the apparent wage cuts – that significant nominal rigidity might be a feature of the UK labour market.[9] We thus have potentially conflicting pieces of evidence from the same data source: frequent wage cuts indicating flexibility, but notable zero spikes indicating rigidity. This in part motivates two aims of this paper: first, to assess whether the wage cuts are believable and, second, to investigate whether the flexibility–rigidity contrast reflects a two-piece labour market, which might also be reflected in a cut–rise split.

How do the statistics presented above compare with data from other UK and overseas sources? Nickell, Jones and Quintini (1999) provide evidence on the extent of real pay cuts in the UK using the New Earnings Survey (NES). These worker-level data should be almost free of measurement error as they result from a survey of employers (although they relate to 'latest' pay which may include volatile elements). They find that during 1992–96 – almost the same time frame as in this paper – 9.5% of stayers have real hourly wage cuts in excess of 10% and 4.3% of stayers suffer real cuts over 30%.[10] They also find that the frequency of

[8]Part-timers and part-time/full-time status switchers are particularly likely to have large hours changes. Analysis of outliers indicates that 44% are associated with part-time working both years and a further 22% with status switching ('outliers' are defined for these statistics as nominal monthly pay cuts exceeding 50% or rises greater than 100%, which account for about 1% of observations at each end of the pay growth distribution).

[9]Smith (2000) investigates this further and finds that up to 90% of the zero spike can be attributed to symmetric, menu-cost-type factors such as long-term contracts and rounding behaviour and measurement error. The unexplained 10% would then be the maximum attributable to downward nominal rigidity.

[10]NES figures rise to 14.4% and 7.9% respectively for continuously-employed movers, and to

cuts is higher in the 1990s than in the previous decade. For stayers alone, raw BHPS data suggest that 20.0% have real hourly pay cuts greater than 10%, and 4.7% take cuts over 30%.[11] The difference between BHPS and NES figures may reflect several factors. The NES figures are based on 48,502 stayers; BHPS figures are based on 17,096 stayers. The NES does not cover workplaces with fewer than 25 employees. This is potentially a serious flaw, since it seems likely that workers in small workplaces might well be most prone to wage cuts. Some may have a significant financial interest in their firm, such that they are willing to take cuts to keep the business running. Other small workplaces might employ largely temporary or casual workers – low-paid, with little human capital or bargaining power. Finally, the BHPS figures might reflect measurement error. The figures suggest that if this is the case, measurement error is worse for hourly than for monthly pay, in accordance with additional measurement error in hours. However, the BHPS measurement error-free pay-slip-seen subsample also shows more frequent cuts than the NES. Among stayers in the BHPS sample whose pay-slip was checked both years, 16.9% have hourly real pay cuts in excess of 10%, and 2.9% over 30%.[12]

US data from the individual-level Panel Study of Income Dynamics (PSID) show that 43% of all workers report real cuts – very similar to BHPS figures – but only 19% take nominal cuts (Lebow, Stockton and Wascher (1995), using data covering 1971–88).[13] As in the BHPS, PSID data show that real cuts are slightly more frequent among stayers (44%), and nominal cuts are less common (18%). Figures from the US Current Population Survey (CPS) are similar, indicating that nominal cuts affect between 12% and 20% of all hourly-paid workers, depending on the location of the pay growth distribution (i.e. on median pay growth) (Card and Hyslop (1997), using data from 1979 to 1993).

As indicated in Section 1, a number of recent papers have argued strongly against the reliability of this type of individual-level panel survey data in which

40.4% and 27.9% for movers with an intervening spell of unemployment (Nickell, Jones and Quintini (1999)). The authors do not report the overall proportion of cuts.

[11] Figures for stayers' monthly pay are 15.8% and 3.7% respectively. Table 1 in this paper refers to all workers.

[12] Figures for monthly real pay are 11.5% and 2.3% respectively.

[13] These statistics are weighted to take account of sample selection and non-response.

workers are asked their pay level in consecutive waves. One of the best sources of evidence against downward nominal wage flexibility is Akerlof, Dickens and Perry (1996). They present a battery of alternative data sources which all suggest that nominal pay cuts are rare. Some ethnographic survey evidence (Bewley (1995; 1998); Blinder and Choi (1990)) suggests managers are unwilling to cut pay due to adverse morale and motivation effects, and only do so in extreme circumstances. Blinder and Choi also find evidence of money illusion, which accords with well-known psychological evidence from Kahneman, Knetsch and Thaler (1986), experimental evidence (Fehr and Tyran (1999)) and questionnaire surveys (Shafir, Tversky and Diamond (1997)): most people consider nominal pay cuts unfair except in extreme circumstances, although most do not consider an equivalent real reduction that did not involve a nominal cut unfair. Union contract data hardly ever show pay cuts – although Akerlof, Dickens and Perry (1996) find one year (1983) when 15% of all private sector union settlements monitored by the US Bureau of Labour Statistics involved nominal cuts in the first year (p. 9). Recent analysis of the UK Confederation of British Industry (CBI) Pay Databank shows that between 1979 and 1998 not a single settlement (among 1500 observations on unionised and non-unionised settlement groups) was reported by managers to have resulted in a nominal cut in the earnings (including bonus payments) of a typical employee (Brown, Ingram and Wadsworth (1999)). Many settlements involve *ex post* real cuts: up to 90% in times of high inflation and recession (1991–92), but as few as 3% in times of low-inflationary growth (1987–88).[14]

Two recent papers specifically attempt to remove measurement error from individual survey data. Akerlof, Dickens and Perry (1996) conducted their own telephone survey for the Washington area. Only 2.7% of the 409 respondent 'stayers' said their basic pay had fallen over the last twelve months. Slightly at odds with this was the fact that 14.7% of them knew someone who had taken such a cut. Akerlof *et al.* showed that if one believes their telephone survey accords with the truth, measurement error can explain virtually all cuts in individual

[14]It matters quite a lot whether the inflation rate over the last year is used, or that over the period of the settlement. These figures use the former.

survey data such as the PSID and BHPS. Adding a normally-distributed random error (of reasonable standard deviation and affecting a reasonable proportion of observations) to their survey-derived pay growth distribution produced a dirtied distribution with more apparent cuts than are found in individual panel surveys. Altonji and Devereux's (1999) econometric technique showed that, under certain assumptions about the form of the measurement error, the true probability of a nominal cut was between zero and 3.5%.[15] Many of the errors were by implication made by workers whose nominal pay was in fact rigid; their corrected estimates of nominal rigidity range from 13% to 43%. Altonji and Devereux also examined the personnel files of a large financial corporation and found virtually no occurrences of nominal cuts in basic pay that did not involve a change in part-time status or bonus payments.

Those who wish to defend individual data face tough opposition. It may prove an impossible task. But this paper aims to do what it can to investigate whether all those cuts really are just an illusion.

4. Do reported pay cuts reduce satisfaction?

Reported job satisfaction should accord with reported pay growth.[16] The question of causality is difficult – are unhappy people more likely to suffer cuts because of their attitude? – but does not need to be addressed here.[17] All we want to do is check that the expected relationship exists. We investigate both overall satisfaction and satisfaction specifically with pay as checks on the believability of pay cuts. We expect satisfaction with pay to be particularly related to pay growth.

[15] Altonji and Devereux's method assumes that measurement error is normally distributed and only applies to some observations. They admit this ignores the consequences of rounding of pay levels and the possibility that measurement error is correlated with the wage. See Smith (2000) for an analysis of the former.

[16] It is possible that workers have a wrong idea about their pay which affects their perceived satisfaction, in which case there will be correlation between the errors in both. Workers are asked about satisfaction in the same part of the BHPS questionnaire as they are asked about pay. But it seems more likely that reported satisfaction reflects workers' actual circumstances, whereas their pay might be more likely to contain error.

[17] The statistics in this section relate to variations across individuals as well as for a given individual. Causality is more difficult to assess using pooled data than if we used only 'within' variation. This does not affect data-reliability implications, but it does mean that insofar as they relate to the determination of satisfaction, our results should be qualified.

Pay levels will also matter, as will pay history, but the most recent growth rate should significantly affect satisfaction. Given the nature of the BHPS questionnaire, overall satisfaction here relates to the worker's job, so overall satisfaction is also likely to reflect pay growth. All satisfaction indicators are measured on an ordinal scale from 1 to 7, with 1 representing "not at all" satisfied and 7 representing "completely" satisfied.[18]

It is rare for workers to report themselves "not at all" or "not" satisfied overall. Table 4 reports the relevant proportions. Only 4.4% of the more than 23,800 for whom we have pay growth data are not at all or not satisfied (categories 1 and 2). Workers are much more likely to report themselves "completely" or "almost completely" satisfied: just over 60% do so (categories 7 and 6). A greater proportion of those who take pay cuts are not satisfied compared with those who enjoy rises (for example, 4.8% of those who take real hourly cuts are not satisfied compared with 4.1% who experience real hourly rises, which represents a difference significant at the 1% level). Also, a significantly smaller proportion of those who take cuts report themselves satisfied.

Dissatisfaction *with pay* is much more common. 11.3% of all workers report themselves not at all or not satisfied with their pay, compared to 40.1% completely or almost completely satisfied. Again dissatisfaction with pay is more common, and satisfaction with pay less common, among workers who apparently take pay cuts. For example, 12.8% of those who report real hourly cuts are dissatisfied compared with 10.2% of those who have real hourly rises. 37.5% of those who report real hourly cuts are satisfied compared with 42.0% of those who enjoy rises.

A simple ordered probit of overall satisfaction on a binary dummy indicating whether a worker had a cut or a rise shows that taking a pay cut significantly reduces satisfaction, as would be expected given the tests for differences in proportions described above.[19] Some might be surprised that these differences, although

[18] See Section 2 for a description of the satisfaction data.

[19] Following a cut in nominal monthly pay, for example, the probability of a worker being not or not very satisfied (categories 1 and 2) rises by 14% (0.6 percentage points), whereas the probability of their being completely or almost completely satisfied falls by 4% (2.4 p.p.). The probability of dissatisfaction *with pay* increases by 21% (2.2 p.p.), and the chance of satisfaction declines by 10% (4.3 p.p.). For both measures of satisfaction, the effects of a nominal, real, monthly and hourly pay cuts are similar.

significant, are not larger.

An ordered probit of overall satisfaction on the continuous variable 'pay cuts' (i.e. on pay growth, restricting the sample to those who report cuts) suggests that those who take larger cuts are less satisfied (e.g. for nominal monthly, coefficient -0.37, z statistic -4.9). Surprisingly, larger pay cuts appear to have a less significant effect on satisfaction *with pay* (e.g. for nominal monthly, coefficient -0.13, z statistic -1.8). This pattern of a significant reduction in overall satisfaction associated with larger pay cuts, but no significant (at 5%) reduction in satisfaction with pay, is repeated for all pay measures (nominal and real, monthly and hourly). These statistics suggest that reported pay cuts are not entirely spurious, but the lack of satisfaction-with-pay response to larger pay cuts remains surprising.

We investigate the relationship between satisfaction and the size of the cut in Tables 5a and 5b, which report ordered probit coefficients from regressions of various aspects of satisfaction on the size of nominal pay cuts (monthly and hourly respectively).[20] The base case to which all effects are relative is a worker who has a pay rise (of any size) or exactly zero pay growth. The regressors are dummy variables taking value 1 when the pay cut is at least as big as the first value and smaller than the second. For example, the coefficient on '0-1%' gives the effect on satisfaction of a pay cut up to 1% compared to no cut, while the coefficient on '1-2%' gives the effect on satisfaction of a pay cut greater than or equal to 1% but less than 2%. Larger cuts are grouped together into broader point ranges, as there are relatively few of these large cuts. The tables show the number of workers who suffered cuts in the specified range during 1992–97 (who also reported overall job satisfaction).

It is predominantly smaller reported pay cuts (of less than 10%) which are significantly associated with reduced overall satisfaction. Large reported cuts typically leave overall satisfaction unchanged, or even in a few cases are estimated to raise overall satisfaction. Certain larger cuts – for example, 15% and 20%

[20] Coefficients significant at the 5% level are highlighted. We acknowledge the rather arbitrary nature of this choice of significance level; the intention is simply to ease observation of the pattern in the data. Ordered logit (and indeed ordinary least squares regression) gave very similar results. It also makes little difference whether or not Huber/White/sandwich robust standard errors are used.

declines in nominal monthly pay – significantly reduce overall satisfaction. Large cuts are more likely to have a detrimental effect on satisfaction with pay, but again most of the significant reductions in satisfaction with pay result from small pay cuts. Small real cuts – involving nominal rises – are associated with satisfaction declines of the same size as nominal cuts (average inflation over 1992–97 was 2.8%). This might surprise those who believe downward rigidity is prevalent: if it were, we should find that nominal cuts make people feel much worse than real cuts involving no nominal fall. We explore this further in Section 4.1.

We control for age, age squared, number of years' education, marital status, race, gender, health and non-labour income.[21] These have all previously been found to affect satisfaction. Cantril (1956) asked an open-ended question about what people wanted in life. Three-quarters valued their material well-being, half valued family concerns and one third valued health. Recent econometric work has found that satisfaction also varies with education, gender and race (see Blanchflower and Oswald (2000)).

We find that overall satisfaction declines with age until the early thirties, then rises, exhibiting the U shape found previously in panel data by Blanchflower and Oswald (2000). More educated people are less satisfied overall, which would be consistent with higher, unfulfilled, aspirations. Married people are more satisfied, as are women. Non-whites are less satisfied. A larger non-labour income raises satisfaction. People in poor health are less happy. The magnitudes of our coefficients are in most cases similar to those previously found by Blanchflower and Oswald (2000) using the US General Social Survey and the British Eurobarom-

[21] There is some debate in the satisfaction literature about the merits of controlling for other factors when, for example, assessing the effect of income. Oswald (for example, Blanchflower and Oswald (2000)) is strongly pro-controls; Easterlin (2000) believes they are not necessary. The issue is not straightforward when it comes to pay (or income). Pay is affected directly by the factors we would use as controls, such as age, health, marital status, and so on. The inclusion of controls means that we capture the *ceteris paribus* effect of pay – the effect of pay holding age, health, marital status etc. constant – rather than the effect of *any* change in pay including those associated with changes in the controls. For the purposes of this paper it may be best to include controls. We want to capture the common-sense idea that people are less happy when their earnings fall. Perhaps people expect their pay to fall as they get older or as their health declines, in which case satisfaction with pay might remain constant even though pay has fallen (for these reasons). The inclusion of controls raises the size and significance of the negative effect on satisfaction with pay of larger pay cuts.

eter survey (they disaggregated some variables more finely and did not include health). The major exception is education; we find a negative effect, whereas the only coefficient reported by Blanchflower and Oswald (for the United States) is positive. Our results for satisfaction *with pay* differ from overall satisfaction in that more education *raises* satisfaction with pay and age makes no difference. The excess happiness of women is slightly lower regarding their pay than overall. The negative effect of ill health on pay satisfaction is also a bit lower than on overall job satisfaction. Non-whites are much more unhappy, relative to whites, with their pay than with other aspects of their jobs.[22]

The most surprising part of our findings is the absence of evidence that the reduction in satisfaction is greater for larger pay cuts. This is seen most clearly in graphs of the estimated coefficients (see Figure 1). The coefficients remain roughly constant and negative up to cuts of around 20% (we have shown standard error bands around coefficients for overall satisfaction and nominal monthly pay for illustration). In fact, the hypothesis that there is no difference between the coefficients over this range cannot be rejected.[23] As cuts get larger above 20%, coefficients relating to overall satisfaction become less negative – the pattern is upwards, with increased volatility.[24] For nominal monthly pay and overall satisfaction, the average coefficient for cuts of size 1-20% is -0.12, which is significantly lower than the average of the 12 coefficients for larger pay cuts of 0.03.[25] The main difference between overall satisfaction and satisfaction with pay is that for satisfaction with pay, as cuts get larger above 20% they become increasingly volatile rather than less negative.[26] There is more sign of decreased satisfaction with larger cuts in monthly pay than in hourly pay. The average overall satisfaction-

[22]Women are 17% (6.1 p.p.) more likely than men to say they are completely or almost completely satisfied with their pay, compared to a differential of 21% (11.6 p.p.) for overall job satisfaction. Healthy-poorly differentials are 10% for pay and 14% overall. The proportionate differences between whites and non-whites are 22% for pay and 10% overall.

[23]For example, for nominal monthly pay and overall satisfaction, the average coefficients over 1-5%, 6-10%, 11-15% and 16-20% are -0.13, -0.10, -0.13 and -0.11 respectively; p-values for χ^2_1 tests of equality of these averaged coefficients range from 0.45 to 0.99.

[24]The rise in volatility is especially marked for nominal pay due to smaller samples.

[25]For this test, $\chi^2_1 = 17.97$, p-value less than 0.001.

[26]For nominal monthly pay and satisfaction with pay, the average coefficient for cuts 1-20% is -0.15, which is no different to the average of the 12 coefficients for larger pay cuts of -0.11 ($\chi^2_1 = 1.72$, p-value 0.19).

related coefficient for hourly pay cuts over 20% is 0.7, which is much more positive than the 0.03 for monthly pay.[27] Overall, then, what should we conclude from these results? One possible conclusion is that they indicate that many large cuts are spurious, and small cuts may reflect reality. Essentially, that the fact the fact that pay data contain error confuses the relationship between satisfaction and pay change where error is prevalent.

Older workers might be responsible for some of the happiness rise with larger cuts. Perhaps some older workers are happily having some sort of demotion, taking a lower wage before retirement, for a quieter life. We can investigate by allowing all coefficients to differ between younger and older workers (the latter defined as aged fifty and above) (results, not reported, refer to nominal monthly pay). Pay cuts of almost any magnitude depress older workers about their pay less than younger: almost all interaction terms involving cut dummies are positive (two are insignificantly negative).[28] Large pay cuts of most sizes significantly reduce the satisfaction of younger workers with their pay, although there is still no sign that large cuts matter more than small cuts. Older workers are significantly happier (in terms of pay) to accept large cuts. Results for overall satisfaction differ, though: there appears to be no difference in the impact of pay cuts on job satisfaction in general between young and old workers, possibly because of some other compensating change in the job. Results relating to the controls (for overall satisfaction) are interesting. Older women have greater excess happiness over comparable-aged men than their younger counterparts. Older non-whites are no different to younger. The old place much more value on non-labour income, but – perhaps surprisingly – react no differently to sickness. Older people are a little less happy to be married.

We also investigate whether women differ from men in their response to pay cuts. We find that women's level of dissatisfaction with (nominal monthly) pay cuts does not differ from that of men (results not reported). Other results (for overall satisfaction) are worth comment. Women have a lower-curvature U shape

[27] For satisfaction with pay, the hourly-pay coefficient is -0.05, compared to the more negative -0.11 for monthly pay.

[28] We group cuts together in sixteen slightly wider bands. A χ^2_{16} test for their joint insignificance is rejected at below the 1% level.

relation between satisfaction and age. Interestingly, almost all the negative effect of education on satisfaction we found in the full sample comes from women: for men there is no significant effect (the coefficient magnitudes have ratio 6:1, women:men) More education even makes women less satisfied *with their pay*, whereas men become more happy with their pay (we found the latter effect dominates in the full sample). Marriage boosts both sexes' happiness, but women's by more. Non-white women are no less unhappy relative to white women than non-white men are to white men. Men and women place the same value on non-labour income. Women complain less when they are ill.

Splitting the sample into stayers and movers gives quite striking and informative results (see Figure 2). We focus on satisfaction with pay for simplicity.[29] Up to cuts of around 20%, stayers' satisfaction declines slightly, the greater the cut.[30] For larger cuts, there are markedly lower falls in satisfaction. As for the full sample, satisfaction *increases* appear to accompany very large pay cuts (the OLS slope for cuts greater than 16%, taking mid-points of bands for larger grouped cuts, is 0.004, significant at the 2% level). Data showing large cuts for stayers demonstrate likely unreliability. The failure of large cuts to adversely affect satisfaction is repeated for hourly pay.

The picture for movers differs in two major respects. First, the same cut has a more detrimental effect on movers' happiness than on stayers' (see Figure 2 and Table 6). Experiencing a 5% nominal monthly cut raises the probability a stayer will say they are dissatisfied with pay from 11% to 16%, and reduces the chance they are satisfied from 40% to 30%. These are changes of 6 percentage points (or 53%) and 10 p.p. (25%) respectively. Movers have a lower probability of being dissatisfied, but it falls faster with a cut: from 9% to 15% on a 5% cut – a rise of 7 p.p. or a huge 79%. Similarly, their probability of satisfaction falls 13 p.p. (29%) from 44% to 31%. The dispersion of pay growth among movers is high. Some job moves are accompanied by large pay rises, whereas other job changes involve pay cuts (especially where there has been an intervening spell of unemployment

[29]For stayers, the effect of cuts on *overall* satisfaction is very similar, but movers' overall satisfaction falls less as cuts get larger.

[30]The OLS regression line through the coefficients below 17% has a slope of about -0.006 for nominal monthly pay, but this is only significantly different from zero at about the 20% level.

– see Nickell, Jones and Quintini (1999)). Because of this dispersion, the fall in happiness of movers who take cuts relative to movers who enjoy rises is greater than the corresponding differential for stayers.

The fall in movers' satisfaction continues to get larger right through the scale. The slope of the OLS regression line through movers' satisfaction-with-nominal-hourly-pay coefficients is -0.003, significant at the 2% level, compared to the stayers' slope of +0.004, significant at the 1% level.[31] We speculate that some movers really do experience large cuts, whereas these are really almost non-existent among stayers. Thus large cuts represent error when they refer to stayers, but may represent the truth when they refer to movers.

We can assess whether this is the case by looking at the pay-slip-seen subsample. When error is removed we would expect to see satisfaction falling further with larger cuts, for all workers. Figure 3 confirms that this is indeed the case for the error-free pay data. (Because of reduced sample size we have to group cuts into broader bands.) Figure 3 also shows that the potentially error-contaminated data – where the pay-slips are not checked – feature an upward slope. Measurement error is clearly important and influential. But it seems there are pay cuts which may well be valid. There are cuts which significantly reduce satisfaction. Furthermore, there are some very large cuts which reduce satisfaction a great deal.

We have seen that the change in satisfaction can be used as a reliability check for pay growth data. In order for pay cuts to have economic impacts and not be associated with compensating changes elsewhere, satisfaction should fall as pay does. In almost three-quarters of cases, individuals' satisfaction with their pay does not rise following a nominal monthly pay cut (there is a roughly even split between those whose satisfaction falls and those whose remains the same).[32]

In summary, this section has confirmed empirically that pay cuts reduce sat-

[31] The nominal monthly pay coefficient is negative but only significant at the 27% level for movers, but is again significantly positive for stayers. The same pattern is repeated for real pay. For overall satisfaction there is no significant change in movers' coefficients as hourly pay cuts increase (although the point estimate from a regression through the coefficients is negative), but stayers' are again estimated to become significantly less negative (i.e. the slope is positive).

[32] There is some suggestion in the distribution of the cuts that those associated with rises in satisfaction reflect errors in reported pay. 17% of these cuts are of more than 30%, compared to 14% of those where satisfaction does not rise. 54% of cuts where satisfaction rises are greater than 10%, compared to 51% where satisfaction does not rise.

isfaction. We have investigated and resolved the surprising finding that the raw data show – markedly for those that remain in the same job – that this did not hold for large cuts. We have interpreted this as implying that measurement error in large cuts is severe, especially for stayers. Other results in this section have confirmed and elaborated on previous correlates of satisfaction.

4.1. A test of downward nominal rigidity

The notion of downward nominal rigidity relies fundamentally on the hypothesis that workers dislike nominal cuts. Bewley (2000) has recently emphasised the adverse satisfaction effects of a nominal cut. He reports findings from interviews with over 300 businessmen, union leaders, job recruiters and unemployment counsellors in the north-eastern United States during the recession of the early 1990s. Bewley concludes that it is *managers* who are responsible for downward pay rigidity.[33] Managers do not want to cut pay because they think this would reduce morale. The morale of those who suffer cuts is reduced by the decline in their standard of living and a reduction in self-esteem (both of which seem likely to be particularly marked following a nominal, rather than a real, pay cut). The fall in morale raises staff turnover and has an adverse effect on productivity: morale is necessary for efficient information flows and teamwork. Firms rely on morale because monitoring is too difficult even to rely on threats or financial inducements. This is very similar to the gift exchange model of Akerlof (1982), and to related efficiency wage models.[34]

Bewley's evidence is persuasive. It is the decline in morale following a nominal cut that is important. Previously the best evidence that nominal cuts are disliked

[33]Nominal rigidity is not due to unions, nor insiders, nor implicit insurance contracts, nor efficiency wages insofar as they are supposed to act as a worker discipline device. Bewley also finds that workers have little idea of comparators' pay, and argues that this goes against Keynes' (1936) idea that downward rigidity results from a concern not to lose out relative to others.

[34]Bewley argues that morale is lowered less by layoffs (only the laid-off suffer) than by pay cuts (stayers suffer). Bewley's theory implies cuts are more likely when labour demand is price-sensitive, i.e. when competition is greater. In addition, cuts are more likely when workers do not need to become attached to the firm (i.e. where morale effects on productivity are lower), such as for those on short-term contracts. We shall see in Section **??** that in Britain during the 1990s there was little evidence of short-term contracts or temporary labour being associated with downward flexibility.

has come from laboratory experiments (see, for example, Fehr, Kirchsteiger and Riedl (1993); Fehr, Gachter and Kirchsteiger (1997)) and other surveys (Blinder and Choi (1990)). The BHPS is, to our knowledge, one of the very few panel surveys (with the GSOEP) which includes data on both pay and satisfaction. Thus it provides a great opportunity to test this fundamental tenet of downward nominal rigidity.

In the satisfaction regressions reported above we combined the nominally rigid with those that had rises (for nominal pay), or we treated them in exactly the same way as others who received very small nominal cuts or rises but fell in the same 1-percentage-point interval (for real pay). Downward nominal rigidity implies that the well-being of the nominally rigid should differ significantly from those with negative pay growth.

We compare the relative satisfaction of the nominally rigid with others by regressing satisfaction on a dummy for a nominal rise (of any size) and cut dummies (see Table 7). The omitted category is nominal rigidity (1,084 of the cases used in the regression have nominally rigid monthly pay). Satisfaction should be significantly lower for those who take nominal cuts than for this base case. We use the same controls as in Table 5. We include similar cut dummies to those shown Table 5, but we group cuts below 25% into 5-percentage-point bands for simplicity.[35] We report only figures relating to monthly pay; our conclusions are unchanged when hourly pay is used. The reported results refer to all workers. Results for stayers are no different (except that, in accordance with Figure 2, satisfaction is significantly higher among stayers who report very large cuts). 953 of the 1,084 nominally rigid remain in the same job.[36]

Overall satisfaction is significantly lower for those who suffer cuts than for the nominally rigid. The reduction in satisfaction appears greater for those with small nominal cuts of less than 4% than for those with larger cuts, however. Predicted probabilities show that those taking small cuts are 19% more likely

[35] We have seen above that there is little difference between the coefficients in this range. It would be possible to group all cuts together in a single dummy, but this would risk contaminating our results with the measurement error discovered above to affect large cuts.

[36] It is surprising that 131 movers report no pay change. We speculate that this might be due to rounding and measurement error (see Smith (2000)).

to be completely dissatisfied with their job than are the nominally rigid, and are at least 20% less likely to record any positive level of job satisfaction – they are 37% less likely to be completely satisfied, for example (see Table 8). But satisfaction *with pay* is no different among those who suffer nominal cuts than among those whose pay does not change. No range of nominal cuts leads to significantly different satisfaction with pay compared to nominal rigidity. When we separate the lowest cut range by percentage point, all four cut magnitudes reduce overall satisfaction (results not shown). The strongest effects come from cuts of magnitudes 3–4% and lower than 1%. Again we find no small cut has significantly different satisfaction-with-pay effects from nominal rigidity.

The difference in overall satisfaction between those who have nominal *rises* and those who have no nominal pay growth is insignificant, which is surprising: the raw data imply that nominal rises do not make workers better off. But satisfaction with pay is much higher among those who enjoy nominal rises than among the nominally rigid.

We conduct several experiments to test the robustness of these results. First, it might be argued that it is the change in morale for individual workers that should be affected by pay cuts. But those who are nominally rigid are found to be no different in the way their satisfaction changes from one year to the next, compared to those who suffer pay cuts (results not shown). In contrast, workers whose pay rises have a greater chance of becoming more satisfied than the nominally rigid. These results hold for overall job satisfaction as well as satisfaction with pay. Second, if we eliminate cases which might be affected by measurement error, we find absolutely no evidence that pay cuts make workers unhappy relative to those whose pay remains fixed in nominal terms (see Table 7).[37]

Third, an objection might be raised that we only have data on total earnings. Perhaps people don't mind if their earnings decline because they are working less overtime than last year, for example. Perhaps they don't even mind when their bonuses are cut. We can investigate what happens if we consider only cases where overtime and bonuses are not responsible for pay cuts.[38] We also here report

[37] Cut bands are widened due to reduced sample size. 192 workers who checked their pay-slips both waves had no change in nominal monthly pay between one interview and the next.

[38] See Table 2 and the discussion in Section **??** for details. We also consider only cases where

results for hourly pay on similar grounds: that people might not mind earnings reductions that are simply due to fewer hours being worked.[39] The results, shown in Table 7, are quite similar to those for the full sample, albeit slightly weaker. Overall satisfaction is lower for those who take small cuts than for the rigid, but there is no sign of such a relationship for satisfaction with pay. But again, once measurement error is controlled for, the negative effect of small cuts on overall satisfaction disappears.

In summary, there is mixed evidence that British workers who suffered pay nominal cuts were worse off than those whose pay did not change in nominal terms. We found that overall satisfaction was lower among those who took cuts, and this applied to cuts which likely involved basic hourly pay as well as to total earnings. However, this effect disappeared when we controlled for measurement error. Given the importance of measurement error, this is not encouraging (one defence might be that the pay-slip-seen sample could be unrepresentative). Although overall job satisfaction will capture 'morale' effects, it is surprising that there is no sign of more dissatisfaction with pay among those who take cuts compared to the nominally rigid. We conclude that there is not a great deal of support from these data for the fundamental assumption behind downward nominal rigidity. This should not be taken to imply that we do not believe downward rigidity exists. We simply pose the question: why, if it exists, is the relationship it relies on (between cuts and morale) not more strongly evident in these data?

5. Do correlates imply voluntary or involuntary pay cuts?

We now turn to econometric models of pay cuts. These will identify factors associated, in some cases causally, with pay cuts. In addition, we can explicitly include factors inducing error as explanatory variables and assess their influence. Alternatively, we can drop cases we believe to be affected by error.

We assume there is some unobservable propensity to take a pay cut, y^*_{it}, given by

overtime and bonuses are not responsible for nominal rises, although this should not affect the cut–rigid comparison.

[39] As noted above, however, results for hourly pay are very similar to those for monthly pay.

$$y_{it}^{*} = \mathbf{x}_{it}'\beta + \upsilon_{it} \qquad i = 1, 2, ..., n \text{ and } t = 1, ..., T. \tag{5.1}$$

What we observe is

$$y_{it} = 1 \text{ if } y_{it}^{*} > 0 \text{ and 0 else.}$$

Assuming normality of the errors, we have a probit model of pay cuts. We assume the same model structure applies to nominal and real, monthly and hourly, cuts. The factors influencing the propensity to take a cut in nominal terms may of course differ from those influencing the propensity to take a real cut. For the pooled probit, the marginal effect for continuous variable x_j is given by

$$\frac{\partial \left[\Pr\left(y_{it} = 1 \mid \mathbf{x}_{it}\right)\right]}{\partial x_{jit}} = \frac{\partial \left[\Phi\left(\mathbf{x}_{it}'\beta\right)\right]}{\partial x_{jit}} = \phi\left(\mathbf{x}_{it}'\beta\right)\beta_j.$$

For continuous variables, the means of the x_{it}s are used. The pooled probit marginal effect for categorical dummy variable x_d is calculated as the difference in the predicted probability in the presence of the relevant characteristic compared to its absence: $(\mathbf{x}_{it}'\beta^{*}|x_{dt} = 1) - (\mathbf{x}_{it}'\beta^{*}|x_{dt} = 0)$ (the means of all other variables are used). We also estimate random-effects probit models.

Our binomial classification of pay growth assumes that the same factors are responsible, to the same extent, for all cuts no matter what size. This might be inaccurate. The size of the cut might matter. We consider this particularly likely for *real* cuts, given the evidence for downward nominal rigidity found elsewhere.

The sample of job stayers whose pay is observed in two consecutive periods is clearly selected. Decisions during the year concerning changing job (and thus tenure) will depend on the wage growth that either did or did not (if the worker moved job) materialise. The effective selection criteria are that the individual be an employee in two consecutive periods, that their pay is reported in those periods, and that they remain in the same job. We are particularly interested in controlling for biases that might be induced by workers leaving their previous job because if they had stayed their pay would have been cut. In that context, individuals are de-selected if they change job or if they move from employment to unemployment, self employment or out of the labour force. Out of the labour

force includes retired from paid work altogether, on maternity leave, looking after family or home, full-time student or at school, long-term sick or disabled, on a government training scheme, or some other situation. We also de-select job stayers whose wage is not observed in one period. In order to control for selection bias we can estimate a sample selection model. The propensity to be selected according to our criteria is

$$y_{it}^{*select} = \mathbf{z}_{it}'\gamma + \varepsilon_{it} \qquad \text{where } (\upsilon_{it}, \varepsilon_{it}) \backsim \Phi_2(0, 1, \rho)$$

where Φ_2 is the cumulative distribution function of the bivariate standard normal. As before, we only observe the selection outcome

$$y_{it}^{select} = 1 \text{ if } y_{it}^{*select} > 0 \text{ and } 0 \text{ else.}$$

Some of the factors $\mathbf{z}_{it}$ which determine the probability of selection (i.e. the probability of changing job, becoming unemployed or self employed, leaving the labour force, and failing to report pay) may be the same as those that affect the likelihood of taking a pay cut in the current job. Some will clearly relate to the individual's job last year.[40] In addition, we hypothesise that selection is affected by commitments preventing job search or preventing a new job being taken during the last year.

The basis for our model of pay cuts is a human capital wage equation, taking into account that we are modelling pay growth rather than levels. We expect the propensity to take a cut to be raised by negative productivity shocks. We allow the possibility of heterogeneity in preferences, constraints and institutional arrangements determining willingness to accept and ability to resist pay cuts. These might relate to an individual's attitudes and opinions, mobility, characteristics of the job, occupation, firm, industry and location. Firms may also differ in their desire to allow the possibility of pay cuts. Again institutional arrangements and job, occupation, firm, industry and regional characteristics might capture this. Factors which increase human capital and factors which raise bargaining power or fall-back options can be termed 'do-better' factors: they will generally reduce the

[40] In fact, we have to use job-related variables dated $t-1$ because otherwise these are missing for non-employees at t.

probability of cuts. In contrast, negative productivity shocks and mobility constraints are 'do-worse' factors: they will typically move workers down the wage growth distribution. Company characteristics and preferences may have a variety of effects which are discussed below.

There are also statistical, data-related factors we need to take into account. Measurement error takes various forms: reporting error (and recording error), imputation error and other calculation error (specifically calculation of gross pay from net values), and features of the data relating to the timing of surveys. Classical measurement error will increase actual and apparent volatility in wage growth. Rounding, which can form part of measurement error or might be due to menu costs or near-rationality, will (over some period) increase nominal rigidity and reduce cuts, as will long-term contracts.

6. Results

Table 10 reports the marginal effects and standard errors relating to maximum likelihood coefficients for pooled probits. We first discuss results for monthly pay, ignoring the possibility of error, the issues of non-basic-pay elements and hours changes. We then deal with each of these. In discussing the monthly results, it should be borne in mind that rather than modelling pay cuts we could be constructing an elaborate model of the incidence of measurement error, overtime, bonuses or hours changes.

Human capital Human capital theory suggests that age, experience, job tenure, education, qualifications and recent training will all raise wage levels. Their effect on wage growth is in some cases more complex.

Training related to the current job, and possibly general training, will raise productivity and hence increase the probability of a pay rise (for previous evidence see, for example, Arulampalam and Booth (1998)). We find that having had on-the-job training during the last year does reduce the chance of a real cut – by around 7% – but has no effect on whether or not a nominal cut will occur. Other training has no significant effect.

Productivity-enhancing human capital can be acquired through labour market

experience (which will be correlated with age). But age- and experience-earnings profiles are notoriously convex. Real earnings growth is fastest for the young, peaking (implying real stability) in mid-to-late working life, then falling (implying real wage cuts) as the worker nears retirement (see, for example, Card (1998) for a recent survey and Mincer (1974) for a classic exposition). The existence of an 'age plateau' will mean that pay rises fastest for the young (less experienced), whereas older (more experienced) people are more likely to have rigid pay or even pay cuts. This reflects decreasing returns to labour market experience and negative productivity or productivity growth effects of increasing age. We use age dummies to capture non-linearities.[41] Results clearly confirm that being young helps avoid pay cuts. Those under twenty-five are 6% (11%) less likely, and those between twenty-five and thirty-five 2% (4%) less likely, than those aged over thirty-five (but more than five years to retirement) to experience nominal (real) cuts. As we saw in Section 4, older workers do not mind cuts as much as the young. Those within five years of statutory retirement age are 2-3% more likely than the middle-aged to take cuts (although these effects are only significant at the 20% level or above).

Pay will rise fastest for those whose tenure is short, reflecting the faster rate of skill accumulation of those who recently started a job. However, some previous work has found tenure to have no significant effect on wage growth (see Arulampalam and Booth (1998); Booth (1993)).[42] We find that those who have been in the job less than two years are less likely to take cuts (4% for nominal pay, 7% for real). Those with tenure between two and five years are 1% less likely to suffer nominal cuts and 3% less likely to suffer real cuts.

The amount of education possessed by an individual is likely to be positively correlated with their ability to acquire productivity-enhancing skills. Earnings growth for the young is faster the higher their education, and the tail-off is later and less (see Card (1998)). During the 1990s there has been a continuation of

[41] The sample is restricted to individuals between 16 and 65 inclusive.

[42] Tenure, like actual labour market experience, is problematic in that it is potentially endogenous (see Dustmann and Meghir (1999)). Dustmann and Meghir benefit from a unique dataset including complete employment history which allows them to control for the endogeneity of tenure. We do not have this information, so the best we can do is control for selection into staying in the job.

the well-documented rise in the returns to skill. Both demand and supply-related explanations have been put forward; in either case it will show up in the data as a lower probability of wage cuts for more highly-educated individuals. Our results confirm that those who left school or further education later – a usual measure of the level of education – are less likely to suffer nominal cuts.[43] Each extra year of education makes it 3% less likely that the individual will suffer a nominal cut. Interestingly, though, the level of education seems to have no significant effect on the probability of real cuts.

When qualifications are substituted for our education measure, we find that certain qualifications offer more protection against cuts than others (results not reported; the effects of other variables are unchanged). Those with a first degree, another higher qualification, A-levels or O-levels are less likely to experience nominal cuts than those with no qualifications. The differences ranged from 7% to 4% for nominal cuts and 5% to 3% for real cuts, with the higher qualifications having more effect. A teaching qualification reduced the probability of nominal cuts by 6% but, as expected, during the 1990s offered no protection against real cuts. Nursing and commercial qualifications offer no protection against either real or nominal cuts.

Individual productivity shocks Individuals' productivity can be adversely affected by personal shocks, such as illness. Wage rises will be less likely for individuals whose health is poor.[44] Those whose health is poor or very poor are 4% (3%) more likely to suffer additionally through nominal (real) pay cuts. Young children might also affect productivity, although they may also act as a constraint on mobility (discussed further below). The presence of children aged less than two in the household substantially raises the likelihood of pay cuts (a child in this age group raises the probability by 5% for both real and nominal pay). Other

[43] Years of education is measured as age when left full-time education (further or schooling) minus five.

[44] The BHPS also indicates when respondents felt the amount or type of work they were able to do had been adversely affected by ill-health, but we never found this to be significantly correlated with the probability of cuts.

commitments might restrict workers' hours, again reducing their productivity.[45] Not surprisingly, commitments that reduce hours have a very large effect on the likelihood of monthly pay cuts (up 15% for nominal and 11% for real).

Bargaining power and fall-back options By raising worker bargaining power, trade union recognition in bargaining is likely to reduce the chance of pay cuts and raise the probability of rises. But unions may care about equity and therefore reduce the variance of pay growth. Indeed, Arulampalam and Booth (1998), using National Child Development Study data, found that unions contribute to lower wage growth, while Booth and Frank (1996) suggest that unions act to flatten age-wage profiles. Conditional on workplace union recognition, whether or not an individual is actually a member of a union (the recognised one or another) should not affect their bargaining power: typically a bargain achieved by a recognised trade union will apply to all workers, non-members as well as members. But union membership might confer informational advantages or might indicate a worker's degree of determination to obtain pay rises. Surprisingly, our results suggest that unions do not reduce the chance of nominal pay cuts, although recognised unions might help prevent real cuts. Being a member of a trade union seems to be associated with increased chance of real cuts (union members are estimated to be 2% more likely to experience real cuts than non-members).

Being married and having children are likely to reduce mobility. Both these involve joint location problems, with possible spousal job and with schools. Less mobile workers will have fewer options other than to accept relatively low pay rises, or even pay cuts. Their fall-back option will be lower. However, we find the empirical effect of being married to be insignificant. As noted above, very young children do seem to increase willingness to accept cuts. The older are the children, the less they adversely affect parents' pay chances. We find that those with older

[45] The BHPS questionnaire asks individuals whether "In the last year have household or family responsibilities ever (a) prevented you from looking for a job? (b) prevented you from accepting a full-time job that you were offered? (c) prevented you from changing jobs? ... (f) required you to work fewer hours?" In our empirical work we investigated all of these. We combined (b) and (c) as these both reflect commitments preventing changing jobs. We also examined the effect of commitments preventing job search (a). However, neither of these significantly affected the likelihood of pay cuts.

children may even have a better chance than the base group (the childless and those with grown-up children) of avoiding pay cuts (real and nominal).

Home owners (and possibly local authority and housing association renters) are likely to be less mobile than private renters and therefore have lower fall-back options (see Oswald (1996)). However, we find that home owners (our base case) are 3% less likely than those who rent their homes to suffer nominal pay cuts. Effects for real pay are not precisely estimated but the signs are reversed: home owners seem more likely to take real cuts. It is possible that these results are picking up the fact that home owners will tend to be in relatively stable jobs which do not feature nominal cuts (although we have controlled separately for broad occupation), but are more willing to accept smaller nominal rises than renters due to their relative lack of mobility.

An individual might be more willing to take risks and pay fluctuations if their fall-back option is higher because there are alternative sources of income available – for example, if their spouse is employed, if another household member is employed, if they themselves have a second job, or if non-labour income is substantial. This 'insurance' could have one of two effects: facing a negative productivity shock, a worker might be more likely to quit, knowing they had income during a period of search, or a worker might be more willing to take a pay cut on the grounds that the proportional impact on household income would not be so great. Whether other sources of income reduce or increase the probability of pay cuts is an empirical matter. For nominal pay, the directions of the effects are that one's spouse having a job increases the likelihood of cuts, but someone else in the household being employed reduces it (these effects are only significant at around the 20% level). Any other employee in the household reduces the probability of real cuts (by around 1%). The individual themselves having a second job has no significant effect.

Risk and bargaining power are also related to local labour market conditions. The higher is unemployment in the local region, the more likely is the worker to accept a pay cut, given imperfect labour mobility and information. We find that the level of unemployment does not affect the chance of nominal cuts, but there is some suggestion that higher unemployment increases the likelihood of

real cuts. Time effects seem very important. Nominal cuts were least frequent in 1991–92, controlling for all other factors. Relative to that year, 1993, 1996 and 1997 all featured 5% more nominal cuts (precision is not good for the 1997 estimate). During 1994 and 1995, around 3% more nominal cuts were reported. The distribution of real cuts across years is rather different. Real cuts were again (5%) more likely in 1997 but were also 5% more likely in 1995, relative to all other years. There are no significant differences across regions in the probability of real cuts, but nominal cuts are most common in Scotland and the West Midlands, controlling for all other factors.

Company and job characteristics Pay might be more stable in larger firms, who are able to insure workers against adverse fluctuations in their marginal product, stemming for example from product demand shifts and general cyclical factors (implicit contract theory would support this). Efficiency wages are higher than the competitive level, but in terms of wage growth their most important feature could be stability: efficiency wages will typically respond less to demand, supply or productivity shocks as their payment depends largely on the extra effort they elicit. Efficiency wage theory also supports upward-sloping experience-earnings profiles, which imply fewer or no cuts. Efficiency wages are more likely to be paid in large firms where monitoring is more difficult and teamwork more likely. Arulampalam and Booth (1998) find wage growth 15–16% higher in large firms (with more than 100 employees). For nominal pay, we find that those working in very small establishments employing ten people or less are about 3% *less* likely to suffer nominal pay cuts than those in very large establishments (over 1000 employees). Beyond this, we do not find the size of the firm has any impact. There is no significant difference in the probability of real cuts across different-sized establishments. One explanation might be that those in smaller establishments have a better idea of within-establishment comparators than those in larger workplaces. Combined with a dislike of relative cuts, this could cause the observed differences.

A bargaining model of wage determination might suggest that better company performance would result in higher wage growth. So workers in high-growth industries will tend to have a lower probability of suffering pay cuts, and a higher

likelihood of rises. We use industry dummies to proxy these factors. In terms of avoiding nominal cuts, it is possibly advisable to work in the Transport and Communications or Other Services industries (which are estimated to reduce the likelihood of cuts relative to Engineering – and by implication all other industries since they are found to be no different – by 4% and 3% respectively, although the precision of these estimates is not very good). Other Services appear to feature small nominal rises, as this is the industry in which workers are most likely to experience real cuts (6% more likely than Engineering). Real cuts are also relatively likely in Energy and Water, Distribution, Hotels and Catering and Transport and Communication industries.

Public sector employers are traditionally perceived as more paternalistic, reducing the chance of pay cuts. However, during the 1990s, public pay policy meant that many public sector workers' pay did not increase as fast as inflation. Nevertheless the fixed pay scales of most public sector workers mean their basic pay should not fall in nominal terms, although there might be fluctuation reflecting overtime pay, for example. Despite the figures presented in Section **??** that the proportions of cuts were very similar in the private and public sectors, it is clear that once we control for other factors, public sector workers are at an advantage. Public sector workers are 5% less likely than those in the private sector to take nominal pay cuts, and 6% less likely to suffer real cuts.[46]

Payment schemes vary by firm and by job. Reasons include technology, motivation, comparability and history. Some jobs feature incremental pay scales. This should eliminate the possibility of nominal cuts (although possibly only in basic pay).[47] However, workers who report they are on incremental pay scales are no significantly less likely to experience nominal cuts than others, but they are less likely to have real pay reductions. Overtime and bonus schemes are considered below.

Most occupations are more likely than Managers and Administrators to take

[46]The definition of the public sector differs slightly here: we include non-profit and other organisations, which were shown in Section **??** to have significantly fewer cuts than the rest of the public sector.

[47]The BHPS note an interpretation problem among respondents for this question. Some respondents took it to refer to RPI-linked pay rises. Nevertheless, we should still see a cut-probability-reducing effect.

nominal pay cuts. Craft workers are 8% more likely, Personal and Protective occupations and Operatives 7%, Other occupations 6%, Associate Professionals 3%, and Clerical workers 1% more likely. Only Sales-related occupations have a similarly low rate of nominal cuts to Managers. Only Other occupations and Personal and Protective Services are more likely than Managers to experience cuts in real terms; Professionals seem somewhat less likely to have real cuts.

Some individuals' pay will be volatile because of the nature of their jobs. Pay volatility is likely to be high for seasonal workers, those on short-term contracts, part-timers, and possibly also for those with non-standard working arrangements involving working elsewhere than the employer's premises.[48] Seasonal jobs are found to be much more likely to feature pay cuts than others (11% for nominal pay, 10% for real). Contract workers are 5% more likely, and part-timers 3% more likely, to suffer nominal cuts than permanent and full-time employees respectively (the effect for contract workers is not very well-determined). But contract workers do not suffer any more real cuts than permanent employees – presumably, then, these results reflect pay volatility for contract workers relative to permanent staff. Part-timers suffer in real terms too, though, again being 3% more likely than full-timers to take real cuts. Employees working at the establishment are no less likely than others working from home, travelling and elsewhere to experience real pay declines, but they have a 3% lower probability of nominal falls.

Exclusion Discrimination- or exclusion-forced employment in low-grade jobs would imply a reduced chance of real rises and more frequent cuts among affected workers. Women and non-white employees are potentially particularly subject to discrimination. Women have a 2% lower chance than men of suffering nominal cuts. Non-whites are almost 6% more likely than whites to receive nominal cuts. There is anecdotal evidence of employment-related discrimination against those from some council housing estates, but, as reported above, in general local authority renters' pay behaves similarly to private renters'.

Excluded workers' jobs will typically be in the private sector, which as seen above raises the probability of pay cuts. Excluded workers might not work regu-

[48] This dummy takes value zero if the individual works from home, from home and at other places, driving, travelling or other places.

lar daytimes, they may be on short-term contracts or have seasonal employment. Workers on unusual work patterns (including shift working, for example) are 2% more likely to suffer nominal cuts, but are no different from workers on regular daytimes when it comes to the chance of real cuts. We have already noted above that short-term contracts and seasonal employment lead to high pay volatility involving greater chance of nominal cuts. Excluded workers might typically be hourly-paid: salaried workers (whose stated pay period is likely to be a month or a year) might well be in 'better' jobs with fewer pay fluctuations and greater chance of pay rises than hourly-paid workers.[49] We find that those who are probably hourly-paid are more likely to suffer nominal cuts, but those who are most likely salaried workers have if anything a greater chance of real cuts (although in the latter case the coefficient on pay period is not well-determined). The chance of a nominal cut is more than 5% higher for those who state their pay per week compared to those who state an annual pay figure. Excluded workers will typically have jobs with no managerial responsibility. Not surprisingly, workers who say their job involves managerial responsibility have less chance of a pay cut, although the probability of a nominal cut falls by only around 1%. The effect of reported managerial responsibility is swept up by occupation controls when these are included, so it is omitted from reported regressions.

Opinions and preferences If workers think inflation is important this indicates they are aware of inflation. They may push particularly hard for their pay growth to match inflation.[50] We examine the impact of views of inflation's importance relative to unemployment.[51] A greater concern for unemployment could indicate a worker whose perceived fall-back option is relatively low. Workers who are more concerned about inflation than unemployment are 2% less likely than

[49] Stated pay periods range from a fraction of a week to 52 weeks.

[50] Alternatively, their concern may stem from their realisation that their pay is failing to match price rises.

[51] 'Inflation more important than unemployment' takes value unity if the individual thinks that inflation is more important than unemployment or is more concerned about "the rising price of food and other consumer goods" than "the high rate of unemployment". The variable takes value zero if vice versa or if neither or both are of equal concern. The second variant of the question is used for 1991 and 1993–1995, the first for 1992 and 1996. Neither question was asked in 1997.

others to take real cuts (significant at the 10% level) but are no less likely to suffer nominal cuts (results not reported; other variables unchanged). The former is consistent with these people being more aware of the inflation rate and wishing their nominal pay to keep pace with it.

We include regional inflation in models for nominal cuts.[52] The higher the rate of inflation, the lower we would expect the chance of anyone having nominal cuts, as the pay growth distribution will shift to the right. (This would not be true if there were perfect downward nominal rigidity and error-free data.) We find that higher inflation does reduce the probability of cuts.

Overtime and bonuses Some jobs and some firms pay overtime. Some firms operate bonus schemes, sometimes for only certain occupations. These will both tend to increase the variability of pay. Working overtime last year but not this and the scrapping of a bonus scheme might both be responsible for pay cuts. Experiencing a fall in paid overtime raises the probability of a nominal cut by 16% and the chance of a real cut by 11% (results not reported). However, the scrapping of a bonus scheme has no significant effect (and the coefficients are negative, perhaps reflecting the fact that only 'better' jobs ever pay bonuses). Being on a bonus scheme last year also has no effect (and a negative coefficient). Having worked overtime last year raises the chance of nominal (real) cuts by 6% (3%).[53]

Statistical, data-based explanations We first investigated the effect of error by including dummies for imputation, calculation of gross pay from net, a change in the degree of rounding, both pay-slips having been checked, and a continuous variable capturing the time between interviews (results not reported). BHPS gross pay data include imputed values and values calculated from stated net pay. In both cases there will be measurement error, which will be of the classical form that tends to increase variance, which since the pay growth distribution has a

[52] Regional inflation is the percentage change in regional price indices excluding housing costs (from The Reward Group "Cost of Living Report: Regional Comparisons", Table 7C: Regional indices – consumer prices).

[53] See Table 2 and the related discussion in Section **??** for the rationales behind these variables.

positive mean will increase the probability of cuts. The effect of imputation is very imprecisely estimated (p-value 0.6), but the marginal effect indicates that imputed pay (in either year) increases the likelihood of nominal cuts by 8%. (The marginal effect and significance are much lower for real pay.) Calculation of gross pay from net significantly raises the chance of cuts, in the case of nominal by 13% and in the case of real by 5%.

Measurement error will affect the data. There are two types of effect. Some interviewees might not know their exact pay and might round to what they consider the approximate amount. It is likely the tendency to be unaware of one's exact pay is persistent, reflecting individual and pay-system characteristics. It is also likely that the approximation will remain valid from one year to the next. Hence people who round in error are quite likely to appear to have rigid pay. Rounding error is not classically distributed. Alternatively respondents might simply make an error which does not involve rounding. This type of error can be assumed to be classically distributed, increasing the variance of pay growth and reducing nominal rigidity. We find that a change in the degree of rounding is estimated to raise the probability of reported nominal cuts by 4% (but also real cuts, by 5%).[54] Those whose pay statements are free of error are 4% less likely to suffer nominal cuts and 5% less likely to experience real declines.

The time between interviews varies. The '360-day effect' could (falsely) attribute nominal rigidity to individuals whose previous interview occurred after their last pay settlement and current interview occurred before the current round's pay settlement. This will only affect a small fraction of workers, as many will not have settlement dates near interview dates. But for those that do, it is most likely to affect individuals interviewed at less-than-12-month intervals, although delayed settlements could mean some individuals are affected even though their interviews are more widely spaced. In some cases individuals are interviewed at very large intervals – up to 19 months. Some of these could be affected by a counteracting '370-day effect': they might have had two pay settlements in the intervening period. We find that the time between interviews matters. Each ad-

[54]The degree of rounding relates to underlying pay. Underlying pay is the pay that was stated by the respondent in the interview (i.e. not divided by pay period or converted from net to gross).

ditional month between interviews reduces the probability that a nominal (real) cut will be reported by 1% (2%).

Random effects models We estimated random effects models, but the estimated ρ – the ratio of the variance of the individual effects to the overall variance (v_{it} in equation 5.1) – was insignificantly different from zero, so the random effects coefficients (suitably scaled – see Arulampalam (1999)) were identical to those of the pooled models.

Sample selection We investigated the impact of only observing those who remain in the same job. Our estimates appear unaffected by this selection. Consistent estimates obtained in a two-step procedure by the addition of the inverse Mills' ratio (the non-selection hazard) to the probit equation for pay cuts were almost identical to those obtained without the selection correction. (Mills' lambda was obtained from the estimation of a probit model of sample selection as described in Section 5.) The correlation between the errors of the two probit equations was small (0.08), and the effect of the inverse Mills' ratio was insignificant.

Summary In summary, there are certain factors which appear to raise the probability of both real and nominal cuts – these are 'do-worse' factors. There are other variables which affect only one of these. Do-worse factors include being older, having been in the same job longer, being male, having very young children, being ill, having commitments that restrict hours, working in the private sector, having a seasonal or a part-time job, being hourly-paid, being a trade union member. Apart from union membership and gender, these are all in accordance with theoretical and intuitive rationales discussed above. Working in Craft, Personal and protective or Other occupations, or in the Distribution, hotel and catering, Transport and communications or Other services industries, or living in Scotland or possibly the East Midlands are all adverse influences on pay growth. These industries and most of these occupations are often cited for their relatively low and volatile pay. Pay growth was also particularly low in 1994–95.

Factors which just increase the probability of a nominal cut include low education, other training, not being white, not being a head of household, the absence

of older children, renting (privately or through housing associations or the local authority), being on a short-term contract, not working at one's employer's premises, not working regular daytimes, not working at small establishments and living in the West Midlands. The proportion of cuts was higher in 1993, 1994 and 1996 than in other years. Many of these factors are suggestive of excluded or disadvantaged workers; some also indicate possible volatility in non-basic payments or hours. Regarding the influence of older children, it is possible that the resources required to keep older children mean that their parents simply cannot accept cuts.

There are few factors which raise the probability of a real cut without affecting the likelihood of nominal cuts. They include not having had job-related training in the last year, not being on an incremental pay scale, working in the Energy and water or Other manufacturing industries, and possibly higher unemployment.

7. Conclusions

This paper has investigated the extent, validity and causes of individuals' pay cuts. It is clear that pay cuts do occur, for various reasons. Certainly hours reductions lead to falls in monthly pay, unpaid hours increases reduce hourly pay, and overtime and bonus changes can also lead to cuts. These are all recognised means of achieving labour market flexibility, and the data presented here suggests they are used extensively. But they do not account for all cuts. The controversy arises over whether the remaining cuts (in essentially basic hourly pay) do actually occur, or whether they are simply due to error.

Using a pay-slip check for pay reports from a British individual panel survey, we have established that only 7% of nominal cuts which are not due to overtime, bonuses and hours changes can be validated. The figure for real cuts is 9%. This is far lower than the proportions found in 'raw' survey data (28% nominal and 41% real cuts). But we argued that relying on the pay-slip check could understate the true extent of cuts.

We used satisfaction data to check the validity of cuts. Those who take cuts should be less happy than those who don't. We confirmed that this is in general true, but found that there were rises in satisfaction accompanying large cuts.

This was identified as stemming from reporting error. Large cuts do not happen as often as they are reported, and are particularly unlikely for those who stay in the same job.

One reason for scepticism about the validity of nominal cuts is a belief that downward nominal rigidity is prevalent. This relies on workers disliking nominal cuts intensely. Using data on satisfaction, we tested whether this is true. In the raw data, overall job satisfaction did seem lower among those whose nominal pay declined, but only after small cuts. Surprisingly, satisfaction with pay did not seem lower. And after controlling for measurement error and other factors, we could find no evidence that those who took nominal cuts were worse off than those whose nominal pay did not change.

We compared cuts among public and private sector workers. Because British public sector workers' pay settlements have involved annual nominal rises, we expected to see few nominal cuts. But the proportions hardly differed between the sectors, even after controlling for overtime, bonuses, hours and measurement error. We found evidence that some public sector cuts likely resulted from contracting-out, but this was not the whole explanation. Error (other than in pay) could be responsible. But the possibility remained that cuts might simply be more prevalent than is commonly believed.

Finally, we investigated why pay cuts occur. We found many similarities in the factors which increase the probabilities of nominal and real cuts. Deterioration in human capital (being older and of longer tenure) and negative productivity shocks (illness, young children, other commitments) were found to raise the chance of a worker suffering a cut. Part-time, hourly-paid and seasonal jobs, and jobs in the Distribution, Hotels and Catering, Transport and Communication and Other Services industries were also most prone to cuts. By occupation, Craft and Personal and protective industries were most likely to feature cuts. Trade union membership was not found to protect against cuts.

Overall, this study has presented a picture of downward flexibility in pay among some workers. Some workers do not feel unhappy about accepting such cuts. Most workers who take nominal cuts appear relatively disadvantaged. It is impossible to tell whether these features existed in the United Kingdom prior to

the 1990s, but the sheer extent of the cuts and workers' reaction to them suggests that downward flexibility in pay might not be a new phenomenon.

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