The impact of downward nominal wage rigidity on quits and layoffs

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Abstract

This paper assesses the cost of downward nominal wage rigidity (DNWR) by investigating whether individual workers are more likely to be laid off after their pay has been frozen and whether nominal cuts prevent layoffs. The relationship between DNWR and quits is also investigated – in particular whether nominal cuts lead to quits. Micro data are used, in the hope of resolving the so-called 'micro-macro' puzzle: the contrast between the plentiful evidence of DNWR from micro data but the general failure of empirical macroeconomic studies to find any unemployment-related cost of this rigidity. Results suggest that DNWR does have significant economic costs: DNWR impacts on individual separation decisions in the theoretically-predicted manner.

Keywords: labour turnover, wage rigidity
JEL codes: J30, J63

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1 Introduction

The main aim of this paper is to investigate the costs of downward nominal wage rigidity by examining its impact on separation decisions. The costs of downward nominal wage rigidity (DNWR) result from the fact that DNWR holds real wages 'too high' relative to the real wage warranted by the value of the worker's productivity. The firm's labour costs are excessive, and – given nominal rigidity – the only way the firm can reduce its wage bill is to make workers redundant. DNWR should lead to layoffs. DNWR will also affect quit behaviour. Workers will be less likely to quit, given that downward nominal rigidity holds their wages higher than is warranted. The counterpart to DNWR is nominal cuts. If some firms are not affected by DNWR, resulting nominal cuts will influence separation decisions. The relationships between rigidity, cuts, layoffs and quits are discussed further in Sections 2 and 3.

Some previous work has investigated the unemployment cost of nominal wage rigidity at the aggregate level. In aggregate studies, the cost of DNWR tends to be measured indirectly, via the relationship between unemployment and real wages. The greater the extent of downward nominal wage rigidity, the more average real wage growth will exceed the market-clearing level, and so the greater the adverse impact on aggregate unemployment. The impact of DNWR can be seen in the slope of the Phillips curve and how this varies with inflation. If inflation is low, then (for any given distribution of shocks) more workers' wages 'should' fall; if these wages are held up by downward nominal wage rigidity, aggregate real wage growth will be higher than it would otherwise have been, so a bigger rise in unemployment would be needed to get real wage growth back down to its market-clearing level. Thus under DNWR, the Phillips curve will be flatter at low inflation. There have been a number of studies that have investigated this.[1] Surprisingly, however, many of these have not found substantial or robust evidence for macroeconomic effects of DNWR (an exception is Akerlof, Dickens and

[1] Akerlof, Dickens and Perry (1996), Card and Hyslop (1997), Lebow, Saks and Wilson (1999), Fares and Lemieux (2000), Knoppik and Beissinger (2003), Brown, Ingram and Wadsworth (2004), Smith (2005).

Perry 1986). The contrast between this relative absence of evidence that DNWR is costly and the extensive microeconomic evidence of the prevalence of DNWR has been termed the 'micro-macro puzzle' (Lebow, Saks and Wilson 1999).

Given the puzzling lack of success in finding costs of DNWR using macro data, this paper turns to micro data to test directly the hypotheses that nominal rigidity leads to layoffs and that cuts lead to quits.[2] Surprisingly few papers have explicitly investigated the costs of nominal wage rigidity at a micro level. The only previous work attempting a similar test appears to be a section of Altonji and Devereux (2000), which has since been replicated by Kornelissen and Hübler (2005). Altonji and Devereux's (2000) results do not show a consistent effect of rigidity on layoffs, although they do find a consistently negative coefficient on rigidity in quit models. Kornelissen and Hübler (2005) also find a negative effect of rigidity on quits, but surprisingly also find a negative effect on layoffs, which they explain using core-periphery ideas of the labour market. Altonji and Devereux (2000) concluded that "research on whether nominal wage rigidity has real effects on employment, mobility patterns, and relative wages deserves a high research priority" (p 26).

Although there has been very little work specifically on the turnover consequences of DNWR, the empirical literature on separations and wage determination is large. Recently most focus has been on the returns to tenure and experience in wage equations (Altonji and Shakotko 1987, Topel 1991, Altonji and Williams 1998, Dustmann and Meghir 2001, Dustmann and Pereira 2003, Williams 2004, Altonji and Williams 2005). This literature has made some progress in dealing with heterogeneity and endogeneity, and is discussed further below. There is also a large body of work focusing on the determinants of separation decisions and a variety of theoretical literature addressing issues relating to separations, most focusing on quits.

Separation behaviour continues to be a topic that challenges, particularly empiri-

[2] Elsby (2004) presents an alternative ingenious solution to the micro-macro puzzle, focusing on the compression in wage raises that could occur if employers take into account the possibility of future DNWR.

cal economists. Arguably, not much progress has been made in more than a decade since Devine and Kiefer (1993) remarked that "the job-exit process is still not well understood" (p 17). Most of the lack of progress stems from the absence of ideal data. Data are typically collected infrequently and often do not contain all the information that would be required to estimate (full information) structural models of separation decisions. The simultaneous determination of separations, pay and tenure presents serious problems when trying to recover true underlying relationships. This paper is not immune to these challenges. An attempt is made to deal with endogeneity and heterogeneity issues using an two-step estimation techniques.

Results suggest that DNWR does impact on separation decisions. If an individual's pay is held above the warranted level by DNWR, the individual is more likely to be laid off. If the individual accepts a nominal pay cut, their chance of being laid off is lower than if their pay had been frozen, but they do face a higher probability of layoff than workers whose pay rises in nominal terms. Nominal cuts encourage quits among movers – that is, an individual who has accepted a pay reduction with a job change is more likely to subsequently quit to a new job. But job stayers who accept nominal cuts are not significantly more likely to quit than those who receive raises. Nominal rigidity does not prevent quits.

The remainder of this paper is structured as follows. Section 2 sets out a model of DNWR and Section 3 a model of separations. These are combined in a model for econometric estimation in Section 4. Heterogeneity and endogeneity issues are also discussed in Section 4. Section 5 outlines the data used and Section 6 presents the results. Section 7 concludes.

2 Nominal rigidity

A simple model of downward nominal wage rigidity is sufficient to set out the key ideas behind the empirical work. The model is similar to that set out by Altonji and Devereux (2000) and later adopted by a number of researchers (Fehr and Goette 2000, Knoppik and Beissinger 2003, Barwell and Schweitzer 2004, Kornelissen and Hübler 2005). Only Altonji and Devereux (2000) and Kornelissen and Hübler (2005) went on to investigate the real implications of nominal rigidity in terms of mobility decisions.

In the absence of nominal rigidity, worker i in firm j at time t would be paid their 'warranted' (or 'notional') log wage W^*_{ijt}, which will be directly related to the worker's productivity and is determined by the worker's and firm's observable and unobservable characteristics:

$$W^*_{ijt} = \mathbf{X}^{W^*}_{ijt}\boldsymbol{\lambda} + \varepsilon^*_{ijt} \tag{1}$$

Nominal rigidity prevents affected workers from receiving their warranted wage when that would be below the wage they received last period. Nominal rigidity prevents warranted nominal cuts, and the nominal pay of affected workers is rigid (frozen) instead. In the case of perfect downward nominal rigidity, when all warranted cuts are prevented, the log wage received by worker i in job j at time t would be:

$$W_{ijt} = \begin{cases} W^*_{ijt} & \text{if } W^*_{ijt} - W_{ijt-1} \geq 0 \\ W_{ijt-1} & \text{if } W^*_{ijt} - W_{ijt-1} < 0 \end{cases} \tag{2}$$

Altonji and Devereux (2000) introduced the idea that some part of (all) warranted cuts might be enacted if the cut was big enough in absolute terms. Knoppik and Beissinger (2003) also extended the model so that a proportion of all warranted cuts were fully enacted. Neither of these modifications alter the nature of the effect of downward nominal rigidity on labour turnover.

Theoretical rationales for nominal rigidity include the idea that firms avoid cutting pay because of the adverse consequences on turnover and morale of remaining workers. It would seem that if nominal cuts did occur, the need for redundancies would be reduced. The idea that cuts might prevent layoffs is controversial following the work of Bewley (1999), who surprisingly found that managers do not typically say they treat layoffs and cuts as alternatives. Bewley reports that a common reaction to asking directly about the choice between layoffs and pay cuts was "puzzlement" (p 181). One human resource manager replied, "What do pay cuts have to do with layoffs? A layoff is used when you don't have sufficient work for certain skills. What would you do with the extra help? It would be unfair to them to keep them around... How could you ask everyone to take a pay cut in order to keep some idle people around?" (Bewley 1999 p 185). Other responses stressed that "layoffs do less damage to morale and productivity than do pay cuts" (Bewley 1999 p 183). The owner of a small manufacturing company explained that "a wage cut would give rise to morale problems. The employees would have a chip on their shoulders and would lose the fire in their bellies" (Bewley 1999 p 175). Some managers also mentioned that layoffs can be used to get rid of the least-wanted workers. Of course, it is possible to be sceptical that managers' reports represent the truth about underlying mechanisms. In any case, econometric investigation of the cut-layoff relationship seems warranted.

If cuts are implemented, then if shocks are firm-specific, or if there is money illusion, workers receiving cuts will be more likely to quit than those whose pay does not fall. A finding that nominal cuts lead to quits would be consistent with morale theories of DNWR based on the idea that workers have a particular dislike of nominal cuts. However, it could be that cuts are only made when workers accept them, in which case we would not see any association between cuts and quits (in this case, morale theories could still hold, but with the caveat that very adverse circumstances will override individuals' distaste for nominal cuts).

3 Separation decisions

Intuitively, most people would probably agree that quits are voluntary mobility decisions by the worker, whereas layoffs are involuntary from the worker's point of view, the layoff decision being taken by the firm. However, McLaughlin (1991) argues that layoffs need not be involuntary, in the sense that although the firm makes the decision to lay off the worker, the worker is in fact better off after the layoff. McLaughlin defines layoffs as firm-initiated separations, and quits as worker-initiated separations, and shows that neither need be inefficient. McLaughlin identifies involuntary separations with inefficiency, and shows that wage rigidity lies behind the notion that layoffs are involuntary. If wages are (downwardly) flexible, some layoffs will still occur but they will be efficient. It is worth emphasising that imperfections are needed to generate any layoffs: without imperfections, all separations are quits. McLaughlin's (1991) efficient turnover model relies on informational asymmetries. The model used here also incorporates (downward nominal wage) rigidities.

A general framework for thinking about separations, based on McLaughlin (1991), is as follows. In this model, workers are assumed to compare the current benefits of working in the current and other jobs. Current wages are used to represent these benefits. Current values will adequately represent the expected discounted present value of all future benefits when wages are a random walk. Alternatively, it can simply be assumed that wage growth is equal in all jobs. Note also that although the fundamental determinant of mobility decisions is the difference between the current real wage and the best alternative real wage offer, this is equivalent to the difference in the nominal values of these wage variables, as the same price level is used to deflate both.

Let W_{ijt} be worker i's current nominal log wage in job j at time t. Consider a two-firm model for simplicity, and let W_{it}^{A} be the the log wage offer for worker i at time t from the other firm. Both W_{ijt} and W_{it}^{A} will be related to the worker's productivity in the current firm and in the other firm, respectively y_{ijt} and y_{it}^{A}. There is stochastic variation in productivity, both over time and across firms. It is easiest for expositional purposes to assume that workers receive all rents from matches,[3] to assume no turnover costs, and to normalise the price level to unity, so that $W_{ijt} = y_{ijt}$ and $W_{it}^{A} = y_{it}^{A}$. Nonparticipation is ignored for simplicity.

Without informational or other imperfections, worker i will quit job j if they see $W_{it}^{A} > W_{ijt}$ (above the 45° line in Figure 1); otherwise, they will stay. The worker will accept a pay cut ($W_{ijt} < W_{ijt-1}$) as long as they see that their productivity in job j

[3] McLaughlin (1991) shows that rent sharing does not change the separation decision mechanics, but greater bargaining power does make it less likely that a worker will receive a better offer elsewhere, hence reducing quits and increasing layoffs.

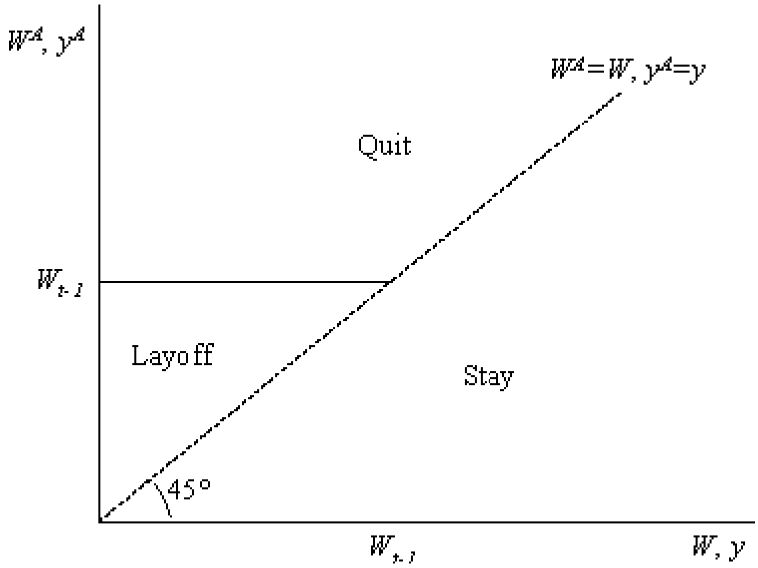

Figure 1: Quits and layoffs in the efficient turnover model (McLaughlin 1991)

has fallen (in relation to their productivity last period) and see that their productivity (ie wage) would not be higher elsewhere ($y_{ijt} < y_{ijt-1}$ and $y_{ijt} \geq y_{it}^A$).

The addition of informational asymmetries enables layoffs to occur. Assume the worker does not know y_{ijt}, and the firm does not know W_{it}^A (or related outside productivity y_{it}^A), as in McLaughlin's (1991) efficient turnover model. Quits occur if workers demand a wage increase because they see $W_{it}^A > W_{ijt-1}$ and if the firm is not prepared to match that demand because $y_{ijt} < W_{it}^A$ (so $W_{it}^A > W_{ijt}$) (see Figure 1). Layoffs occur if the firm demands a wage cut because it sees $y_{ijt} < W_{ijt-1}$ but workers are not prepared to accept that cut because they can see that $W_{it}^A > W_{ijt}$. There is continued employment in the current firm if $W_{ijt} > W_{it}^A$. Note that in this model there is downward wage flexibility in that cuts are acceptable to workers as long as the wage in the current firm is greater than that obtainable elsewhere.

In contrast, in a model with perfect DNWR, layoffs occur if the firm needs to reduce the wage below last period's value because of a negative productivity shock and if the worker does not want to quit as wages elsewhere are below their current wage, which is held at last period's level due to DNWR (ie $y_{ijt} < W_{ijt} = W_{ijt-1}$ and

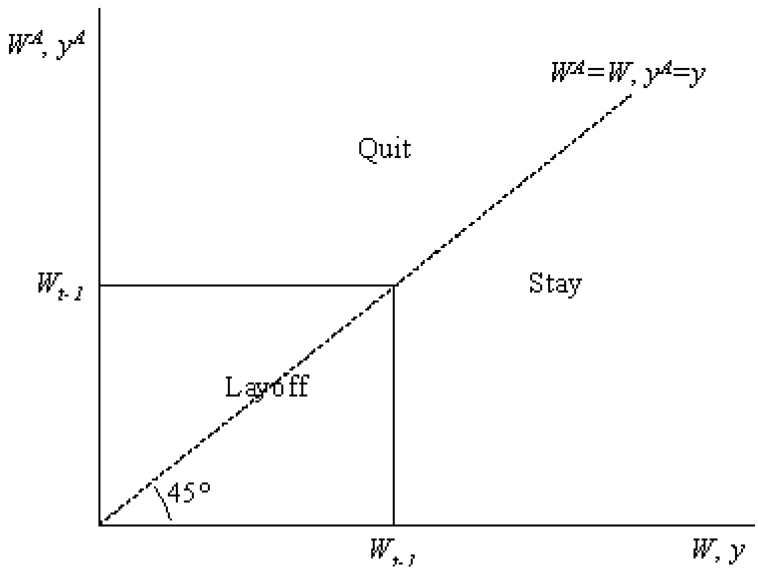

Figure 2: Quits and layoffs in a model with perfect downward nominal rigidity

$W_{it}^A < W_{ijt} = W_{ijt-1}$) (see Figure 2). Quits occur if another firm receives a positive productivity shock such that $W_{it}^A > W_{ijt} \geq W_{ijt-1}$ and if workers want to leave the current firm (which they will if $W_{it}^A > W_{ijt} \geq W_{ijt-1}$). Workers stay in the current firm if the current firm does not receive a negative productivity shock and so is prepared to keep the wage the same or raise it, $y_{ijt} = W_{ijt} \geq W_{ijt-1}$, and workers do not want to quit because $W_{ijt} > W_{it}^A$. Some layoffs are inefficient in the model with downward nominal rigidity: it would be efficient for workers in the triangle below the 45° line in Figure 2 to accept a wage cut and stay, as $y_{ijt} > y_{it}^A$.[4]

In McLaughlin's (1991) efficient turnover model, firms offer pay cuts rather than layoffs. This is not the case in the perfect-DNWR model: for some reason the firm does not wish to offer cuts, opting simply to lay off workers. There are several types of evidence that are relevant to deciding which model best describes reality. First, McLaughlin (1991) could not find strong empirical evidence for the efficient turnover model, using PSID data. Second, evidence suggests that displaced workers suffer wage

[4]McLaughlin (1991) also proposes a model including rigidities, but his model includes upward wage rigidity, in which case more workers quit and fewer workers stay – the additional quits are where $W_{it}^A > W_{ijt} = W_{ijt-1}$. This generates additional inefficiency.

losses (Topel 1990), which is consistent with the involuntary nature of layoffs in the DNWR model.[5] Finally, there is the interesting evidence from the mouths of managers, collected by Bewley (1999), suggesting that managers do not view cuts as a feasible alternative to layoffs (this was discussed in Section 2).

4 An econometric model of quits and layoffs

The model of separations and wage determination that forms the basis of the empirical work can be represented as follows. Take the decision of worker i to quit from job j at time t. There is some unobserved continuous latent response variable $Quit^*_{ijt}$, which can be regarded as the propensity to quit. Assume for the moment that the quit occurs precisely at time t. The propensity to quit is determined in part, according to the model above, by the difference between the wage in job j and the maximum outside offer at time t. This wage differential should capture the expected pecuniary benefits of quitting, but there may be other, non-pecuniary, differences between the current and other jobs. These other factors $\mathbf{X}^Q_{ijt}$ may affect the propensity to quit:

$$Quit^*_{ijt} = \alpha_1(W_{ijt} - W^A_{it}) + \mathbf{X}^Q_{ijt}\boldsymbol{\alpha}_2 + u_{ijt} \tag{3}$$

Measuring the variables in equation (3) with the available data is not always straightforward. Individuals are interviewed once a year and are asked about any changes in labour force spells during the last year. If they have a job they are asked about their current wage. Although the data include the date that the quit occurred between one interview and the next, the exact wage that prevailed at the time of the separation is not known. All that is observed is the wage at the interview prior to separation.

More fundamentally, measuring the difference between the current wage and the maximum outside offer $(W_{ijt} - W^A_{it})$ is not straightforward, even if the assumption is made that the currently prevailing wage captures the expected present value of future

[5]However, even in McLaughlin's (1991) efficient turnover model, all layoffs involve taking a lower pay in the new job compared to the previous wage W_{ijt-1}, even though the new job offers better pay than the previous firm was prepared to offer in the current period.

earnings. The outside offer distribution at the time of quitting is not observable. Realised wages in the new job at $t+1$ are observable for those that separate, but of course for these workers the wage in the old job j at $t+1$ is not observed (and with annual data the wage at $t+1$ will only represent the wage in the job that was quit to if there was only one job-to-job transition during that period). For stayers, no outside offers are observed.[6] The approach adopted here is to model the maximum wage offer econometrically (as has been done previously – for example Abowd and Kang 2002).

In the absence of DNWR, the nominal wage of worker i in job j at time t is simply the warranted nominal wage from equation (1), W^*_{ijt}. The determinants will be identical. In the following expression, the observable determinants are split into individual-level variables $\mathbf{X}_{it}$ and job (or firm) characteristics $\mathbf{Z}_{jt}$. The warranted wage is also affected by unobservable individual characteristics μ_i and unobservable person-firm or job-match factors ϕ_{ij}.

$$W^*_{ijt} = \mathbf{X}_{it}\boldsymbol{\beta}_1 + \mathbf{Z}_{jt}\boldsymbol{\beta}_2 + \beta_3\mathbf{T}_{ijt} + \mu_i + \phi_{ij} + \eta_{ijt} \tag{4}$$

Specifically, $\mathbf{X}_{it}$ includes education, experience, race, gender, marital status, number of children, home ownership, local unemployment rate, region and occupation. $\mathbf{Z}_{jt}$ includes firm size and sector. $\mathbf{T}_{ijt}$ is job (employer) tenure. η_{ijt} is a transitory random error.

The maximum offer wage W^A_{it} will have some determinants in common with the current wage W_{ijt}, but there are differences in determinants, and it is also possible that the degree to which common factors influence current and best-offer wages will differ. Common factors will include: unobserved individual characteristics, demographic factors, labour market experience, education, macroeconomic conditions including unemployment, occupation and region. Tenure in the current job is often thought not to influence offers (e.g. Abowd and Kang 2002), because conditional on overall labour market experience, tenure merely reflects accumulated specific human capital. Thus

$$W^*_{ijt} - W^A_{it} = \mathbf{X}_{it}\boldsymbol{\gamma}_1 + \mathbf{Z}_{jt}\boldsymbol{\gamma}_2 + \gamma_3\mathbf{T}_{ijt} + \phi_{ij} + \zeta_{ijt} \tag{5}$$

[6] Marshall and Zarkin (1987) discuss this absence of data further.

where γ_1 will be zero if observable variables varying by individual and time affect current and offer wages equally. This would be consistent with the search model of Burdett (1978) in which the returns to tenure and experience (wage growth) are assumed to be homogenous across jobs, so the decision whether to accept the offer depends only on the job-match component (intercept and job-specific variables), plus accumulated returns to tenure on the current job. Unobserved individual-specific factors μ_i are assumed to influence current and offer wages equally.

Since nominal wage rigidity makes the actual nominal wage W_{ijt} deviate from the warranted nominal wage W^*_{ijt}, rigidity will also alter $(W_{ijt} - W^A_{it})$, assuming that not all firms are affected equally. The preferred conception of heterogeneity in the effects of DNWR is that it potentially affects only some firms, other firms' wages being flexible. Downward nominal rigidity will mean that $W_{ijt} > W^*_{ijt}$, so the probability of the realised current real wage lying above the best alternative offer, $\Pr(W_{ijt} > W^A_{it})$, will rise. Given that DNWR affects only a subset of firms, a worker in a firm that cuts wages will have a reduced $\Pr(W_{ijt} > W^A_{it})$.

Therefore $(W_{ijt} - W^A_{it})$, the difference between the actual current wage for worker i in job j and the maximum offer wage for that worker at time t, is determined according to the following expression:

$$W_{ijt} - W^A_{it} = \mathbf{X}_{it}\boldsymbol{\delta}_1 + \mathbf{Z}_{jt}\boldsymbol{\delta}_2 + \delta_3\mathbf{T}_{ijt} + \delta_4 RIGID_{ijt} + \delta_5 CUT_{ijt} + \phi_{ij} + \zeta_{ijt} \qquad (6)$$

where the 'rigidity dummies' $RIGID_{ijt}$ and CUT_{ijt} are defined as:

$$RIGID_{ijt} = \begin{cases} 1 & \text{if } W_{ijt} = W_{ijt-1} \\ 0 & \text{otherwise} \end{cases} \qquad (7)$$

$$CUT_{ijt} = \begin{cases} 1 & \text{if } W_{ijt} < W_{ijt-1} \\ 0 & \text{otherwise} \end{cases} \qquad (8)$$

The cut dummy can be regarded as capturing the set of firms with flexible wages which suffer adverse shocks (shocks that could be person- or firm-specific). The rigid dummy is intended to capture the set of firms with downwardly rigid wages which suffer adverse shocks. The omitted category, firms with nominal raises, can be regarded as the set of firms which do not suffer adverse shocks. (Note that it is for notational simplicity that the unobserved firm-specific effects continue to be denoted ϕ_{ij}.)

Bewley (1999) suggested that nominal cuts would have an effect which can be considered additional to their impact on the actual wage, namely an 'insult' effect (pp 174-175). 26 out of 151 businesses (17%) Bewley interviewed thought that pay cuts would be insulting. In terms of the impact on turnover, 62 businesses (41%) said that turnover would rise following a cut. Most (45) thought employees would leave slowly. 5 businesses were worried employees would "quit out of rage". 18% believed that the best employees would leave. This implies that the rigidity dummies could have an effect on separations above and beyond their effect on $W_{ijt} - W_{it}^A$.

Returning to the model of quits: the quit propensity itself is not observed; what is observed is the actual quit decision embodied in the binary variable

$$Quit_{ijt} = \begin{cases} 1 & \text{if } Quit_{ijt}^* > 0 \\ 0 & \text{otherwise} \end{cases} \tag{9}$$

The quit model can be estimated as a probit if we assume that $u_{ijt} \sim N(0,1)$. A 'reduced form' model could be estimated:

$$\Pr\left(Quit_{ijt}\right) = \mathbf{X}_{it}\boldsymbol{\theta}_1 + \mathbf{Z}_{jt}\boldsymbol{\theta}_2 + \theta_3\mathbf{T}_{ijt} + \theta_4 RIGID_{ijt} + \theta_5 CUT_{ijt} + \mathbf{X}_{ijt}^Q\boldsymbol{\theta}_6 + \nu_{ijt} \tag{10}$$

The layoff equation has a structure identical to the quit equation. In this case the wage differential $W_{ijt} - W_{it}^A$ is considered the difference between the actual cost of employing the worker and the value of the worker to the firm.

A multinomial logit model can also be estimated for the three alternatives quit, layoff or stay. The multinomial logit model assumes that the each alternative is independent of the others. If any of the alternatives are not independent, parameter estimates will be inconsistent if that alternative is included. If all alternatives are independent, the estimates of the multinomial logit model will be more efficient than the simple biniomal probit models in which one of the alternatives is excluded (although the probit estimates would be consistent in this case).

4.1 Heterogeneity and endogeneity

The error terms in the separation equations include individual- and job-match-specific components. Allowing for unobserved individual and job-match heterogeneity raises the issue of bias if single-equation probit or multinomial logit methods are used to estimate the equations. The unobserved individual effects μ_i can be interpreted as unobserved ability. Low-μ_i individuals will have low productivity and will be more likely to be laid off (conditional on the wage). Quit propensity is typically assumed to be decreasing in μ_i; this is normally justified with reference to motivation, perseverance, or unobserved health status. Furthermore, if prospective employers infer that job applicants are likely to be low-ability workers laid off by previous employers, prospective employers will face an adverse selection problem. Unless high-ability workers can signal their high ability, they may be reluctant to quit in the face of this adverse selection.

Much of the focus in prior literature has concerned the endogeneity of job tenure: tenure is a function of past separation decisions. A lower probability of layoff or quit entails longer tenure, so tenure is positively correlated with the individual-specific effect in a separation equation: $cov(T_{ijt}, \mu_i) > 0$. Experience could also be correlated with the individual effect: it is sometimes argued that because experience is simply the sum of tenures over jobs held, the more frequent are job changes, the more likely are non-work spells to occur, and these will reduce lifetime labour market experience, resulting in $cov(X_{ijt}, \mu_i) > 0$ (see Dustmann and Pereira 2003 pp 6-7 for the wage equation case). Most of the previous literature, however, assumes $cov(X_{ijt}, \mu_i) = 0$, which would hold if job changes did not involve significant non-work spells (Altonji and Shakotko 1987, Topel 1991, Altonji and Williams 1998; note that this literature focuses on US data, and it may be more reasonable to assume no break between employment spells in the relatively flexible US labour market than elsewhere).

Now consider biases arising from the job-match specific effect ϕ_{ij}. Good job matches are likely to pay high wages, and quits from these are unlikely. Firms may also share in the benefits of a good match, in which case layoffs are also unlikely. Thus tenure and the job-match effect will be positively correlated: $cov(T_{ijt}, \phi_{ij}) > 0$. Assuming a worker continuously receives job offers, the longer a worker has been in the labour market, the more likely he is to have found a good job match, leading to a positive correlation between experience and the job-match effect: $cov(X_{ijt}, \phi_{ij}) > 0$. However, this same feature of 'job shopping' means that a disproportionately high number of low-tenure workers will have a good match (Topel 1991, Altonji and Williams 1998), which implies $cov(T_{ijt}, \phi_{ij}) < 0$.

The estimated tenure and experience coefficients in a separation probit would exhibit bias. These biases result from unobserved individual and job-match heterogeneity, and in both cases the estimated coefficient would reflect biases resulting from correlations between the endogenous regressor and the error components, and correlations among the endogenous regressors.

The usual way to evade the adverse consequences of endogeneity is to use instrumental variables. Given that observable factors affecting separation decisions will also determine tenure, finding a suitable instrument is difficult. The technique adopted here follows previous literature (begun by Altonji and Shakotko 1987 and followed by many others including Altonji and Williams 1998, Parent 2000, Dustmann and Pereira 2003, Williams 2004). Tenure can be instrumented with DT_{ijt}, its difference from job-match mean $\overline{T_{ij}}$ (where the job-match mean is the mean of the sample observations over job match ij). Thus $DT_{ijt} = T_{ijt} - \overline{T_{ij}}$ is used as an instrument for T_{ijt}. Because both the error components ϕ_{ij} and μ_i are fixed within the job, deviations from job-match means are orthogonal to both of these error components. Deviations from job means are also obviously correlated with the original variable. Thus they are valid instruments. Note that the difference from job-match means of experience cannot also be used as an instrument, as this would be perfectly collinear with DT_{ijt}. In some contexts there is a disadvantage of using deviations from job-match means as an instrument in that it removes one possibly-important source of sample variation in the affected variables, namely that between jobs. The instruments capture only variation within job matches.

This is less of a problem in the present context as the focus is on determinants of job changes. Given that a decision has to be made about whether to instrument tenure or experience, this paper follows previous work in choosing to instrument tenure. Unfortunately that means that any correlation of experience with individual or job-match effects will result in biased estimates of the impact of not only experience, but also tenure, as tenure and experience are correlated (see Dustmann and Pereira 2003 for a full discussion of this in the context of a wage equation). Positive correlation between experience and the job-match effect will tend to positively bias the experience coefficient and negatively bias the tenure coefficient.

The rigidity dummies may also be endogenous. Consider the model of warranted wage determination (1). The error term will contain individual and job-match specific components: $\varepsilon^*_{ijt} = \mu_i + \phi_{ij} + \eta_{ijt}$. The rigidity dummies will be affected by μ_i or ϕ_{ij} if the probability of a warranted nominal cut $\Pr(\Delta W^* < 0)$ is affected by μ_i or ϕ_{ij}. As μ_i and ϕ_{ij} are both constant over time, differencing equation (1) would imply they might not influence ΔW^*. However, it is possible that wage growth is positively correlated with unobserved ability μ_i. Workers with a good job match (high ϕ_{ij}) might also have higher wage growth, for example via more rapid enhancement of human capital on the job. Both of these would imply that cuts are negatively correlated with μ_i and ϕ_{ij}, but do not directly imply anything about rigidity. If firms want to retain high-ability or high-job-match workers, they may be more willing to hold their wages rigid in the face of a negative shock, which would imply a positive correlation between the rigid dummy and the error components. The counterpart would be a negative correlation between nominal cuts and the individual and job-match effects.

As before, it is difficult given available data to find suitable instruments for the rigidity dummies. Because $RIGID_{ijt}$ and CUT_{ijt} are dichotomous, their instrumenting equations will need to be estimated by probit (or similar). Differences from job-match means would perfectly predict $RIGID_{ijt}$ and CUT_{ijt}, and so cannot be used as instruments. Instead, the deviation from the job mean of the nominal pay change and the deviation from the job mean of the real pay level can be used as instruments. Neither of these variables are correlated with the unobserved error components μ_i and ϕ_{ij}, and both may be correlated with the rigidity dummies. Note that although most bias should be removed by this method, if the rigidity dummies are correlated with experience then remaining correlation between experience and the error components will bias the estimated coefficients on the rigidity dummies. However, there are limited theoretical or empirical grounds for believing that experience affects the likelihood of rigidity, so this bias is unlikely to be substantial.

Estimation of a model of quits controlling for endogeneity is complicated in the present case by the fact that the (potentially endogenous) rigidity dummies are not continuously observed. A variety of approaches are attempted in the face of this problem. To clarify, the structure of the problem is as follows (subscripts are omitted for simplicity). The equation of interest is

$$y_1^* = \alpha_1 y_2 + \alpha_2 y_3^* + \mathbf{x}\boldsymbol{\alpha}_3 + \varepsilon_1 \tag{11}$$

where y_2 and y_3^* are endogenous, while $\mathbf{x}$ is a set of exogenous variables. Reduced form equations for the endogenous explanatory variables y_2 and y_3^* are as follows, including the common exogenous variables $\mathbf{x}$ and other exogenous variables $\mathbf{z}_1$ and $\mathbf{z}_2$ specific to each:

$$y_2 = \mathbf{z}_1\boldsymbol{\beta}_1 + \mathbf{x}\boldsymbol{\beta}_2 + \varepsilon_2 \tag{12}$$

$$y_3^* = \mathbf{z}_2\boldsymbol{\gamma}_1 + \mathbf{x}\boldsymbol{\gamma}_2 + \varepsilon_3 \tag{13}$$

y_2 is continuously observed, but y_1^* and y_3^* are not. Instead the binary indicator variables y_1 and y_3 are observed, where

$$y_1 = \begin{cases} 1 & \text{if } y_1^* > 0 \\ 0 & \text{otherwise} \end{cases} \quad \text{and} \quad y_3 = \begin{cases} 1 & \text{if } y_3^* > 0 \\ 0 & \text{otherwise} \end{cases}$$

Note that the 'structural form' for y_2 depends on y_3^*, not y_3 (this mirrors Rivers and Vuong 1988 and Smith and Blundell 1986). Also, for simplicity (11) is shown as including only one continuous and one dichotomous endogenous variable; the extension to more than one of each is straightforward.

If $\alpha_2 = 0$, the model is identical to that considered in Rivers and Vuong 1988 and is 'Model 3' in Maddala (1983 pp 244-245) (note though that here it is only the parameters $\boldsymbol{\alpha}$ that are of interest). Maddala suggests estimating (12) by OLS and substituting the resulting fitted values in place of y_2 in (11). This is the instrumental variables probit estimator described in Rivers and Vuong (1988 p 351). Alternatively, a two-stage conditional maximum likelihood estimator could be used (Rivers and Vuong 1988 pp 352-353). Again the procedure is straightforward. As before, the first step consists of estimation of (12) by OLS. In the second step, the residuals from this OLS regression $\hat{\varepsilon}_2$ are included in (11) which is estimated by probit. The estimable parameters in this case are $\alpha_1 / \sigma_{\varepsilon_4}$ and $\alpha_3 / \sigma_{\varepsilon_4}$ where σ_{ε_4} is the standard deviation of the residuals from the reduced form equation for y_1. A useful feature of this two-step conditional maximum likelihood estimator (2SCML) is that a simple test for exogeneity of can be conducted via an exclusion test of the first-step residuals $\hat{\varepsilon}_2$. Rivers and Vuong (1988) show that 2SCML will be efficient if (11) is just identified, ie if the number of excluded exogenous variables ($\mathbf{z}_1$) equals the number of endogenous variables on the right hand side of (11) (the alternative condition for efficiency of 2SCML in this case is that y_2 in fact prove exogenous).

If $\alpha_1 = 0$, the model is 'Model 6' discussed in Maddala (1983 pp 246-247). Again it would be possible to obtain consistent estimates of the parameters of interest in (11) using a two-step instrumental variables method in which (13) is estimated by probit and the predicted values included in (11) in place of y_3^*. In this case the estimable parameters take the form $\alpha_2 \sigma_{\varepsilon_3} / \sigma_{\varepsilon_4}$ and $\alpha_3 / \sigma_{\varepsilon_4}$ where σ_{ε_3} is the standard deviation of the residuals from (13) and and σ_{ε_4} is the standard deviation of the residuals from the reduced form equation for y_1.

Since in the problem addressed in this paper $\alpha_1 \neq 0$ and $\alpha_2 \neq 0$, an extension to the two-step instrumental variables estimators just discussed is applied. (12) is estimated by OLS and (13) is estimated by probit, with all the exogenous variables $\mathbf{z}_1$ and $\mathbf{z}_2$ included in both equations. The respective fitted and predicted values are included in place of y_2 and y_3^* in (11). A similar extension to the two-step conditional maximum likelihood estimators is also used. 'Residuals' from the first-step probits are calculated as actual values minus predicted probabilities. To my knowledge, there are no consistency (or efficiency) results for the relatively complex case dealt with here. For consistency, the two-step approaches require joint normality of the error terms in all equations. It is unlikely that the two-step estimates will be efficient.

Although Altonji and Devereux's (2000) and Kornelissen and Hübler's (2005) subsequent investigations of the impact of DNWR on separations did not explicitly deal with endogeneity, their approach does effectively allow for the endogeneity of rigidity. They use model estimates to calculate the (conditional expectation of the) difference between actual and notional wages $E\left(W_{ijt} - W_{ijt}^*\right)$ (conditional on the current and lagged variables explaining the notional wage) and use this as their measure of rigidity in linear probability models of separation decisions decisions.[7] This difference has been termed 'wage sweep-up': it measures the amount wages have been held up by DNWR (in Altonji and Devereux's model of rigidity – similar to (1) and (2) above – the actual wage W_{ijt} is always at least as great as W_{ijt}^*, in the absence of measurement error). As an alternative measure of rigidity, Altonji and Devereux use the predicted probability of a nominal freeze. Although they estimate the predicted probability of a cut, they do not also include this in their separation models, possibly on the grounds that having accounted for measurement error this predicted probability is quite small. Altonji and Devereux (2000) admit that unobserved heterogeneity may bias the coefficients on other variables, but argue that all they need is the total effect of particular variables on wages, controlling for the other observed variables.

The use of 'wage sweep-up' means that Altonji and Devereux's (2000) estimates not only capture the effect of DNWR per se on separations but also control for the size of the impact of DNWR on wages. However, in the model set out in Section 3,

[7] In addition to allowing for rigidity in the actual wage, Altonji and Devereux (2000) also allow for measurement error. They confine themselves to the study of job stayers. Kornelissen and Hübler (2005) estimate probits, allowing for heteroskedasticity. Neither set of authors appears to correct standard errors to account for the fact that their key regressor has been estimated.

the size of the wage sweep-up makes no difference to separation decisions: instead it is the simple fact of rigidity that matters. When considering which approach is preferred it is important to bear in mind that wage sweep-up is not the same as the difference between current and offer wages and that it is the latter differential that drives separation decisions. Wage sweep-up relates only to wages in the current job (it is the difference between the actual wage and the wage that would counterfactually be paid in the absence of DNWR). The extent to which the current wage is swept up by DNWR will not affect the differential between the current wage and the maximum wage offer, as this is determined by job-specific effects and job- or individual-specific shocks. These affect the current wage-offer wage differential but do not affect wage sweep-up. Thus the use of a simple rigidity dummy would seem preferable.

5 Data

The primary source of data is the British Household Panel Survey (BHPS). Around 10,000 individuals were surveyed in 1991, and these people have been interviewed (whenever possible) on an annual basis since then. New adult members of these individuals' households are also interviewed, as are children once they reach the age of 16. This study uses only data relating to that original sample, as it was designed to be representative of the British population south of the Caledonian Canal, and subsequent changes in household composition should have helped maintain that feature. The Welsh, Scottish, Northern Irish and ECHP additional samples are not used as they are not representative. The first twelve waves of data are used, up to 2002. The sample is restricted to working age (16-60) employees, and includes both males and females and full and part time workers. Pay cuts bigger than 75% and pay raises bigger than 500% are excluded (trimming less than 0.5% of the sample at either end), as are cases where reported pay period was less than 1 week.

Quits and layoffs are distinguished by means of a question asked to all employees whose labour force status has changed since 1 September the previous year. In this paper the main quit variable is coded 1 if the individual left for a better job. A broader definition of quits is also used which also includes "left to have baby", "children/home care", "care of other person", "moved area", "started college/university" and "other reason".[8] The degree to which these reasons can be validly considered quits depends on the degree to which they were chosen voluntarily, via a considered evaluation of their benefits vis a vis remaining in the previous job. A layoff is coded if the individual was made redundant. A broader definition of layoffs is also used which includes those who were dismissed or sacked. Other stated reasons for leaving a job are ignored. These include the ending of a temporary job,[9] retirement, health reasons, and other undefined reasons. Ill health, and possibly also retirement, is considered an involuntary reason for termination, not implying optimal decision making behaviour. Promotions are also not included in the analysis.[10] The definition of quits and layoffs used here requires that individuals did not continue to work for the same employer, ie that the labour force status of the previous job was not "different job, same employer". Some further possibly erroneous observations were eliminated by ensuring that all those who gave reasons for stopping the job also were recorded as employed prior to that job change.

The information about labour force status changes is taken from the individual's job history file. This records details of all spells between one year and the next. The particular employment spell that is chosen to determine separation status is the one

[8] "Other reason" is included because during the early part of the sample "moved area" and "started college/university" were not available as choices, but responses were later recoded into these catgories from the "other reason" category by the BHPS. When these categories are explicitly introduced, numbers in them fall, and numbers in the "other reason" category rise, suggesting either error in recoding or a belief by the BHPS that "other reasons" primarily relate to those categories.

[9] The ending of a temporay job is not included here in either quits or layoffs as this separation is not necessarily consistent with either, although it is considered a layoff by Booth and Francesconi (2000).

[10] It has been suggested (Altonji and Devereux 2000) that promotions could be used when pay for the job is rigid. The often-cited paper by Solon, Whatley and Stevens (1997) also makes this point, but a crucial part of that paper's data relies on the fact that promotions involve raises: in firms making promotions, it is not individual pay that is rigid, but pay for the job. (In principle it is not clear what the firm's rationale is in maintaining the freeze in pay for the job, given it is able to raise the wage bill by other means.) Treating promoted individuals as stayers makes absolutely no difference to the results. Two other possibilties, to include promotions with quits and to study them separately, are not pursued: the focus of this paper is on wage rigidity and there seems little a priori relationship between rigidity and promotion.

that was current at the time of the interview, as other (explanatory) data relate to that job. It is ensured that the start date of the relevant spell was before this interview, and that its end date was after this interview and before the next interview.

The focus on nominal wage rigidity prompts a distinction between job stayers and job movers. For the study of the impact of DNWR it is necessary to define job stayers as individuals who were not promoted, did not change grades, and did not change employer. The specific question asked by the BHPS ensures this. This definition is strict relative to most countries' surveys, and should help ensure that the sample of stayers really has no change in job that would necessitate a pay change. 67% of employees are stayers on this definition, on average over 1991-2002. It is the pay of job stayers that theory suggests might be rigid. There is no relevant theory that could explain why the nominal wage of a job mover might remain the same across jobs. The definition of 'stayer' that is implied by the tenure variable used here is different. For the definition of tenure, a stayer is defined as someone remaining with the same employer; that individual could change grades or jobs within the firm. This definition accords with the idea of employer-specific human capital. If the definition of 'stayer' that is relevant for DNWR were used, tenure would decline to zero after promotion, which does not accord with a sensible measure of specific human capital acquisition.

The main pay variable used to calculate rigidity is usual gross weekly pay. From 1999, the BHPS asked hourly-paid workers to state their basic hourly wage rate. On average, 41% of employees report themselves to be hourly-paid. It could be argued that the basic hourly wage rate is the variable that should be studied when investigating rigidity. It is certainly true that the basic rate is less likely to change than total pay, as total pay includes overtime, bonuses, performance-related pay, and shift payments. On theoretical grounds, it is possible to make arguments for studying either pay measure. Legal considerations can explain rigidity in the basic wage rate, but clearly do not apply to overtime or bonus payments for example, thus suggesting that it is the basic rate that will reflect the extent of rigidity. But from the point of view of theories that justify pay rigidity on 'fairness' grounds, it would seem reasonable to study total pay, as this determines workers' utility. In principle, total pay per hour worked would also be relevant in this case. Hourly total pay measures can be calculated by dividing usual gross weekly pay by number of hours worked per week. Unfortunately, hours are reported with even more error than pay, so hourly pay is more error-ridden and probably less reliable (Bound, Brown, and Mathiowetz 2001). An idea of the greater error in hourly pay can be seen from its greater variation in Table 1. To get as comprehensive picture of the effects of pay rigidity on separations as possible, all pay measures are initially investigated.

Variable	Mean	Std dev	Min	Max	Mean	Std dev
	All workers				Stayers	
quit	0.061	0.239	0	1	0.046	0.210
layoff	0.030	0.172	0	1	0.027	0.161
rigid	0.071	0.256	0	1	0.083	0.276
cut	0.242	0.428	0	1	0.237	0.425
real weekly pay (£)	333.75	267.45	0.80	17,309.28	334.18	275.01
real hourly pay (£)	8.35	5.50	0.02	303.67	8.41	5.42
real hourly basic wage rate (£)	6.04	2.61	2.04	90.00	6.10	2.66
tenure (years at employer)	6.3	7.1	0.04	46.8	8.1	7.4
experience (years)	17.7	11.4	0	49.8	19.8	11.0
education (years)	13.0	3.9	5	49	13.0	3.9
married	0.72	0.45	0	1	0.75	0.43
white	0.97	0.18	0	1	0.97	0.17
female	0.51	0.50	0	1	0.51	0.50
union coverage	0.51	0.50	0	1	0.54	0.50
union membership	0.33	0.47	0	1	0.37	0.48
workplace employment 25-200	0.36	0.48	0	1	0.37	0.48
workplace employment >200	0.31	0.46	0	1	0.31	0.46
private sector	0.69	0.46	0	1	0.67	0.47
regional unemployment rate	0.059	0.026	0.016	0.121	0.061	0.026
number of children in household	0.68	0.95	0	8	0.66	0.94
hours	38.8	13.2	1	161	38.6	13.1
inflation	0.024	0.006	0.013	0.035	0.024	0.006

Table 1: Descriptive statistics

Note: Sample includes males and females employed (full time or part time) both this and last interview; excludes pay cuts bigger than 75% and pay raises bigger than 500%; excludes pay period less than 1 week; and excludes Welsh, Scottish, Northern Irish and ECHP additional samples; excludes cases with unknown workplace size. Real pay is measured at 2002 prices. Main source of data: BHPS 1992-2002. Basic hourly wage rate is only available 1999-2002.

Descriptive statistics are shown in Table 1. On average over 1992-2002, 6.1% of employees quit their job ("left for a better job"). This is a slightly higher proportion to that reported for Germany during 1984-93 by Clark, Georgellis and Sanfey (1998) (5.4%, although this includes some quits to unemployment or out of the labour force; only quits to retirement are excluded). In Britain during 1992-2002, on average 3.0% of employees were laid off each year. On average over that period, 7.1% of employees had pay that was rigid in nominal terms from one year to the next (this figure includes stayers and movers). The data suggest that 24.2% of employees suffered nominal pay cuts over this period.[11]

The use of data for stayers brings up the question of sample selection. Table 1 indicates that stayers have longer tenure and greater labour market experience than job movers. Stayers are less likely to suffer a nominal cut and more likely to experience pay freezes. Perhaps surprisingly, these differences do not seem to result in substantially different pay levels. Stayers are more likely to be unionised, and are more likely to be married. Stayers are much less likely to quit and somewhat less likely to be laid off. No other features appear to markedly distinguish stayers from movers. With the exception of the wage rate, these differences in characteristics match those found for US National Longitudinal Survey of Young Men (NLSYM) data by Marshall and Zarkin (1987). Because quits and layoffs are likely to be affected by whether nominal wage floors are binding, there is the potential for selection bias when the analysis is confined to stayers. It would be possible to estimate turnover equations on the sample of prior-year stayers allowing for selection, but that is not attempted here. Whether one is interested in stayers or in all workers depends on the question being addressed. As argued above, theories of DNWR do not make sense when applied to movers, so for the purposes of examining these theories it is stayers who are of interest. However, the full sample is relevant for an examination of the influences on labour market mobility.

[11] For previous evidence on UK nominal wage rigidity, see Smith (2000) and Nickell and Quintini (2003).

6 Results

Before presenting regression results, Table 2 shows the results of preliminary data investigations. The variation in quit (or layoff) proportions is assessed according to whether the individual previously had rigid nominal pay, a nominal pay cut, or a nominal raise. A test of the difference in proportions can be used to judge significance – a one-sided test comparing the situations after rigidity and after cuts is used, based on null hypotheses derived from the theory outlined above. It would be consistent with the theory to find that quits are higher after cuts than after rigidity, and that layoffs are higher after rigidity than after cuts. The results should be particularly strong for stayers, to back up theories of DNWR. Statistics are presented for rigidity in various pay variables, for all individuals and for previous-year stayers alone. The tables only consider separations that occur in the year after a particular nominal wage change was enacted. It is certainly possible that response lags could mean that some separations prompted by the nominal pay change occur after this time, and it is also possible that separations might occur in anticipation of a particular pay change. Nevertheless, it is felt that the timing that is studied here will tend to pick up any relevant behaviour. It would be possible and interesting to investigate timing issues further, but that is left to future research.

Layoffs appear more likely following nominal pay rigidity than they are following a cut, for stayers and for movers. For stayers, rigidity does not seem to prevent quits: for only one pay measure are quits higher after cuts than they are after rigidity, and even this difference is insignificant. However, for all workers, quits are higher among those who suffered cuts. The implication of this is that job movers who switch to a lower-paying job are more likely to quit that job within a year. This would be consistent with the lower-paying job being a stop-gap while the individual searches for a better job match. It is also consistent with previous evidence that some individuals have a greater propensity to move jobs.

	quit (%)			z-statistic
	cut	rigid	raise	(1-sided p-value)
weekly pay	6.7	5.7	5.7	1.4 (0.92)
hourly pay	6.4	4.5	5.8	1.9 (0.97)
hourly basic wage rate	10.5	9.1	9.0	0.5 (0.67)

	layoff (%)			z-statistic
	cut	rigid	raise	(1-sided p-value)
weekly pay	3.6	4.5	2.8	-1.6 (0.94)
hourly pay	2.9	4.2	3.1	-1.8 (0.97)
hourly basic wage rate	3.3	7.2	3.5	-1.8 (0.96)

Table 2a: Quits and layoffs by prior nominal wage change: all workers

Notes: See Table 1 for definition of sample. Sample includes both stayers and movers in prior year. The z statistic is for the difference in proportions cut and rigid. p-value is for one-sided test of nulls that quits are lower after rigidity, and layoffs are higher after rigidity.

	quit (%)			z-statistic
	cut	rigid	raise	(1-sided p-value)
weekly pay	4.6	5.5	4.5	-1.3 (0.10)
hourly pay	4.7	4.0	4.7	0.7 (0.76)
hourly basic wage rate	7.3	7.6	6.4	-0.1 (0.46)

	layoff (%)			z-statistic
	cut	rigid	raise	(1-sided p-value)
weekly pay	3.1	4.3	2.5	-1.9 (0.97)
hourly pay	2.8	3.9	2.8	-1.4 (0.92)
hourly basic wage rate	1.5	6.5	3.1	-2.1 (0.98)

Table 2b: Quits and layoffs by prior nominal wage change: (prior-year) stayers

Notes: See Table 1 for definition of sample. Sample includes only job stayers in prior year. The z statistic is for the difference in proportions cut and rigid. p-value is for one-sided test of nulls that quits are higher after rigidity, and layoffs are lower after rigidity.

Table 3 presents results of reduced-form probits, with no allowance for potential endogeneity or heterogeneity.[12] Results are presented for all workers and then for stayers alone. It is worth recalling that the effect of a variable could reflect its impact on any of the wage in the current firm, the maximum offer wage, or non-pecuniary factors affecting mobility decisions.

[12] The standard errors reported do not allow for clustering by individual, but results were almost identical when such clustering was allowed for. Allowing for clustering also made no difference to multinomial logit results.

A random effects probit produced almost exactly the same estimates as the basic probit, which is not surprising as the estimated ρ (ratio of standard deviation of individual effects to the total error) was insignificantly different from zero. (The number of observations per individual ranges from 1 to 10, with an average of 4.) Random effects estimates will be inconsistent if, as we have argued, the regressors are correlated with the individual-specific effect.

A nominal cut increases the chance that a worker will quit by 0.9 percentage points, relative to workers who receive nominal raises. The effect of nominal rigidity on quits is insignificantly different from that of nominal raises, although numerically not much lower than the effect of cuts. Nominal rigidity increases the likelihood of layoff by 1.8 percentage points (compared to nominal raises). Accepting a pay cut reduces the likelihood of layoff compared to rigid pay, but workers who take cuts are nevertheless more likely than those with raises to be laid off. These results contrast with the findings of Altonji and Devereux (2000) and Kornelissen and Hübler (2005). Altonji and Devereux's results (using US PSID data) were quite mixed. Rigidity appeared not to have a significant effect on layoffs, and coefficient signs were not consistent. Rigidity did appear to consistently negatively and generally significantly affect quits. These results are themselves in complete contrast to those of Kornelissen and Hübler (2005), who replicate Altonji and Devereux's method using German GSOEP data. Kornelissen and Hübler find limited effect of rigidity on quits (finding no significant effect in the full sample they undertake a variety of sample splits and find no significant effect except for the subsample of full-time workers). They do find a significant effect on layoffs, but wage sweep-up (the payment of a higher wage than is warranted) surprisingly seems to deter layoffs. Kornelissen and Hübler explain this with reference to a 'core-periphery' view of the labour force, proposing that there is a core group of (high-ability or job-match) workers with both high wage sweep-up (high wages) and job security.

Table 3 indicates that longer tenure appears to reduce both voluntary and involuntary job separations, although it is necessary to bear in mind that endogeneity will tend to negatively bias this coefficient. A worker with 10 years' tenure is 6.7 percentage points less likely to quit than a worker who has just joined a firm. In contrast, experience appears to have very little if any effect on quit propensity, and none on the likelihood of layoff either. The contrast between the impacts of these two variables is in accordance with the theory discussed above, whereby tenure impacts only on the current wage and not on wage offers, whereas experience might raise both wages equally.

The other direct measure of human capital included, education, also has no effect on quits or layoffs.

The results strongly suggest that unions encourage workers and firms to maintain existing employment relationships. Union coverage at the workplace reduces the probability of a worker quitting by 1.7 percentage points and the probability of a worker being laid off by 1.1 percentage points. Married workers are less likely to quit and also less likely to be laid off. The former could reflect greater mobility costs. White men are more likely to quit, which would be consistent with their having better labour market opportunities (although race is insignificant at conventional levels). White people are if anything less likely to be laid off. Women are also less likely to be laid off, which could possibly reflect selection into certain types of job.[13] Workers at larger workplaces (over 200 employees) are less likely to quit and also less likely to be laid off – both probabilities are 0.8-0.9 percentage points lower than those at smaller workplaces. The regional unemployment rate is strongly related to layoffs: a 1 percentage point increase in the unemployment rate increases the probability of layoff by 15.3 percentage points. The unemployment rate does not appear to significantly affect the quit rate.

[13] The number of children below the age of 16 in the household and the type of housing tenure (home ownership) were found to have no significant effect on quits or layoffs.

	quit		layoff	
	probit	MEs	probit	MEs
rigid*	0.088	0.007	0.248	0.018
	(1.2)	(1.1)	(3.3)	(2.7)
cut*	0.108	0.009	0.133	0.009
	(2.5)	(2.3)	(2.6)	(2.4)
tenure	−0.164	−0.013	−0.074	−0.004
	(−4.9)	(−4.5)	(−2.6)	(−2.6)
$tenure^2/10$	0.118	0.009	0.065	0.004
	(1.9)	(1.9)	(1.8)	(1.8)
$tenure^3/100$	−0.039	−0.003	−0.023	−0.001
	(−1.0)	(−1.0)	(−1.4)	(−1.4)
$tenure^4/1000$	0.004	0.0003	0.003	0.0001
	(0.5)	(0.5)	(1.1)	(1.1)
experience	0.005	0.0004	−0.026	−0.002
	(0.2)	(0.1)	(−0.6)	(−0.6)
$experience^2/10$	−0.034	−0.003	0.005	0.0003
	(−1.0)	(−1.0)	(0.1)	(0.1)
$experience^3/100$	0.015	0.001	0.003	0.0002
	(1.2)	(1.2)	(0.2)	(0.2)
$experience^4/1000$	−0.002	−0.0001	−0.001	−0.00004
	(−1.4)	(−1.4)	(−0.5)	(−0.5)
education	−0.001	−0.0001	−0.003	−0.0002
	(−0.2)	(−0.2)	(−0.4)	(−0.5)
union coverage*	−0.216	−0.017	−0.189	−0.011
	(−5.2)	(−5.0)	(−3.8)	(−3.8)
married*	−0.069	−0.006	−0.108	−0.007
	(−1.5)	(−1.5)	(−2.0)	(−1.9)
white*	0.150	0.010	−0.180	−0.013
	(1.2)	(1.4)	(−1.4)	(−1.2)
female*	−0.158	−0.012	−0.128	−0.008
	(−3.8)	(−3.7)	(−2.5)	(−2.5)
workplace employment 25-200*	−0.013	−0.001	−0.050	−0.003
	(−0.3)	(−0.3)	(−1.0)	(−1.0)
workplace employment >200*	−0.118	−0.009	−0.135	−0.008
	(−2.3)	(−2.4)	(−2.3)	(−2.4)
regional unemployment rate (%)	−0.700	−0.055	2.57	0.153
	(−0.8)	(−0.8)	(2.6)	(2.6)
McFadden's Pseudo R^2	0.116		0.051	
Log Likelihood	-2,669.2		-1,810.8	
Observations	14,059		13,706	

Table 3a: Quits and cuts: probit estimates: all workers

Notes: See Table 1 for definition of sample. Sample includes both stayers and movers in preceding year. Regressions also include standard region and 1-digit occupation dummies. * indicates dummy variable. Estimated coefficients are relative to the state of staying with the same employer. t-statistics are in parentheses. MEs are marginal effects. Marginal effects for dummies are calculated for the discrete change from 0 to 1. For continuous variables, marginal effects are evaluated at the mean.

	quit		layoff	
	probit	MEs	probit	MEs
rigid*	0.096	0.006	0.235	0.016
	(1.2)	(1.1)	(2.9)	(2.4)
cut*	0.072	0.004	0.161	0.010
	(1.3)	(1.2)	(2.8)	(2.5)
tenure	−0.037	−0.002	−0.030	−0.002
	(−0.5)	(−0.5)	(−0.7)	(−0.7)
$tenure^2/10$	−0.091	−0.005	0.011	0.001
	(−0.7)	(−0.8)	(0.2)	(0.2)
$tenure^3/100$	0.090	0.005	−0.0004	−0.0002
	(−1.0)	(1.2)	(−0.02)	(−0.02)
$tenure^4/1000$	−0.022	−0.001	−0.0003	−0.0002
	(−1.2)	(−1.5)	(−0.1)	(−0.1)
experience	0.064	0.003	−0.072	−0.004
	(1.3)	(1.2)	(−1.2)	(−1.2)
$experience^2/10$	−0.083	−0.004	0.054	0.003
	(−1.8)	(−1.7)	(1.1)	(1.1)
$experience^3/100$	0.029	0.002	−0.015	−0.001
	(1.8)	(1.7)	(−0.9)	(−0.9)
$experience^4/1000$	−0.003	−0.0002	0.001	0.00008
	(−1.7)	(−1.7)	(0.8)	(0.8)
education	0.003	0.0002	0.005	0.0003
	(0.5)	(0.5)	(0.7)	(0.7)
union coverage*	−0.241	−0.013	−0.176	−0.010
	(−4.8)	(−3.6)	(−3.1)	(−3.1)
married*	−0.034	−0.002	−0.076	−0.005
	(−0.6)	(−0.6)	(−1.2)	(−1.2)
white*	0.195	0.009	−0.028	−0.002
	(1.3)	(1.5)	(−0.2)	(−0.2)
female*	−0.158	−0.012	−0.134	−0.008
	(−3.8)	(−3.5)	(−2.3)	(−2.5)
workplace employment 25-200*	0.006	0.0003	−0.031	−0.002
	(0.1)	(0.1)	(−0.5)	(−0.5)
workplace employment >200*	−0.083	−0.004	−0.072	−0.004
	(−1.3)	(−1.3)	(−1.1)	(−1.1)
regional unemployment rate (%)	−1.436	−0.077	2.081	0.118
	(−1.4)	(−1.4)	(1.8)	(1.8)
McFadden's Pseudo R^2	0.107		0.042	
Log Likelihood	-1,816.4		-1,380.9	
Observations	11,206		11,027	

Table 3b: Quits and cuts: probit estimates: stayers

Notes: See Table 1 for definition of sample. Sample includes only job stayers in preceding year. Regressions also include standard region and 1-digit occupation dummies. * indicates dummy variable. Estimated coefficients are relative to the state of staying with the same employer. t-statistics are in parentheses. MEs are marginal effects. Marginal effects for dummies are calculated for the discrete change from 0 to 1. For continuous variables, marginal effects are evaluated at the mean.

Multinomial logit results (Table 4) are very similar to those from separate probits, so these are only presented for all workers. Cramer-Ridder (1991) tests indicate that pooling any two of the states would not be valid.

	quit multinomial logit	MEs	layoff multinomial logit	MEs
rigid*	0.155 (1.0)	0.005 (0.9)	0.557 (3.4)	0.017 (2.8)
cut*	0.224 (2.5)	0.007 (2.3)	0.306 (2.6)	0.008 (2.4)
ln real monthly pay	−0.026 (−0.4)	−0.001 (−0.4)	−0.002 (−0.02)	−0.00002 (−0.01)
tenure	−0.316 (−4.5)	−0.001 (−4.0)	−0.173 (−2.7)	−0.004 (−2.5)
tenure^2/10	0.211 (1.6)	0.007 (1.5)	0.155 (1.9)	0.004 (1.8)
tenure^3/100	−0.069 (−0.8)	−0.002 (−0.8)	−0.054 (−1.5)	−0.001 (−1.4)
tenure^4/1000	0.006 (0.4)	0.0002 (0.3)	0.006 (1.2)	0.0001 (1.2)
experience	0.011 (0.2)	0.0004 (0.2)	−0.039 (−0.4)	−0.001 (−0.4)
experience^2/10	−0.066 (−0.9)	−0.002 (−0.9)	−0.007 (−0.1)	−0.0001 (−0.1)
experience^3/100	0.031 (1.2)	0.001 (1.1)	0.013 (0.5)	0.0003 (0.4)
experience^4/1000	−0.004 (−1.3)	−0.0001 (−1.3)	−0.002 (−0.7)	−0.00005 (−0.6)
education	−0.002 (−0.2)	−0.0001 (−0.2)	−0.008 (−0.6)	−0.0002 (−0.6)
union coverage*	−0.431 (−4.9)	−0.013 (−4.5)	−0.431 (−3.8)	−0.010 (−3.6)
married*	−0.122 (−1.3)	−0.004 (−1.2)	−0.258 (−2.1)	−0.006 (−2.0)
white*	0.328 (1.3)	0.009 (1.6)	−0.437 (−1.6)	−0.013 (−1.3)
female*	−0.334 (−3.5)	−0.010 (−3.3)	−0.294 (−2.3)	−0.007 (−2.1)
workplace employment 25-200*	−0.008 (−0.1)	−0.0002 (−0.1)	−0.120 (−1.0)	−0.003 (−1.0)
workplace employment >200*	−0.238 (−2.2)	−0.007 (−2.2)	−0.315 (−2.2)	−0.007 (−2.3)
regional unemployment rate (%)	−0.879 (−0.5)	−0.033 (−0.6)	6.033 (2.64)	0.145 (2.7)
Pseudo R^2	0.089	Cramer	Quit:Layoff	239.0
Log Likelihood	-4,512.5	-Ridder	Quit:Stay	695.4
Observations	14,488	LR tests	Layoff:Stay	197.1

Table 4: Quits and cuts: multinomial logit estimates: all workers

Notes: See Table 1 for definition of sample. Sample includes both stayers and movers in preceding year. Regressions also include standard region and 1-digit occupation dummies. * indicates dummy variable. Estimated coefficients are relative to the state of staying with the same employer. t-statistics are in parentheses. MEs are marginal effects. Marginal effects for dummies are calculated for the discrete change from 0 to 1. For continuous variables, marginal effects are evaluated at the mean. Cramer-Ridder LR tests are LR tests of the assumption that the intercept is the only coefficient that differs between the relevant alternatives (in which case they can be pooled). The test statistic is asymptotically distributed χ^2_{37}.

Tables 5 and 6 present coefficients on rigidity dummies for various different wage measures; weekly pay results are repeated from Table 3 for ease of comparison.[14] Re-

[14] There were insufficient observations to reliably estimate separation equations for stayers including rigidity measures calculated from the hourly basic wage rate.

sults are shown from pooled probits as these seem robust to possible alternative modelling assumptions. For layoffs, the results using hourly basic wage rate tend to support theories of DNWR: cuts reduce layoffs, whereas (although insignificant at conventional levels) pay freezes tend to increase layoffs. For this pay measure, quits seem to be increased (although not significantly) by rigidity and are not affected by cuts. Rigidity measures calculated using hourly total pay do not significantly affect separations, which is not surprising given the measurement error problems affecting this pay measure.

	Weekly pay		Hourly pay		Hourly basic wage	
	probit	MEs	probit	MEs	probit	MEs
rigid	0.088 (1.2)	0.007 (1.1)	−0.049 (−0.4)	−0.004 (−0.4)	0.355 (1.7)	0.025 (1.0)
cut	0.105 (2.4)	0.009 (2.2)	0.0004 (0.01)	0.00003 (0.01)	0.070 (0.4)	0.004 (0.3)
Pseudo R^2	0.116		0.114		0.178	
Log likelihood	-2,669.2		-2,581.9		-198.0	
Observations	14,059		13,600		866	

Table 5a: Coefficients in quit probits: all workers

Notes: See Table 1 for definition of sample. Regressors are as in Table 3. Sample includes both stayers and movers in preceding year. Estimated coefficients are relative to the state of staying with the same employer. t-statistics are in parentheses. MEs are marginal effects. Marginal effects are calculated for the discrete change from 0 to 1.

	Weekly pay		Hourly pay	
	probit	MEs	probit	MEs
rigid	0.096 (1.2)	0.006 (1.1)	−0.106 (−0.8)	−0.005 (−0.9)
cut	0.072 (1.3)	0.004 (1.2)	0.002 (0.04)	0.0001 (0.04)
Pseudo R^2	0.116		0.106	
Log likelihood	-2,669.2		-1,745.9	
Observations	14,059		10,835	

Table 5b: Coefficients in quit probits: stayers

Notes: See Table 1 for definition of sample. Regressors are as in Table 3. Sample includes only job stayers in the preceding year. Estimated coefficients are relative to the state of staying with the same employer. t-statistics are in parentheses. MEs are marginal effects. Marginal effects are calculated for the discrete change from 0 to 1.

	Weekly pay		Hourly pay		Hourly basic wage	
	probit	MEs	probit	MEs	probit	MEs
rigid	0.248	0.018	0.185	0.013	0.473	0.021
	(3.3)	(2.7)	(1.7)	(1.4)	(2.0)	(1.3)
cut	0.134	0.009	−0.034	−0.002	−0.626	−0.011
	(2.6)	(2.4)	(−0.7)	(−0.8)	(−1.5)	(−1.9)
Pseudo R^2	0.051		0.048		0.167	
Log likelihood	1,810.8		-1,767.9		-103.6	
Observations	13,706		13,264		792	

Table 6a: Coefficients in layoff probits: all workers

Notes: See Table 1 for definition of sample. Regressors are as in Table 3. Sample includes both stayers and movers in preceding year. Estimated coefficients are relative to the state of staying with the same employer. t-statistics are in parentheses. MEs are marginal effects. Marginal effects are calculated for the discrete change from 0 to 1.

	Weekly pay		Hourly pay	
	probit	MEs	probit	MEs
rigid	0.234	0.016	0.177	0.012
	(2.9)	(2.4)	(1.5)	(1.3)
cut	0.161	0.010	0.002	0.0001
	(2.8)	(2.6)	(0.03)	(0.03)
Pseudo R^2	0.042		0.037	
Log likelihood	1,380.9		-1,356.9	
Observations	11,027		10,671	

Table 6b: Coefficients in layoff probits: stayers

Notes: See Table 1 for definition of sample. Regressors are as in Table 3. Sample includes only job stayers in preceding year. Estimated coefficients are relative to the state of staying with the same employer. t-statistics are in parentheses. MEs are marginal effects. Marginal effects are calculated for the discrete change from 0 to 1.

Tests were carried out to investigate (weak) exogeneity. These tests simply consist of adding the residuals from a regression (or probit) of the potentially-endogenous variable(s) on the instruments and exogenous variables to the (second-stage) main model (Rivers and Vuong 1988 discuss these tests in the case of probit models; Smith and Blundell 1986 independently developed them for the Tobit model; see also Newey 1987). Under the null that the regressors are exogenous (and the model is correctly specified), these residuals should have no explanatory power. The relevant exclusion restriction(s) can then be tested, the test statistics being distributed χ^2 under the null, with degrees of freedom equal to the number of potentially endogenous variables. As discussed above, the rigid and cut dummies are instrumented with deviations from job-match means of nominal pay growth and real pay level. Powers of tenure and experience are instrumented using their deviations from job-match means. Note that these tests require joint normality of the errors from the first and second steps (although Rivers and Vuong 1988 show that a weaker normality condition applying only to the residuals of the second stage is adequate).

Table 7 reports separate tests for the potentially endogenous variables. With respect to layoffs, the rigid dummy does appear to be exogenous, and the cut dummy is borderline exogenous, but both are endogenous with respect to quits. Tenure and experience strongly reject exogeneity. Results for job stayers alone were very similar and are not shown. It seems wise to allow for endogeneity of all these variables.

	Test statistic [p value]	
	Quit	Layoff
rigid* (χ^2_1)	36.8 [<0.001]	1.3 [0.26]
cut* (χ^2_1)	39.9 [<0.001]	4.1 [0.04]
rigid* and cut* (χ^2_2)	47.6 [<0.001]	4.1 [0.13]
tenure – tenure^4 (χ^2_4)	583.5 [<0.001]	280.3 [<0.001]
experience – experience^4 (χ^2_4)	514.3 [<0.001]	248.5 [<0.001]

Table 7: Exogeneity tests: all workers

Notes: See Table 1 for definition of sample. Sample includes both stayers and movers in preceding year. Tests are for the exclusion from second stage quit or layoff probits of first-stage residuals. * indicates residuals are calculated as the difference between actual values and predicted probabilities from probits for relevant variable on relevant instruments and all exogenous variables. For other variables, residuals are taken from OLS regressions of the relevant variable on relevant instruments and all exogenous variables. Instruments are: for rigid and cut, differences from job-match means of nominal pay growth and real pay; for tenure, difference from job-match means of tenure; for experience, difference from job-match means of experience. Rigidity dummies are calculated using weekly pay data.

Two-step instrumental variables (IV probit) and limited information maximum likelihood (2SCML) methods are used to allow for endogeneity. The latter method is the one used to generate the exogeneity test statistics in Table 7, and is a variant of the methods discussed in Rivers and Vuong (1988) and Smith and Blundell (1986). The standard errors for these models should be treated with caution as they do not allow for the fact that some of the regressors are fitted values from the first step. Maddala (1983) discusses correction of standard errors for two-stage estimation methods and shows that it is not straightforward: that the form of the correction is not standard and unfortunately needs to be derived separately for each type of model (p 252). Also note that marginal effects are not reported.

Consistent with the finding that rigid and cut dummies are borderline exogenous, allowing for the endogeneity of these hardly alters the results, whereas allowing for the endogeneity of tenure does change the results (full results from these investigations are not reported). The results in Table 8 allow for endogeneity in all these variables.[15]

Although the results are qualitatively similar, there are numerical differences between coefficients estimated by the two methods. But the most striking aspect of the results in Table 8 is the change in the signs and magnitudes of the coefficients on the endogenous variables compared to for example the pooled probit results in Table 3. The effect of rigidity appears to be amplified. Cuts are surprisingly now estimated to have a significantly negative effect on quits. Also unexpectedly, the linear effect of tenure is now positive, and that of experience negative. Both tenure and experience now seem to have stronger influences on separations. With the exception of the rate of unemployment, the estimated effects of the exogenous variables are much less changed, largely retaining their previous signs, with significance, and magnitudes not greatly altered.

In Section 4 it was pointed out that when two-step methods involve the estimation of probits in the first step, the estimable coefficients relating to the endogenous regressors will differ from the standard probit case. This is unlikely to explain all the differences between Tables 3 and 8: when powers of tenure alone were treated as endogenous, which involves first-step regressions and so does not involve different estimable coefficients, the tenure coefficients were similar to those in Table 8.

[15] The significance of the residuals included in the 2SCML equations in Table 8 is an indicator of the endogeneity status of the relevant variable.

	quit		layoff	
	IV probit	2SCML	IV probit	2SCML
rigid*†	1.345 (1.3)	0.868 (0.6)	1.338 (1.3)	2.087 (1.7)
cut*†	−0.476 (−3.7)	−0.425 (−3.0)	−0.067 (−0.5)	−0.055 (−0.4)
tenure†	0.684 (10.4)	0.627 (8.4)	0.602 (7.6)	0.679 (6.4)
$\text{tenure}^2/10$†	−0.681 (−8.1)	−0.550 (−5.1)	−0.547 (−5.7)	−0.531 (−4.4)
$\text{tenure}^3/100$†	0.266 (6.8)	0.218 (3.6)	0.209 (5.0)	0.182 (3.6)
$\text{tenure}^4/1000$†	−0.035 (−5.9)	−0.030 (−2.5)	−0.027 (−4.5)	−0.021 (−3.0)
experience	−0.220 (−4.1)	−0.174 (−2.9)	−0.200 (−3.0)	−0.227 (−2.6)
$\text{experience}^2/10$	0.102 (2.1)	0.043 (0.8)	0.106 (1.9)	0.126 (1.8)
$\text{experience}^3/100$	−0.024 (−1.4)	−0.007 (−0.4)	−0.028 (−1.6)	−0.036 (−1.7)
$\text{experience}^4/1000$	0.002 (1.0)	0.0004 (0.1)	0.003 (1.5)	0.004 (1.7)
education	0.003 (0.4)	0.015 (1.6)	0.009 (1.3)	0.022 (2.9)
union coverage*	−0.423 (−8.1)	−0.697 (−7.9)	−0.468 (−6.6)	−0.445 (−5.4)
married*	−0.008 (−0.2)	−0.009 (−0.2)	−0.112 (−1.8)	−0.027 (−0.4)
white*	0.253 (1.7)	0.473 (2.8)	−0.029 (−0.2)	0.151 (0.8)
female*	−0.126 (−2.7)	−0.087 (−1.5)	−0.016 (−0.3)	0.018 (0.3)
workplace employment 25-200*	−0.013 (−0.2)	−0.039 (−0.6)	−0.036 (−0.5)	0.062 (0.8)
workplace employment >200*	−0.091 (−1.4)	−0.143 (−1.9)	−0.074 (−1.0)	0.050 (0.5)
regional unemployment rate (%)	−5.465 (−4.5)	−13.249 (−5.1)	−4.318 (−3.4)	−6.879 (−4.4)
$\varepsilon_{\text{rigid}}$		−0.712 (−0.5)		−1.465 (−1.4)
ε_{cut}		0.710 (4.5)		0.202 (1.2)
$\varepsilon_{\text{tenure}}$		−1.211 (−13.2)		−0.905 (−9.7)
$\varepsilon_{\text{tenure}^2}$		1.143 (8.5)		0.796 (6.8)
$\varepsilon_{\text{tenure}^3}$		−0.458 (−6.2)		−0.288 (−5.6)
$\varepsilon_{\text{tenure}^4}$		0.061 (4.4)		0.034 (4.7)
Pseudo R^2	0.130	0.248	0.093	0.137
Log Likelihood	-2,110.1	-1,823.2	-1,293.0	-1,231.4
Observations	11,562	11,562	11,251	11,251

Table 8: Two-step estimates: all workers

Notes: See Table 1 for definition of sample. Sample includes both stayers and movers in preceding year. Regressions also include standard region and 1-digit occupation dummies. * indicates dummy variable. † indicates variable treated as endogenous. Instruments include deviations from job-match means of nominal pay growth, real pay level, tenure and powers thereof, and all exogenous regressors. Estimated coefficients are relative to the state of staying with the same employer. t-statistics are in parentheses. Standard errors, log-likelihood and R^2 relate to final-stage probits. For IV probit, continuous endogenous variables are replaced by their fitted values from first step regressions and endogenous dummy variables are replaced by their predicted probabilities from first step probits. ε denotes residual from relevant first-step regression or difference between actual values and predicted probabilities from first-step probit.

	quit		layoff	
	IV probit	2SCML	IV probit	2SCML
rigid*†	1.436 (1.1)	1.639 (1.0)	1.846 (1.6)	1.644 (1.5)
cut*†	−0.517 (−3.5)	−0.539 (−3.2)	−0.092 (−0.6)	−0.062 (−0.4)
tenure†	0.861 (9.4)	1.018 (6.7)	0.800 (7.5)	0.517 (6.6)
$\text{tenure}^2/10$†	−0.804 (−7.3)	−1.066 (−4.8)	−0.707 (−5.9)	−0.401 (−4.2)
$\text{tenure}^3/100$†	0.302 (6.2)	0.506 (3.8)	0.260 (5.2)	0.140 (3.3)
$\text{tenure}^4/1000$†	−0.039 (−5.4)	−0.082 (−3.1)	−0.032 (−4.7)	−0.017 (−2.7)
experience	−0.172 (−2.4)	0.116 (0.8)	−0.261 (−3.0)	−0.175 (−2.6)
$\text{experience}^2/10$	0.054 (0.8)	−0.239 (−1.7)	0.160 (2.3)	0.078 (1.4)
$\text{experience}^3/100$	−0.010 (−0.4)	0.091 (1.8)	−0.047 (−2.2)	−0.019 (−1.1)
$\text{experience}^4/1000$	0.001 (0.3)	0.013 (−1.9)	0.005 (2.2)	0.002 (1.0)
education	0.007 (0.9)	0.057 (2.5)	0.019 (2.5)	0.013 (1.8)
union coverage*	−0.474 (−6.7)	−0.988 (−4.8)	−0.406 (−5.2)	−0.511 (−6.8)
married*	0.034 (0.5)	0.110 (1.3)	−0.031 (−0.4)	−0.106 (−1.6)
white*	0.215 (1.3)	0.712 (2.9)	0.065 (0.3)	0.038 (0.2)
female*	−0.184 (−3.2)	0.058 (0.5)	0.002 (0.02)	0.002 (0.03)
workplace employment 25-200*	0.038 (0.6)	0.110 (1.2)	0.043 (0.6)	−0.022 (−0.3)
workplace employment >200*	−0.049 (−0.6)	−0.051 (−0.5)	0.031 (0.3)	−0.058 (−0.7)
regional unemployment rate (%)	−6.374 (−4.5)	−28.256 (−3.2)	−4.519 (−3.1)	−7.119 (−5.0)
$\varepsilon_{\text{rigid}}$		−1.476 (−0.9)		−1.954 (−1.6)
ε_{cut}		0.840 (4.4)		0.258 (1.4)
$\varepsilon_{\text{tenure}}$		−1.390 (−9.9)		−1.146 (−9.0)
$\varepsilon_{\text{tenure}^2}$		1.135 (5.5)		0.979 (6.7)
$\varepsilon_{\text{tenure}^3}$		−0.327 (−2.4)		−0.342 (−5.6)
$\varepsilon_{\text{tenure}^4}$		0.007 (0.2)		0.040 (4.8)
Pseudo R^2	0.139	0.223	0.088	0.128
Log Likelihood	-1,438.0	-1,296.4	-1,009.3	-965.3
Observations	9,320	9,320	9,153	9,153

Table 8: Two-step estimates: stayers

Notes: See Table 1 for definition of sample. Sample includes only job stayers in preceding year. Regressions also include standard region and 1-digit occupation dummies. * indicates dummy variable. † indicates variable treated as endogenous. Instruments include deviations from job-match means of nominal pay growth, real pay level, tenure and powers thereof, and all exogenous regressors. Estimated coefficients are relative to the state of staying with the same employer. t-statistics are in parentheses. Standard errors, log-likelihood and R^2 relate to final-stage probits. For IV probit, continuous endogenous variables are replaced by their fitted values from first step regressions and endogenous dummy variables are replaced by their predicted probabilities from first step probits. ε denotes residual from relevant first-step regression or difference between actual values and predicted probabilities from first-step probit.

7 Conclusion

This paper assesses the cost of downward nominal wage rigidity (DNWR) by investigating whether individual workers are more likely to be laid off after their pay has been frozen and whether nominal cuts prevent layoffs. The counterpart hypotheses are also investigated – in particular whether nominal cuts lead to quits. Micro data are used, in the hope of resolving the so-called 'micro-macro' puzzle: the contrast between the plentiful evidence of DNWR from micro data but the general failure of empirical macroeconomic studies to find any unemployment-related cost of this rigidity.

Results suggest that DNWR does impact on separation decisions. If an individual's pay is held above the warranted level by DNWR, the individual is more likely to be laid off. If the individual accepts a nominal pay cut, their chance of being laid off is lower than if their pay had been frozen, but they do face a higher probability of layoff than workers whose pay rises in nominal terms. Nominal cuts encourage quits among movers – that is, an individual who has accepted a pay reduction with a job change is more likely to subsequently quit to a new job. But job stayers who accept nominal cuts are not significantly more likely to quit than those who receive raises. Nominal rigidity does not prevent quits.

These results do suggest that there are real costs of DNWR. There is no 'micro-micro' puzzle: the puzzling lack of macroeconomic evidence for the costs of DNWR is not repeated at the micro level.

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Data Appendix

This Appendix gives details of the data used that are omitted from the main text.

Pay: Three measures of pay are used, one weekly and two hourly: usual gross weekly pay, usual gross hourly pay and the hourly basic wage rate. The BHPS derived monthly pay variable is not used, as that includes observations that are imputed and others that are calculated from stated net pay. Both these procedures induce error, which even if very small might erroneously cause a pay change to be recorded when in fact pay was frozen. Weekly pay is defined as latest gross pay divided by the relevant pay period, if latest gross pay was usual, or as usual gross pay divided by the relevant pay period if latest gross pay was unusual. In a small number of observations, the net pay period is used where the gross pay period is not available. Hourly pay is obtained by dividing this measure of usual gross weekly pay by the sum of normal weekly hours (excluding overtime) plus normal overtime hours per week. The hourly basic wage rate is taken directly from BHPS data. The rigid dummy is coded one if there is no growth in the relevant (nominal) pay measure between last year and this. A pay cut is defined as a fall in the pay measure. Real pay measures are created by dividing nominal pay by the RPI index (all items).

Education: The number of years of education is defined as the age left school, or the age left further education if this is greater, minus 5.

Establishment size: The base category is less than 25 employees. Two other categories define medium-sized firms (at least 25 but less than 200 employees) and large firms (200 or more employees). The BHPS category "don't know but more than 25" is recoded to missing.

Experience: Work experience is the sum of spell durations in employment and self-employment. This experience measure takes account of all job spells, and is measured to the nearest month. Experience is taken from an updated version of the 'reconciled' employment histories data set for the individual's main activity produced by Paull (2003).

Female: Is coded 1 if the individual is female, 0 if male.

Marital status: The base category is never married, divorced, separated or widowed: the latter three categories were found to have indistinguishable effects on turnover to those of 'never married'

Regional unemployment: Is measured by the claimant count for each of 11 standard regions (including Greater London). The ILO measure, also for 11 standard regions, which is available from 1993 onwards, was also used, with almost no difference in results. Unemployment rates measured on a quarterly basis are averaged over Q4-Q3 to give an average figure that relates to the unemployment that prevailed at the time of any turnover (or pay) decision between one interview and the next.

Tenure: Tenure is tenure with the current employer. With the aid of information about when job spells started between interviews, tenure is measured to the nearest month. Tenure is taken from an updated version of the 'reconciled' employment histories data set for the individual's main activity produced by Paull (2003).

Union coverage: The individual's workplace is coded as covered if there is a relevant recognised union or staff association at the workplace. Between waves 2 and 4, unionisation questions were not asked of employees in the same job as the previous year. It is assumed that unionisation status remained the same during this period for stayers. *Union member*: An individual's trade union membership is coded 1 if they are a member of a workplace union, and 0 if they are not or if there is no union or staff association at the workplace.