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Estimating the Wage Costs of Inter- and Intra-Sectoral Adjustment

by

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Abstract

The proposition that labour market adjustments to intra-industry trade are less costly than adjustments to inter-industry trade is a widely-held belief amongst trade economists. If it is the case that there are significant sector-specific skills, then this 'smooth adjustment hypothesis' seems intuitive. However, direct evidence relating to this issue remains largely anecdotal. In this paper we adopt the methodology of the micro-econometric labour literature to estimate the returns to tenure within firms, industries and occupations in order to predict the costs, in terms of wage losses, of moving jobs between and within sectors. To do this we use a large panel of individual workers for the UK over a long period (1975–1998), which enables us to control for unobserved fixed effects which may jointly determine the propensity to move jobs and the wage level.

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4. Some Previous Estimates
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1 Introduction

The proposition that labour market adjustments to intra-industry trade are less costly than adjustments to inter-industry trade is a widely-held belief amongst trade economists. This proposition is based on the idea that factors of production, such as labour, can be reallocated within industries more easily than between industries. However, as noted by Brülhart, Murphy & Strobl (1998, p.1), "there exists no formal theoretical underpinning for this assumption . . . empirical tests of [this hypothesis] have been crude and rather indirect."

There is however a well-developed theoretical and empirical literature in labour economics concerned with the relationship between job tenure and wages which has a direct bearing on this question. Returns to tenure within a firm are usually interpreted in terms of returns to specific human capital, although they are also consistent with a number of other theories of worker compensation. Estimates of the wage returns to firm tenure are common, either by examining within-job wage growth, or by examining the changes in wages which occur when individuals change jobs. As noted by Neal (1995), however, there is far less work which measures the value of industry-specific skills.[1] If skills do have a significant industry-specific component, this provides direct evidence that adjustment costs will be lower for workers who move jobs within industries compared to those who move jobs between industries.

It also seems reasonable to suppose that some skills may be specific to occupations, and therefore an additional question of interest is the relative importance of returns to occupational tenure. This question too appears to have received little attention in the literature.

In this paper we analyse the extent to which wages increase with tenure not only within firms, but also within industry and occupation. By doing this we are able to provide estimates of the potential 'cost' to workers of changing jobs, industry and occupation. We use a large panel dataset of UK employees over the period 1975–1995, which enables us to examine the consequence of different assumptions about likely biases which may result from correlations between the unobservable determinants of wages and the measures of tenure which we use.

In Section 2 we outline some basic theory about the relationship between tenure and wages, and in Section 3 we present an econometric framework for analysing this relationship. Some previous estimates are presented in Section 4. Section 5 describes the data we use, and our results are presented in Section 6. Section 7 concludes and suggests some additional relevant research questions.

[1] Neal (1995), Kletzer (1996) and Kim (1998) are exceptions. There appear to be no measures of the returns to industry tenure in the UK literature.

2 The wage effects of worker mobility

Workers who are involuntarily displaced from their jobs suffer wage losses, and these losses tend to be higher for more senior workers. This fact lends support to the idea that part of a worker's remuneration consists of a return to tenure, and this is foregone if the employment relationship is severed. The most common explanation for these returns to tenure is that workers accumulate human capital specific to a particular job (Becker 1962). Increasing wages reflect in part increasing productivity, and also a means by which any match-specific rents generated by training are shared. General human capital, that which is not specific to a particular job, also accumulates, and this explains the positive relationship between wages and total labour market experience.

In this context, it seems natural to consider whether some proportion of the observed increase in wages with firm tenure is due to the accumulation of industry-specific human capital. As noted by Neal (1995, pp.653–654): "All firms in a given manufacturing industry may value a common set of skills that are vital to the production process in that industry. However, these same skills may not be valued by firms that manufacture different product lines." As well as being of importance to the individual worker, the extent to which skills are industry specific is clearly of great interest in determining the cost of aggregate adjustment where many workers move from one set of industries to another.

However, there are a number of reasons why the observed relationship between job tenure and wages might in fact be misleading. First, high wage jobs may last longer. Second, more able individuals may change jobs less often, and hence tend to have longer tenure. Third, workers who choose to move jobs will tend to do so for higher wages.

For example, the mover/stayer model (Blumen, Kogan & McCarthy 1955) argues that workers with low productivity have greater levels of mobility than those with high productivity. Wages appear to be negatively correlated with mobility because it is the low productivity, low wage workers who are moving. However, if sufficient controls could be made for worker quality then wages and tenure would in fact be uncorrelated.

In contrast, the search model of job matching (Burdett 1978, Jovanovic 1979*a*) assumes that workers voluntarily move to jobs for which they are better suited, since higher quality matches yield higher wages. This can lead to wages and tenure appearing to be *negatively* related even though wages are assumed to be constant over the course of a job, and even though better matches last longer. The negative correlation arises because job movers do so for higher wages, and job movers tend to have lower tenure. In fact, wages rise only with total experience, because workers with more experience are more likely to have found a good match. The model also predicts that mobility decreases with age and experience, since younger workers will tend to have a lower quality of match.

Similarly, the experience-good model of job matching (Jovanovic 1979*b*) suggests that workers are more likely to move when the true quality of the worker-firm match proves to be lower than anticipated. Rather than suffer the consequent low wage, the individual (or firm) seeks out a more productive relationship.

It may therefore be difficult to determine whether the observed relationship between tenure and wages is an overestimate of the true relationship because more able people are less mobile, or an underestimate because job movers do so for wage gains. Some recent estimates have concluded that returns to tenure are negligible (Altonji & Shakotko 1987), while others argue that they are large and significant (Topel 1991). Similar problems will also be associated with identifying returns to industry and occupational tenure. In the next section we lay out a framework which makes explicit how these problems arise.

3 A framework for estimating returns to industry tenure

A simple relationship between wages, experience and tenure can be written as:

$$w_{ijt} = X_{ijt}\gamma_1 + T_{ijt}\gamma_2 + I_{ijt}\gamma_3 + O_{ijt}\gamma_4 + \mathbf{x}'_{ijt}\boldsymbol{\beta} + \varepsilon_{ijt}, \tag{1}$$

where w_{ijt} is the (log) wage for individual i on job j at time t, X_{ijt} is total labour market experience, T_{ijt} is firm tenure, I_{ijt} is industry tenure and O_{ijt} is occupational tenure. The precise relationship between experience and tenure is unlikely to be linear as shown above, but we leave investigation of this issue for the empirical work. $\mathbf{x}_{ijt}$ is a vector of other measurable characteristics thought to influence wages. The unmeasured component of this relationship, ε_{ijt}, can be decomposed into three separate terms:

$$\varepsilon_{ijt} = \phi_{ijt} + \mu_i + \nu_{ijt}. \tag{2}$$

ϕ_{ijt} is the unobserved component of wages due to a specific worker-firm pair. This can be thought of as reflecting the unobserved value of a particular match between a worker and a firm.[2] In this framework ϕ_{ijt} is allowed to vary over the course of a job, but a common

[2] In principal there might also be an unobserved component to a particular match between an individual and an industry, or between an individual and an occupation. However, here we assume that these differences are

restriction is to assume that it is constant within jobs so that $\phi_{ijt} = \phi_{ij}$. μ_i is the unobserved person-specific component of wages, and is assumed not to vary over time. ν_{ijt} accounts for any other unobserved component of wages.

We are interested in estimating the returns to experience and the three different forms of tenure and in particular γ_3, the returns to industry tenure. If all skills are specific only to a particular job, then $\gamma_2 > 0$ and $\gamma_3 = \gamma_4 = 0$. In this case there is no 'cost' to moving between industries or occupations above that which occurs when workers move job.

Within this framework, biases will arise in the estimation of the parameters on X, T, I and O if a correlation exists between the unobservables and these variables. There are at least three reasons why elements of ε_{ijt} might be correlated with experience and tenure in our data.

First, if unobserved fixed effects μ_i are correlated with tenure. For example, if workers with higher unobserved ability are more likely to move jobs, then they will also tend to have lower values for tenure. This would not, however, lead to any correlation between μ_i and total experience, X_{ijt}. But if the correlation between μ_i and tenure occurs because workers with low values of μ_i are more likely to have periods of unemployment, then ε_i would also be correlated with total experience X_{ijt} as well as the individual elements of tenure. The panel data we use (see Section 5) are particularly prone to individuals not being recorded in a particular year, even if they are in employment. If the probability of not being in the sample is also correlated with the unobserved fixed effect, a similar problem ensues.

Second, if worker-firm match quality ϕ_{ijt} and tenure are correlated. Altonji & Shakotko (1987) argue that OLS estimates of γ_2 (and in our case γ_3 and γ_4 as well) will be biased upwards because workers with high values of ϕ_{ijt} are less likely to quit, and hence ϕ_{ijt} and T_{ijt} will be positively correlated. However, Topel (1991) shows that OLS estimates will actually be biased downwards because ϕ_{ijt} and T_{ijt} are negatively correlated: individuals who move jobs do so in order to obtain higher values of ϕ_{ijt}, and movers have low tenure. Similar arguments apply to the correlation between ϕ_{ijt} and I_{ijt} and O_{ijt}. The correlation between ϕ_{ijt} and total experience X_{ijt} is more clear cut. Workers who have been in the labour market for longer are more likely to have received offers of jobs with high values of ϕ_{ijt}, and therefore ϕ_{ijt} and X_{ijt} will be positively correlated.

Third, it may be the case that *past* values of ν_{ijt} may be correlated with current values of tenure and experience, even if current values are uncorrelated. In the context of Equation (1)

adequately represented by ϕ_{ijt}, since the majority of changes between industries and occupations occur between rather than within firms.

this is not a difficulty. However, it should be apparent that any model which estimates changes in wages over time, or which uses within-group means, may produce biased estimates if shocks to past values of ν_{ijt} influence decisions to move between jobs, industries or occupations.[3] In the context of the panel data we use, this type of correlation may be a particular problem, because transitory shocks to ν_{ijt} may influence the probability of appearing in the sample. The measures of experience and tenure are generated by summing responses over each time period (see Section 5 for more detail). If a temporary downward shock to earnings reduces the probability of not appearing in the sample at t, then this will reduce the calculated values of X, T, I and O for future values of t.

4 Some previous estimates

Previous estimates of γ_1 and γ_2 tend to come from one of two sources. Direct estimates of models similar to Equation (1) come from the literature on returns to seniority, (e.g. Abraham & Farber 1987, Altonji & Shakotko 1987, Topel 1991). Alternatively, the literature on the wage effects of job displacement calculates wage changes following job moves (e.g. Kletzer 1989, Jacobson, LaLonde & Sullivan 1993, Neal 1995). For example, a first-differenced version of Equation (1) for workers who change from job k to job j, but remain in the same industry and occupation, yields:

$$\Delta w_{ijt} = (\gamma_1 + \gamma_3 + \gamma_4) - T_{ikt-1}\gamma_2 + \Delta \mathbf{x}'_{ijt}\boldsymbol{\beta} + \Delta\varepsilon_{ijt}, \qquad (3)$$

where $\Delta w_{ijt} = w_{ijt} - w_{ikt-1}$, and T_{ikt-1} refers to tenure on the previous job at time $t-1$. In a model such as this, the cost of worker dislocation is a function of previous job tenure. Common alternative specifications regress wages on dummy variables recording displacement events in previous periods, which allow for the identification of 'scarring' effects of displacement.

In principal it makes no difference whether estimates of γ_2 are taken from Equation (1) or (3), although in practice results will depend on how the estimates deal with the possible biases resulting from the correlation of ε_{ijt} with measures of tenure. One important difference between the two methods is in the choice of sample. By definition, a sample of displaced

[3] This problem is familiar from the empirical literature on the measurement of the impact of interventions in the labour market — see, for example, Heckman & Smith (1999). If those who change jobs do so because they experience a temporary dip in their earnings, then any type of differenced estimate will tend to exaggerate the impact of changing jobs, and hence produce a downward bias on the returns to tenure.

workers have not moved voluntarily between jobs, and therefore the correlation between ϕ_{ijt} and tenure is likely to be different. Indeed, it seems more plausible to assume that ϕ_{ijt} and T_{ikt-1} are uncorrelated for displaced workers. The problem of a permanent ability bias μ_i still remains, however, and may be increased if workers of lower ability are more likely to be displaced.

Altonji & Shakotko's (1987) approach to the selectivity problem is to adopt an instrumental variables method to estimate γ_1 and γ_2. If it is assumed that match specific effects are fixed within jobs, so that $\phi_{ijt} = \phi_{ij}$ then this implies

$$\sum (T_{ijt} - \bar{T}_{ij})(\phi_{ij} + \mu_i) = 0$$

and so valid instruments for T_{ij} are provided by $T_{ijt} - \bar{T}_{ij}$, deviations from mean tenure within each job. Instrumental variables estimates of Equation (1) are calculated using the GLS estimator suggested by Hausman & Taylor (1981). The results obtained in Altonji & Shakotko suggest that the returns to job tenure are small once heterogeneity bias is corrected for and the main influence on wage growth is provided by general labour market experience.[4] However, if match-specific effects are not constant within jobs ($\phi_{ijt} \neq \phi_{ij}$) then they may still be correlated with deviations from mean tenure or experience (Topel 1991). If these correlations are positive then the resulting downward bias in the estimated returns to tenure may explain Altonji & Shakotko's results.

Topel (1991) uses a two-step method which attempts to provide a lower bound on the returns to tenure. Consider a version of Equation (1) without measures of I_{ijt} and O_{ijt}. As before, match characteristics are assumed to be constant within jobs, and so a first-differenced estimate of wage growth within jobs gives an unbiased estimate of $(\gamma_1 + \gamma_2)$:

$$w_{ijt} - w_{ijt-1} = \gamma_1 + \gamma_2 + (\varepsilon_{ijt} - \varepsilon_{ijt-1}). \tag{4}$$

The second step of Topel's method involves estimation of a current wage equation. This will depend on the initial wage, $X_0\gamma_1$, plus any additional returns to tenure, $T_{ijt}(\gamma_1 + \gamma_2)$. Thus if $\hat{\gamma}_1 + \hat{\gamma}_2$ is estimated from Equation (4), as a second stage γ_1 may be estimated using

$$w_{ijt} - T_{ijt}(\hat{\gamma}_1 + \hat{\gamma}_2) = X_0\gamma_1 + e, \tag{5}$$

[4]Note that this method is very similar to a standard fixed-effect estimate of Equation (1). Any type of first-difference or fixed-effects estimate of Equation (1) will remove the bias associated with the correlation between μ_i and tenure. In addition, if $\phi_{ijt} = \phi_{ij}$ then estimates of wage changes within jobs will also purge the correlation between match-specific unobservables and measures of tenure. We adopt this method in Section 6.

where $e = \varepsilon + T[(\gamma_1 + \gamma_2) - (\hat{\gamma}_1 + \hat{\gamma}_2)]$. If match quality rises with experience then X_0 and e will be positively correlated, and hence Equation (5) only provides a lower bound on the returns to seniority. Even so, Topel finds that the average returns are substantial, with 10 years of tenure raising the average wage by 25%, a finding that does not vary across occupations. Further he finds that for unionists, 10 years of seniority raise the average wage by 40%.

A number of problems remain with Topel's methodology. If within-job wage growth has a strong permanent component which is firm specific, then workers in firms with high wage growth will be less mobile. If this is the case then the sample used in the first stage regression will be a non-random sample, and will tend to consist of better jobs. Equation (4) will therefore overstate the average wage increase and ($\hat{\gamma}_1 + \hat{\gamma}_2$) will be biased upwards. In addition, Topel's method does not deal with the fact that individual unobservable effects may be correlated with tenure.

An example of the displacement literature is provided by Jacobson *et al.* (1993). Focusing on high tenure workers (6 or more years of tenure) they estimate an equation of the form

$$w_{ijt} = \alpha_i + \lambda_t + \mathbf{x}'_{it}\boldsymbol{\beta} + \sum_{k \geq -m} D^k_{it}\delta_k + \varepsilon_{it}. \tag{6}$$

In this model the dummy variables represent the event of displacement in each time period: $D^k_{it} = 1$ if, in period t, worker i had been displaced k periods earlier. These dummies also measure future displacement if $k < 0$. The parameter δ_k is the measure of the effect of displacement k quarters following (or preceding) its occurrence. As in Equation (1), $\mathbf{x}_{it}$ represent other time-varying personal characteristics. The α_i represent both observable and unobservable fixed effects, and the λ_t are time dummies.

In order to compare their estimates with earlier researchers who use the Displaced Worker Survey (DWS), they split their sample according to whether movement is voluntary or enforced. Although they do not know the exact form of each separation (quits, layoffs etc), they use firm level employment data to identify 'distressed' firms. They therefore create a 'mass-layoff' sample of displaced workers. For all workers they find that high tenure workers experience substantial earnings losses that depend on local labour market conditions, the business cycle and the extent of unionisation. Individuals in distressed firms additionally suffer substantial earning losses prior to displacement and persistent long term losses following job loss. Note that the method of Jacobson *et al.* has the same potential biases as the other papers discussed. For example, estimates will be biased if firms selectively lay off workers whose performances are poor.

As noted in the introduction, there are few estimates of γ_3 or γ_4 in the literature: very few papers consider estimating returns to industry tenure in addition to returns to job tenure. An exception is Neal (1995), who uses the DWS. The advantage of this sample in comparison with the data set used by Jacobson *et al.* is that the cause of job separation is not open to question. However estimates obtained from this sample are likely to suffer from sample selection as it consists of workers that by definition involuntarily lose their jobs. Neal runs a separate regression for industry switchers and industry stayers and compares the returns to tenure in the two subsets. This is equivalent to entering industry switching as an interaction term. Using this methodology however he obtains estimates of the returns to tenure of a similar magnitude to Topel (1991).

Instead of trying to find instruments for tenure, Neal treats the bias that results from the correlation between unobserved ability and industrial mobility as a selection problem (Heckman 1979). That is, he estimates a selection equation using a Probit model to determine whether an individual who is displaced also changes industry. Identification of the model relies on exclusion restrictions placed on the wage regression, and to this end Neal uses total number of jobs and rate of job growth in the pre-displacement industry as predictors of the probability of moving sector which are not thought to directly influence wages.[5]

5 Data

The data that we use come from the UK New Earnings Survey Panel Dataset (NESPD). The NESPD is a panel of a random sample of approximately 1% of civilian employees in employment in Great Britain from 1975 to 1998. The data are collected from employers under the Statistics of Trade Act 1947, which ensures a generally very high response rate.[6] Although the sample is large, and covers a long time period, the NESPD contains only a limited amount of information on the individuals. Most seriously, we have no information concerning educational attainment.

A second drawback to the NESPD is that the sample under-records individuals who have recently changed employers (Elias & Gregory 1994). That is, individuals not recorded in the panel in a particular year may not necessarily be unemployed or out of the labour force,

[5] Restrictions on the functional form of the wage equation may also be used to identify the model.

[6] A detailed description of the NES and the NESPD can be found in Gregory & Thomson (1990) and Elias & Gregory (1994).

but instead be employed with a new firm. Thus we can expect that measures of total labour market experience calculated from the NESPD are underestimates. Set against this, however, is the fact that the survey is carried out at a particular point in time in each year. Individuals may therefore be recorded as being employed at t and $t+1$ even if they were unemployed for some period between the two points. This will lead to overestimates of total labour market experience.

For the purposes of this study, the sample consists of all male workers who *entered* the labour market between 1975 and 1995. Although this reduces the sample size considerably, it enables us to construct a measure of total labour market experience which does not rely on making assumptions about time spent out of the labour market before the start of the panel in 1975. We concentrate on males in this analysis because the employment records of females in the NESPD are generally thought to be less reliable. The last three years of the data (1996 to 1998) are excluded because a change of industry classification makes the calculation of a consistent measure of industry difficult.

For each individual we calculate a measure of total labour market experience, tenure with current firm, tenure in current industry and tenure in current occupation. Occupations are defined using the 22 sub-major groups of the 1980 Standard Occupational Classification (Elias & Gregory 1994, pp.49–50). Industries are defined from one of the 42 2-digit 1980 Standard Industrial Classifications.[7] Total labour market experience X_{ijt} is defined as the total number of years since 1975 in the labour force: employers are asked if each employee had been working for the present firm for more than 12 months, and firm tenure, T_{ijt}, is calculated by summing these responses across years. Similarly, by comparing occupation and industry codes between years we can determine whether an individual has moved industry or occupation, and hence calculate industry and occupational tenure, I_{ijt} and O_{ijt}.

One drawback with models of panel data, and particularly models which rely on differencing, is that measurement error may increase the inconsistency of the estimates relative to OLS, even though the inconsistency due to correlation of μ_i and the right hand side variables is removed. This occurs because differencing data measured with error can reduce the signal-to-noise ratio of the data (Hsiao 1986, pp63–64). In our case, we might worry whether errors in recorded firm tenure, industry or occupation might bias our estimates. This will always be

[7]Some grouping of industries is necessary to ensure greater consistency across all years of the data; the groupings used are listed in Table A.1. From 1975 to 1982 the NESPD used the 1968 SIC classification; from 1982 to 1995 the 1980 SIC classification was used. Cross-coding was achieved by comparing the codes for 1982, which contained both definitions.

a problem, but there are several reasons for hoping that data from the NESPD are less prone to measurement error than other surveys. First, the data on tenure are created by cumulating year-on-year responses rather than relying on recall from more than 12 months in the past. Second, the data are collected from employers rather than employees, whom we would presume are more likely to be able to accurately describe the activity of the company. Third, the data on occupations have an inbuilt 'stability' in the sense that the worker's occupation at t was only coded as different to the occupation at $t-1$ if the respondent explictly stated that the worker's job had changed from the previous year. Thus any measurement error in occupation is likely to have serial correlation, which as Hsiao notes, lessens the problem associated with first-differencing.

Many individuals do not have a complete work history in every year. Individuals may be missing from the panel in a particular year for a variety of reasons. First, they may be unemployed or out of the labour force. Second, they may not have been located by the survey, possibly because they recently changed employer. Third, their earnings may not have been sufficient to qualify them for income tax and National Insurance contributions, in which case they fall outside the scope of the survey. In addition, individuals may have missing information on a variable in a particular year, or their pay may have been affected by absence or part-time working. We cannot use these observations in estimates of wage equations, but it is important that they *are* used in calculation of the tenure variables. Thus, for example, we do not assume that an individual who was not in the panel at $t-1$ must have changed employer at t. In addition, we create a series of variables which record each individual's status at t and $t-1$, in case absence from the panel or missing data are correlated with earnings. These variables are described in more detail in Section 6.

Table A.2 shows the basic sample used, means of the age, tenure and experience variables and the numbers moving into and out of the panel at each point in time. The sample increases each year as new entrants enter the labour market. Note that re-entrants are quite a high proportion of the sample. From 1980 to 1995, between 10%–14% of the sample at t were not in the sample at $t-1$. As noted earlier, this is an overestimate of the proportion who did not have employment in the previous year. The sample also ages as time passes, because we only observe individuals as they enter the labour market from 1975 onwards. Thus in 1975 everyone is aged 16 or under. As a result, average measures of experience and tenure also increase over time. Total experience must increase faster than any of the other measures. The average tenure within an industry is slightly longer than average tenure within an occupation, which is slightly longer again than average firm tenure. Changes between firms are therefore the most common occurrence, followed by changes between occupation, and finally changes between industry.

Some descriptive statistics on the whole sample are shown in Table A.3. The total sample consists of 61,061 individuals, who are in the panel (on average) for 8.6 years. About 6.6 years of this time are spent in the labour market. Note that the number of person-years is not given by the product of the number of individuals and average labour market experience, as might be expected, because the number of person-years excludes years with missing data or where pay was affected by absence. The average probability of starting a job in a new firm is 0.285, largely because the sample are young: this probability falls from 0.51 for 18 year-olds to around 0.15 for 36 year-olds.[8] A similar negative relationship also exists between age and the probability of changing industry and occupation.

The final panel of Table A.3 shows the joint probabilities of moving between firms, industries and occupations. There is a strong association between the three: of the 28.5% who move firms, 16.7% also move industry and occupation. This will in part be due to measurement error, since a respondent in a new firm is less likely to describe that firm's activity, and that worker's occupation in exactly the same way. This is particularly true for information on occupation because occupational coding for workers who had worked for their current employer for more than 12 months was only altered if the respondent explictly stated that the worker's job had changed from the previous year.

More surprising is the fact that of the 71.5% who do *not* change firms, 6.3% appear to change industry. Further examination of the data reveals that the majority (about 60%) of these apparent switches are caused by gaps in the data: individuals re-enter the panel with a different industry code, but are reported as working for their current firm for more than 12 months. It seems likely therefore that these individuals changed industry in a year in which they do not appear in the panel. A further 13% of these switches occur one year after changing firm, suggesting that perhaps the change in industry did in fact coincide with a change of firm. In both these cases, although the precise year of the move between industries is not recorded accurately, the effect on our measurement of industrial tenure will be small. It would appear that genuine coding error is not the usual cause of these apparent industry switches, since only about 5% are coded as returning to the original industry at $t + 1$.

[8]Data from the LFS suggests that gross job-to-job flows are between 6 and 11% per year, while flows in and out of the labour market are between 6 and 13% (Greenaway, Upward & Wright 1999). Assuming that on average flows into jobs are half total flows in and out of the labour market, the average probability of starting a job in a new firm should fall between 9 and 17.5%.

Some simple evidence on the relationship between earnings and the constructed measures of tenure is presented in Table A.4. This shows the average change in log wages for movers and stayers between firms, occupations and industries, split by the appropriate tenure of the previous spell of employment. The measure of earnings used is gross hourly earnings, including overtime payments and overtime hours. The difference between the wage changes for movers and stayers is a raw difference-in-difference estimate of the parameters γ_2, γ_3 and γ_4, without controlling for any observable characteristics or selection bias. Average wage changes for movers and stayers decline strongly with tenure, partly because of age effects. More importantly, wage changes for movers are almost always positive and *greater* than wage changes for stayers. This strongly suggests that the majority of these job changes are quits rather than layoffs, and that among young workers mobility is associated with greater wage increases. As argued in Section 3, this does not imply that returns to tenure are negative, but rather that sample selection characterises the data: movers change jobs because of higher wages available elsewhere.

6 Results

6.1 OLS estimates

A straightforward starting point for the estimation of γ_3 is to estimate Equation (1) by OLS. However, some care needs to be taken in dealing with time, age and cohort effects, and in the specification of the vector $\mathbf{x}_{ijt}$ which contains other elements thought to influence wages.

We begin by splitting the data into cohorts: the youngest cohort are 16 in 1975, the oldest are 16 in 1995.[9] The most unrestricted specification would allow for different effects of tenure on wages across different cohorts and ages.[10] This involves estimating Equation (1) separately for each age and cohort. Results suggest that while there are strong differences in estimates of γ_1, γ_2, γ_3 and γ_4 across age, cohort (or year) effects are less important. For the purposes of simplicity, we therefore group together cohorts and age groups, and include measures of age and year dummies in the vector $\mathbf{x}_{ijt}$. In addition, because of the strong relationship between age and returns to tenure, our preferred specification includes interaction terms between each of the measures of tenure and age.

[9] As noted in Section 5, the sample is restricted to those individuals who were 16 or younger in 1975.
[10] A particular cohort-age combination identifies a particular year.

In all the following reported results, the vector $\mathbf{x}_{ijt}$ contains measures of age, age-squared, year dummies, sector (public or private), union coverage, occupation (dummies for 22 major groups), industry (10 dummies) and region (10 dummies). In order to control for any possible effects from non-appearance in the panel in the previous year, $\mathbf{x}_{ijt}$ also includes four additional dummies for new entrants and re-entrants, as follows.

New entrants to the panel will by definition have experience and all measures of tenure will be set to one, but we also include a dummy variable "New entrant" to determine whether there is an additional effect on wages in the first year of employment. The dummy "Re-entrant (1)" is a crude measure of recent unemployment experience which records whether an individual was not in the data at $t-1$, but is not a new entrant. Table A.2 shows that about 12% of the sample in each year are not in the sample in the previous year, which we know from other data is an overestimate of the proportion who were unemployed at $t-1$.[11] "Re-entrant (2)" records whether an individual had missing data at $t-1$, and is required because individuals who have missing values for any variables cannot be included in any regressions, and we wish to control for the possibility that the occurrence of missing values is correlated with wages. Finally, "Re-entrant (3)" records whether individuals had a different employment status at $t-1$. These individuals were either working part-time or had pay affected by absence at $t-1$.

Table A.5 reports pooled OLS estimates of a variety of specifications for Equation (1). The simplest estimate, specification (1), is included for comparison with other work which does not include measures of industrial and occupational tenure. As usual, returns to experience and firm tenure are positive, although returns to experience are far higher. Wage profiles are concave in both experience and firm tenure, although both profiles flatten out, with significant positive cubic terms.

All four dummy variables which record reasons for missing data at $t-1$ have significant and negative coefficients. Individuals who have not been in the panel before earn 0.6% less, after controlling for age, while individuals who were missing from the panel at $t-1$ (and who may therefore have been unemployed) earn 3.9% less. The significant coefficient on "Re-entrant (2)" suggests that missing data is non-random in relation to wages: individuals with missing wage data at $t-1$ earn 1.8% less. Finally, the effect of working part-time or having pay affected by absence at $t-1$ is the largest negative effect, at over 9%.

[11] Gregory & Jukes (1997) provide more evidence and additional data on the effects of recent unemployment experience on wages.

In specifications (2)–(5) we include more variables to test how robust these results are, and to include measures of industry and occupational tenure. An obvious problem with specification (1) is that it does not include measures of education, which may well be correlated with tenure and experience. Our prior would be that individuals with later school-leaving ages will have lower average experience and longer average tenure. In specification (2) we introduce a measure of the age when each individual first entered the panel. This is intended to proxy time spent in education, since individuals with more education will tend to enter later. Of course, it might also be the case that individuals who enter late do so because they have been unemployed. However, the estimated coefficient on this variable is positive and significant, supporting the idea that later entrants do better. If those with more education also have longer firm tenure, we would expect the returns to tenure to fall when we include this variable, and this is the case. Conversely, those who enter the labour force later will tend to have less total experience, and so the inclusion of this variable increases estimated returns to tenure.

In specification (3) we introduce our measures of industry tenure. Wage-tenure profiles for industry tenure are also positive and concave, but the inclusion of these terms completely wipes out any firm tenure effect. Clearly industry and firm tenure are highly correlated, but it appears to be the industry effect which dominates. In specification (4) we also include occupational tenure, which serves to drive down returns to firm tenure even further.

As noted above, age effects for all these tenure measures are pronounced, and in specification (5) we include various age interaction terms. Returns to experience and firm tenure are both increasing with age, so that although the coefficient on firm tenure is negative, the estimates predict positive returns for ages above 39. In contrast, returns to industry tenure appear quite flat, with an (insignificant) positive return. Returns to occupation are also positive, but actually appear to decline very slowly with age. Despite the fact that our measures of tenure are strongly collinear, their relationship with age appears to be quite different. The increasing return to firm tenure is consistent with the idea that young workers are more mobile, and that the mobility of young workers is associated with wage gains. These results also suggest that even after including measures of age, experience and firm tenure, there is a small additional industry tenure effect.

There are also strong age interaction effects with the variables measuring when individuals enter the panel. The effect of ‘Age first entered panel’ increases with age, supporting the hypothesis that this variable is picking up education effects. Individuals who enter the panel at a later age benefit more from late entry because they are more likely to have received further and higher education. Age effects for re-entrants, on the other hand, are all negative.

This suggests, for example, that the wage penalty for having not been in the panel at $t-1$ is increasing with age, consistent with the idea that older workers suffer more from spells out of the labour market.

6.2 Correlation between unobservables and tenure

The OLS results in Table A.5 may suffer from the various potential biases outlined in Section 3. In this section we start with the OLS estimates from specification (5) and investigate whether there is any evidence that unobservables in Equation (2) are correlated with our measures of experience and tenure.

As we argued in Section 3, OLS estimates of γ_2 will be biased upwards if matches with high values of ϕ_{ijt} last longer (Altonji & Shakotko), but will be biased downwards if workers who move do so for better matches (Topel). We also need to consider whether unobservable time-invariant characteristics, μ_i are likely to be correlated with experience and tenure. Altonji & Shakotko argue that μ_i and T_{ijt} will be positively correlated because high ability workers are "less likely to experience layoffs or quits." While the former might be true, it is not clear whether high μ_i workers are more or less likely to experience quits. If high μ_i workers generate more job offers, for example, then they are more likely to find jobs offering higher values of ϕ_{ijt}, and hence more likely to quit. Set against this is the fact that μ_i may have a significant impact on an individual's appearance in the sample, and hence on our calculated values of tenure and experience. Individuals with low μ_i may have lower total experience because they experience longer spells of unemployment, for example.

Table A.6 compares estimates from the preferred OLS specification (5), with a standard fixed-effects (within) and a random effects GLS model (StataCorp. 1999, Vol Su-Z pp.437–438). To aid interpretation, we also plot the predicted relationship between returns to experience, tenure and age for each of the specifications, shown in Figure 1.

Specification (6) is an estimate of

$$w_{ijt} - \bar{w}_i = (X_{ijt} - \bar{X}_i)\gamma_1 + (T_{ijt} - \bar{T}_i)\gamma_2 + (I_{ijt} - \bar{I}_i)\gamma_3 + (O_{ijt} - \bar{O}_i)\gamma_4 + (\mathbf{x}_{ijt} - \bar{\mathbf{x}}_i)\boldsymbol{\beta}_1 + (\varepsilon_{ijt} - \bar{\varepsilon}_i), \quad (7)$$

where means are within-individual means e.g. :

$$\bar{w}_i = \frac{\sum_{t_i=1}^{T_i} w_{ijt}}{T_i}.$$

In this model the error term consists of

$$\varepsilon_{ijt} - \bar{\varepsilon}_i = (\phi_{ijt} - \bar{\phi}_i) + (\nu_{ijt} - \bar{\nu}_i), \tag{8}$$

so the individual effect μ_i has been removed, but it may still be the case that $(\phi_{ijt} - \bar{\phi}_i)$ is correlated with measures of tenure and experience in Equation (7). For example, a job with a high average value of ϕ_{ijt} will also have high values of $(\phi_{ijt} - \bar{\phi}_i)$, and if that job also has longer (or shorter) average tenure, then there will be a correlation with $(T_{ijt} - \bar{T}_i)$ and the other tenure measures.[12] Specification (7) is an estimate of a random-effects GLS model, which provides more efficient estimates than the fixed-effects model because it utilises between as well as within variation, but relies on the assumption that the individual time-invariant characteristics are uncorrelated with the other right hand side variables. Intuitively therefore we might expect estimates from the random-effects model to lie in between OLS and fixed-effects estimates.

Specification (6) has quite different effects on returns to experience, and the three different measures of tenure (see Panel (a) in Figure 1). As predicted, the fixed-effect model greatly reduces returns to experience, but it does so by removing the increasing returns to experience with age rather than by shifting the constant effect. By the age of 40, the fixed effect model predicts returns to experience of about 7%, compared to 13% from the OLS model. This suggests that returns to μ_i are also increasing with age, which is consistent with the idea that workers with high μ_i are less likely to experience spells of unemployment, and that this effect is cumulative. In contrast, the fixed-effect model *increases* the returns to firm tenure (Panel (b)) by a constant amount, but has little effect on the age-tenure relationship. This provides some support for the hypothesis that μ_i and T_{ijt} may be negatively correlated, and that therefore OLS estimates of γ_2 are biased downwards, in this case by about 1% for all age groups. But estimated returns to firm tenure are still very small, only reaching 0.5% by the age of 35.

The fixed-effect model increases the age effect of industry tenure (Panel (c)), resulting in higher predicted returns after the age of about 30. That is, $\gamma_3^{OLS} > \gamma_3^{FE}$ for young workers, but $\gamma_3^{OLS} < \gamma_3^{FE}$ for older workers. This tends to suggest that more able young workers are actually less mobile than less able young workers, and therefore tend to have longer industry tenure. Returns to industry tenure are negligible for young workers, but increase and become larger than returns to firm tenure for older workers. Specification (6) also increases the age effect of occupational tenure (Panel (d)). OLS estimates suggest occupational tenure is slightly

[12] Unless the individual has only one job, in which case ϕ_{ijt} is indistinguishable from μ_i.

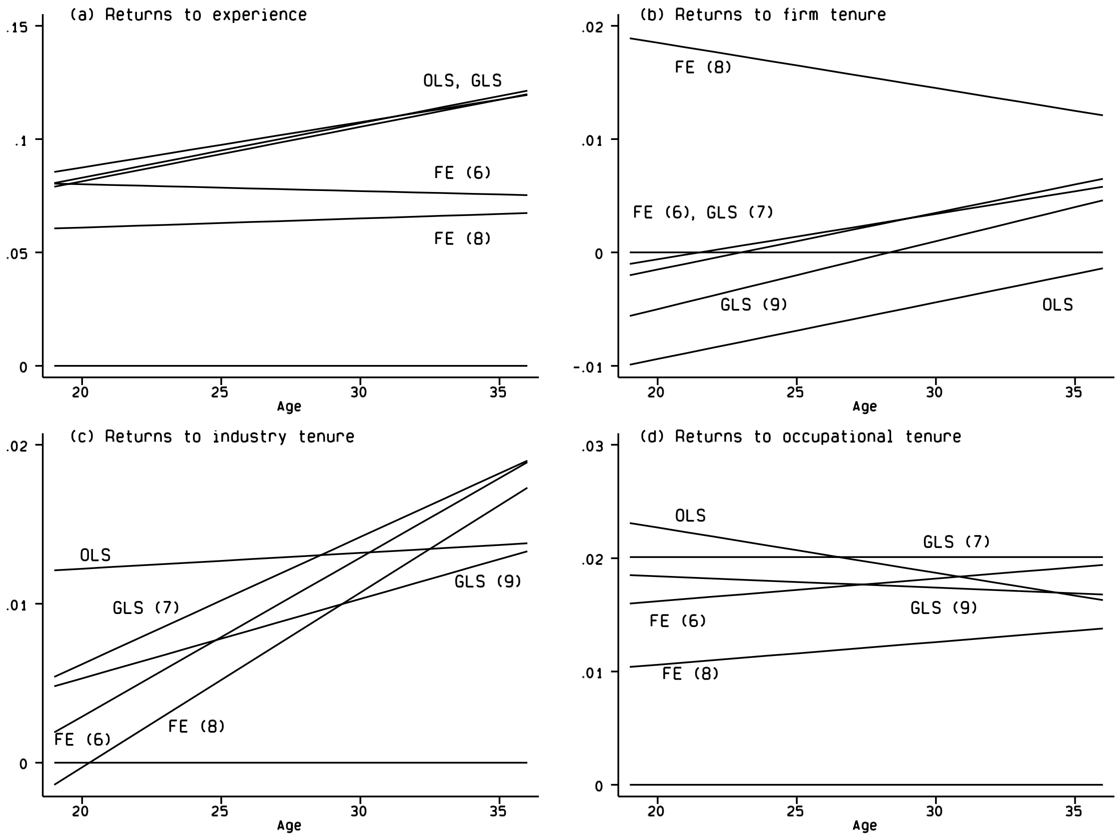

Figure 1: **Interactions between experience, tenure and age**

higher for younger workers, whereas fixed-effects estimates suggest it is increasing with age. But in general age effects for occupational tenure are much smaller than for industrial tenure.

In Table A.6 we also report estimates of a random-effects GLS model, specification (7). A test of the equality of coefficients between specifications (6) and (7) is easily rejected (Hausman 1978), suggesting that the assumption that the μ_i and the right hand side variables is uncorrelated is causing the divergence between the fixed-effects and random-effects estimators. In the case of returns to experience, GLS and OLS estimates are very similar, but this is not generally true. Estimates of γ_2, γ_3 and γ_4 are all reasonably similar whether estimated by fixed- or random-effects models. Focussing on estimates of γ_3, GLS estimates also show a much stronger age effect, confirming the hypothesis that while younger workers do not experience additional wage penalties from moving between industries, workers above the age of 35 would lose an *additional* 2%, on top of firm and occupational tenure effects.

As noted in Equation (8), the error term from specification (7) may still be correlated with measures of tenure because of job-specific unobserved effects, ϕ_{ijt}. Altonji & Shakotko's proposed solution to this problem is to use instruments based on deviations from within-job tenure. A more straightforward solution is to estimate a fixed-effects model using differences from within-job means rather than differences from within-individual means. Instead of Equation (7), we estimate

$$w_{ijt} - \bar{w}_{ij} = (X_{ijt} - \bar{X}_{ij})\gamma_1 + (T_{ijt} - \bar{T}_{ij})\gamma_2 + (I_{ijt} - \bar{I}_{ij})\gamma_3 + (O_{ijt} - \bar{O}_{ij})\gamma_4 + (\mathbf{x}_{ijt} - \bar{\mathbf{x}}_{ij})\boldsymbol{\beta}_1 + (\varepsilon_{ijt} - \bar{\varepsilon}_{ij}), \quad (9)$$

where means are given calculated by summing within each job j rather than within each individual. If match-specific effects are constant within jobs, then this method removes ϕ_{ij} from the error term.

Estimates of Equation (9) are reported as specification (8) in Table A.6, and the predicted relationships with age are shown in Figure 1. Estimates of γ_1, γ_3 and γ_4 are fairly similar whether estimated by Equation (7) or Equation (9). However, as might be expected, the use of within-job fixed effects has a significant impact on estimates of γ_2, returns to firm tenure. OLS estimates and within-individual fixed-effects predict very small or even negative firm tenure effects. Once match-specific fixed effects are removed, however, firm tenure effects increase to about 2%, a similar magnitude to our estimates of γ_3 and γ_4. The fact that $\gamma_2^{OLS} < \gamma_2^{FE(6)} < \gamma_2^{FE(8)}$ supports the hypothesis that both individual-specific and job-specific fixed effects are negatively correlated with tenure: more able individuals are more mobile, and better matches are more likely to be observed at low values of T_{ijt}.

Finally, specification (9), also summarised in Figure 1, provides estimates of a random-effects version of (9). Results are generally similar to specification (7) and lie between OLS estimates and specification (8).

Table 1 summarises our main findings with regard to the four parameters of interest for the OLS estimate and the two fixed-effects specifications. Returns to experience, $\hat{\gamma}_1$, are lower in specifications (6) and (8), suggesting a positive bias between unobserved components and experience. This seems plausible, both because the quality of match is likely to increase with experience, and because individuals who move in and out of the sample (and who therefore have lower experience) are likely to be those with lower unobserved earning potential. In contrast, the bias associated with returns to firm tenure is negative: OLS estimates are lower than fixed-effects estimates. This seems to support Topel's interpretation rather than Altonji & Shakotko's.

Table 1: **Summary of results**

	Age	*OLS (5)*	*FE (6)*	*FE (8)*	*Suggested bias* Cov(μ_i, x)	Cov(ϕ_{ijt}, x)
$\hat{\gamma}_1$	20	0.081	0.080	0.061	$+$	$+$
	35	0.117	0.076	0.067	$+$	$+$
$\hat{\gamma}_2$	20	-0.009	-0.001	0.019	$-$	$-$
	35	-0.002	0.005	0.013	$-$	$-$
$\hat{\gamma}_3$	20	0.012	0.003	0.000	$+$	$+$
	35	0.014	0.018	0.016	$-$	$-$
$\hat{\gamma}_4$	20	0.023	0.016	0.011	$+$	$+$
	35	0.017	0.019	0.014	$-$	$+$

Returns to industry tenure exhibit greater variation with age than any of the other estimates. 35-year-old workers earn between 1%-2% per year more for each additional year of tenure in an industry, while younger workers gain no significant wage increase.[13] Thus, even after including measures of firm tenure, there does appear to be a significant industry effect for workers above a certain age. Although it would be possible to make out of sample predictions for older workers based on these data, this might be unwise, since we have adopted an extremely simple linear age-returns relationship. Thus we only make predictions up to the age of 35. These estimates are not affected greatly by the use of the two fixed-effects specifications, although as Panel (c) of Figure 1 shows, the fixed-effects estimates reduce returns to industry tenure for younger workers, and (slightly) increase returns for older workers. Returns to occupational tenure are of a similar size for older workers, but are also positive for young workers, suggesting additional wage losses of between 1% and 2% for workers who change occupation. Fixed-effect estimates also tend to reduce the OLS estimates, but only by a small amount.

How large are these effects? Because of the quadratic and cubic terms in experience and tenure, as well as the age interaction effects, it is difficult to interpret these results in terms of actual predicted wage changes. In Table 2 we compute predicted wage changes for individuals

[13] Linearized prediction: returns to industrial experience are concave.

aged 20 and 35 who have the average characteristics of that age group.[14]

Table 2: Predicted wage changes

		Predicted wage change		
	Age	*OLS*	*FE (6)*	*FE (8)*
New firm, same industry, same occupation ($-\hat{\gamma}_2\bar{T}$)	20	$+1.06\%$	-0.12%	-1.26%
	35	$+1.27\%$	$+1.17\%$	$+0.53\%$
New firm, new industry, same occupation ($-\hat{\gamma}_2\bar{T} - \hat{\gamma}_3\bar{I}$)	20	-0.86%	-0.77%	-1.31%
	35	-5.07%	-6.27%	-6.01%
New firm, same industry, new occupation ($-\hat{\gamma}_2\bar{T} - \hat{\gamma}_4\bar{O}$)	20	-2.38%	-2.69%	-2.83%
	35	-1.68%	-1.18%	$+0.33\%$
New firm, new industry, new occupation ($-\hat{\gamma}_2\bar{T} - \hat{\gamma}_3\bar{I} - \hat{\gamma}_4\bar{O}$)	20	-4.31%	-3.33%	-2.87%
	35	-8.01%	-8.62%	-6.20%

The first two rows give an estimate of $-\gamma_2\bar{T}$, the wage change associated with moving firm without changing industry or occupation. These effects are actually positive in most cases, as a result of the negative coefficient on T_{ijt} in nearly all the estimates, even those which attempt to control for unobserved fixed effects. The use of fixed effect models tends to decrease the wage gains from moving (increase the wage losses), as would be expected if fixed effects and tenure are negatively correlated. However, Table 2 also shows that the absolute differences between the estimates are small, albeit in an intuitive direction.

The second two rows give an estimate of $-\gamma_2\bar{T}-\gamma_3\bar{I}$, and illustrate that relative to job effects, industry effects are large and negative. However, in absolute terms γ_3 is still small: we predict a wage loss of only 5%–6% for a 35-year-old with typical tenure who changes employer and industry. Once again, note that industry effects are much larger for older workers. The third two rows give an estimate of $-\gamma_2\bar{T}-\gamma_3\bar{O}$, and illustrate that there is also an additional (small) penalty for workers who change occupation as well as firm. The total ‘cost’ of mobility is illustrated in the final two rows. The overall effect of including fixed-effects is actually to reduce the estimated cost of moving by about 2 percentage points.

[14]Predicted values are calculated as $(\bar{\mathbf{x}} - \bar{\mathbf{x}}^*)'\boldsymbol{\beta}$, where $\bar{\mathbf{x}}$ are the average characteristics of an age group, and $\bar{\mathbf{x}}^*$ are the same characteristics with the appropriate tenure variables reset to one.

7 Conclusions

Almost all previous estimates of the effects of seniority on wages, and hence on the relationship between seniority and wage loss following displacement, have concentrated on total labour market experience and firm tenure. But it seems plausible that some skills are specific to industries and occupations as well as to individual firms. If this is the case, then workers who move between industries or occupations will experience greater wage losses than those who change jobs within sectors, and this loss will increase with industry and occupational tenure. This idea is central to the "smooth adjustment hypothesis", which argues that factors of production, such as labour, can be reallocated within industries more easily than they can be reallocated between industries. Using a large panel of young workers over a long time period for the UK, our results suggest that there are some significant, albeit small, additional returns to tenure within industries and occupations above those that accrue to general experience and firm tenure.

Our results also give rise to some additional research questions. First, why do older workers appear to have greater returns to industry tenure? This is a potentially significant finding, because it suggests that older workers will suffer greater wage losses on being separated from their current industry than younger workers even if they have the same level of industry tenure. To confirm this result one would want to conduct the analysis on the full age-range of workers: at present we are restricted to analysing only those workers who enter the labour market from 1975 onwards. There are two possible reasons for this result. It may be that workers who enter the labour market earlier are different, and we have failed to account for these differences. For example, older workers, who come from earlier cohorts, entered industries which required greater specific skills. We have of course controlled for all the measurable differences possible, but it seems possible that there are also unmeasured differences. Or, it may reflect a genuine measure of the extent to which skills become less easily adaptable for older workers.

A second question is: do these estimates provide a reasonable estimate of the wage effects of worker mobility? As Tables A.3 and A.4 illustrate, these data are characterised by frequent moves between firms, industries and occupations, which tend to result in wage increases rather than decreases. We cannot distinguish between voluntary moves (which result in wage increases) with enforced moves (which may result in wage losses). However, we would argue that to focus only on displaced workers results in a different (and opposite) bias. In addition, the effects of changing labour demand brought about by external factors such as shifts in trading patterns are not met purely by worker displacement: many workers will still be able to make ‘voluntary’ moves even from declining sectors.

A third research question is whether the results are dependent on the definitions of industries used. One would wish to investigate whether the size of the returns to industry tenure are affected by the degree of aggregation in industry definition. For example, it seems possible that some of the two-digit industries used here do share skills, production processes and so on. Moves between more disparate industries should in theory produce greater wage losses, and evidence of this would provide more support for the notion of industry-specific skills.

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Tables

Table A.1: **Industry groupings**

16,17	Production & distribution of electricity, gas & other forms of energy (16); Water supply industry (17)
23,21	Extraction of minerals not elsewhere specified (23); Extraction & preparation of metalliferous ores (21)
25,15	Chemical industry (25); Nuclear fuel production (15)
33,34,37	Manufacture of office machinery & data processing equipment (33); Electrical & electronic engineering (34); Instrument engineering (37)
41,42	Food, drink & tobacco manufacturing industries
61,62,63	Wholesale distribution (except dealing in scrap & waste materials) (61); Dealing in scrap & waste materials (62); Commission agents (63)
74,75	Sea transport (74); Air transport (75)
83,85	Business services (83); Owning & dealing in real estate (85)
91–95	Public administration, national defence & compulsory social security (91); Sanitary services (92); Education (93); Research and Development (94); Medical and other health services (95)
	All other industries are defined at the 1980 SIC 2-digit level

Table A.2: NESPD sample used 1975–1995

Year	*N*	*Stayers*[a]	*New entrants*[b]	*Re-entrants*			*Age*		*Total experience*		*Firm tenure*		*Ind. tenure*		*Occ. tenure*	
				(1)[c]	*(2)*[d]	*(3)*[d]	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
1975	846	0.000	1.000	0.000	0.000	0.000	15.98	(0.14)	1.00	(0.00)	1.00	(0.00)	1.00	(0.00)	1.00	(0.00)
1976	2262	0.249	0.726	0.000	0.003	0.022	16.63	(0.51)	1.27	(0.45)	1.20	(0.40)	1.24	(0.43)	1.22	(0.41)
1977	3840	0.381	0.551	0.023	0.005	0.040	17.21	(0.79)	1.58	(0.71)	1.42	(0.63)	1.49	(0.67)	1.46	(0.66)
1978	5675	0.427	0.457	0.053	0.011	0.052	17.74	(1.07)	1.85	(0.94)	1.58	(0.83)	1.68	(0.88)	1.63	(0.86)
1979	7541	0.479	0.381	0.074	0.010	0.055	18.26	(1.32)	2.14	(1.16)	1.72	(0.98)	1.87	(1.08)	1.81	(1.05)
1980	9546	0.501	0.329	0.097	0.009	0.064	18.81	(1.60)	2.44	(1.38)	1.89	(1.13)	2.01	(1.27)	1.91	(1.23)
1981	11015	0.553	0.259	0.115	0.005	0.068	19.42	(1.86)	2.83	(1.60)	2.16	(1.31)	2.29	(1.45)	2.15	(1.39)
1982	12500	0.620	0.186	0.105	0.005	0.085	20.17	(2.04)	3.33	(1.81)	2.46	(1.51)	2.54	(1.67)	2.51	(1.59)
1983	13853	0.630	0.186	0.111	0.005	0.069	20.79	(2.31)	3.71	(2.09)	2.65	(1.75)	2.71	(1.90)	2.81	(1.81)
1984	15613	0.626	0.185	0.117	0.008	0.064	21.37	(2.56)	4.02	(2.36)	2.85	(1.97)	2.93	(2.10)	3.04	(2.05)
1985	16727	0.633	0.163	0.119	0.015	0.070	21.99	(2.81)	4.40	(2.62)	3.06	(2.18)	3.21	(2.31)	3.27	(2.30)
1986	19319	0.621	0.160	0.136	0.010	0.073	22.59	(3.05)	4.69	(2.87)	3.17	(2.37)	3.32	(2.55)	3.39	(2.52)
1987	21309	0.648	0.152	0.129	0.007	0.064	23.14	(3.30)	4.95	(3.11)	3.27	(2.55)	3.43	(2.75)	3.48	(2.71)
1988	24800	0.631	0.156	0.136	0.009	0.068	23.69	(3.56)	5.15	(3.36)	3.24	(2.69)	3.41	(2.93)	3.49	(2.87)
1989	26436	0.675	0.125	0.132	0.006	0.062	24.25	(3.81)	5.54	(3.56)	3.26	(2.82)	3.48	(3.08)	3.58	(3.00)
1990	28746	0.679	0.128	0.128	0.007	0.058	24.82	(4.07)	5.85	(3.80)	3.29	(2.91)	3.69	(3.20)	3.67	(3.14)
1991	29628	0.719	0.093	0.123	0.009	0.055	25.55	(4.24)	6.41	(4.00)	3.51	(3.04)	4.05	(3.39)	3.75	(3.31)
1992	29580	0.761	0.061	0.120	0.005	0.053	26.37	(4.36)	7.05	(4.15)	3.90	(3.17)	4.55	(3.56)	4.17	(3.44)
1993	30273	0.750	0.068	0.126	0.006	0.051	27.07	(4.53)	7.53	(4.38)	4.20	(3.34)	4.89	(3.76)	4.47	(3.61)
1994	32074	0.747	0.066	0.136	0.003	0.047	27.74	(4.69)	7.96	(4.61)	4.40	(3.54)	5.18	(3.97)	4.73	(3.79)
1995	32876	0.759	0.069	0.122	0.003	0.047	28.40	(4.90)	8.38	(4.87)	4.55	(3.73)	5.43	(4.22)	4.91	(3.99)

[a] In the panel at t and $t-1$.

[b] Not been in the panel at any point before t.

[c] Not in the panel at $t-1$, but were in the panel at some point before.

[d] In the panel at t and $t-1$, but with missing data on earnings at $t-1$.

[e] In the panel at t and $t-1$, but whose pay was affected by absence or part-time working at $t-1$.

Table A.3: **Descriptive statistics**

Total person-years	326,603
Number of individuals i	61,061
Number of job spells j	112,207
Average number of years in panel	8.641
Average labour market experience $\bar{X}$	6.668
Average firm tenure $\bar{T}$	3.258
Average industry tenure $\bar{I}$	3.591
Average occupation tenure $\bar{O}$	3.494
New firm	0.285
New industry	0.258
New occupation	0.264
Same firm, same industry, same occupation	0.624
Same firm, same industry, new occupation	0.028
Same firm, new industry, same occupation	0.033
Same firm, new industry, new occupation	0.030
New firm, same industry, same occupation	0.051
New firm, same industry, new occupation	0.039
New firm, new industry, same occupation	0.028
New firm, new industry, new occupation	0.167

Table A.4: **Wage changes of individuals changing jobs**

	$\ln(w_t) - \ln(w_{t-1})$					
Years of tenure on previous job[a]	*Same Firm*	*New Firm*	*Same Industry*	*New Industry*	*Same Occupation*	*New Occupation*
1	0.109	0.132	0.117	0.124	0.118	0.121
2	0.087	0.117	0.093	0.110	0.096	0.101
3	0.069	0.103	0.074	0.087	0.077	0.083
4	0.055	0.091	0.059	0.079	0.061	0.073
5	0.037	0.070	0.043	0.070	0.045	0.061
6–10	0.031	0.054	0.033	0.057	0.036	0.037
11–15	0.020	0.040	0.023	0.050	0.027	-0.014
16–20	0.006	-0.019	0.012	0.016	0.015	-0.118

[a] Tenure on previous job refers to firm tenure for those changing firm, occupational tenure for those changing occupation, and industry tenure for those changing industry.

Table A.5: OLS estimates

	(1)		*(2)*		*(3)*		*(4)*		*(5)*	
Experience	0.0694	[0.000]	0.0987	[0.000]	0.0889	[0.000]	0.0830	[0.000]	0.0334	[0.000]
Experience^2/10	−0.0657	[0.000]	−0.0772	[0.000]	−0.0725	[0.000]	−0.0679	[0.000]	−0.0813	[0.000]
Experience^3/100	0.0199	[0.000]	0.0220	[0.000]	0.0207	[0.000]	0.0195	[0.000]	0.0204	[0.000]
Firm tenure	0.0079	[0.000]	0.0063	[0.000]	0.0006	[0.719]	−0.0074	[0.000]	−0.0194	[0.000]
Firm tenure^2/10	−0.0087	[0.001]	−0.0086	[0.001]	−0.0061	[0.025]	0.0017	[0.572]	−0.0016	[0.590]
Firm tenure^3/100	0.0016	[0.159]	0.0020	[0.091]	0.0017	[0.170]	−0.0004	[0.766]	−0.0002	[0.887]
Industry tenure					0.0178	[0.000]	0.0122	[0.000]	0.0102	[0.010]
Industry tenure^2/10					−0.0100	[0.000]	−0.0043	[0.137]	−0.0057	[0.054]
Industry tenure^3/100					0.0022	[0.073]	0.0005	[0.683]	0.0008	[0.491]
Occupation tenure							0.0238	[0.000]	0.0307	[0.000]
Occupation tenure^2/10							−0.0241	[0.000]	−0.0205	[0.000]
Occupation tenure^2/100							0.0069	[0.000]	0.0062	[0.000]
Age first entered panel			0.0195	[0.000]	0.0173	[0.000]	0.0165	[0.000]	−0.0133	[0.004]
New entrant	−0.0062	[0.025]	−0.0066	[0.018]	−0.0014	[0.609]	0.0030	[0.278]	−0.1545	[0.000]
Re-entrant (1)	−0.0394	[0.000]	−0.0083	[0.000]	0.0012	[0.551]	0.0095	[0.000]	0.1353	[0.000]
Re-entrant (2)	−0.0176	[0.000]	−0.0169	[0.000]	−0.0156	[0.000]	−0.0150	[0.000]	0.0712	[0.000]
Re-entrant (3)	−0.0936	[0.000]	−0.0911	[0.000]	−0.0818	[0.000]	−0.0765	[0.000]	0.1411	[0.000]
Age	0.1249	[0.000]	0.0924	[0.000]	0.0947	[0.000]	0.0957	[0.000]	0.1281	[0.000]
Age^2	−0.0184	[0.000]	−0.0146	[0.000]	−0.0146	[0.000]	−0.0147	[0.000]	−0.0269	[0.000]
Age $\times$ Experience									0.0024	[0.000]
Age $\times$ Firm tenure									0.0005	[0.000]
Age $\times$ Industry tenure									0.0001	[0.467]
Age $\times$ Occupation tenure									−0.0004	[0.028]
Age $\times$ Age first entered panel									0.0010	[0.000]
Age $\times$ new entrant									0.0081	[0.000]
Age $\times$ re-entrant (1)									−0.0053	[0.000]
Age $\times$ re-entrant (2)									−0.0035	[0.000]
Age $\times$ re-entrant (3)									−0.0094	[0.000]
Sample size	326603		326603		326603		326603		326603	
Number of individuals	61601		61601		61601		61601		61601	
R^2	0.5711		0.5745		0.5760		0.5767		0.5780	
MSE	0.2980		0.2968		0.2963		0.2961		0.2956	

[a] All standard errors are robust (White 1980), calculated assuming independence between but not within individuals (StataCorp. 1999, Vol U. pp.256–260).

[b] All regressions also include year, occupation, industry, region, public sector and union coverage dummy variables.

Table A.6: Comparison of within-individual and within-job fixed and random effects estimates

	Fixed-effects (6)		*Fixed-effects (8)*		*Random Effects GLS (7)*		*Random Effects GLS (9)*	
Experience	0.0861	[0.000]	0.0530	[0.000]	0.0475	[0.000]	0.0350	[0.000]
Experience^2/10	−0.0613	[0.000]	−0.0644	[0.000]	−0.0807	[0.000]	−0.0812	[0.000]
Experience^3/100	0.0192	[0.000]	0.0207	[0.000]	0.0206	[0.000]	0.0213	[0.000]
Firm tenure	−0.0086	[0.000]	0.0165	[0.000]	−0.0115	[0.000]	−0.0170	[0.000]
Firm tenure^2/10	−0.0103	[0.000]	−0.0041	[0.056]	−0.0092	[0.000]	−0.0070	[0.000]
Firm tenure^3/100	0.0019	[0.019]	0.0002	[0.762]	0.0016	[0.053]	0.0000	[0.984]
Industry tenure	−0.0171	[0.000]	−0.0223	[0.000]	−0.0098	[0.000]	−0.0047	[0.050]
Industry tenure^2/10	−0.0081	[0.000]	−0.0034	[0.069]	−0.0069	[0.000]	−0.0036	[0.036]
Industry tenure^3/100	0.0007	[0.314]	−0.0012	[0.102]	0.0008	[0.270]	0.0000	[0.981]
Occupation tenure	0.0122	[0.000]	0.0066	[0.022]	0.0201	[0.000]	0.0204	[0.000]
Occupation tenure^2/10	−0.0246	[0.000]	−0.0205	[0.000]	−0.0224	[0.000]	−0.0210	[0.000]
Occupation tenure^2/100	0.0067	[0.000]	0.0062	[0.000]	0.0064	[0.000]	0.0067	[0.000]
Age first entered panel					−0.0146	[0.000]	−0.0089	[0.002]
New entrant	−0.2088	[0.000]	−0.1985	[0.000]	−0.1694	[0.000]	−0.1707	[0.000]
Re-entrant (1)	0.1275	[0.000]	0.0846	[0.000]	0.1199	[0.000]	0.0915	[0.000]
Re-entrant (2)	0.1048	[0.000]	0.0614	[0.000]	0.0947	[0.000]	0.0654	[0.000]
Re-entrant (3)	0.0220	[0.388]	0.0015	[0.953]	0.0657	[0.006]	0.0493	[0.033]
Age	0.0776	[0.000]	0.1011	[0.000]	0.1267	[0.000]	0.1397	[0.000]
Age^2	−0.0198	[0.000]	−0.0217	[0.000]	−0.0272	[0.000]	−0.0289	[0.000]
Age × Experience	−0.0003	[0.108]	0.0004	[0.193]	0.0020	[0.000]	0.0024	[0.000]
Age × Firm tenure	0.0004	[0.000]	−0.0004	[0.049]	0.0005	[0.000]	0.0006	[0.000]
Age × Industry tenure	0.0010	[0.000]	0.0011	[0.000]	0.0008	[0.000]	0.0005	[0.000]
Age × Occupation tenure	0.0002	[0.006]	0.0002	[0.080]	0.0000	[0.748]	−0.0001	[0.122]
Age × Age first entered panel	0.0021	[0.000]	0.0017	[0.000]	0.0012	[0.000]	0.0010	[0.000]
Age × new entrant	0.0104	[0.000]	0.0098	[0.000]	0.0086	[0.000]	0.0084	[0.000]
Age × re-entrant (1)	−0.0048	[0.000]	−0.0028	[0.000]	−0.0045	[0.000]	−0.0032	[0.000]
Age × re-entrant (2)	−0.0042	[0.000]	−0.0024	[0.000]	−0.0040	[0.000]	−0.0028	[0.000]
Age × re-entrant (3)	−0.0028	[0.010]	−0.0010	[0.384]	−0.0051	[0.000]	−0.0040	[0.000]
Sample size	326603		326603		326603		326603	
Number of individuals	61601		61601		61601		61601	
R^2	0.4568		0.4369		0.5603		0.5682	
Hausman[b]					10530.75	[0.000]	10766.51	[0.000]

[a] All regressions also include year, occupation, industry, region, public sector and union coverage dummy variables.

[b] Test that random effects model is correctly specified and that fixed effects uncorrelated with independent variables (Hausman 1978).