Job Mobility and Earnings Growth

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The relationship between job mobility and earnings growth is a theoretically important but empirically neglected issue. How important are job shifts as a mechanism in the development of earnings over the life cycle? We estimate the impact of internal and external mobility on earnings growth from age 26 to 35, a crucial formative period in work-life careers. We present a new specification for measuring long-term effects of job shift sequences, and report the following main findings on the basis of data on male wage-earners in Sweden. First, internal and external job shifts are distinct pathways in work-life careers; very few individuals pursue both routes. Secondly, both kinds of mobility increase earnings growth. Internal mobility has the strongest impact. While the economic returns of internal job shifts increase with their frequency, the returns of changing employer diminish rapidly. Thirdly, the impact of firm tenure and external mobility should be considered simultaneously, otherwise the effects of both are biased downward. However, the tenure effect on earnings is largely unrelated to internal job shifts. Fourthly, the impact of internal mobility on earnings growth chiefly operates net of occupational advancement, while the effects of external mobility to a considerable extent run via occupational attainment.

Introduction

At the heart of many theoretical models of labour-market contracts and compensation lie strong assumptions about the relationship between job mobility and earnings. A core conception in sociological theories of inequality is that rewards are tied to positions in social structure rather than to the persons occupying these positions. Movement between jobs is therefore, in this view, a vital cause of individual earnings growth. The distinction between positions and persons may indeed be the major contribution of sociological theory to research on social inequality (Sørensen, 1994: 230). This distinction is absent in, for instance, orthodox human-capital theory and standard production theory in economics (Lazear, 1995: 78). In these economic theories, individual productivity growth is sufficient to explain earnings progression.

The potential importance of job mobility for earnings growth goes beyond the distinction between positions and persons, however. Although differences in rewards between positions are likely to be a significant source of mobility effects on earnings, we find it reasonable to expect that they are not the only mechanism involved. As developed further later on, we see the process of matching persons to jobs as important not only for what position an individual attains, but also for the amount of rewards that the individual earns in that position. Accordingly, we expect to find an impact of mobility on earnings even when positional differences in rewards are accounted for. We follow the standard practice in stratification research of operationalizing position in occupational terms (see further below). Hence, earnings effects of mobility are seen as positionally determined to the extent that they may be accounted for by shifts between differentially rewarded occupations. Net of such occupational attainment, we see the growth in earnings due to job shifts as 'pure' mobility effects.

In view of its theoretical importance, empirical knowledge on the relationship between job mobility and earnings growth is surprisingly scarce.[1] In fact,

job mobility and earnings determination have mostly been studied in isolation from each other, at least in the sociological literature. This paper provides estimates of the impact of job-shift sequences on long-term earnings growth in Sweden on the basis of retrospective employment history data matched with tax records of earnings. Our primary motive in using a Swedish data-set is the exceptionally strong link between information on mobility and information on income that these data provide.

We start from two empirical regularities. First, among workers with similar amounts of general labour-market experience, those who have spent the longest time with their current employer tend to have the highest earnings. Employer seniority apparently pays off in earnings, although the magnitude of this pay-off is a matter of controversy.[2] A second repeated finding is that shifts of employer entail a significant wage increase.[3] These two findings are not contradictory, since selection processes of various kinds are at work in the labour market. Individuals who profit by staying with the same employer choose to do so if they can, while others face alternative pay-off structures and act accordingly. Most previous research is limited, however, to examining short-term earnings effects of employer shifts and employer tenure. Not much is known about the long-term pay-offs of different kinds of mobility and stability.

We make three main contributions in the present paper. First, we connect the research topics of job mobility and earnings determination. In making this connection, we produce empirical findings that significantly advance our understanding of reward determination in the labour market. Secondly, we break new ground in the study of labour-market dynamics. By modelling the long-term effects of job-shift sequences, we move beyond the focus on isolated events that dominates contemporary research on work-life mobility. Thirdly, we make use of powerful methodological techniques to correct for several sources of potential bias in parameter estimation. These techniques are applicable to a wide range of empirical analyses, but are still rarely used in sociology.

Since we analyse data from a single country, we call for future research to put our results in comparative perspective. We believe, however, that our findings for Sweden are of general interest. The Swedish labour market is characterized by a highly compressed wage structure, strong unions, a high degree of centralized bargaining, and extensive legal regulations (see e.g. Bellmann and Möller, 1995; Edin and Topel, 1997). Comparative studies have shown, though, that the Swedish pattern of wage determination in several respects is similar to, for example, the US case. There are stable and consistent earnings effects of gender, schooling, experience, tenure, firm size, and industry among Swedish employees, although the coefficients of these determinants generally are weaker than in the US labour market (see e.g. Blau and Kahn, 1995; Edin and Holmlund, 1995; le Grand, 1989; Westergård-Nielsen, 1995). Therefore, our study on Sweden probably provides conservative estimates of the more general case.

The paper is organized as follows. We first state our view of how earnings growth over the life cycle is determined, and specify the role played by job mobility in this process. Then we present our analytical strategy to examine earnings determination empirically, and describe the data-set that we use. In reporting results, we begin with a regression model estimated with ordinary least squares, and then successively take various sources of potential bias into account. Finally, we summarize and discuss our findings.

Tenure, Mobility, and Earnings

Theoretical explanations of the positive seniority–wage slope tend to be based on different notions of firm internal labour markets. At least three explanatory accounts have been suggested in this respect. First, earnings grow with seniority due to accumulation of firm-specific human capital (Becker, 1964). Secondly, the positive slope of the seniority–wage gradient is a strategic employer device to elicit effort and loyalty from employees who might otherwise be difficult to control (Lazear, 1979, 1981). Thirdly, employers facing high turnover expenditures (for recruitment and training) may reduce their transaction costs by using a prospective reward structure (Williamson, 1975). All three perspectives imply that it is rational for (some) employers to give employees economic incentives to stay with the firm.

Aside from these accounts of internal labour markets, a frequent perspective in the labour economics literature on the seniority-wage effect is based on the concept of match quality. In this view, workers move between jobs in order to find a good match between a job and their own skills and aptitudes. When a satisfactory match is found the worker will tend to stay with that job. Hence, tenure is positively associated with match quality and therefore with earnings, but this association is based on sorting and search, not on a causal connection between seniority and wages (Burdett, 1978; Jovanovic, 1979).

Despite many studies of internal labour markets, the nature of internal job mobility is not well known (Baker and Holmstrom, 1995). It has been much more common to examine the determinants of job shifts than their consequences (see the review in Rosenfeld, 1992). One of the least extensively studied aspects of mobility in internal labour markets is how job shifts are connected to earnings careers. The empirical separation of alternative theoretical accounts has rarely been attempted.

Hannan, Schönmann, and Blossfeld (1990) analyse job and wage careers among a sample of workers in West Germany, and conclude that employees in firm internal labour markets (distinguished by firm size and industry) do not have faster wage growth than other workers, either between or within jobs. Baker, Gibbs, and Holmstrom (1994), analysing longitudinal data from a single large US firm, discover that a promotion from one organizational level to the next pays off significantly less than the difference in mean wages between levels. This is due to a substantial wage variation within levels, where the promotee tends to have high relative pay within his or her origin level, but starts with low relative pay at the destination level. The implication is that wages vary both between and within jobs, but that a job shift is required to advance beyond the wage ceiling of each level. Hence, the instantaneous earnings effect of an internal job shift is small, although the long-term effect may be substantial. According to Lazear's (1999) case-study, however, wage growth within the firm is discontinuous due to instantaneous and dramatic effects of internal promotion.

In sum, the interrelations between tenure, mobility, and earnings are not well established empirically, which may be one reason why they continue to be subject to theoretical controversy. To move research forward, we think it is essential to simultaneously consider tenure, internal mobility, and external mobility in examining the determination of earnings. It is to the development of such a model that we now turn.

Earnings Determination over the Life Cycle

In principle, one may distinguish four main determinants of earnings growth during an individual's career. First, there is the general rate of real wage growth (or decline) in the national economy. This, in turn, is partly but not entirely an outcome of changes in productivity and the economic situation at the national level. Secondly, individuals increase their general productivity over time by accumulating experience and skills. Although there are exceptions to this rule, it probably applies to most individuals at least during the early and formative parts of their working life. Thirdly, individuals change position in the labour market. If some positions are better paid than others, net of the characteristics of individual workers, movement among positions will in many cases affect the rate of earnings growth. Fourthly, net of the pay-off to change in positions, there is a conceivable impact of job mobility as such. In other words, it is important to distinguish between earnings effects of job shifts across and within positions.

We operationalize position as occupation. This is the standard operationalization in sociology, although there is, of course, also a variation in positional characteristics within occupations. Industry and establishment size are examples of factors that might be used to distinguish (some of) this variation, but we will not attempt to do so in the current paper. Occupation is a much more powerful determinant of earnings than is either industry or size, at least in Sweden.[4] Further, in contrast to the two latter factors, occupation varies across positions both within and between establishments. This is important since establishment-internal careers is one of our main concerns. In addition, the data-set we use (see further below) has several highly desirable characteristics, but a rather small number of

observations. Therefore, we keep the empirical models as simple as possible with regard to the number of variables involved.

The direction of the impact of 'pure' (within-position) mobility is not clear on theoretical grounds. There are arguments for expecting both positive and negative earnings effects of mobility. A negative effect will occur if mobility implies that accumulated specific human capital cannot be used in the new job. A positive effect, net of the difference in rewards tied to positional characteristics of the origin and destination jobs, is based on two conceivable mechanisms. First, a job shift might improve the quality of the job-worker match, *inter alia* due to a better use of the worker's skills. Secondly, we suggest that transfer premia may be paid at the destination in order to induce a shift. Such premia need not be based on improved matching, but rather on the relation between demand for and supply of different kinds of labour. The filling of expensive vacancies may be a sufficient reason for employers to assume high recruitment costs.

On average, job shifts probably carry a combination of positive and negative effects, with the net balance varying across specific situations. It is this net effect that we will estimate in the present paper. To achieve greater precision, we attempt to distinguish empirically between voluntary and involuntary job shifts. There is no direct (self-reported) measure of this distinction in our data. Instead, we rely on information on the occupation held at each job. We see job shifts that involve a loss in expected occupational earnings (see further below) as involuntary, and all other moves as voluntary. Since the sample we use consists of men only, we believe that such an operationalization is satisfactory, although not perfect. There may exist some voluntary job changes that involve earnings losses, for example because the spouse has changed job. We believe, however, that the number of such job changes is very small for men.[5]

Analytical Strategy

We focus on longer-term rather than short-term earnings change. Although the short-term case – how earnings change between two consecutive jobs or between two time-points given job changes in the interim – may be easier to deal with methodologically, it is the longer-term case that is the more important, for two reasons. First, in the perspective of individual utility or welfare, total earnings during a long interval are more consequential than episodic earnings changes. Secondly, the impact of mobility on earnings is likely to be long-range in character. When workers choose between staying at a job or leaving it, they normally consider expected rewards not just during one year but several or even many years ahead. And from the converse side, so do the employers. Indeed, it is probably not uncommon that workers consider more than two jobs at a time – not just the current and the contemplated next, but also how the latter might affect chances for further steps (cf. Spilerman, 1977). Regardless of how amenable careers actually are to rational planning, however, job shifts (or their absence) are likely to have long-term consequences, in a sequential and perhaps unforeseen manner.

While we favour the long-term perspective in our analyses, we also carry out alternative estimations of our main models. These alternatives are based on more conventional short-term considerations. Useful comparisons between our longer-term results and the outcomes of other kinds of approaches may be carried out by estimating fixed-effects models (cf. e.g. Greene, 1997; Hsiao, 1986; Petersen, 1993); see further below.

We look at a crucial phase in the careers of workers: the period between the ages of 26 and 35. At the start of this phase, education is usually completed and an early period of more or less erratic job shopping has ended. At the end of the 26–35 age-span, work-life careers have typically stabilized and change little thereafter. In between, a formative period evolves that is important to investigate empirically. However, since the relative importance of internal and external mobility for earnings growth might differ across stages in work-life careers, we do not generalize the findings beyond the age group analysed here.

There are two main sources of potential bias in the estimations of the impact of job mobility on earnings growth. The first is that there may be unmeasured variation across individuals in characteristics that influence both mobility and earnings. This unobserved heterogeneity will bias the parameter estimates of interest by adding a

spurious component of unknown direction and size. The second problem is that the presumed predictor (job mobility) is endogenous with respect to the presumed outcome (earnings growth). In other words, while it is reasonable to assume (as we do in this paper) that mobility may have a causal impact on earnings, it is equally plausible that there is a causal influence in the opposite direction. Specifically, the higher the rate of earnings growth at the job presently held, the less likely, *ceteris paribus*, is a voluntary exit from that job. This means that the error (residual variation) in earnings growth is correlated with job mobility, which violates a standard requirement of ordinary least squares (OLS) estimation. These two sources of bias both come about as consequences of non-experimental data design, and there is a set of methodological devices for dealing with them (see e.g. the review in Winship and Morgan, 1999). Within this set, we have chosen the following two techniques.

The standard procedure to control for time-invariant unobserved heterogeneity is fixed-effects estimation. The purpose of this technique is to remove the effects of unmeasured individual characteristics that do not change over time, such as socioeconomic background, innate ability, and deeply rooted attitudes. Hence, the 'fixed-effects' refer to the unique impact of unmeasured but stable characteristics of each individual (see England *et al.*, 1988; Rosenfeld and Nielsen, 1984 for early sociological applications of fixed-effects models). The removal of the stable part of the individual's residual is accomplished by transforming all variables of the earnings equation into deviations at each specific point in time from the individual's mean for all covered time-points (see the Appendix). Thus, specifying the regression equations this way effectively (by definition) eliminates time-constant heterogeneity. We follow this procedure here.

However, the problems of time-variant unobserved heterogeneity and of endogeneity remain. As an attempt to overcome these sources of bias, we use instrumental variable estimation. The idea is to replace the endogenous predictor (job mobility) with another variable (the instrument) that is assumed to have two important properties: (*a*) to be strongly related to the endogenous predictor, but (*b*) uncorrelated with the error (residual variation) of the outcome variable (earnings growth). We return to these issues below.

Data

Data come from self-reported retrospective employment histories collected among a national probability sample in the Swedish Level of Living Survey, 1991.[6] Yearly earnings information from tax registers is available for the survey respondents for the period 1951 to 1990, the last complete year in the employment data.[7] The respondents who were between 26 and 35 years old during this period were thus born between 1925 and 1955. We restrict the sample to men who were wage-earners in June of the relevant years, thus excluding women, the self-employed, and non-employees. This restriction is motivated by the lower correspondence between income as registered by the tax authorities and actual earnings from work in comparable time units among the excluded individuals. Practically all of the selected individuals have been employed for the entire year in which they were employees in June. Although we lack information on working hours, it is reasonable to assume that a large majority of male employees in the considered age-span work full-time.

The earnings data concern calendar years, and we have adjusted the employment history information accordingly. Data on education, labour-force experience, employer tenure, and occupation are all read off at June – the mid-point of the included calendar years. The number of job shifts during the period is estimated as the number of times that an individual has changed job from age 26 to 35, with separate counts for internal (to the employer) and external moves. In effect, the data structure is a panel of ten adjacent years starting at age 26. There are 742 individuals in this panel.[8]

As already indicated, the strategic advantage of the data-set that we use is the tight connection between information on mobility and information on income. First, the respondents report explicitly on all internal and external job shifts they have made since their first steady job. Thus, the occurrence of job shifts need not be inferred from other kinds of information, such as on tenure or on occupation. This is an advantage that retrospective

surveys on work-life histories tend to have in common. Secondly, the information on income is not retrospective, but is based on contemporaneous tax registers (matched with the survey data by the equivalent of an individual social security number). This is important because retrospective information on income tends to be quite unreliable (see e.g. Hannan *et al*., 1990). The combination of direct self-reported information on job shifts and reliable register information on income makes the data-set very well suited to our purposes in this paper.

Measuring the Long-term and Short-term Effects of Mobility

When analysing the long-term effects of mobility, all of the dependent and independent variables, except mobility, are measured as growth defined as the difference between the average value of the variable at age 27 to 35 and the value at age 26, divided by the value at age 26 (see the Appendix for a more detailed exposition of our methodological approach). If earnings grow at a constant rate during the interval we study, this ratio is highly correlated with the slope of the growth curve. Of course, the growth rate will tend to fluctuate significantly across years in many cases, but the proposed ratio appears to be a useful and simple approximation of the actual growth pattern.[9] Although averaging certainly involves a loss of information in a strict sense, we believe that much (or perhaps most) of the short-term fluctuations in recorded earnings is noise. Hence, the use of averages over a number of years is at least as likely to increase as to decrease the strength of the information signal transmitted by the earnings data.

The determinants of earnings growth are defined in the same temporal context. As stated above, there are four main factors to consider. The economy-wide development is measured as the growth rate of average real wages among male manual workers in the manufacturing industry for the period when the individual respondent was 26 to 35 years old. The second and third explanatory factors are the growth rates of individual human capital and occupational standing. To ease our model specifications, we use a linear framework as far as possible.[10] In order to achieve this, we transform the human-capital and positional variables into 'earnings values' as explained in the Appendix. In short, the earnings value of years of experience is given by the predicted earnings for different lengths of experience, given an average (sample mean) education. In a converse fashion, the earnings value of education is obtained (predicted earnings given average experience). The earnings value of tenure is constructed on the basis of an earnings regression where tenure and its square are included in addition to the earnings values of education and experience.

The positional variable used in these analyses is occupational standing, measured as the earnings value of the occupation (see the Appendix). The resulting measure indicates the expected earnings of an individual in a given occupation, net of occupational variation in human capital of the incumbents and net of variations over historical time in occupational distributions.

Involuntary mobility is measured as a single dummy – whether at least one job shift, internal or external, has involved a loss in occupational standing. All other job shifts (i.e. those lateral or upward in the occupational structure) are defined as voluntary. Voluntary internal mobility is measured via two dummy variables: (*a*) whether the respondent has experienced one (and only one) internal shift between age 26 and age 35, and (*b*) whether he has made two or more internal shifts during the same period. Voluntary external mobility is measured in the same way – a dummy for one external and a dummy for two or more external job shifts. Altogether, then, mobility is indicated by five dummy variables with zero moves as the reference category.

For the alternative analyses, the specification of the fixed-effects models is based on equation (6) of the Appendix. In these models, the variables are measured as immediate changes between adjacent time-points (years in our case) for each individual. More specifically, all variables are transformed to deviations from the individual mean for all included time-points, which implies that the time-constant part of the individual-specific residual disappears from the equation.

Before we go on, some remarks on the causal structure of our model of long-term change are in order. One problem with the our proposed model is that some fraction, possibly large, of the earnings

change that constitutes the dependent variable might have occurred before the events (including job shifts) that are used to explain it. Thus, (part of) the effect would seem to temporally precede (part of) the cause. We have two comments on this matter. First, to some extent there appears to be a trade-off between the interest in long-term sequences of events and the interest in causally clear-cut data structures. Our model will, we believe, produce descriptively interesting information on the co-variation between mobility and earnings. Secondly, the time order of cause and effect is not so obvious as it may seem. Rational individuals anticipate (the probability of) future events and take these into account when acting in the present. For example, as assumed in human-capital theory, individuals consciously forego present earnings when enrolling in education in order to increase their chances of getting a well-paid job later on. These foregone earnings should be included in an assessment of the lifetime pay-off of education, despite the fact that they take place before the educational credential is achieved. They are, in a sense, an effect that occurs before the cause. Likewise, and closer to the concern with job mobility, improving career chances in an internal labour market may require an initial period of training with low wages before promotion begins. Although less well explored, similar patterns may obtain in other kinds of labour markets as well. In sum, intentional explanation, sometimes seen as a special case of causal explanation, blurs the time order of cause and effect.

In the fixed-effects models, as used in this paper, the variables are measured as immediate changes between adjacent time-points (years in our case) for each individual. Instead of one long-term change measure, as in our main strategy, all one-year changes are recorded and used in the model. This makes things easier in one way; reverse causality would appear to be less of a problem (although the potential bias due to endogeneity of mobility remains just as serious). As just discussed, however, this short-term approach also has its drawbacks. Nonetheless, for comparative purposes estimates from such a specification are useful as a complement.

Results

Descriptive Overview

The development of real earnings, education, experience, tenure, and occupational standing during the age-span 26 to 35 is shown in Table 1. (Recall that the second time-point is not age 35, but the average for age 27 through 35. In this sense, the length of the period is five years – 26 to 31, the mid-point of 27 and 35 – rather than nine.) Real earnings increase on average by 18.5 per cent. More than half of this increase appears to be accounted for by the general growth in real wages in the economy as a whole. Not surprisingly, the amount of education changes very little after age 26, as evident both from the change in educational earnings values (about 1 per cent) and the absolute change in years of education (0.2 years). Of course, general labour-force experience changes much more. Most of the workers considered have been employed for the entire time-span. Although the absolute number of years of experience goes up by more than 60 per cent on average, the earnings

Table 1. *Earnings, human capital, and occupational standing from age 26 to age 35: relative and absolute changes* (N=742)

	Age 26	Mean age 27–35	Std. dev. of change
Real earnings	100	118.5	31.3
Real wage, national average	100	110.2	10.7
Education, earnings value	100	101.1	3.2
Experience, earnings value	100	108.1	2.0
Tenure, earnings value	100	100.8	2.0
Occupation, earnings value	100	102.1	8.2
Education, years	10.3	10.5	0.6
Experience, years	7.7	12.6	0.5
Tenure, years	3.8	6.5	2.8

value of experience changes considerably less because of decreasing economic returns to experience over the life-cycle. Average tenure (years spent with the current employer) increases by 70 per cent. As in the case of general experience, the earnings value of tenure changes much less than tenure measured by time. Occupational standing, finally, expressed as earnings values, improves by a modest 2 per cent on average. The variance of this increase, however, is considerably larger than the variance of the increase in human capital.

The frequency of job shifts from age 26 to 35 is evident from Table 2. About two-thirds of the workers have changed jobs at least once during the period, and about one-third have made two job shifts or more. External moves, i.e. changes of employer, are more common than internal shifts. In fact, seven out of ten job changes are between employers. One in two workers have spent the entire period with the same employer, and among these more than two-thirds have held only one job. Interestingly, there is a strong negative correlation between the number of internal shifts and the number of external shifts. Among workers who have changed jobs at least once, the correlation between the frequencies of internal and external moves is −0.51. Very few workers change jobs at a generally high rate, as may be seen by the prevalence of empty cells in the lower right-hand part of the cross-tabulation in Table 2. Rather, some individuals tend to change employers frequently, while others pursue internal careers. Apparently, these are two distinct job shift patterns. This finding underscores the need to take them both into account when assessing the impact of job mobility on earnings growth.

We now turn to examining the impact of job mobility on the growth in earnings between the ages of 26 and 35. As a preliminary, consider the earnings curves of Figure 1, showing the average growth in yearly earnings for three types of workers: those who never changed jobs from age 26 to 35, those who made at least one internal (within employer) job shift during this period, and those who changed employer at least once.[11] Both the internal and the external job changers turn out to have a much steeper earnings progress than the stable workers do. Thus, while the stayers earn about 15 per cent more at age 35 compared to age 26, the corresponding earnings growth for internal and external movers is almost twice as large – 29 and 27 per cent, respectively.

The figure gives the impression that job mobility is a very important tool to improve one's earnings. As discussed above, however, we must be very careful about drawing conclusions about causality, since it is reasonable to assume that selection processes will explain at least part of these mobility effects. These issues will be considered in the following sections.

Regression Models of Long-term Change

In what follows, elaborate controls are implemented in order to arrive at purer estimates of mobility effects on earnings growth on the basis of a number of regression models. We concentrate on our main approach of looking at long-term change, and then

Table 2. *Number of job shifts from age 26 to age 35, percentage distribution* (N=742)

N of external shifts	N of internal shifts					
	0	**1**	**2**	**3**	**4**	**Total**
0	34.4	10.4	3.4	0.9	0.3	49.3
1	23.7	4.4	1.1	0.4		29.6
2	10.6	2.3	0.4	0.1		13.5
3	5.1	0.4				5.5
4	1.3	0.1				1.5
5	0.4					0.4
6	0.1					0.1
Total	75.7	17.7	4.9	1.5	0.3	100.0

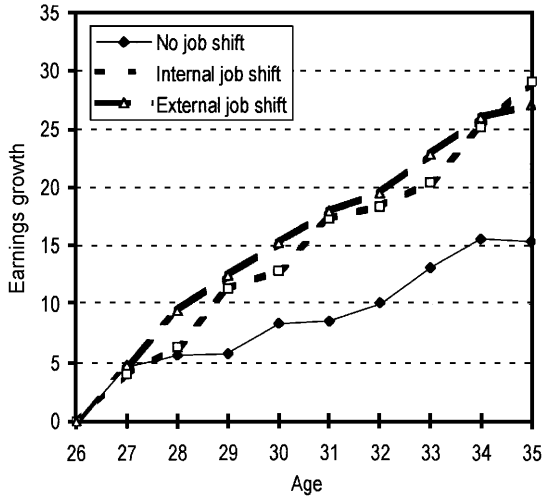

Figure 1. Income profiles by type of mobility: average earnings growth from age 26 to 35 (%)

turn to fixed-effects models of episode (person-year) data as points of comparison. Finally, we present the results of models of long-term change with instrumental variable estimation.

Regression models based on the first analytical strategy are shown in Table 3. All independent variables except the job-shift dummies are measured as deviations from their means. Hence, the intercept term in the following regressions is an estimate of the predicted growth in earnings for workers with average values on all determinants who have not changed jobs during the period. The regression coefficients of the change in economy-wide real wages, the change in individual human capital, and the change in occupational standing are estimates of elasticities: how much earnings growth in percentage terms, on top of the average growth rate, does a 1 per cent increase in each of the determinants lead to? For the job shift indicators, the regression coefficients reveal how much additional (or how much less) earnings growth (in per cent) is achieved by changing jobs as compared to remaining in the same job for the entire period.

The estimates of Model 1 in Table 3 show that the average worker gains slightly less than 20 per cent in real earnings over the period considered. The elasticity of real wage growth in the economy is close to unity, as might be expected. The standard error of the estimate is very small, indicating that almost all workers receive the standard share of economy-wide wage increases. This is probably in part a reflection of the centrally coordinated wage-determination process in Sweden. In other countries with different bargaining systems, the standard error of the national wage-growth estimate would probably be larger. Increases in education give a marginally significant pay-off, although the estimate is clearly below 1. Gains in experience also add to earnings growth. The point estimate is far above 1 and significant.

Tenure is added in Model 2. The pay-off of increases in tenure is not much below unity, but not quite significant judging by this equation. Apparently, the relationship between tenure and earnings is far from uniform across workers in different parts of the labour market. As is evident from the results of Model 3, the impact of increases in occupational earnings value is numerically smaller than the tenure effect, but highly significant. A 1 per cent increase in occupational standing leads to an almost 0.5 per cent gain in earnings growth. The impact of tenure is somewhat larger when changes in occupation are taken into account, and becomes significant at the 5 per cent-level.

Model 4 gives base-line estimates of the impact of job mobility on earnings growth. There is a very large positive effect of changing jobs internally at least twice between the ages of 26 and 35. These workers gain an additional increase in earnings of almost 27 per cent, arriving at a total growth of almost three times the standard rate. It is a small category, only $3\frac{1}{2}$ per cent of the included sample (26 out of 742 individuals). Nonetheless, the effect is highly significant. As we shall see, it will remain so throughout the various models that we consider. Only one internal job shift, however, is not enough to gain a significant earnings pay-off. We return to this finding below. External mobility also gives a gain in earnings growth. One external shift leads to an average addition of 7 per cent to the standard growth rate, which is significant by a good margin. Interestingly, those who make two or more external job shifts receive an earnings gain of the same magnitude as the pay-off of a single shift of employer. Hence, in contrast to the case of internal mobility, the economic returns of external job shifts appear to be rapidly diminishing.

Table 3. *Regression analyses (OLS) of real earnings growth (%) from age 26 to 35: unstandardized regression coefficients (t-values in parentheses)* (N=742)

	1	2	3	4	5	6	7
Intercept	18.57**	18.57**	18.57**	14.23**	15.86**	15.10**	15.93**
	(22.6)	(22.7)	(23.0)	(10.7)	(12.7)	(11.7)	(12.3)
Δ Average wage	0.84**	0.85**	0.85**	0.72**	0.84**	0.85**	0.85**
	(10.3)	(10.4)	(10.6)	(8.5)	(10.4)	(10.5)	(10.7)
Δ Education	0.50	0.56*	0.57*		0.50	0.56*	0.57*
	(1.9)	(2.1)	(2.2)		(1.9)	(2.2)	(2.3)
Δ Experience	3.02**	2.92**	2.53*		2.68**	2.54*	2.34*
	(2.9)	(2.8)	(2.4)		(2.6)	(2.4)	(2.3)
Δ Tenure		0.85	1.02*			1.13*	1.17*
		(1.9)	(2.3)			(2.4)	(2.5)
Δ Occupation			0.47**				0.39**
			(4.6)				(3.6)
One internal shift				3.92	2.11	2.39	0.99
				(1.6)	(0.9)	(1.1)	(0.4)
Two or more IS				26.78**	19.45**	19.39**	17.42**
				(5.6)	(4.3)	(4.3)	(3.9)
One external shift				6.98**	5.96**	6.69**	5.02*
				(3.4)	(3.1)	(3.5)	(2.5)
Two or more ES				7.33**	3.30	5.04*	2.15
				(2.8)	(1.3)	(2.0)	(0.8)
Involuntary shift				−0.43	−1.33	−0.31	0.84
				(0.2)	(0.7)	(0.2)	(0.4)
R^2 (adj.)	0.489	0.491	0.505	0.433	0.504	0.507	0.515

Notes: All reported job shift variables, except involuntary shift, refer to moves that are lateral or upward with respect to occupational earnings values. The reference category for all job shift variables is zero moves.
All models control for the starting values (age 26) of earnings and (except Model 4) experience.
All variables except the job-shift dummies are measured as deviations from their means.
Average wage is the national average of real wages.
Education, experience, tenure, and occupation are measured as relative change (%) in earnings values.
Significance levels (two-tailed tests): * ⩽ 0.05; ** ⩽ 0.01.

Education and labour-force experience are added in Model 5. This brings the estimated returns to job mobility down a good deal. Even given controls for human capital, however, the pay-off to frequent internal job moves is very large, close to 20 per cent. The effect of frequent external job shifts drops by more than half when human capital is taken into account, and becomes non-significant. A single employer shift is clearly worthwhile, however, leading to an average earnings gain of about 6 per cent.

Model 6 adds tenure. When considered jointly with the occurrence of job shifts, the impact of increases in tenure on earnings growth is stronger than in Model 2, where mobility covariates are not included. The pay-off of internal moves is largely unaffected by the inclusion of tenure, but the estimated impact of external shifts increases somewhat. Frequent shifts of employer now have a significant effect of about 5 per cent, but this estimate is still smaller than the impact of a single external move. Thus, Model 6 indicates, in line with Topel (1991), that it is important to jointly take tenure and employer shifts into account. Since they are negatively correlated with each other (recent between-employer movers by definition have short tenure), but both apparently pay off in earnings, including only one of them in the model leads to downwardly biased estimates of their effects. A more surprising result is that the effects of tenure

and internal mobility seem to be only weakly interrelated. This finding needs elaboration.

As discussed above, part of the impact of job mobility on earnings growth may be expected to run via movement between differentially rewarded occupations. In Model 7 we attempt to estimate how much of the job shift effects are indirect in this sense. Adding change in occupation to the model should decrease the estimated returns of job mobility. This turns out to be true, especially for external job shifts. However, the two strongest mobility effects – of changing jobs internally at least twice, and of changing employer once – remain significant and numerically substantial even when occupational change is taken into account. Frequent external moves, by contrast, seem to affect earnings growth chiefly by increases in occupational standing. When occupational change is taken into account, changing employer at least twice does not contribute significantly to the rate of earnings growth.[12] Altogether, the main conclusion of Model 7 is that the dominant part of the impact of job shifts on earnings growth is a pure (within-occupation) mobility effect. Such a conclusion is preliminary, of course, since there are other positional variables than occupation that may need to be taken into account.

Fixed-Effects Models

As an alternative to the analyses presented above, we have estimated fixed-effects models based on short-term (yearly) changes. In order to keep the specification as similar to the long-term change analyses as possible, we use logarithmic transformations of the earnings values for all independent variables, except the job-mobility dummies.

Table 4 presents the results of five fixed-effects models. First, a model with only economy-wide real wages plus schooling, experience, and tenure. Secondly, the same model with occupation included. Thirdly, a model with the job shift variables and economy-wide real wages as predictors. Fourthly, a model including both the human-capital indicators of Model 1 and the job-shift variables. Fifthly, a full model, in which occupation is added to all the other predictors.

The job shift effects in Tables 3 and 4 do not differ much from each other. In other words, the substantive conclusions of the fixed-effects models seem to be similar to those we reached earlier. According to the specification in Model 3 of Table 4, more than one internal job shift implies an earnings increase of about 19 per cent, while the estimated earnings effect of more than one external job shift is almost 11 per cent. As seen from Model 4, however, part of the earnings effect of internal job shifts is explained by schooling, experience, and tenure, although the effects of external job mobility are not much affected by the inclusion of the human-capital variables. By contrast, when we control for occupation in Model 5, the external mobility coefficients are greatly reduced. Thus, most of the earnings gain associated with external mobility seems to be due to upwardly directed occupational shifts. This is true to a much lesser extent for internal mobility.

Model 1 of Table 4 indicates that tenure does not have any significant impact on earnings. When occupation is included in Model 2, however, the tenure effect increases somewhat and is now significant at a conventional level. Moreover, when we include tenure together with mobility in Model 4, the coefficient of tenure is almost doubled. More detailed analyses (not shown here) reveal that almost all of this increase in the tenure effect is due to the inclusion of external job shifts. In sum, the results concerning employer tenure in Table 4 show the same pattern as the long-term change analyses of Table 3, and thus give further support to the argument that tenure and employer shifts should be taken into joint account.

Instrumental Variables Estimation of Long-term Growth

One important purpose of the fixed-effects models in the previous section was to take time-constant unobserved heterogeneity across individuals into account. As already indicated, however, this is only a partial correction for the potential bias involved in our estimations. In addition, we need to consider the twin problems of (*a*) time-variant unobserved heterogeneity and (*b*) endogeneity of job mobility with respect to earnings growth. The device we

Table 4. *Effects of internal and external job shifts on (log) yearly income from age 26 to 35: fixed-effects estimations (t-values in parentheses)*

	1	2	3	4	5
Intercept	0.868	−4.519*	7.565**	0.893	−4.280*
	(0.4)	(2.2)	(64.7)	(0.4)	(2.1)
ln(Average wage)	0.895**	0.895**	0.955**	0.884**	0.891**
	(30.6)	(31.0)	(35.8)	(30.4)	(30.8)
ln(Education)	−0.237*	−0.186		−0.253*	−0.204
	(2.1)	(1.7)		(2.3)	(1.9)
ln(Experience)	0.613**	0.504**		0.326**	0.306**
	(10.5)	(8.6)		(4.5)	(4.2)
ln(Tenure)	0.214	0.261*		0.517**	0.495**
	(1.6)	(2.0)		(3.5)	(3.4)
ln(Occupation)		0.467**			0.429**
		(12.3)			(9.5)
One internal shift			0.056**	0.042**	0.013
			(5.5)	(4.0)	(1.2)
Two or more IS			0.186**	0.154**	0.120**
			(7.8)	(6.3)	(4.9)
One External Shift			0.060**	0.055**	0.030**
			(7.8)	(6.3)	(3.3)
Two or more ES			0.105**	0.092**	0.041**
			(8.5)	(6.7)	(2.8)
Involuntary shift			−0.011	−0.017*	0.015
			(1.4)	(2.0)	(1.6)
F-value	509.43	447.81	348.09	240.45	228.58
R^2 within	0.245	0.263	0.250	0.257	0.267

Notes: The number of individuals is 742, and the number of observations is 7022.
All reported job shift variables, except involuntary shift, refer to moves that are lateral or upward with respect to occupational earnings values. The reference category for all job shift variables is zero moves.
Average wage is the national average of real wages.
Education, experience, tenure, and occupation are measured as relative change (%) in earnings values.
Significance levels (two-tailed tests): * ⩽ 0.05; ** ⩽ 0.01.

will use as a remedy against these problems is instrumental variables estimation.[13] We proceed as follows.

First, we transform our five job-shift dummies into a single variable that simply measures the total number of (voluntary and involuntary) job shifts, external or internal. This is done to avoid the complexities involved in instrumenting several variables in the same model. In Table 5, Model 1, we give the results of a model of long-term earnings change that is identical to Model 6 of Table 3, except that mobility is measured by the simple job shift count instead of the set of five dummy variables. The estimated linear effect of mobility is 2.4 per cent earnings gain by each job shift, which is significant by a good margin. Secondly, in Model 2 of Table 5, we take individual fixed effects into account by adding a variable that measures the predicted individual-specific residual from the fixed-effects model in Table 4 (Model 5). In this way, following Arai (1998), we attempt to control for the time-constant part of unobserved heterogeneity.[14] We find that the estimated effect of mobility is more or less unchanged. Apparently, unmeasured time-constant individual traits that affect earnings growth are not highly correlated with the frequency of job shifts.[15]

Thirdly, we instrument the mobility variable. The task is to select an alternative variable, an instrument, that is strongly correlated with mobility but (unlike mobility) uncorrelated with the error (residual variation) of earnings growth. We have chosen employer tenure at age 26 (the start of the period under consideration) as an instrument. It fits

Table 5. *Regression analyses of real earnings growth (%) from age 26 to age 35: controlling for fixed effects, and IV-estimates. (t-values in parentheses)* (N=742)

	1	2	3	4
Intercept	15.82**	15.84**	15.41**	14.25**
	(13.1)	(17.7)	(9.4)	(11.7)
Δ Average wage	0.84**	−0.17*	0.84**	−0.18*
	(10.4)	(2.4)	(10.4)	(2.4)
Δ Education	0.53*	0.78**	0.53*	0.77**
	(2.0)	(4.1)	(2.0)	(4.0)
Δ Experience	2.73**	1.80*	2.70**	1.69*
	(2.6)	(2.3)	(2.6)	(2.2)
Δ Tenure	1.34**	0.91**	1.41**	1.20**
	(2.8)	(2.6)	(2.8)	(3.2)
Number of Job Shifts	2.40**	2.44**	2.75*	3.83**
	(3.1)	(4.2)	(2.2)	(4.2)
Individual fixed effect		88.29**		88.32**
		(24.8)		(24.7)
R^2 (adj)	0.497	0.726	0.497	0.724

Note: Models 3 and 4 show instrumental variables estimates where the instrument is log(earnings value of tenure) at age 26. Else, see Table 3. Significance levels (two-tailed tests): * ⩽ 0.05; ** ⩽ 0.01.

the criteria of a suitable instrument quite well, since it (*a*) has a strong (negative) effect on subsequent mobility, and (*b*) arguably is weakly (if at all) related to the error of earnings growth. In Model 3 of Table 5, the outcome of this instrumentation, carried out by two-stage least squares (2SLS) estimation, is shown. The result should be compared to Model 1, which contains the uninstrumented (OLS) version of the same model. The point estimate of the mobility effect in the instrumental variable (IV) model is not significantly different from the corresponding effect in the OLS model.[16] This indicates that there is no strong selection of job movers with respect to earnings growth.

The final model in Table 5 (column 4), is an instrumented (2SLS) version of the OLS model in column 2. The rationale behind Model 4 is that, although the correlation between the instrument (tenure at the start of the period) and the error in the outcome variable (earnings growth) is probably not high, it is arguably especially low if time-constant unmeasured individual traits are controlled for. To see this, consider the assumed causal structure of the main variables involved in our analyses. There is (*a*) a two-way (reciprocal) causal relation between job mobility and earnings growth, and (*b*) a set of unmeasured individual traits that influence both mobility and earnings. While it is unlikely that earnings growth after the start of the period causes tenure at the start of the period (our instrument), they may still have a common unmeasured cause (such as ability). Such a cause, however, should be time-constant over the considered period, because changes after the start of the period (such as acquired skills) are unlikely to cause conditions at the start of the period. Therefore, if we control for time-constant unmeasured traits, we can be more confident that our instrument is uncorrelated with the error in our outcome variable. This reasoning leads us to believe that the best correction for bias is to simultaneously use an instrumental variable estimation and include the measure of individual fixed effects in the model.

The results of such a model support our previous conclusion: there is a strong effect of job mobility on earnings growth. The point estimate of this effect is almost 4 per cent, to be compared with the 2.4 per cent OLS estimate in Model 1. We do not wish to emphasize this increase, although it seems reasonable enough since it implies that job movers are adversely selected with respect to earnings growth at the outset, in line with our expectations. Instead,

we draw the more conservative conclusion that the significant effect of job mobility on earnings growth, according to the OLS estimate, does not seem to be upwardly biased by unobserved heterogeneity, or by endogeneity of mobility with respect to earnings growth.

Conclusions

The primary issue that we raised at the outset of this paper was whether job mobility is a vital cause of earnings growth, as theories claiming that rewards are tied to positions rather than to persons would predict. Our empirical findings strongly support this prediction. A second issue we raised was whether the impact of mobility goes beyond what differences in rewards across positions imply. We tentatively conclude that it does. Job shifts significantly increase earnings even within detailed occupations. Such 'pure' mobility effects have received insufficient theoretical attention in the literature, and should be explicitly dealt with in future research.

More specifically, our main empirical findings are the following:

1. Internal and external job shifts are distinct pathways in work-life careers. Very few individuals pursue both routes.
2. Both kinds of mobility have a positive effect on the rate of earnings growth. Internal mobility, if sufficiently frequent, has the strongest impact. These effects are very robust across various types of model specifications and estimation techniques.
3. The impact of firm tenure and external mobility should be considered simultaneously. Otherwise, the effects of both are biased downward. However, the tenure effect on earnings is largely unrelated to internal job shifts.
4. The impact of internal mobility on earnings growth chiefly operates net of occupational advancement. By contrast, the effects of external mobility to a considerable extent run via occupational attainment.

Of course, these conclusions are preliminary. We use a small sample of male workers over a long historical period in a single country; we focus on a limited, if crucial, phase of work-life careers; and we need to control for additional potentially confounding factors. Nonetheless, our findings are suggestive. Much further work at the intersection of the research fields of work-life mobility and earnings determination is called for. We end by offering some further comments on the basis of our findings.

A significant contribution of our results is that they demonstrate the importance of firm internal mobility for the process of individual earnings growth. Evidently, changes of employer are seriously incomplete as an indicator of job mobility. The effects of internal job shifts also appear to be distinct from the earnings–tenure relationship. Even in the absence of job shifts, earnings grow somewhat with time spent in the current firm; in part, perhaps, due to accumulation of firm-specific human capital. But if internal job shifts take place at a sufficiently frequent pace, the rate of earnings growth increases dramatically. This implies that, although there may be something also to human-capital theoretical explanations of wage–tenure profiles, structural versions of internal labour-market theory seem to be more important for understanding earnings growth. One explanation of the negligible earnings effect of a single internal move may be that a significant proportion of single moves may be non-promotions. Promotions probably occur in a more sequential, step-wise manner than other internal moves.

Changes of employer are quite a different matter. They pay off less than internal job shifts, and their economic returns diminish rapidly with their frequency. Already after one move, additional changes of employer do not, on average, improve the rate of earnings growth (net of the advantages that follow from shifting occupation). Indeed, some model specifications imply that frequent external job shifts may even have a negative impact on earnings growth. By contrast, the returns of internal job shifts increase during the age span we consider. One explanation may be that successful matching is more difficult to achieve by external than internal moves, because the quality of information available to employers and workers is better in internal labour markets (see e.g. Althauser and Kalleberg, 1981).

A common trait of internal and external mobility, as revealed by our models of long-term earnings growth, is the significant discontinuities in the

estimated impact of job shifts. We think that this finding carries implications for how to think about mobility processes. The diminishing economic returns of employer shifts and the increasing returns of internal moves (if corroborated by further research) need to be taken into account in standard mobility models. The common practice of estimating models of the duration until a specific kind of mobility event occurs, such as changing employer, loses much of its attraction if events of the same apparent kind are strongly dissimilar in their consequences. Our results add to the concerns that have occasionally been expressed about the loss of information that occurs by splitting a sequence of events into its constituent parts without keeping track of their interrelations (see e.g. Abbott and Hrycak, 1990). According to our estimates of the impact of job shifts on earnings growth, the place of an event within a sequence of events is of crucial importance for its outcome.

We have shown that job mobility is a significant source of earnings growth. Who are the workers taking advantage of this? Individuals change jobs on the basis of three kinds of factors: preference, capacity, and opportunity. Preferences and capacities are characteristics of persons. As such, their impact is taken into account in our models, to the extent that we have succeeded in eliminating unobserved heterogeneity among individual workers. The remaining factor of conceivable importance for the issue of who becomes a mover rather than a stayer is thus opportunity. No doubt some structural settings in the labour market are more conducive than others to individual careers. For example, large establishments typically provide better opportunities for internal mobility than smaller establishments do. And some kinds of occupations and industries probably offer better chances than others of beneficial shifts between employers. To examine such variations in mobility opportunities across different parts of the labour market is a natural and important extension of the analyses presented here.

Notes

1. The empirical literature takes off with Bartel (1980), Borjas (1981), Bartel and Borjas (1981), and Mincer and Jovanovic (1981). All of these, as well as much later work, use a human-capital framework. These studies tend to equate job mobility with employer change, and hence to neglect the issue of firm internal job shifts that is one of our major concerns in the present paper. A recent exception is McCue (1996). An early sociological study is Rosenfeld (1983).
2. The controversy starts with Abraham and Farber (1987) and Altonji and Shakotko (1987) who claim that the economic returns to tenure are much smaller than standard cross-sectional estimates imply. Topel (1991) is a forceful reply, concluding that the true returns actually are close to what cross-sectional studies show. This came to be the accepted view for some time. Recently, Altonji and Williams (1997) reassessed Topel's work, and found tenure returns closer to what they found in their earlier work than to Topel's estimates. The matter does not appear to be settled. Hutchens (1989) gives a useful overview of the issue and its theoretical repercussions. See also Farber (1999) and Malcolmson (1999).
3. The dispersion tends to be large though, with some movers incurring losses; see e.g. Mincer (1986). For Sweden, see Björklund and Holmlund (1989); Holmlund (1984).
4. In the 1991 Level of Living Survey, which we use below for our empirical analyses, occupation (183 three-digit ISCO-type categories) explains 40.4% of the variation in (the log of) hourly wages. Adding industry (32 categories) and establishment size (8 categories) increases the proportion explained variance by a mere 2.2%.
5. Some voluntary moves certainly involve pecuniary losses that are compensated by non-pecuniary gains. However, such moves are likely to be exceptions: pecuniary and non-pecuniary job rewards tend to be strongly and positively correlated with each other. One of the most comprehensive sociological studies available (Jencks, Perman, and Rainwater, 1988) reports a correlation of 0.50 between earnings and a summary measure of non-pecuniary job rewards and a 0.73 correlation between earnings and overall job 'desirability'. In fact, according to the same study, earnings explain about twice as much of the variance in overall job desirability as occupational status does.
6. The sample consisted of 6,710 individuals aged 18–75 years, of whom 5,306 (79.1%) responded by personal interviews lasting between one and two hours. A subset of these respondents, born between 1925 and 1965, reported retrospectively on their work-life history. Information of this kind was obtained from 3,466 individuals (65.3 % of all respondents). There are no work-life history data available for other age groups.

7. See Björklund (1993), for a detailed description of these data. The available tax register information for this period does not contain earnings (income from work) in a strict sense, but rather total income (taxable income from all sources) minus deductions for interest payments (rents on loans). The Swedish term for this income measure is *sammanräknad nettoinkomst* (total net income). However, for the group of workers we consider (male wage-earners 26 to 35 years old), earnings make up the overwhelming majority of this income. From 1967 on we also have information on yearly earnings (*inkomst av tjänst* in Swedish). The correlation between total net income and earnings for this period is high at 0.93.

8. Of the 3,466 individuals who gave information on their work-life histories, 3,078 gave complete reports on growth in human capital (education, experience, and tenure). 1,544 of these are men of whom 970 remain after selection of the considered age-span (26–35 years). Of these, 876 were employees at age 26 and during at least four years between ages 27 and 35. The number of included individuals is reduced to 742 by internal missing values on occupation, industry, or establishment size. Although we do not use the two latter variables in the empirical analyses below, we required information on them because we find it reasonable to assume that if the respondent cannot recall establishment size and industry for a particular job, the information he gives on the mobility sequence containing that job will tend to be unreliable. A comparison between the individuals included in our subsample and those who were excluded due to internal missing values shows (*a*) no significant differences between the groups in education and experience at age 26, (*b*) longer tenure at age 26 (1.2 years longer) among the excluded workers, (*c*) fewer job shifts between age 26 and 35 among the excluded individuals (on average 0.7 moves compared to 1.2 moves in the selected subsample), and (*d*) lower earnings (about 13% lower) at age 26 as well as a lower rate of earnings growth between age 26 and 35 (about 5% lower given group differences on exogenous variables) among the excluded workers. However, these differences between the categories have no significant bearing on our main results. In other words, the estimated impact of job mobility on earnings growth does not depend on the particular subsample chosen among male workers in the specified age-range. In fact, close to half of the difference in earnings growth between the groups (excluded vs. included individuals) can be attributed to the difference in mobility rates.

9. Extensions to take alternative growth curves into account are conceivable (see e.g. Altman and Casella, 1995), but will not be pursued here.

10. Including square terms or other deviations from linearity in the growth regressions would require complexities in the model specifications that are difficult to handle with the rather small data-set that we use.

11. Workers who made both internal and external shifts (they are not many, as shown in Table 2) are excluded from the figure.

12. The diminishing economic returns to employer changes apply both when an occupational shift is involved in the moves and when it is not. In the former case, the size of the coefficient of frequent (two or more) employer shifts is slightly more than half of the size of the effect of one employer change, and in the case of within-occupation mobility somewhat less than half. In both cases, the effect of frequent employer shifts is insignificant when change in occupational standing is taken into account (as in Model 7 of Table 3).

13. This approach has well-known drawbacks (see e.g. Angrist and Krueger, 1999), but we see no obvious superior alternatives.

14. Note that the standard errors become biased downward with this procedure (cf. Murphy and Topel, 1985). We have not corrected for this bias in the table, however, since the t-value of the job shift coefficient is sufficiently large (4.2) to make the effect of mobility very unlikely to become non-significant (at conventional levels) even with correction of its standard error. Given that the coefficients are significant, our main interest is to compare point estimates across models.

15. When we include the individual fixed-effects indicator, the coefficient for average wage growth drops from 0.84 to −0.17. This is due to a complex relationship between earnings growth, the starting value of earnings (at age 26), average wage growth, and the fixed-effects indicator. If the starting value of earnings is removed from the model, the effect of average wage growth is strong and positive, as in our other models, while the mobility coefficient is unaffected.

16. A Hausman test of the difference between the OLS and IV estimates shows that they are not significantly different at the 5%-level (see Kennedy, 1992: 148).

Appendix: Methodological Approach

Long-term Effects of Mobility

All of the dependent and independent variables, except mobility, are measured as growth:

$$\Delta X_i = \left[\left(\sum_{27}^{35} X_{it}/9\right) - X_{i26}\right]/X_{i26} \quad (1)$$

where X_{it} is the variable value of individual i at age t which goes from 26 to 35. (Hence, for example, the dependent variable, earnings growth, is defined as the difference between the average earnings at age 27 to 35 and the earnings at age 26, divided by the earnings at age 26.)

In order to achieve a linear framework, we have transformed the human-capital and positional variables into 'earnings values' as follows. Labour-force experience and education (both measured in years, transformed to deviations from the sample mean) enter a standard human-capital regression with the logarithm of yearly earnings as the dependent variable. This regression is estimated on pooled cross-sectional information consisting of all person-year observations (N=8,122) in our data-set:

$$\log \mathrm{EARN}_{it} = \beta_1 + \beta_2 \mathrm{SCH}_{it} + \beta_3 \mathrm{EXP}_{it} + \beta_4 \mathrm{EXP}_{it}^2 + \alpha Z_c + \varepsilon_{it}, \quad (2)$$

where EARN_{it} is the yearly earnings of individual i at age t, SCH is the years of schooling, EXP is years of labour-force experience, and EXP^2 is the square of experience. Z includes two control variables – the average economy-wide real earnings for the calendar year, c, of each observation and a linear year term. (The rationale for including average economy-wide real earnings and a linear year term is that human capital grows over historical time, as do real earnings, and we want to eliminate the time component of this covariation since it is largely spurious at the individual level.) ε is the error term.

The earnings value of years of experience (EXP*) is then given by the exponent of the predicted earnings for different lengths of experience, given an average (sample mean) education, i.e.,

$$EXP^*_{it} = \exp(\beta_1 + \beta_3 EXP_{it} + \beta_4 EXP_{it}^2 + \beta_2 \overline{SCH}) \quad (3)$$

where $\overline{SCH}$ is the overall mean of years of education. In a converse fashion, the earnings value of education (SCH^*) is obtained as:

$$SCH^*_{it} = \exp(\beta_1 + \beta_2 SCH_{it} + \beta_3 \overline{EXP}_{it} + \beta_4 \overline{EXP^2}) \quad (4)$$

The earnings value of tenure is constructed on the basis of an earnings regression where tenure and its square are included in addition to the earnings values of education and experience.

Occupational standing ($\mathrm{OCC_n}$) is measured as the earnings value of the occupation in these analyses. The occupations are based on a detailed classification schema. The schema is a cross-classification of three-digit ISCO (NYK in Sweden) with five categories of 'social class' (SEI in Sweden; three skill levels of white-collar occupations and two skill levels of blue-collar occupations). This cross-classification contains 281 cells and 8,122 person-year observations. Each occupation in this schema is given an earnings value by dividing the raw earnings mean in the data for that category ($\overline{\mathrm{EARN}}_n$) by the predicted mean ($\overline{\mathrm{EARN}}^*_n$) from a regression with education, experience (including a square term), average economy-wide real earnings for the year of each observation, and a linear year term. This ratio is then multiplied by the overall earnings mean in the data ($\overline{\overline{\mathrm{EARN}}}$). The resulting measure thus indicates the expected earnings of an individual in a given occupation, net of occupational variation in human capital of the incumbents and net of variations over historical time in occupational distributions.

$$\mathrm{OCC_n} = \frac{\overline{\mathrm{EARN}}_n}{\overline{\mathrm{EARN}}^*_n} \times \overline{\overline{\mathrm{EARN}}} \quad (5)$$

The measure of occupational standing is also used as the basis for distinguishing between voluntary and involuntary job shifts. Involuntary moves are defined as job shifts that involve a loss in occupational standing, while all other moves are defined as voluntary.

Together with the economy-wide real wage growth variable, education, experience, tenure, and occupational standing (all measured as growth rates as explained above) are used as predictors in a regression of individual earnings growth; see Table 3. (Regression to the earnings mean is taken into account by including raw earnings at age 26 among the predictors.) With all these held constant, the direct (pure) earnings effect of job mobility is given by the earnings growth associated with the number of internal and external (to the establishment) job shifts. The total (direct and indirect) effect of

mobility is obtained by the coefficients of internal and external mobility in a model where only the three human-capital variables are controlled for. Hence, the indirect effect of job mobility, i.e. the impact of mobility via occupational change, is the difference between the mobility coefficients in the latter and the former models.

Fixed-effects Models

For the alternative analyses, the specification of the fixed-effects models is based on the following equation:

$$\mathrm{EARN_{it}} = \beta_1 + \beta_{2j}\mathrm{X_{itj}} + \beta_{3k}\mathrm{MOBILITY_{itk}} + \upsilon_i + \varepsilon_{it}, \quad (6)$$

where X_{itj} is the value of control variable X_j for individual i at age t and $\mathrm{MOBILITY}_k$ is the set of five dummy variables for job shifts as explained above. In this model, the residual is divided into two components – υ_i which is the individual-specific residual assumed to be constant over time, and ε_{it} which is the remaining residual of noise assumed to be random across time and individuals. The fixed-effects models are then estimated by transforming all variables to $(X_{it} - \overline{X}_i)$, i.e. to deviations from the individual mean for all time periods, which implies that υ_1 disappears from the equation since it is constant over time.

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