Skill mismatch and wage growth

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Abstract

We examine the impact of skill mismatch on wages in Sweden in the context of static and dynamic versions of the ORU model. The empirical analyses, based on cross-sectional and panel data from the Swedish Level of living surveys 1974-2000, are guided by two main hypotheses: (a) that skill mismatch reflects human capital compensation rather than real mismatch, and (b) that skill mismatch is real but dissolves with time spent in the labor market so that its impact on wages tends toward zero over a typical worker's career. Our findings give mixed support to these two hypotheses. First, while there are some indications that overeducated (undereducated) workers are less (more) able than correctly matched workers, significant differences in contemporaneous economic returns to education across match categories remain even after variations in ability are taken into account. Second, there is some evidence that rates of wage growth are not lower for mismatched workers than for others, but we find no evidence that their growth rate is higher. Our main conclusion is that the overeducated are (on average) penalized early on by an inferior rate of return to schooling from which they do not recover.

Introduction

Reward attainment in the labor market is dependent on how well worker skills are matched with job requirements. The economic payoff to worker skills (such as education) depends positively on the skill requirements of the job (Duncan and Hoffman 1981, Hartog 2000). At an aggregate level, changes in the distribution of wages are commonly believed to be closely tied to changes in the balance between skill demand and skill supply (Katz and Murphy 1992, Acemoglu 2002).

It is well-known that the average level of education has risen significantly in advanced industrial nations in recent decades (Barro and Lee 2001, Bassanini and Scarpetta 2001). There is also a wide-spread but contested view that the average skill level among jobs has risen even faster, due mainly to two developments: technological change (Acemoglu 2002) and globalization (Wood 1994, Feenstra and Hanson 2001). The supposed excess demand for skills is widely used as an explanation for the observed increase in wage dispersion in several (but not all) countries (Acemoglu 2003). The relationship between skill and wage movements is assumed to be especially tight in labor markets with relatively uncoordinated (”flexible”) systems of wage determination (Freeman 1994, Krugman 1994).

On the basis of previous research from several OECD countries (see the overview in Tåhlin 2004) we may conclude (a) that the skill requirements of jobs have increased significantly in recent decades (certainly in Sweden, the UK and the US, and most probably in other OECD countries), and (b) that the supply of skills as measured by the educational attainment of workers has also increased significantly (with the exception of the US in recent years).

An important issue is how these two trends are related to each other. In the literature, there are two main strands. The first is the overeducation perspective. The sense that educational expansion was outstripping the demand for skills in the labor market dates back at least as far as the late 1940s (see Harris 1949). In the wake of the rapid growth of student enrollment at colleges and universities this impression became a wide-spread view in the 1970s (Berg 1970, Freeman 1976, Collins 1979), and began to be documented empirically. A literature on overeducation and earnings started with a paper by Duncan and Hoffman (1981) and has since become substantial. There is by now a large body of international evidence on the incidence and wage effects of over-schooling (see further below). Most recently, Rubb (2003a) provides a meta-analysis of many empirical studies of the effects of skill mismatch on earnings.

The second major strand in the literature on skill matching is the upgrading view, i.e., that skill demand is increasing at a higher rate than skill supply (education). The starting point of this perspective was the growth in wage inequality, in particular across skill or education categories, in the United States and Britain in the 1980s. In a standard supply-and-demand model, the joint occurrence of rising returns to education and an increase in skill supply can only be explained by an even faster growth in skill demand. The main rationale behind such a growth in demand is skill-biased technological change (SBTC), i.e., changes in production processes and work organization favoring employment of high-skill workers. The rapid expansion of information technology is seen as the prime feature of this development. In addition to SBTC, globalization (in particular increased international trade, of which especially across the north-south divide) is viewed as a cause of skill bias in the evolution of labor demand in advanced countries.[1]

The currently dominant view within the upgrading strand is that technological change is the clearly more important of these driving forces. Acemoglu (2002) gives an extensive overview of the technology-inequality literature, while Autor et al. (2003) provide a large-scale and long-term analysis of changes in the US industrial and occupational structure, documenting a link between the expansion of information technology and a rise in skill demand. Gallie et al. (2002) draw similar conclusions based on British survey data.[2] Important dissenting accounts, where the link between computerization and growing wage inequality is questioned on a number of empirical grounds (in the US), are Bernstein and Mishel (2001) and Card and DiNardo (2002).

A fair amount of overeducation appears to exist in Western labor markets, both in the United States and in Europe. Between 20 and 50 percent of all workers (depending in part, of course, on the definition used) seem to have more schooling than their job requires, with the American rate tending to be higher than the European. Trends in overeducation are poorly

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[1] See Feenstra and Hanson 2001 for an overview of the trade-inequality literature and Alderson and Nielsen 2002 for a general review of explanations of trends in income inequality.

[2] See also Fernandez (2001), a recent case study establishing a connection between within-firm technological change and an increase in internal wage dispersion.

established. What we know is mainly confined to the US and a rather small number of countries in Europe, including Britain, the Netherlands, Portugal, Spain and Sweden. These studies indicate that overeducation has increased in Europe in recent decades, while it appears to have decreased in the United States. (On the US case, Gottschalk and Hansen 2003 is the most recent analysis.)

To the extent that this preliminary comparative evidence is valid, it is interesting to note that changes in wage inequality are broadly consistent with trends in skill mismatch rates, in that wage inequality has increased more in the US than in Europe while overeducation appears to have increased in Europe but not in the US (Hartog 2000). This empirical pattern might also be part of the explanation of why research on overeducation is active and growing in Europe (see, e.g., Büchel at al. 2003) but is currently not a major field in American education and labor studies. It may also be noted that the moderate increase in wage inequality in several European countries (or even the stronger increase in the US) is not necessarily incompatible with a stronger growth in the level of skill supply than in the level of skill demand (see Hartog 2000). In addition to changes in levels, changes in distributions should be taken into account, and at least for some countries in Europe the distribution of skill demand has grown faster (or fallen slower, e.g. in Sweden) than the distribution of supply.

In Sweden, the rate of overeducation has increased at a rapid pace during recent decades (le Grand, Szulkin and Tåhlin 2001, 2004; Åberg 2003). In 2000, close to half (48 percent) of all employees had an education at least one year longer than required in the job they held. About one third (35 percent) had an amount of schooling at least two years in excess of their job requirements. These rates have more than doubled since the 1970s, while the proportion correctly matched with respect to education has fallen considerably. Undereducation rates, by contrast, have been roughly stable during most of the period.

To what extent does the growth in skill mismatch pose a problem for society and for the workers concerned? A large number of studies from several countries shows that, in the short run as revealed by cross-sectional information, overeducated workers have significantly lower wages and lower job satisfaction compared to similarly educated individuals with a better job match (see, e.g., Büchel et al. 2003). But overeducated workers also tend to be significantly younger than others (see, e.g., Sicherman 1991). This indicates that many of them will improve their match over time, so that the problems associated with excess schooling may be merely temporary. Indeed, in recent policy discussions of ’life-long learning’ and flexibility (see, e.g., OECD 2004), overeducation may be seen as an asset of growing importance in adjusting to changing labor market conditions.

The purpose of this paper is to assess the degree to which skill mismatch, in particular overeducation, has a negative impact on wages in the Swedish labor market. We start from the general finding in the international literature that the economic returns to education are significantly larger for years of schooling up to the required level of the job than for education above that level. Two main hypotheses as to why this difference appears are then considered. The first hypothesis says that the degree of skill mismatch is overestimated due to typically unobserved heterogeneity across individuals, such that apparently overeducated workers are in reality less able than others (while apparently undereducated workers are in reality more able than others). When ability differences are taken into account, wage returns to education are hypothesized to be independent of the skill requirements of the job. The second hypothesis says that skill mismatch is at least in part real, but that it affects wages only in the short run. Over time, mismatch is expected to dissolve through career processes and job mobility such that higher rates of wage growth compensate for initial wage losses.

We examine these hypotheses empirically on the basis of cross-sectional and panel data from four waves of the Swedish Level of living surveys, 1974 to 2000. Our findings give mixed support to the two hypotheses. First, while there are some indications that overeducated (undereducated) workers are less (more) able than correctly matched workers, significant differences in contemporaneous economic returns to education across match categories remain even after such variations in ability are taken into account. Second, there is some evidence that rates of wage growth are not lower for mismatched workers than for others, but we find no evidence that their growth rate is higher. Our main conclusion is thus that the overeducated are (on average) penalized early on by an inferior rate of return to schooling from which they (on average) do not recover.

The remainder of the paper is organized as follows. Next section contains information on the data and variables that we use in the empirical analysis. We then give an overview of previous research on the impact of skill mismatch on wages and discuss some implications and limitations of past findings. As we turn to an empirical investigation of the two main hypotheses we begin by providing some descriptive evidence before going on to more explanatory analyses. In the concluding section we offer some reflections on our findings.

Data and variables

The data come from the Swedish Level of living surveys (LNU) from 1974, 1981, 1991, and 2000. At each occasion, a national probability sample of about 6,000 adults (15-75 years 1974 and 1981, 18-75 years 1991 and 2000) residing in Sweden were interviewed (by personal visits) about their living conditions along several dimensions, such as education, working conditions, health, housing, and family life. The non-response rate was 14.8 percent in 1974, increasing to 23.4 percent in 2000. The samples have a panel structure, such that all individuals in the sample at t1 (1974-1991) are included in the sample at t2 (1981-2000) if still within the targeted age range and residing in Sweden. New members of the sample are drawn at each time-point, entering either through age or immigration. In the analyses in this paper, we use data on 6,426 individuals who were employed respondents aged 19-65 on at least one of the four interview occasions: 3,112 in 1974, 3,285 in 1981, 3,326 in 1991, and 3,060 in 2000. 2,622 of these respondents have participated (and been employed) once, 1,944 twice, 1,167 three times, and 693 individuals have responded (and been employed) at all four occasions.

Table 1 shows descriptive statistics for all variables used in the empirical analyses below. ED_t (t=1974-2000) is the respondent's attained number of years of full-time education beyond compulsory school. RE_t is the required ED in the worker's current (t) job, according to the respondent's own assessment. RE is thus a crucial variable, on which all of the empirical results depend. The variable is based on two interview questions, phrased:
(a) ”Is any schooling or vocational training above elementary schooling necessary for your job?”.[3] (Yes – No.) (b) ”About how many years of education above elementary school are necessary?”[4] (Number of years, ungrouped.) Respondents answering ’No’ to (a), or ’Yes’ to (a) but less than ’1’ to (b), are assigned RE=0, while respondents answering ’Yes’ to (a) and at least ’1’ to (b) are assigned RE=x, where x is the response to (b). This information is of high quality, as indicated by both reliability and validity tests. First, in LNU 1991 re-

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[3] ”Behöver man någon skol- eller yrkesutbildning utöver folk- eller grundskola i din befattning?”

[4] ”Ungefär hur många års utbildning utöver folk- eller grundskola behöver man?”

interviews were made with a random subsample of respondents. The outcome of the double interviews showed that indicator (a) had a Cohen's kappa of 0.82 (N=133), while indicator (b) had a Pearson's r of 0.88 (N=76), not much less than ED (r=0.95); see Bygren (1995). Second, RE correlates highly with external judgments of the respondents' occupation (r=0.70 with the SEI code of Statistics Sweden, indicating typical educational requirements by occupation as listed by the Swedish public employment exchange). Third, RE is a very strong predictor of wage rates (r=0.51 with ln(wage/hour) in LNU 1991), significantly stronger than the corresponding value for ED (r=0.33), and even as strong by itself as a full Mincer model (ED plus experience and its square; R=0.51).

GO ON ….

Dynamic reward effects of skill mismatch

The reasoning behind hypotheses on dynamic effects of skill mismatch is rooted in the theoretical literature on career mobility (Tuma 1976, Sörensen 1977, Sicherman and Galor 1990). A large amount of individual resources (human capital) relative to the requirements of the currently held position is seen there as an unstable situation highly conducive to future positional improvement. Sicherman (1991) explicitly links the career model to overeducation, and finds empirical support for significant catch up over time among the initially overschooled in relation to better matched workers. Some subsequent longitudinal studies have reached the same conclusion (e.g., Robst 1995, Hersch 1995).

As shown by Büchel and Mertens (2004), however, the findings from these longitudinal studies (all on US data) are not convincing, due to specification problems of the empirical models. In essence, the difficulty lies in distinguishing between the impact of mismatch and the effects of other attributes of the starting position on future reward attainment. In models of occupational mobility, which are the focus of Sicherman (1991) and several followers, one should control for the reward level of the current job when examining reward changes, which these studies did not. This omission leads to mismatch effects being confounded with the impact of vertical scale limits (floor and ceiling) and of regression to the mean. Specifically, Sicherman (1991, table 3) controls for schooling when estimating the effect of overeducation. This means that the overeducation indicator will simply reflect a low job level rather than mismatch, and it is well-established from a large amount of previous research (see the overview in Rosenfeld 1992) that the starting job level is inversely related to the direction of subsequent job shifts for purely technical reasons.

Büchel and Mertens (2004) first replicate the US findings on German data, and then show how the positive impact of overeducation on upward occupational mobility disappears when starting occupation is controlled. They then proceed to examining wage growth as a further test of the career mobility argument. We agree that wage growth is a better measure of reward change than occupational mobility.[5] However, we are not convinced that the empirical wage growth model used by Büchel and Mertens is properly specified. They conclude that overeducated workers have a significantly lower wage growth than correctly matched employees, who in turn have a lower growth rate than undereducated workers. Surprisingly enough, given the authors' arguments, this result seems driven by insufficient attention to starting position attributes. The problem is, once again, that schooling is included in the empirical model, so that the overeducation indicator (just like in Sicherman 1991) reflects low occupational rank rather than mismatch. And although workers in low occupational positions have relatively high rates of upward job mobility (which makes Sicherman's result hard to interpret and potentially spurious), their average wage growth rates are typically weaker than others (see, e.g., le Grand and Tåhlin 2002) which makes Büchel and Mertens' finding questionable.

The empirical implementation of the ORU model

To move forward on this issue, we apply the standard model (ORU) for cross-sectional estimation of wage effects of skill mismatch to the dynamic case of wage growth. In an influential article, Duncan and Hoffman (1981) decompose attained education (in years) into three parts defined in relation to the educational requirements of the job held. This decomposition is expressed by the equation

[5] It is sometimes claimed that occupation, for instance measured by status (or prestige) scales, is a more comprehensive indicator of job rewards than earnings are, and is therefore a superior measure of attainment. A common argument for this claim is that earnings to some extent compensate for negative non-pecuniary job traits. While such compensation is significant in some cases (see, e.g., Duncan and Holmlund 1983, Tåhlin 1991), the main tendency is quite the opposite: pecuniary and non-pecuniary job rewards correlate positively (and strongly) with each other. The most comprehensive (as far as we know) study available (Jencks, Perman, and Rainwater 1988) reports (on US data) a correlation of .50 between earnings and a summary measure of non-pecuniary job rewards and a .73 correlation between earnings and overall job "desirability" (as subjectively assessed by survey respondents). In fact, according to the same study, earnings explain about twice as much of the variance in overall job desirability as occupational status does.

$$AE = RE + OE - UE, \quad (1)$$

where AE denotes attained education, RE is the required amount of education in the job that the worker holds, OE is the amount of education attained by the worker that is in excess of what the current job requires, and UE is the amount of education required by the job that is in excess of what the worker has attained. Hence, OE is zero for correctly matched and undereducated workers, while UE is zero for correctly matched and overeducated workers. The equation thus reduces to AE = RE for the correctly matched, to AE = RE + OE for the overeducated, and to AE = RE – UE for the undereducated.

There are two attractive traits of this decomposition. First, conceptually, it combines the information on attained and required education while fully retaining the continuous character of both dimensions. This allows an assessment of separate payoffs to years of attained education dependent on the nature of the job match as revealed by earnings (or other rewards) regressions. Second, empirically, the main pattern of results from this model has turned out to be remarkably robust across both time and countries.

In these analyses, the three types of education defined in the DH formulation have been introduced into a standard Mincer wage equation producing the ORU wage equation

$$W_{ti} = \beta_1 RE_{ti} + \beta_2 OE_{ti} + \beta_3 UE_{ti} + \gamma X_{ti} + \varepsilon_{1i} \quad (2)$$

Here X is a vector of independent variables including a constant, γ is a corresponding vector of coefficients, and ε_1 a standard error term. For correctly matched workers, β_1 indicates the total schooling return. For mismatched workers, the effects β_2 and β_3 are to be interpreted in conjunction with β_1 to arrive at estimates of the total impact of their education. The total return to schooling among overeducated workers is thus β_1 for the years of schooling corresponding to the job requirements together β_2 for the additional years. Among undereducated workers the total return to schooling is given by β_1 again indicating the return to the years of schooling corresponding to the job requirements but less β_3 for the missing years of schooling.

The following results from cross-sectional wage regressions have been found in virtually all published studies, regardless of time and place (see Rubb 2003a for a recent overview):

(a) the wage effects of both RE and OE are positive while the wage effect of UE is negative, and (b) the impact of RE exceeds the impact of OE and UE. In terms of (2) β_1 and $\beta_2 > 0$ while $\beta_3 < 0$ and $|\beta_3| < \beta_1 > \beta_2$. Put differently, (a) overeducated workers earn more than correctly matched workers in the same kind of jobs, but less than correctly matched workers with a similar amount of education; while (b) the converse pattern holds for undereducated workers: they earn less than correctly matched workers in the same kind of jobs, but more than correctly matched workers with a similar amount of education.

These consistent results of a large number of studies are interesting in several ways. To begin with, they clearly contradict structural accounts of reward attainment in the labor market as well as standard human capital models. Specifically, (a) is incompatible with the former approach while (b) is incompatible with the latter. In structural research, rewards are exclusively determined by job traits, so the impact of RE should be significant while the effects of OE and UE should both tend toward zero. In a standard human capital framework, by contrast, rewards are exclusively determined by traits of individual workers, so the impact of RE should not differ significantly from the effects of either OE or UE. Apparently, there is something to be said for both perspectives, but neither account seems to tell the whole story, at least not without additional specification.

Further, the consistent pattern of empirical findings suggests several hypotheses on the nature and causes of skill mismatch. The most straightforward interpretation in a standard human capital frame of mind would be to emphasize that formal schooling is an incomplete measure of individual productive capacity. Apparently overeducated workers might in reality use their ”surplus” schooling to compensate for deficient human capital in other respects, while the converse might be true of seemingly undereducated workers. Therefore, the ”overeducated” are less productive than others with the same amount of education and so receive a smaller payoff to each year of schooling than the correctly matched workers do, while the ”undereducated” are more productive than others with similar schooling and are therefore penalized less than the standard amount for each ”deficit” year of schooling.

Along the same lines, but from the viewpoint of jobs rather than individual workers, educational requirements are obviously not a perfect measure of the skill level of jobs. Hence, the true skill level may be underestimated for ”overeducated” workers and overestimated for ”undereducated” workers, so the mismatch is again overestimated which attenuates the OE and UE coefficients.[6]

We now turn to an empirical examination of these issues. We begin by considering the static case of contemporaneous wages, and in this connection we attempt to evaluate the hypothesis of human capital compensation. In a second step, we proceed to the dynamic case of wage growth, in the context of which we try to assess the career mobility hypothesis. In both cases, we start out by providing some descriptive information, and then go on to more explicit tests of the different explanatory perspectives that we seek to evaluate.

Human capital compensation: descriptive results

In general, we use the ORU model to examine the empirical association between skill mismatch and other human capital related dimensions. For ability, we use three indicators of which the first two are standard items: labor market experience, indicating general skills, and tenure (time spent with current employer), indicating firm-specific skills. The third indicator is a measure of verbal ability based on two indicators: (a) self-rated verbal ability (capacity to write a complaint)[7] and (b) self-reported amount of books at home[8]. This composite measure correlates (in LNU 1991) .33 with the log of hourly wages, .35 with years of education, and has a highly significant positive effect if added to a standard Mincer regression. (All variable constructions are listed in the Data and variables section above. NB: still incomplete in this version.)

Table 2 shows the model results, all based on cross-sectional data from LNU 1991. We see, firstly, that undereducation is positively related to experience, while overeducation is negatively related to this measure of general skills (model 1). For each year of ”deficit” schooling, experience increases by about three years. Conversely, for each year of ”surplus” schooling, experience decreases by on average 1.7 years. Compared to the association between matched years of education and experience, estimated as minus 0.6 years, the undereducated have 3.6 more years of experience per deficit year of schooling, while the

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[6] We intend to test both of these hypotheses in a companion paper based on data from the International Adult Literacy Survey (IALS), a data set containing unusually detailed information on both individual capacity and job demands.

[7] ”Could you take it upon yourself to write a letter appealing against a decision made by a public authority?” (yes/no).

[8] ”Do you have at least 5 running metres of books [at home], not counting works of reference?” (yes/no).

overeducated have about one year of deficit experience per surplus year of schooling. These associations accord well with previous empirical findings from other countries and time-points (e.g., Sicherman 1991 on US data from the PSID), although they have not previously been estimated on the basis of the ORU specification.

The results on firm-specific skills, indicated by years of tenure, are slightly different. In this case as well, there is some evidence that apparent skill mismatch works as human capital compensation: in model 2, undereducation is associated with relatively long tenure and overeducation with comparatively short tenure. There is no relation, however, between tenure length and matched years of education. But obviously tenure is a subset of total experience, and in order to isolate the specific-skill part it is useful to compare tenure levels at similar levels of experience. When experience is held constant (model 3), only matched years of education are significantly related to tenure. The association is positive, indicating that the amount of firm-specific skills is larger among the well-educated, but only to the extent that their job requires the achieved level of education. These results thus indicate that educational mismatch has a compensatory function with respect to general but not to firm-specific skills.

Finally, we examine the link between skill mismatch and verbal ability in model 4. The pattern of results resemble the standard wage results (as described above). There is thus a strong positive association between education and ability up to the schooling level required by the job, and somewhat less strong but still significantly positive above that level, while workers with deficit schooling are significantly less able than correctly matched workers at the same job level but by a smaller amount than expected from their education alone. According to this pattern of coefficients, then, the overeducated workers are more able than correctly matched workers at similar job levels (and with similar amounts of experience), but by a slightly smaller amount than their achieved level of schooling indicates. An illuminating way of using the ability measure to assess the human capital compensation hypothesis would be to examine whether the wage payoffs to UE, RE and OE tend to converge when including ability in the model. This test will be carried out in the next section. [NB: Not included in this version.]

Controlling for unobserved heterogeneity by panel data

When estimating eq. (2) above, unobserved productivity differences become part of the error term ε_1. Decomposing the error term ε_1, we can write

$$W_{ti} = \beta_1 RE_{ti} + \beta_2 OE_{ti} + \beta_3 UE_{ti} + \gamma X_{ti} + (\rho_{ti} + \varepsilon_{2ti}) \quad (3)$$

with ρ being an indicator of productivity. With a negative correlation between ρ and OE and a positive between ρ and UE the estimates of the educational effects produced by the OLS analyses of the ORU specification (2) would be biased, with the absolute magnitude of both β_2 and β_3 being underestimated.

If ρ is a time invariant person specific factor (i.e. $\rho_{ti} = \rho_{t+1i}$) unbiased estimates could be obtained through the estimation of a standard fixed effects model. One way of specifying the fixed effects model is the first difference model, where

$$\begin{aligned} W_{t+1}\text{-}W_t = {} & \beta_1(RE_{t+1}\text{-}RE_t) + \beta_2(OE_{t+1}\text{-}OE_t) + \beta_3(UE_{t+1}\text{-}UE_t) \\ & + \gamma(X_{t+1}\text{-}X_t) + (\rho\text{-}\rho) + (\varepsilon_{2t+1}\text{-}\varepsilon_{2t}) \end{aligned} \quad (4)$$

and the individual index i has been dropped to simplify the notation. The effect of the time invariant factor ρ here cancels out, and the resulting estimates are unbiased.[9]

Estimates of the OLS and fixed effects versions of the standard ORU model are presented in Table 3. The OLS model has here been estimated with robust standard errors to take account of the fact that we have multiple observations per respondent, and the fixed effects model is the deviations-from-mean model rather than the first difference model of (4). These Swedish results replicate the results found in other countries. In the OLS version the effect of RE is positive, the effect of OE is also positive although smaller than the effect of RE, while the

[9] Bauer (2002) estimates such a model using two different measures of overeducation, and finds that the absolute values of b_2 and b_3 either decrease or increase depending on which measure is used. Irrespective of which result is regarded as the most valid, the results suggest that wage effects of mismatch in the DH model (to some extent) are spurious. How plausible is the assumption that the productivity factors are time invariant, and what alternative assumptions could be made? Bauer tests the fixed affects specification against a random effects version, rejecting the latter. However, the random effects model is based on an assumption of no correlation between P and the mismatch variables. This seems unlikely, and is of course precisely what the unobserved ability model does not assume. Instead the fixed effects assumption would appear rather reasonable. Other qualms about Bauer's analyses include the measures of required education used (and implicitly on the OE and UE measures as well), which focus on the educational level of employees rather than on the educational requirements of the jobs.

effect of UE is negative and smaller in absolute size than the effect of RE. Both OE and UE are furthermore significantly different from RE.

The same relations also hold for the fixed effects specification, although the absolute values of all the estimates decrease. That the point estimates from the between-person comparison in the OLS model are greater than the estimates from the within-person comparison in the fixed effects model may suggest that unobserved time-invariant factors, personal and/or others, are part of the explanation for the differences in the rate of return to the three types of skill match. An alternative interpretation when comparing the estimates such as these is that the changes over time examined in the fixed effects model may involve such a slow process that the time span between interviews may not be enough to capture the full effect of changes in mismatch (Petersen ????). Measurement error, which might be larger in the panel data case than in the cross-section, could also be part of the picture.

However, there are some more subtle differences between the two models that also may explain the differences in the estimates. While the FE model has the desirable property of controlling for unobserved fixed effects, the focus on changes over time in the independent variables is not unproblematic. Recall that the attractiveness of the ORU model was based in the decomposition of attained education, AE = RE + OE – UE, and the interpretation of the schooling estimates in relation to this decomposition. The move to the FE model involves a shift from between-person comparison to within-persons comparison among individuals who have changed educational level or job. Take the case of a change in RE. This would entail a change in the required qualifications together with a change in educational level of the equal magnitude. The typical case would probably be further education matched by a promotion. A change in OE would instead imply an increased educational level without a corresponding increase in required qualifications, i.e. further education without a subsequent promotion. Variation in UE would instead be a promotion without any corresponding addition to educational qualifications.

The differences in the RE estimates could thus also be interpreted such that the promotional payoff to further education in the FE model is less than the return to schooling in terms of starting level evident in the OLS model. The negligible OE estimate in the FE-specification in turn indicates that the return to further education is almost nil if one fails to land a promotion.

The reduced UE estimate finally suggests that the UE-deduction is less among those who have proven their mettle on the job.

The relatively small reduction of the UE estimate is in this context nonetheless somewhat surprising. The undereducated workers are often thought of as employees who lack the formal qualifications but who still are highly productive along some unobserved dimension. This unobserved productivity is believed to have allowed them to make a career despite their insufficient qualifications. If this is the case, it would seem reasonable to expect these employees who have demonstrated their ability to receive the same payoff to a promotion as the correctly matched employees (in which case we would have $\beta_3 = 0$). Yet they still get a lower bonus, so formal qualifications thus still seem to matter somehow.

These considerations also point to some other drawbacks related to the FE model. First, if education level remains unchanged changes in the three match variables involve vertical job mobility. This would produce a change in RE and therefore also in OE or in UE. While vertical mobility such as promotions is one aspect of the attainment process, the specification fails to model wage growth occurring without an occupational shift. Second, in the remaining cases changes in the match variables involve changes in educational level, i.e. further education among adults. Here it can be argued that this is a qualitatively different variable than the cross-sectional measure of educational level, since the latter will tend to focus on youth education. There are two issues involved: the type and the timing of education. Whereas youth education tends to be more general in nature, adult education tends to be vocationally oriented. The years of schooling included in the analyses would thus measure two different types of education, with the impact of one type not necessarily relevant for conclusions regarding the impact of the other. Similarly, the timing of education may be important if age affects the rate of return, for instance through an effect on motivation. Another aspect in relation to the educational measures is the problem of measurement error. Part of the variation in educational attainment between panels stems from respondents who (implicitly) report that their level of education has decreased between interviews, illustrating that the combination of two faulty measures may be less desirable (although see Allison 1990).

In sum, the differences in the results may be related to the elimination of unobserved fixed factors in the FE model, but they may just as well be due to other differences between the models. Whether or not unobserved factors are part of the story, the fact that the larger pattern of results ($|\beta_3| < \beta_1 > \beta_2$) remains the same indicates that unobserved heterogeneity in any case is not the whole story. What then is the remaining story?

In addition to unobserved ability, a second explanation for the pattern of findings from the standard ORU models such as the ones examined in Table 3 is specifically related to the difference in size between the returns to required education and to overeducation. Sicherman and Galor (1990) and Sicherman (1991) argue that overeducation may be seen in a career perspective, as part of a human capital investment strategy. The occupancy of a job for which one is overeducated could be part of a long-term strategy if the currently reduced rate of return is compensated in the future. Based on the career mobility perspective, overeducated workers might be seen as being in an early phase of an upwardly oriented job career. Therefore, the payoff to their attained education is underestimated by only considering current rewards. That is, overeducation should be associated with greater than average wage growth, and the gap in returns to schooling relative to correctly matched workers will over time decrease and eventually go to zero.

If this is the case we should examine wage growth, for example in the form of

$$W_{t+1}-W_t = \beta_1 RE_t + \beta_2 OE_t + \beta_3 UE_t + \gamma X_t + \varepsilon_{3t} \qquad (5)$$

and expect to find $\beta_2 > \beta_1$. Note that this specification differs from the first difference specification discussed above. Although the left hand side is identical, the right hand side is different in that we would not be examining the effect of changes in OE, RE, and UE.[10] In other words, this model would focus the impact of being (mis-) matched on all forms of wage growth, both in connection to promotions and otherwise. If overeducation is regarded as an investment strategy this should be a more relevant model as it takes all forms of investment return into consideration.

A third explanation discussed by Sicherman (1991) is that the mismatch is temporary, not in the planned career sense discussed above but rather as a result of job search with imperfect

[10] However, rather than testing this hypothesis directly, Sicherman (1991) examines promotion probabilities and the implied negative relationship between the degree of overeducation and promotion. This would seem to be at best a partial test, since promotions are only one (albeit important) aspect of wage growth. Wages may grow even without a job shift, and promotions may come about for other reasons. (In addition, there are other problems with Sicherman's empirical analysis, as discussed above.)

information. If the employee is aware that the current job match is less than perfect this would imply further search and subsequent job mobility conditional on better offers being located. In this scenario we would also expect that estimates based on (5) would yield $\beta_2 > \beta_1$.

These explanations of mismatch predict that overeducated workers should experience greater than average wage growth. In contrast, Büchel and Mertens (2004) argue that overeducation should be seen as an indicator of underachievement: the overeducated are not able to land a job at their supposed skill level. There is thus no career investment nor is there any mismatch. Rather than expecting the "proven" underachievers to suddenly become over-achievers, they should be expected to remain underachievers. This is another version of the unobserved ability argument above, but now applied to wage growth. The expectations regarding the parameters in the model would in this case be the reverse of the above, i.e. we should obtain $\beta_2 < \beta_1$.

Similar predictions would be made based on signaling models as well as on social-psychological models focusing on individual reactions to negative life events. In the former case, potential employers may regard obvious overeducation with disapproval taking it as a signal of potentially low productivity. This could be regarded as a version of the underachievement argument, with the difference that the focus here is on potential rather than actual productivity. The latter hypothesis instead focuses on the reactions of the individual employee, with a drop in self-esteem as a mechanism.

The implication of all these arguments is that rather than the standard application of the ORU model, either in its most frequent OLS form of eq. (2) or in the fixed effects shape of eq. (4), we really should be examining wage growth, i.e. eq. (5). The standard ORU model would simply seem ill suited for testing career predictions. Before turning to the wage growth models, we provide some descriptive evidence on the career mobility perspective in the next section.

Career mobility: descriptive results on the training content of jobs

The human capital compensation explanation of the ORU earnings regression results is based on (usually unobserved) *individual* heterogeneity: that the differences in ability (true productive capacity) between skill match categories of workers are smaller than the education based indicators imply. We saw in a previous section that there is something to this explanation, especially as concerns the (apparently) undereducated. The career mobility explanation, by contrast, is based on (usually unobserved) *job* heterogeneity: the amount of on-the-job training provided by employers is hypothesized to be larger in jobs held by (apparently) overeducated workers than measures based on schooling requirements imply. This training adds significantly to the human capital of the job incumbent, and the sojourn in a seemingly low-skill job is therefore an investment that the worker undertakes in order to enhance her future career.

The LNU surveys contain several measures of on-the-job training that are useful to illuminate this issue empirically. First, there is a standard indicator of formal employer provided training, measured as the (self-reported) number of days (full-time equivalent) that the worker spent in formal training (education provided by or paid for by the employer) during the last (prior to the interview) twelve months. Second, there is an indicator of informal on-the-job training, measured as the (self-reported) time required from entry into the current job until the worker has learnt to carry out the job tasks ”reasonably well”. Third, there is a standard job quality indicator, measuring the (self-reported) extent to which the worker's current job requires that she keeps learning new things. Fourth, there is an explicit prospective career mobility indicator, measuring the (self-reported) extent to which the worker's current job contains prospects of advancement, either within the current firm, outside that firm, or both.[11]

Table 4 shows the results of ORU regression models with the on-the-job training indicators as outcomes. All regressions are based on cross-sectional data from LNU 1991, except the prospective career mobility model which uses data from LNU 2000 (the advancement indicator was not available until that survey). According to all models in the table, the career mobility explanation is unsupported. The entire association between schooling and on-the-job training operates via the skill level of the job as indicated by its educational requirements. Net of these requirements, there is not any significant linkage between the worker's attained level of schooling and the amount of training she receives on the job. In all the four cases considered, the number of matched years of education is positively and strongly (significant by a good margin) related to training opportunities, while the association between training and

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[11] While all of these indicators are based on self-reports, and therefore subject to the various kinds of biases tied to such measures, this should be of less concern here: the career mobility explanation would appear to be based precisely on job characteristics as subjectively assessed by the workers themselves.

mismatched education (either deficit or surplus) is never significantly different from zero. The conclusion of these simple cross-sectional descriptions is thus that jobs do not seem to be heterogeneous with respect to training content in the way that the career mobility hypothesis supposes. All the skill variation in on-the-job training opportunities appears to occur between jobs, with no net variation at all between schooling categories.

The final model in table 4 estimates the relation between skill mismatch and workers' satisfaction with their job. The logic behind this model is the following: the ability explanation of earnings variation across match categories as well as the career mobility explanation state that, due to (usually unobserved) individual and job heterogeneity, respectively, the degree of mismatch is overestimated in simple ORU models. Given that overeducated (unereducated) workers are in reality less (more) able than others, or that the jobs held by overeducated workers are in reality better (have a larger training content) than their schooling requirements indicate, apparently mismatched workers should not be less satisfied with their jobs than other workers. The mismatch explanation, by contrast, states that the mismatch is real (even if temporary). Therefore, job satisfaction should differ significantly between match categories. The overeducated might in this view be expected to be less satisfied than correctly matched workers. In the case of undereducated workers, however, the prediction is less clear. Having attained a job level above one's educational credentials is in one sense a positive achievement, but having less schooling than the position normally requires may also be problematic in several ways. The net outcome of these conflicting mechanisms is an empirical matter.

We find, on the basis of LNU 1991 data, that the mismatch explanation is supported. Education is strongly and positively related to job satisfaction up to the schooling level required by the job, but strongly and *negatively* related to satisfaction beyond that point. This pattern means that both undereducated and overeducated workers are significantly less satisfied than matched workers. So in distinction to the case of on-the-job training (models 1 through 4 in table 4), job satisfaction is not accounted for simply by variation across jobs. Nor is satisfaction explained simply by variation across individuals. Consistent with the mismatch explanation, it is the *interaction* between individual and job characteristics that is crucial. Hence, mismatch to a significant extent seems to be real rather than merely apparent. The degree to which the mismatch is long-term rather than only temporary is, however, another matter. We begin to pursue the dynamic issue in the next section by estimating wage growth models.

ORU models of wage growth

In eq. (5) above, we formulated a dynamic ORU model that expresses wage growth determination. As in the static case, however, we need to take potential unobserved heterogeneity into account and despite the reservations above fixed effects estimation is again a possible solution. Using a first difference model as illustration, we transform (5) into

$$(W_{t+1}-W_t) - (W_t-W_{t-1}) = \beta_1(RE_t-RE_{t-1}) + \beta_2(OE_t-OE_{t-1}) + \beta_3(UE_t-UE_{t-1}) \qquad (6)$$
$$+ \gamma(X_t-X_{t-1}) + (\rho-\rho) + (\varepsilon_{3t+1}- \varepsilon_{3t})$$

Estimates based on the dynamic ORU models described in (5) and (6) are presented in Table 5. According to the OLS model, both matched years of education (RE) and years of surplus education (OE) pay off significantly in wage growth. While the point estimates indicate higher returns to OE than to RE, the difference between them is not significant. Hence, in this specification, the rate of return to overeducation is identical to that of required (and attained) education. This result does not directly contradict the career and search theories, but provides no clear support either since we still do not observe the *greater* wage growth impact of OE than RE implied by these hypotheses. As in the static models, years of deficit schooling (UE) have a negative economic impact. But in distinction to the static case, the magnitude of $|\beta_3|$ is not smaller than that of required (and attained) education. In sum, the OLS estimation of this model of wage growth implies that $|\beta_3| = \beta_1 = \beta_2$, consistent with a conventional human capital perspective. But note that this result *combined with* the finding $|\beta_3| < \beta_1 > \beta_2$ from the contemporaneous wage regression in Table 3 means that the static result holds also in the longer run: the overeducated are (on average) penalized early on by an inferior rate of return to schooling from which they (on average) do not recover. In other words, compared to other workers with the same amount of education, their wage growth curve starts below and then runs parallel to the curve of matched workers.

Turning to the fixed effects estimates of the growth model, we see that they are both markedly different from and quite similar to the OLS results. The difference is that, in the fixed effects case, none of the educational variables affects wage growth significantly, while in the OLS case all of them did. The similarity is then obviously that, in both cases (and in contrast to the static model), $|\beta_3| = \beta_1 = \beta_2$. The fixed effects estimates would seem to suggest that the entire impact of education and job skill level on wage growth observed in the OLS model reflects time-constant unobserved heterogeneity across individuals and jobs. More importantly, just as the dynamic OLS results they would also appear to imply that the initial wage losses of the overeducated are never recovered.

For theoretical reasons, however, the specifications (5) and (6) should be extended. First, the predictions of the career and search theories of overeducation are not that changes in mismatch status should affect wage growth, but that mismatch in itself is associated with different rates of wage growth. This suggests that the dynamic OLS specification (5) is more germane than the fixed effects specification (6) when examining these hypotheses. Secondly, wage careers clearly involve state dependence: the wage rate at t_1 causally affects the wage rate at t_2, primarily due to downward stickiness but also to differential growth rates across starting wage levels. One possibility is for example that the equality of the RE and OE estimates in the OLS model is due to a combination of two different offsetting growth processes. The career and search theories thus postulate greater wage growth among the overeducated. However, the overeducated tend to earn more than the correctly matched employees, see Table 3, and growth rates tend to decrease with staring wage (i.e. a ceiling effect). This would imply a downward pressure on the growth rates of the overeducated, suggesting that the equality just documented may be the outcome of two counteracting processes. In order to reach firmer conclusions, we therefore need to examine the importance of the starting wage level more closely.

Proceeding from eq. (5) and taking the starting level of wages into account yields

$$W_{t+1}-W_t = \beta_1 RE_t + \beta_2 OE_t + \beta_3 UE_t + \gamma X_t + \beta_4 W_t + \varepsilon_{4t} \qquad (7)$$

This model can be shown[12] to be identical to

$$W_{t+1} = \beta_1 RE_t + \beta_2 OE_t + \beta_3 UE_t + \gamma X_t + \beta_5 W_t + \varepsilon_{5t} \qquad (8)$$

[12] See Werts and Linn 1970. Although eq. (8) is similar to (5), they are not identical since in (5) β_5 is constrained to equal 1.

Estimates of (7) using OLS are presented in Table 6. These show a by now very familiar pattern, namely the standard ORU results $|\beta_3| < \beta_1 > \beta_2$. These estimates may be interpreted in relation to the OLS estimates presented in Table 5. For example, among the correctly matched workers in Table 5 those in job requiring higher qualifications could look forward to greater wage growth. We obtain the same result when we compare employees with the same wages, as we do in Table 6, although here the difference is even greater. The latter can be thought of as a comparison of young university graduates with older industrial workers; while wages at t_1 may be identical the former can expect greater wage growth. This type of comparison across models is particularly interesting in relation to the OE estimates. Above we discussed the possibility that the equality of the RE and OE estimates in Table 5 was due to offsetting processes, potentially concealing a greater wage growth among the overeducated. However, as is evident in Table 6 this was not the case. Although the OE estimate increases in relation to the results in Table 5, wage growth among overeducated is now significantly lower than among matched workers. There is thus no indication of greater wage growth associated with overeducation.

However, this dynamic OLS specification turns out to be highly problematic if we take the possibility of unobserved fixed effects seriously (cf. Halaby 2004). Even if the fixed effects are uncorrelated with the exogenous variables, as e.g. assumed by the random effects specification presented by Büchel and Mertens (2004), the estimates (for both the lagged endogenous and the exogenous variables) are biased due to the correlation between W_t and the fixed effect. The bias is even greater if the fixed effects are correlated with the exogenous variables, which of course is what we suspect. Differentiation is no remedy, because the differentiated error term would still be correlated with the differentiated W_t (Halaby 2004).

To solve this problem, i.e. to be able to estimate a dynamic model including lagged dependent variables and allowing for fixed effects, it is generally suggested that one should first differentiate and then use an instrumental variable in place of the differenced lagged dependent variable (Halaby 2004). Differentiation transforms (8) into

$$(W_{t+1}-W_t) = \beta_1(RE_t-RE_{t-1}) + \beta_2(OE_t-OE_{t-1}) + \beta_3(UE_t-UE_{t-1}) + \gamma(X_t-X_{t-1}) + \beta_5(W_t-W_{t-1}) + (\rho-\rho) + (\varepsilon_{5t+1}-\varepsilon_{5t}) \qquad (9)$$

A number of different instruments intrinsic to panel data have been suggested in the literature, such as the twice lagged dependent variable W_{t-1} or the twice lagged difference W_{t-1}-W_{t-2} (Anderson and Hsiao 1981). Both are valid instruments in the sense that they are correlated with the lagged difference in the dependent variable but not with the lagged difference in the error term. Simulation tests do however indicate that using the twice lagged level is more efficient and less biased that using the twice lagged difference (Halaby 2004).

Estimates using both these instruments are presented in Table 6, and evince a startling similarity to the fixed effects results presented in Table 5. Again, none of the educational variables appear to impact on wage growth, and their effects are hence not different from each other.

When considering these estimates, the discussion related to the results in Table 5 is of course relevant. In addition to this, the instrumental variable approach raises some further issues. One is the sample restriction imposed by the instruments. Using the twice lagged level implies that only those respondents who participated in three consecutive surveys are analyzed, and the analysis using the twice lagged difference is based only on those taking part in all four surveys. This of course involves a drastic reduction in sample size. Furthermore, the structure of the sample changes. In the previously reported analyses the age span of the respondents is 19 to 65. When using the twice lagged level the age limits change to 35-65, whereas it in the case of the twice lagged difference is reduced to 45-65. This obviously affects the interpretation of the results. How relevant is for instance wage growth among the middle aged for theories of early career investments? Moreover, the time period covered by the analyses also changes. In the previous analyses the time period encompassed the whole period 1974 to 2000. In the analyses utilizing instrumental variables we in contrast are analyzing the period 1981 to 2000, alternatively 1991 to 2000. This too might affect the interpretations. The last period is a very distinct period covering the decade of mass unemployment as well as comparatively strong wage growth among those staying employed. Finally, there are some more statistical aspects of the models that need further consideration. These include the strength/weakness of the instruments, the possibility of serial correlation of errors, as well as the issue of panel length and the importance of initial conditions (Bond 2004).

Even disregarding these issues, a model such as (9) would still be based on differentiation, which brings us back to the issue of estimates based on changes in the skill match variables discussed above. These models were introduced in order to examine a specific explanation for the standard ORU results, an explanation based on unobserved ability differences. We have also examined various ways of specifying the ORU model, focusing on the relationship between the models and other explanations of the pattern of ORU results. Even if we succeed in obtaining unbiased estimates, the similarities in the predictions developed by e.g. the career and the search models would make it difficult to adjudicate between them.

One way forward would be to focus on the settings in which the various models would seem most likely to be applicable. Age would for instance seem to be a crucial contextual factor. Both the career and the matching models would seem more applicable to employees who have recently entered the labor market, whereas the underachievement, signaling, and dejection models would be more germane to older workers. Other such distinctions would be related to the job. The career model would for example seem most relevant for jobs offering on-the-job-training or jobs which are part of an established internal labor market, while the search model would appear germane to other jobs (see also the discussion of model 5 in table 4 above). MORE …

Conclusion

We have examined the impact of skill mismatch on wages in Sweden in the context of static and dynamic versions of the ORU model. The empirical analysis based on cross-sectional and panel data from the LNU surevys 1974-2000 have been guided by two main hypotheses:
(a) that skill mismatch reflects human capital compensation rather than real mismatch, and
(b) that skill mismatch is real but dissolves with time spent in the labor market so that its impact on wages tends toward zero over a typical worker's career. Our findings give mixed support to these two hypotheses. First, while there are some indications that overeducated (undereducated) workers are less (more) able than correctly matched workers, significant differences in contemporaneous economic returns to education across match categories remain even after such variations in ability are taken into account. Second, there is some evidence that rates of wage growth are not lower for mismatched workers than for others, but we find no evidence that their growth rate is higher. Our main conclusion is thus that the overeducated are (on average) penalized early on by an inferior rate of return to schooling from which they (on average) do not recover.

How might we improve our analyses? Unobserved ability does seem to be important, but it is difficult to properly assess these results given that differencing appears to reduce the models' validity. It would therefore seem that we in principle would be interested in estimating wage growth models controlling for starting wage level and somehow handling unobserved heterogeneity without estimating difference models. One possibility might however be to try to use some instrument for schooling, e.g. using our productivity index. How reliable such estimates are remains to be seen, and also how such a treatment of the schooling variable could be combined with the definition of the match categories. Another extension would be to distinguish between different sub-categories among the mismatched, each linked to theoretically informed hypotheses as to why mismatch appears and how it affects labor market rewards. The overeducated are most probably a quite heterogeneous group; some workers compensating for weak human capital in other dimensions than schooling, others in the beginning of an upwardly oriented career path, yet others with temporary low-skill jobs between education spells, and some stuck in undesirable positions for which they are genuinely overqualified. Several of our results indicate that the last of these groups (and the list could of course be extended) is fairly large, but we so far have no precise estimates, nor any clear theoretical explanation for why such negative states endure.[13] And apart from examining the relative size of various sub-categories in a cross-section, it is important to assess the changes in their size over time. Such an assessment is crucial to the interpretation and policy implications of the upward trend in overeducation, in Sweden and elsewhere.

[13] Assignment models may be useful in this regard (see, e.g., Sattinger 1975, 1993; Teulings 1995), but have so far proved difficult to combine with the ORU-type model specification (see Hartog 2000: 140f).

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Table 1. Descriptive statistics of all used variables, LNU 1974 – 2000.

1974	N	Min	Max	Mean	Std. Dev
ED_74	3111	0	12	1,65	2,395
RE_74	3066	0	12	1,76	2,455
UE_74	3065	0	12	,65	1,411
OE_74	3065	0	12	,53	1,165
FEM_74	3112	0	1	,43	,495
EXP_74	3104	0	52	18,75	13,244
WG_74	2875	33,00	589,26	90,0170	34,14481
LNWG_74	2875	3,50	6,38	4,4480	,30632
1981					
ED_81	3281	0	12	2,35	2,672
RE_81	3178	0	12	2,03	2,449
UE_81	3175	0	11	,51	1,202
OE_81	3175	0	12	,87	1,493
FEM_81	3285	0	1	,47	,499
EXP_81	3281	0	52	18,51	12,640
WG_81	3245	23,25	406,94	89,5074	31,26799
LNWG_81	3245	3,15	6,01	4,4473	,29390
1991					
ED_91	3326	0	12	2,76	2,795
RE_91	3294	0	12	2,39	2,506
UE_91	3294	0	12	,62	1,336
OE_91	3294	0	12	,98	1,542
FEM_91	3326	0	1	,50	,500
EXP_91	3323	0	50	18,59	12,032
WG_91	3283	25,25	516,44	92,5690	32,26751
LNWG_91	3283	3,23	6,25	4,4825	,28658
TEN_91	3311	0	50	10,04	9,664
VA_91	3321	0	3	2,03	1,11
FTR_91	3323	0	365	5,52	21,925
IFTR_91	3305	,05	36,00	13,1107	13,68420
LRN_91	3318	1	5	3,44	1,144
SAT_91	3320	1	5	4,25	,801
2000					
ED_00	3058	0	12	3,78	2,857
RE_00	3028	0	12	3,05	2,632
UE_00	3026	0	10	,59	1,224
OE_00	3026	0	12	1,32	1,804
FEM_00	3060	0	1	,49	,500
EXP_00	3055	0	53	19,59	12,382
WG_00	2949	34,64	1732,10	116,7046	55,51541
LNWG_00	2949	3,55	7,46	4,7019	,31057
ADV_00	3020	0	2	,5487	,81226

Table 2. Experience (years spent in gainful employment), tenure (years spent with current employer), verbal ability (index 0-3), and wage (ln wage/hour) by skill match category (RE=years of schooling matched by job requirements; UE=years of deficit schooling; OE=years of surplus schooling).
OLS regressions, LNU 1991. B-coefficients, standard errors in parentheses.

	1	2	3	4
	Experience	Tenure	Tenure	Verbal ability
UE	3.030 ***	1.684 ***	-0.190	-0.072 **
	(0.161)	(0.137)	(0.118)	(0.016)
RE	-0.611 ***	-0.054	0.244 ***	0.177 ***
	(0.083)	(0.070)	(0.058)	(0.008)
OE	-1.699 ***	-0.892 ***	-0.054	0.145 ***
	(0.128)	(0.109)	(0.091)	(0.012)
R-sq	0.194	0.091	0.401	0.182
N	3,291	3,279	3,276	3,286

Note: All models include a sex dummy. Models 3 and 4 also include experience and its square. Sign. levels: *** <= .001, ** <= .01, * <= .05.

Table 3. Static ORU model. Pooled cross-sections, Sweden 1974-2000. Standard errors in parentheses.

	OLS	Fixed effects
UE	-0.025*** (0.002)	-0.018*** (0.003)
RE	0.067*** (0.001)	0.033*** (0.002)
OE	0.026*** (0.001)	0.008*** (0.002)
R-sq	0.40	0.34

Notes:
No. respondents 6233, no. observations 12124. Dependent variable ln $Wage_t$. In addition to the variables shown, all models include sex, experience and experience squared. Swedish Level of Living Survey 1974 – 2000.

Table 4. Formal on-the-job training (number of days during last 12 months spent in employer provided education), informal OJT (number of months needed in job before carrying out tasks 'reasonably well'), opportunity to learn new things on the job (scale 1-5), prospects for advancement in current job (scale 0-2, where 2 is good prospects both within and outside current firm and 0 is neither), and job satisfaction (scale 1-5) by skill match category (RE=years of schooling matched by job requirements; UE=years of deficit schooling; OE=years of surplus schooling). OLS regressions, LNU 1991 (except model 4; LNU 2000). B-coefficients, standard errors in parentheses.

	1 Formal training	2 Informal training	3 Learning opportunities	4 Advancem. prospects	5 Job satisfaction
UE	-0.149	-0.328	0.010	-0.001	-0.038 **
	(0.345)	(0.183)	(0.017)	(0.014)	(0.012)
RE	0.727 ***	2.059 ***	0.139 ***	0.053 ***	0.048 ***
	(0.170)	(0.091)	(0.008)	(0.006)	(0.006)
OE	0.007	0.252	-0.009	0.012	-0.041 ***
	(0.266)	(0.141)	(0.013)	(0.009)	(0.010)
R-sq	0.010	0.286	0.106	0.081	0.031
N	3,288	3,272	3,283	2,983	3,285

Note: All models include a sex dummy plus experience and its square.
Sign. levels: *** <= .001, ** <= .01, * <= .05.

Table 5. Dynamic ORU model. Pooled cross-sections, Sweden 1974-2000. Standard errors in parentheses.

	OLS	Fixed effects
UE	-0.009***	0.004
	(0.003)	(0.007)
RE	0.008***	-0.008
	(0.001)	(0.006)
OE	0.012***	0.005
	(0.002)	(0.006)
R-sq	0.06	0.03

Notes:
No. respondents 3474, no. observations 5715. Dependent variable ln $Wage_{t+1}$-ln $Wage_t$. In addition to the variables shown, the OLS model includes sex, experience and experience squared, while the FE model only includes the latter two. Swedish Level of Living Survey 1974 – 2000.

Table 6. Dynamic ORU model including lagged wage level. Pooled cross-sections, Sweden 1974-2000. Standard errors in parentheses.

	OLS	Fixed effects - IV (twice lagged level)	Fixed effects - IV (twice lagged difference)
UE	-0.019***	-0.002	0.002
	(0.003)	(0.012)	(0.009)
RE	0.041***	0.003	0.007
	(0.002)	(0.010)	(0.009)
OE	0.022***	0.013	0.002
	(0.002)	(0.009)	(0.008)
R-sq	0.27	0.23	
No. resp.	3474	1670	596
No. obs.	5715	2265	596

Notes:
Dependent variable ln $Wage_{t+1}$-ln $Wage_t$. In addition to the variables shown, the OLS model includes sex, experience and experience squared, while the FE models only includes the latter two. Swedish Level of Living Survey 1974 – 2000.