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A B S T R A C T

We examine the impact of educational mismatch on wages and wage growth in Sweden. The empirical analyses, based on cross-sectional and panel data from the Level of living surveys 1974–2000, are guided by two main hypotheses: (a) that educational mismatch reflects human capital compensation rather than real mismatch, and (b) that educational mismatch is real but dissolves with time spent in the labour market, so that its impact on wages tends toward zero over a typical worker’s career. Our findings do not support these hypotheses. First, significant differences in contemporaneous economic returns to education across match categories remain even after variations in ability are taken into account. Second, we find no evidence that the rate of wage growth is higher among overeducated workers than others. Our conclusion is that the overeducated are penalized early on by an inferior rate of return to schooling from which they do not recover.

1. Introduction

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). During the same period, the average skill level of jobs has risen as well (Acemoglu, 2002; Green, 2006). An important issue, inter alia for the evolution of economic skill premia, is how these two trends are related to each other. There are two main strands in the literature. The first is the upgrading view, i.e., that skill demand is increasing at a higher rate than skill supply (education). The supposed excess demand for skills is widely used as an explanation for the increase in wage dispersion across skill or education categories that has been observed in several (but not all) countries (Acemoglu, 2003). 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. In addition to SBTC, globalization – in particular increased international trade – is viewed as a cause of skill bias in the evolution of labour demand in OECD countries.

The second strand in the literature is the overeducation perspective. The sense that educational expansion is outstripping the demand for skills in the labour market dates back at least as far as the 1940s (Harris, 1949). In the wake of the rapid growth of student enrollment at colleges and universities in the 1960s, this impression became a wide-spread view (Berg, 1970; Freeman, 1976). An empirical literature on overeducation and earnings started with 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 what has been called educational mismatch, overeducation, or overschooling. A fair amount of educational mismatch appears to exist in Western labour markets. Between 20 and 50% of all workers seem to have more schooling than their job requires, with American rates tending to be higher than European. Trends in overeducation are not well established. Available evidence indicates that overeducation has increased in Europe in recent decades, but not in the United States. This pattern might partly explain why research on overeducation is active and growing in Europe (see, e.g., Borghans and de Grip, 2000; Büchel et al., 2003) but is currently not a major topic in American education and labour studies.

How to reconcile the seemingly contradictory upgrading and over-education perspectives? We consider two hypotheses that solve the contradiction by explaining away an observed oversupply of skills. According to the first, all overeducation is apparent rather than real. As pointed out by Duncan and Hoffman (1981), the estimation of a gap...
between the skill requirements of jobs and the skills of workers is based on the “idea of relatively inflexible, technologically determined educational job requirements” (1981:76), as in assignment models (Sattininger, 1993). This runs counter to the traditional analysis of firms’ production decisions, in which labour processes are assumed to be continuously adapted to available input factors, including human capital. With such flexibility on the part of firms, mismatch between job tasks and employee skills seems unlikely. Implicit in this traditional standpoint, consistent with a standard human capital model, is thus the view that ‘overeducation’ is largely due to measurement error. Naively observed educational mismatch, then, would simply reflect unobserved heterogeneity across individuals: apparently overeducated workers are in reality less able than others on other dimensions (while apparently undereducated workers are more able than others). When all relevant ability differences are taken into account, wage returns to education are expected to be independent of the skill requirements of the job. We call this the human capital compensation hypothesis.

An alternative line of argument is that the recorded skill gap may be real, but only in the short run. If the upward shift in overeducation rates is confined to workers in early career phases, it need not be incompatible with increasingly steep skill-wage gradients in the labour market as a whole. Human capital investment strategies as well as labour market search may generate initial mismatch that will, however, typically dissolve through career processes and job mobility. Higher rates of wage growth are therefore expected to compensate for initial wage losses. We call this the career mobility hypothesis.

In what follows, these two explanations of the apparent contradiction between the upgrading and overeducation perspectives are assessed empirically by examining the impact of educational mismatch — in particular overeducation — on wage levels and wage growth. We start from the general finding in the literature that contemporaneous 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 (Rubb, 2003). The two hypotheses as to why this difference appears are then considered. To examine the human capital compensation hypothesis we apply explicit measures of some otherwise unobserved skill dimensions as well as fixed effects and instrumental variable models to account for unobserved heterogeneity. The career mobility hypothesis is tested by including measures of the training and career dimensions of jobs as well as by analyzing the connection between educational mismatch and wage growth.

We use cross-sectional and panel data from four waves of the Swedish Level of living surveys, 1974 to 2000. Sweden is an interesting test case displaying many of the characteristics pertinent to the Swedish Level of living surveys, 1974 to 2000. Sweden is an interesting test case displaying many of the characteristics pertinent to the mobile welfare states debate. Educational expansion since the 1970s has resulted in a significant rise in the average level of education, and the skill requirements of jobs also increased in the same period. The trend in skill supply (education) has been stronger than the growth in job mobility hypothesis is tested by including measures of the training and career dimensions of jobs as well as by analyzing the connection between educational mismatch and wage growth.

The following results from cross-sectional wage regressions have been found in virtually all published studies, regardless of time and place (Rubb, 2003a; Rubb, 2003b; Rubb, 2003c; Rubb, 2003d). Put differently, overeducated workers earn more than correctly matched workers in the same kind of jobs (β2 > 0), but less than correctly matched workers with the same amount of education (β1 < β2). The converse pattern holds for undereducated workers: they earn less than correctly matched workers in the same kind of jobs (β3 < 0), but more than correctly matched workers with the same amount of education (β1 + β3 > 0).

These consistent empirical results would seem to clearly contradict structural accounts of reward attainment in the labour market, such as the job competition model in Thurow (1975), as well as standard human capital models. The most straightforward interpretation from a standard human capital viewpoint is to emphasize dispersion across education categories indicates that market forces alone cannot explain the evolution of economic inequality. Institutional factors must have mattered as well.

The remainder of the paper is organized as follows. Next section contains an overview of previous research on the impact of educational mismatch on wages and discusses some implications and limitations of past findings. Following a description of the data and variables we use in the analysis, we test the two hypotheses outlined above, devoting one empirical section to each of them. We conclude with some reflections on our findings.

2. Educational mismatch and wages: the ORU model

Duncan and Hoffman (1981) decompose attained education into three parts as expressed by the equation

\[
AE = RE + OE - UE
\]

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.

This decomposition into over, required, and undereducation, known as the ORU model, has two attractive traits. 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 (Rubb, 2003).

The three schooling components defined above are then inserted into a standard Mincer wage equation, as in

\[
W_{it} = \beta_1 RE_{it} + \beta_2 OE_{it} + \beta_3 UE_{it} + \gamma X_{it} + \epsilon_{it}
\]

where \(X\) is a vector of independent variables including a constant, \(\gamma\) is a corresponding vector of coefficients, and \(\epsilon\) an error term. For correctly matched workers, \(\beta_3\) indicates the total schooling return. For mismatched workers, the effects \(\beta_3\) and \(\beta_2\) interpreted in conjunction with \(\beta_3\) yield estimates of the total impact of their education. The total return to schooling among overeducated workers is thus \(\beta_3\) for the years of schooling corresponding to the job requirements together with \(\beta_2\) for the additional years. Among undereducated workers the total return to schooling is given by \(\beta_3\) again indicating the return to the years of schooling corresponding to the job requirements but less \(\beta_3\) 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 (Rubb, 2003a; Rubb, 2003b; Rubb, 2003c; Rubb, 2003d). Put differently, overeducated workers earn more than correctly matched workers in the same kind of jobs (\(\beta_2 > 0\)), but less than correctly matched workers with the same amount of education (\(\beta_1 > \beta_2\)). The converse pattern holds for undereducated workers: they earn less than correctly matched workers in the same kind of jobs (\(\beta_3 > 0\)), but more than correctly matched workers with the same amount of education (\(\beta_1 + \beta_3 > 0\)).

These consistent empirical results would seem to clearly contradict structural accounts of reward attainment in the labour market, such as the job competition model in Thurow (1975), as well as standard human capital models. The most straightforward interpretation from a standard human capital viewpoint is to emphasize
that formal schooling is an obviously 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. Therefore, the “over-educated” 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. The converse argument would apply to “undereducated” workers.

Allen and van der Velden (2001) find empirical support on Dutch data for this hypothesis by showing that wages are less strongly tied to survey respondents’ subjective assessment of educational mismatch than to external classifications based on educational attainment and the skill requirements of jobs. Their subjective indicator of over-education is the degree of disagreement with the statement “My current job offers me sufficient scope to use my knowledge and skills” (2001:438). While useful for many purposes, it is less than obvious that indicators of this kind are relevant to our concerns. One problem is that what counts as ‘sufficient’ for the respondent may look very different from the employer’s point of view, and it is reasonable to suppose that the latter is more important than the former in the determination of wages. Bauer (2002) tests the human capital compensation hypothesis by estimating fixed effects models on German data. His results indicate that unobserved worker heterogeneity explains a large part of the difference across match categories in wage returns to education, especially for women. However, the measures of required education are based on the educational level of individual job incumbents rather than on the educational requirements of jobs, and therefore conflate supply and demand side indicators of skill.

Along the same lines as in the human capital compensation hypothesis, but from the viewpoint of jobs rather than workers, educational requirements are obviously not a perfect measure of the skill level of jobs. Hence, due to unobserved job heterogeneity the true skill requirements level may be underestimated for “overeducated” workers and overestimated for “undereducated” workers, so that the mismatch is again overestimated which attenuates the OE and UE rationale was, however, later provided by Sicherman and Galor (1990; see also Sicherman, 1991) with a model in which over-education is 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., Tåhlin, 2007) which makes Büchel and Mertens’ finding questionable.

In sum, previous attempts at testing the hypotheses of human capital compensation and career mobility have produced interesting but inconclusive results. We now turn to empirically examining these issues on the basis of Swedish data (described in the next section). We first consider the static case of contemporaneous wages, and in this connection 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 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.

3 More specifically, Sicherman (1991) examined promotion probabilities and the implied positive 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.
outcome of the double interviews showed that indicator (a) had a

RE = respondents answering themselves at least 10 h per week.

ED

necessary? to (a), or

1991) are included in the sample at

individuals who were employed respondents aged 19

sample are drawn at each time-point, entering either through age or

targeted age range and residing in Sweden. New members of the

was 14.8% in 1974, increasing to 23.4% in 2000. The samples have a

living conditions along several dimensions, such as education, work-

ing conditions, health, housing, and family life. The non-response rate

in LNU 1991) are included in the sample at (number of years, ungrouped.) Respondents answering

Yes

No

of subjective physical and mental health problems, encompassing

ailments from headaches to anxiety to cancer, in each case

health and verbal ability. The former is an index based on 44 indicators

intended to capture crucial dimensions of what in previous studies has

fallen under the rubric unobserved individual heterogeneity; namely

health and verbal ability. The former is an index based on 44 indicators

of objective physical and mental health problems, encompassing

ailments from headaches to anxiety to cancer, in each case

distinguishing between no, minor and severe problems. With no

problems coded as zero and the two degrees of severity coded as 1 and

2 respectively, we have an additive index with a range from zero to

88.4 This index correlates (in LNU 1991) – 15 with log of hourly wages

and – 10 with years of education. Verbal ability is also measured

through an index, this time constructed from five items indicating

various aspects of verbal competence. They include (a) self-rated

capacity to write a complaint to a government agency; (b) self-rated

frequency of book reading; whether one has (c) spoken to a meeting or

d (written an article and; (e) number of books at home. These have

been coded as zero-one indicators, with the additive index

ranging from zero to five. This measure correlates (again in LNU 1991)

.36 with the log of hourly wages, .44 with years of education, and has a

highly significant positive effect if added to a standard Mincer

regression. Thus, although these two measures clearly are less than

complete measures of productive capacity, they nevertheless seem to

capture important wage relevant characteristics that have not

previously been incorporated in analyses using the ORU model.

As for the career based explanations of overeducation, the LNU

surveys contain several measures of on-the-job training, advancement

prospects and job satisfaction 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 s/he keeps learning new things. This

variable has five response alternatives: 1 = not at all, 2 = to a small

extent, 3 = to some extent, 4 = to a large extent, and 5 = to a very large

extent. Fourth, there is an explicit prospective career mobility

indicator, measuring the (self-reported) extent to which the worker’s

residing in Sweden were interviewed (by personal visits) about their

living conditions along several dimensions, such as education, work-

ing conditions, health, housing, and family life. The non-response rate

was 14.8% in 1974, increasing to 23.4% 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 6426

individuals who were employed respondents aged 19–65 on at least

one of the four interview occasions: 3112 in 1974, 3285 in 1981, 3326

in 1991, and 3060 in 2000. 2622 of these respondents have

participated (and been employed) once, 1944 twice, 1167 three times,

and 693 individuals have responded (and been employed) at

all four occasions. The analyses pertain to all employees, where employees are defined as those working for somebody other than

themselves at least 10 h per week.

Table 1 shows descriptive statistics for all variables used in the

empirical analyses below. ED = 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 provided (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 s/he keeps learning new things. This variable has five response alternatives: 1 = not at all, 2 = to a small extent, 3 = to some extent, 4 = to a large extent, and 5 = to a very large extent. Fourth, there is an explicit prospective career mobility indicator, measuring the (self-reported) extent to which the worker’s

Table 1

Descriptive statistics, Swedish Level of living surveys (LNU), 1974–2000

| Year       | Wage (SEK/h) | Ln wage | Education (years) | Required education (years) | Undereducation (years) | Overeducation (years) | Female | Experience (years) | Verbal ability | Health problems | Tenure (years) | Formal training | Informal training | Learning new things | Job satisfaction | Advancement prospects | Mean SD | Mean SD | Mean SD | Mean SD | Mean SD | Mean SD | Mean SD | Mean SD | Mean SD | Mean SD | Mean SD | Mean SD | Mean SD | Mean SD |
|------------|--------------|---------|-------------------|----------------------------|------------------------|------------------------|--------|-------------------|--------------|---------------|--------------|----------------|------------------|------------------|------------------|---------------|------------------|---------|--------|---------|---------|---------|---------|---------|--------|---------|--------|---------|--------|--------|---------|--------|
| 1974       | 89.85        | 4.45    | 1.67              | 1.80                       | 0.67                   | 0.53                 | 0.42      | 18.90             | 1.56           | 5.53           | 10.06        | 5.51            | 13.14            | 3.44             | 4.25             | 0.20          | 0.40          | 89.70    | 4.45    | 1.67    | 1.80    | 0.67    | 0.53    | 0.42    | 18.90  | 1.56    | 5.53    | 10.06  | 5.51   | 13.14  | 3.44   | 4.25    | 0.20   |
| 1978       | 92.45        | 4.48    | 2.76              | 2.39                       | 0.62                   | 0.98                 | 0.50      | 18.59             | 2.14           | 5.77           | 10.06        | 5.51            | 13.14            | 3.44             | 4.25             | 0.20          | 0.40          | 116.55   | 4.45    | 2.76    | 2.39    | 0.62    | 0.98    | 0.50    | 18.59  | 2.14    | 5.77    | 10.06  | 5.51   | 13.14  | 3.44   | 4.25    | 0.20   |
| 1981       | 92.45        | 4.48    | 2.76              | 2.39                       | 0.62                   | 0.98                 | 0.50      | 18.59             | 2.14           | 5.77           | 10.06        | 5.51            | 13.14            | 3.44             | 4.25             | 0.20          | 0.40          | 116.55   | 4.45    | 2.76    | 2.39    | 0.62    | 0.98    | 0.50    | 18.59  | 2.14    | 5.77    | 10.06  | 5.51   | 13.14  | 3.44   | 4.25    | 0.20   |

Note: Descriptive statistics using listwise deletion among all variables within each year.

The questions were “Have you during the preceding twelve months experienced ...?”

The actual wording of the questions were: “Could you take it upon yourself to write a letter appealing against a decision made by a public authority?” (yes/no), “Do you read books in your spare time?” (yes, often=1)/yes, sometimes and no=0), “Have you ever spoken to a meeting of an association or an organisation?” (yes/no), “Have you ever written a letter to the editor or an article in a journal or a newspaper?” (yes/no), and “Do you have at least 5 running metres of books [at home], not counting works of reference?” (yes/no).
current job contains prospects of advancement, either within the
current firm, outside that firm, or both. This measure is coded 1 if
both internal and external options are good, and 0 otherwise. Finally, there
is an indicator of job satisfaction, namely the extent to which
the employee is happy, generally speaking, with his/her current job. The
five response alternatives to this question are: 1=very unsatisfied,
2=fairly unsatisfied, 3=neither satisfied nor unsatisfied, 4=fairly
satisfied, and 5=very satisfied.6

4. Human capital compensation and wages

We now turn to empirically examining the two hypotheses,
beginning with the human capital compensation idea, and then – in
the next section – looking at career mobility. As a general strategy, we
use the ORU model to examine the empirical association between
educational mismatch and other human capital related dimensions.
We start by relating mismatch to four indicators of human capital. The
first two are standard items: labour market experience, indicating
general skills, and tenure (time spent with current employer),
indicating firm-specific skills. The third and fourth indicators are the
measures of health problems and of verbal ability discussed above.

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 and a half years (the
UE effect is evaluated relative to the estimate for matched education,
RE, not relative to zero). Conversely, each year of “surplus” schooling
is associated with on average 1.7 years shorter experience than
among otherwise similar workers (the OE effect is evaluated relative
to zero). 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
differently. In this case as well, there is some evidence that
apparent educational mismatch works as human capital compensa-
tion: 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), 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.

Next, we examine the link between educational mismatch and
verbal ability; see model 4. The pattern of results resembles 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. Finally, in model 5 we show the result with regard to
ill health. When one takes into account that we here study disabilities
rather than abilities, and therefore have a reversal of signs, these
results display clear substantive similarities to the previous ones.
There is in other words a strong negative relationship between the
level of matched education on the job and health problems, and a
similar yet weaker negative association for education above that level.
The difference is not however very well established; the p-value is .15.
In contrast, there is no significant difference between matched and
deficit education (excess requirements) in their association with ill
health, implying that the health of the undereducated is better than
that of workers with similar educational qualifications and equal to
that of their matched co-workers.

Overall, according to these results the overeducated workers seem
seem to be 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. The under-
educated on the other hand appear to be less capable than their
matched co-workers, but more able than other workers with similar
levels of education. The results would thus seem to support the
human capital compensation thesis.

What consequence does this have for the ORU model with respect to
wages? Estimates of the standard ORU model, based on the
combined samples from the four surveys 1974–2000, are presented in
Table 3, model 1. (The OLS model has here been estimated with robust
standard errors to take account of the fact that we have multiple
observations per respondent.) These Swedish results replicate the

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6 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.
results found in other countries. In the OLS version the effect of RE is positive, the effect of OE is also significantly positive although much smaller than the effect of RE, while the effect of UE is significantly negative and thus smaller than the effect of RE, but still significantly above zero (0.067 – 0.025 = 0.042).

In model 2 we then show the results when the health and ability variables are included in the analyses. Despite the evidence of human capital compensation presented above, the inclusion of these additional measures of human capital leaves all the educational estimates unaffected. Although these two variables clearly capture ability variations (as shown above) among employees with different educational backgrounds in different jobs, this seems to be of basically no importance when it comes to wage differences across match categories. While the two variables do not cover a complete range of potentially important skills, this result would still seem to question the unobserved heterogeneity story.

4.1. An FE model of ORU

Does this mean that the human capital compensation perspective is misconceived? Before jumping to this conclusion, it seems useful to examine results from other standard approaches to controlling for unobserved differences. One such approach is of course to apply a fixed effects model. When estimating Eq. (2) above, unobserved productivity differences become part of the error term \( \varepsilon_t \). Decomposing the error term \( \varepsilon_t \), we can write

\[ W_{it} = \beta_1 RE_{it} + \beta_2 OE_{it} + \beta_3 UE_{it} + \gamma X_{it} + (\beta_4 + \varepsilon_{it}) \]  

(3)

with \( \beta \) being an indicator of productivity. With a negative correlation between \( \rho \) and UE and a positive between \( \rho \) 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 \( \beta_2 \) and \( \beta_3 \) being underestimated.

If \( \rho \) is a time invariant person specific factor (i.e. \( \beta_{3t} = \beta_{3t+1} \)) 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

\[ W_{it} - W_{i(t-1)} = \beta_1 RE_{i(t-1)} - RE_{it} + \beta_2 (OE_{i(t-1)} - OE_{it}) + \beta_3 (UE_{i(t-1)} - UE_{it}) + \gamma (X_{i(t-1)} - X_{it}) + (\beta_{3t} - \varepsilon_{it}) \]  

(4)

and the individual index \( i \) has been dropped to simplify the notation. The effect of the time invariant factor \( \rho \) here cancels out, and the resulting estimates are unbiased.

The results obtained from the fixed effects specification, estimated in its deviations-from-mean form rather than the first difference model of Eq. (4), are presented in model 3.\(^7\) These resemble those obtained from the previous two, although the absolute values of all the estimates decrease noticeably. 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, 2004). Another reason for lower within effects may be measurement error, which generally tends to induce attenuation bias in the estimates (Wooldridge, 2002: 312).\(^8\)

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 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 motive on the job.

The relatively small reduction of the OE 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.

\(^7\) The data do not enable us to identify cases where people, such as students, work part-time jobs “on the side”. While the employment definition in the data does exclude short part-time jobs involving few hours per week, some casual employment may still be included. This could induce some “spurious” changes in educational requirements, and to examine this we have conducted two analyses in which we have excluded individuals with less than two or less than five years of employment experience. The results from these analyses were basically identical to the ones shown.

\(^8\) An example of such measurement error is respondents who (implicitly) report that their level of education has decreased between interviews. It is important to note that the attenuation in the estimated effects induced by such errors would tend to reduce significance levels. That the results obtained here are highly significant indicates that measurement error is a relatively benign problem in relation to our conclusions. This is also supported by the fact that analyses of various influence diagnostics (residuals, leverages and combinations thereof) as well as experiments with different recoding and deletion schemes produced substantively similar results.
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.

4.2. An IV model of ORU

In the human capital compensation perspective the choice of educational attainment is related to unobserved personal characteristics: some individuals compensate for traits and skills they lack through greater investment in education. In addition to fixed effects, an alternative and common way of handling unobserved heterogeneity involves the use of instrumental variables (IV).

There are numerous examples of IV analyses of the return to education [see, e.g., Harmon et al., 2003 for a recent overview]. These share the characteristic of having one endogenous variable measuring education that is instrumented using one or more variables. Here the situation is somewhat different. Recall that the definitions of the various mismatch categories are OE=AE−RE if OE<0, UE=RE−AE if UE<0 and RE=AE if OE=0 and UE=0. Since attained education appears in all three mismatch categories we have to treat all three as endogenous. As we for identification need at least one instrument per endogenous regressor we thus need at least three exogenous instruments.

In order for IV analyses to yield acceptable estimates, these instruments have to fulfil two conditions: relevance and exogeneity. Relevance implies a correlation between the instrument and the endogenous regressor, whereas exogeneity implies no correlation between the instrument and the error term in the main regression. Important is also the predictive power of the instruments, as weak instruments have to fulfi l two conditions: relevance and exogeneity.

When considering potential instruments we are limited to the information available in the surveys. We have focused on variables measuring aspects of childhood circumstances, as these seem likely to influence educational careers (i.e., relevant) while at the same time are less likely to be correlated with adult wages given education (i.e., exogenous). Specifically, four instruments are used in the analyses: sibship size, place of residence during childhood, economic problems during childhood is represented through two dummies with the first coded one if the respondent grew up abroad and the second coded one if the respondent grew up in a town with a population of at least 10,000.

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wage growth, education is regarded as an investment strategy this should be a more growth, both in connection to promotions and otherwise. If over-focus on the impact of being (mis-) matched on all forms of wage Büchel and Mertens (2004) argue that overeducation should be seen workers should experience greater than average wage growth. In contrast, of the unobserved ability argument above, but now applied to wage should be expected to remain underachievers. This is another version investment nor is there any mismatch. Rather than expecting the results is based on (usually unobserved) capital compensation explanation of the ORU earnings regression. A third explanation discussed by Sicherman (1991) is that the opportunity that the jobs held by overeducated workers are in reality better (have apparently) overeducated workers than measures based on schooling requirements imply. The training provided on the job may thus add 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.

Models 1 to 3 of Table 4 show the results of ORU regression models with the on-the-job training indicators as outcomes, while model 4 focuses on advancement prospects. 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 and advancement operates via the skill level of the job as indicated by its educational requirements. Of these requirements, there is no 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 surplus education is never significantly different from zero. But note that it is the job rather than the match that is crucial: years of deficit education (excess skill requirements) have the same impact on training content as matched years of education (the UE coefficient, which is evaluated against the matched component RE, is not significant). The conclusion of these simple cross-sectional analyses 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 and advancement 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 educational 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 states that, due to (usually unobserved) individual and job heterogeneity, respectively, the degree of mismatch is overestimated in simple ORU models. Given that overeducated (undereducated) 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 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) under-educated. The career mobility explanation, by contrast, is based on (usually unobserved) job heterogeneity: the career prospects are hypothesized to be better in jobs held by (apparently) overeducated workers than measures based on schooling requirements imply. The training provided on the job may thus add 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.

Table 4

<table>
<thead>
<tr>
<th>Category</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
</tr>
</thead>
<tbody>
<tr>
<td>Formal training</td>
<td>0.010</td>
<td>0.016</td>
<td>0.010</td>
<td>0.038**</td>
<td></td>
</tr>
<tr>
<td>Informal training</td>
<td>0.345</td>
<td>0.183</td>
<td>0.017</td>
<td>0.044</td>
<td>0.012</td>
</tr>
<tr>
<td>Learning opportunity</td>
<td>0.727***</td>
<td>2.059***</td>
<td>0.139***</td>
<td>0.146***</td>
<td>0.048***</td>
</tr>
<tr>
<td>Advancement prospects</td>
<td>0.170</td>
<td>0.091</td>
<td>0.008</td>
<td>0.010</td>
<td>0.006</td>
</tr>
<tr>
<td>Job satisfaction</td>
<td>0.097</td>
<td>0.252</td>
<td>−0.009</td>
<td>0.020</td>
<td>−0.041***</td>
</tr>
<tr>
<td>OE</td>
<td>0.266</td>
<td>0.141</td>
<td>0.013</td>
<td>0.029</td>
<td>0.010</td>
</tr>
<tr>
<td>R-sq</td>
<td>0.010</td>
<td>0.286</td>
<td>0.106</td>
<td>0.091</td>
<td>0.031</td>
</tr>
<tr>
<td>N</td>
<td>3288</td>
<td>3278</td>
<td>3263</td>
<td>2983</td>
<td>3285</td>
</tr>
</tbody>
</table>

OLS (models 1–3, 5) and logistic (model 4) regressions; standard errors in parentheses. Notes: Formal on-the-job training is number of days during last 12 months spent in employer provided education. Informal OJT is number of months needed after entry to current job before carrying out tasks ‘reasonably well’. Opportunity to learn new things on the job is a scale 1–5. Prospects for advancement in current job is an indicator variable with ‘good prospects both within and outside current firm’ = 1; otherwise = 0. Job satisfaction is a scale 1–5. (See further section Data and variables.) RE = years of schooling matched by job requirements; UE = years of deficit schooling; OE = years of surplus schooling. All models include a sex dummy plus experience and its square. Swedish Level of living survey (LNU) 1991; except model 4: LNU 2000. Significance levels: ***, *0.001, ***, *0.01, &–sq, model 4= Nagelkerke.

of wage effects of educational mismatch to the dynamic case of wage growth. More precisely, we examine wage change in the form of

\[ W_{t+1} - W_t = \beta_1 RE_t + \beta_2 OE_t + \beta_3 UE_t + \gamma X_t + \epsilon_t \]  

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. In other words, this model would focus on the impact of being (mis-) matched on all forms of wage growth, both in connection to promotions and otherwise. If over-education is regarded as an investment strategy this should be a more relevant model as it takes all forms of investment return into consideration. If overeducation involves investment, initial over-education should in Eq. (5) be associated with greater than average wage growth, \( \beta_2 > \beta_3 \), and the gap in returns to schooling relative to correctly matched workers should over time decrease and eventually go to zero.

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 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 Eq. (5) would yield \( \beta_3 > \beta_2 \).

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 underachievment: 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., using Eq. (5) we should obtain \( \beta_3 < \beta_2 \).

Before turning to the wage growth models, we provide some cross-sectional evidence on the career mobility perspective. The human capital compensation explanation of the ORU earnings regression results is based on (usually unobserved) individual heterogeneity: that

\[ W_{t+1} - W_t = \beta_1 RE_t + \beta_2 OE_t + \beta_3 UE_t + \gamma X_t + \epsilon_t \]  

12 In distinction to formal and informal on-the-job training, which are both measured with interval scale time measures, the measure of learning opportunities is an ordinal scale item with five values and the indicator of advancement prospects is a dichotomy (see the Data and variables section above). The advancement model is therefore a logistic regression. The learning model should ideally be estimated by ordinal regression rather than – as in Table 4 – by OLS. However, there are several variants of specifying the ordinal model, all of which are mere approximations to the “true” latent continuous distribution. In practice, none of these variants produces results that deviate significantly from the OLS specification.
be problematic in several ways. The net outcome of these conflicting mechanisms is an empirical matter.

We find 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 under-educated and over-educated workers are significantly less satisfied than matched workers.\(^\text{13}\) So in distinction to the case of on-the-job training, informal OJT, learning opportunity, and job satisfaction. Swedish Level of living survey 1974–2000. Significance levels: \(\ast\ast\ast = .001, \ast\ast = .01\).

In Table 5, we move from these descriptive results to estimates based on the dynamic ORU models described in Eq. (5). This is presented in model 1, and according to these estimates 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 \((p\text{-level}=0.29)\). 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 significantly less positive impact on wages than years of matched education; in fact the UE coefficient is not significantly different from zero. But in distinction to the static case, the magnitude of \(\beta_3\) is not smaller than that of matched education. In sum, the OLS estimation of this model of wage growth implies that the career mobility hypothesis in Table 5 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.

Although only available in the last two surveys, that is for the period 1991 to 2000, the variables examined in Table 4 allow us to account for some of the differences between jobs that are supposed to generate wage growth differentials. These have here been included as control variables in the analysis and the results are shown in model 2. (The advancement prospects indicator is only available in the last survey, and therefore it cannot be included in these analyses of wage growth.) The only substantive difference in relation to those in model 1 is that the effect of undereducation relative to matched education here is insignificant, in part probably due to the reduced sample size. More importantly, the point estimate for overeducation is still not significantly different from that of matched education. There is thus still no clear evidence of any catch-up among the overeducated.

Since the career mobility theory focuses on career investment decisions, it would seem plausible that the return to investment would be greater among the relatively young. They would simply have the greatest opportunity to recuperate any short term losses in the return to education. This idea is examined in model 3, where we analyze wage growth among those with less than 15 years of experience. However, the estimate for overeducation is not greater than that for matched education, and here it is not even significantly greater than zero. Support for the theory of career mobility is again lacking.

For theoretical reasons, however, the specification in Eq. (5) should be extended. 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 correctly matched employees with the same job requirements \((\text{see Table 3})\), and growth rates tend to decrease with starting wage \((\text{i.e., a ceiling effect})\). 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

\[
W_{t+1} = \beta_1 W_t + \beta_2 RE_t + \beta_3 OE_t + \gamma X_t + \beta_4 W_t^{−} + \epsilon_t
\]

Estimates of Eq. (6) using OLS are presented in model 4, Table 5. These show a by now very familiar pattern, namely the standard OLS result \(\beta_3 < \beta_1 < \beta_2\). The estimates may be interpreted in relation to the OLS estimates presented in model 1. For example, among the correctly matched workers in model 1 those in jobs 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 model 4, 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

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\(^{13}\) The UE coefficient implies that each year of excess educational requirements has a zero impact on job satisfaction (the difference between the UE and RE coefficients is \(0.10\), which is not significantly different from zero). Hence, the overall implication is that (a) requirements matched by education are positive for satisfaction, \(b\) excess requirements \((\text{i.e., unmatched by education})\) do not matter for satisfaction, and \(c\) excess education \((\text{i.e., unmatched by education})\) are negative for satisfaction.

\(^{14}\) As in the case of learning opportunities, ordinal regression models of satisfaction \((\text{which is measured with an ordinal rather than interval scale item})\) in several different specifications produce results that are identical to \((\text{i.e., not substantively different from})\) the estimates from the reported OLS model.
the RE and OE estimates in model 1 was due to offsetting processes, potentially concealing a greater wage growth among the over-educated. However, as is evident in model 4 this was not the case. Although the OE estimate increases in relation to the results in model 1, wage growth among the overeducated is now significantly lower than among matched workers. There is thus no indication of greater wage growth associated with overeducation.

6. Conclusion

We have examined the impact of educational 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 Level of living surveys (LNU) 1974–2000 have been guided by two main hypotheses attempting to explain away the incidence or importance of overeducation: (a) that educational mismatch reflects human capital compensation rather than real mismatch, and (b) that educational mismatch is real but dissolves with time spent in the labour market so that its impact on wages tends toward zero over a typical worker’s career.

Our findings do not support 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. This has here been done through the inclusion of explicit indicators of ability, or by using fixed effects or instrumental variable estimation. Although each of these analyses have their weaknesses, they nevertheless represent improvements over much of what has been done previously. The fact that none of them dramatically alters the classical ORU results strongly suggests that these results are not entirely due to unobserved heterogeneity. 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. A possible extension of these analyses would be to distinguish between different sub-categories among the mismatched, each linked to theoretically informed hypotheses as to why mismatch appears and between different sub-categories among the mismatched, each linked to theoretically informed hypotheses as to why mismatch appears and how it affects labour market rewards. The overeducated are most negatively states endure.15 And apart from examining the relative size of negative states endured.15 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.

How do our findings relate to the apparent contradiction between the upsizing and overeducation perspectives that we discussed at the outset of the paper? The analyses here do of course indicate that overeducation, and mismatch in general, is a real phenomenon with important economic effects. At least for Sweden, then, our results imply that the rise in skill demand has been sufficiently met by an increase in supply; indeed, perhaps more than sufficiently. Although a significant upgrading of the job structure has occurred in most (if not all) OECD countries, shifts in the balance between skill demand and supply need not have changed in a way that enhances wage inequality. The fact that the dispersion of wages has increased in several countries (Sweden among them) that simultaneously show rising rates of overeducation indicates that market factors alone cannot explain the evolution of economic inequality. Institutional factors – such as the character of wage bargaining – must be taken into account as well.

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