Estimating the returns to educational mismatch with panel data: the role of unobserved heterogeneity

M. Pecoraro

Discussion Paper 2011-36

Institut de Recherches Économiques et Sociales
de l'Université catholique de Louvain

Estimating the returns to educational mismatch with panel data: the role of unobserved heterogeneity

Marco Pecoraro*

IRES, Université Catholique de Louvain,

and SFM, Université de Neuchâtel

June 20, 2011

Abstract

Using longitudinal data from the Swiss Household Panel, this analysis suggests that the cross-sectional estimates of the returns to educational mismatch are significantly biased when unobserved heterogeneity is omitted in the wage equation. The results of the standard fixed effects model indeed demonstrate that the wage returns to education are independent of the job requirements. Hence, this empirical analysis supports the human capital interpretation of the Swiss labour market.

Keywords: Educational mismatch, wages, panel data analysis, human capital

*Address: Marco Pecoraro, Swiss Forum for Migration and Population Studies, Université de Neuchâtel, Faubourg de l'Hôpital 106, 2000 Neuchâtel, Switzerland (email: marco.pecoraro@unine.ch, tel: +41 32 718 39 41). The author especially thanks Profs. Muriel Dejemeppe, Bruno Van der Linden and Bart Cockx for their comments, as well as Boris Wernli from the Swiss Centre of Expertise in the Social Sciences (FORS) for his availability to deal with questions about the data set and William Doelher for his assistance concerning English.

1 Introduction

The analyses of educational mismatch in the labour market and its consequences on individual wages have never been so widespread than nowadays.[1] Educational mismatch arises in the form of overeducation (i.e. workers have more education than is required for their jobs) or undereducation (i.e. workers have less education than is required for their jobs). Since the seminal paper by Duncan and Hoffman (1981), who proposed an extension of the Mincer wage equation in order to estimate the returns to over/required/undereducation, the literature has generally agreed with the following two points. First, overeducated workers earn less than those adequately educated with the same education but more than those adequately educated in the same occupation. Second, undereducated workers earn more than those adequately educated with the same education but less than those adequately educated in the same occupation. Given the growing availability of panel data, some studies have applied the fixed effects estimation method to Duncan and Hoffman's wage equation (Bauer, 2002; Korpi and Tåhlin, 2009). The main idea of this method is to account for unobserved heterogeneity whose omission may lead to biased estimates in cross-sectional analyses. However, neither Bauer nor Korpi and Tåhlin have considered the fact that fixed effects estimates of the returns to required education, overeducation and undereducation may not be properly identified since there is probably little or no variation in actual education over time. The aim of this paper is to address this problem of identification when using fixed effects to estimate the returns to educational mismatch.

How to reconcile the variation need of the fixed effects estimator and the time-invariant nature of actual education? Motivated by the latest meta-analyses of studies on educational mismatch, we rely on a simplified version of Duncan and Hoffman's model proposed by Rumberger (1987) in which years of overeducation and years of undereducation are restricted to have symmetric effects on wages. After rearranging Rumberger's specification, we show that the coefficient associated with required education is the differen-

[1] The recent review of the literature provided by Leuven and Oosterbeek (2011) is a good example revealing the ongoing interest in studying this topic.

tial rate of return between required education and educational mismatch. Since required education is time-varying, the fixed effects method allows the identification of this coefficient. The fixed effects results based on panel data from Switzerland demonstrate that the wage returns to education are independent of the job requirements, i.e. the estimated differential rate of return is equal to zero. Hence, this empirical analysis supports the human capital interpretation of the Swiss labour market.

The next section of this paper provides a description of the data from the Swiss Household Panel Survey and explains how educational mismatch is measured. Section 3 presents the modelling approaches to estimating by fixed effects the returns to educational mismatch under the time-invariant nature of actual education. The regression results are produced in Section 4 and a number of robustness checks are performed in Section 5. Section 6 discusses the significance of our findings and proposes possible extensions.

2 Data

We use the data collected by the Swiss Household Panel (SHP), which is based at the Swiss Centre of Expertise in the Social Sciences (FORS). The SHP is an unbalanced panel where surveyed individuals may temporarily drop out of the sample because certain or all variables are unobserved for some time period. It consists of two samples: the *SHP_I* (the sample of households and individuals interviewed for the first time in 1999) and the *SHP_II* (a refreshment sample of "new" households included in 2004). The individual questionnaires cover a broad range of topics, of which education and employment are our main points of interest.[2]

In the empirical analysis, we restrict our final sample to individuals who belonged to the working age population (18-65 for men, 18-62 for women)

[2] Complete interview data are available for 7,799 and 3,654 individuals in 1999 and 2004, respectively. The net response rates (referring to all called individuals minus those with neutral problems such as invalid telephone number or foreign language) are quite high, attaining 85% in 1999 and 76% in 2004. The random samples were stratified according to seven large regions of Switzerland, proportionally to the number of phone connections in the comprehensive Swiss phone directory. Interviews were carried out in German, French and Italian using computer-assisted telephone interviewing. Further details on the sampling methodology and questionnaires are available at www.swisspanel.ch.

of Swiss origin (i.e. with the Swiss citizenship and at least one parent that is/was Swiss by birth) at the time of the first-wave interview in 1999; in addition, we include workers who were not self-employed, not enrolled in education and reported valid information for the variables of interest (e.g. level of occupation, gross hourly earnings, potential work experience). We consider that, once a person drops out (because he/she does not reply to the questionnaire, moves out of the labour force, becomes self-employed, starts studying again or reports invalid information for the variables of interest), he/she is out forever. Hence, we ignore any subsequent observations on individuals after they leave the sample (dropout is an *absorbing* state). Given the relatively high attrition among selected individuals over time, we focus the remainder of the analysis on the first four waves of the panel (see Table 3 of the appendix for more details on the sample selection).[3]

In the literature, educational mismatch is measured as the difference between workers' actual education and required education for their job. Three methods are generally used for defining required education, each of them presenting arguments for and against (see, e.g., Hartog, 2000; Chevalier, 2003, for critical overviews of each measurement method). The first measure is obtained from the **job analysis** method; according to this *objective* method, the level of education required to perform a particular job is determined by a systematic job evaluation. However, this information is actually unavailable for Switzerland. Second, some surveys include the **worker's self-assessment** of educational requirement; this *subjective* method consists in asking workers directly how much education is required to *get* or *do* their job. But the SHP survey does not provide such information. As a result, we are left with the **realized matches** method that can always be implemented. There are two main measures derived from this *objective* and *statistical* method. First, the required education is defined as a band around the mean level of education within each occupation (Verdugo and Verdugo, 1989). Workers are then overeducated (resp. undereducated) if their actual education expressed in years diverges by more than one standard deviation above (resp. below) the mean value for a given occupation. The required education can also be

[3]Hereafter, we only inspect whether our findings are sensitive to sample selection resulting from attrition.

established from the modal rather than the mean level of education (Kiker et al., 1997). Accordingly, workers are overeducated (resp. undereducated) if their educational attainment falls above (resp. below) the modal value for a specific occupation.

As noted by Kiker et al. (1997) and Mendes de Oliveira et al. (2000), the standard deviation procedure has the disadvantage, with respect to the modal measure, that it is more sensitive to the presence of outliers in the data. In addition, it relies on the strong assumption of symmetry, while the modal level of education within a particular occupation better reflects the potential asymmetry of the distribution. Therefore, we derive required education from the mode of workers' actual educational attainment in each occupation, separately by survey year. The highest level of education achieved consists of 10 levels classified in an increasing hierarchical order; each educational level is translated into the total number of years of schooling (see Table 4 of the appendix). Moreover, we rely on the International Standard Classification of Occupations (ISCO) disaggregated on a 2-digit level with at least 10 observations in a year;[4] this amounts to about thirty occupation levels. As we show in Section 5, the main findings of the empirical analysis do not change depending on whether we use (i) occupations disaggregated on a 3-digit level or (ii) another conversion scale between levels and years of education.

3 Educational mismatch and wages

Duncan and Hoffman (1981) have proposed an extended version of the Mincer wage equation in which years of actual education S^a are decomposed into years of required education S^r, years of overeducation S^o and years of undereducation S^u:

$$\ln w_{it} = \delta + \alpha^r S^r_{it} + \alpha^o S^o_{it} + \alpha^u S^u_{it} + \beta_1 X_{it} + \beta_2 X^2_{it} + \gamma G_i + \epsilon_{it}$$

where w_{it} is the gross hourly wage for individual i at wave t,[5] δ a constant

[4]Workers in occupations with less than 10 observations in a year are excluded from the sample (see Table 3 of the appendix).

[5]Deflated into 2000 Swiss francs, gross hourly wages is obtained from the division of the reported gross monthly wages to the reported number of hours worked per week multiplied by 4.3 (weeks).

term, X_{it} potential years of work experience (= year of interview - year started working without prolonged interruption) and G_i a dummy variable for gender (= 1 if worker is a woman). While S^r_{it} is simply derived from the realised matches method, the following identities hold for S^o_{it} and S^u_{it}:

$$S^o \equiv \max(0, S^a - S^r) \quad \text{and} \quad S^u \equiv \max(0, S^r - S^a).$$

By construction, $S^a = S^r + S^o - S^u$. Adequately educated workers earn a return of α^r for years of required education. Overeduated workers receive a return of α^r for years of required education plus a return of α^o for years of surplus education. And undereducated workers get a return of α^r for years of required education minus a return of α^u for years of deficit education.

Two theoretical frameworks can be tested on the basis of Duncan and Hoffman's wage equation. First, the **human capital hypothesis** (Becker, 1964) implies this joint equality: $\alpha^r = \alpha^o = -\alpha^u$. Accordingly, wages only depend on worker's characteristics and the Duncan and Hoffman specification simplifies to the Mincer wage equation. Second, the **job competition hypothesis** (Thurow, 1975) states that wages are entirely determined by required education: $\alpha^o = \alpha^u = 0$. In this case, workers' characteristics do not matter in the wage determination process. Most earlier studies do not give support to these hypotheses (for the latest surveys of the empirical evidence using the Duncan and Hoffman specification, see McGuinness, 2006; Leuven and Oosterbeek, 2011), in particular: (1) returns to overeducation are positive and lower than returns to required education, i.e. $\alpha^r > \alpha^o > 0$, (2) returns to undereducation are negative and lower than returns to required education in absolute value, i.e. $\alpha^u < 0$ and $\alpha^r > |\alpha^u|$. Compared to those adequately educated, overeducated (resp. undereducated) workers earn less (resp. more) for a given level of actual education but earn more (resp. less) for a given level of occupation.

As most prior researches have relied on a cross-sectional estimation of Duncan and Hoffman's specification and have then assumed that unobserved heterogeneity (including ability, motivation and other unobserved characteristics) is uncorrelated with educational mismatch, they are confronted with the problem of *omitted variable bias* if this assumption fails to hold. Indeed, the omission of unobserved heterogeneity may lead to an underestimation of the rate of return to overeducation if unobserved heterogeneity is negatively correlated with overeducation; conversely, the rate of return to undereducation is probably overestimated in case of a positive correlation between unobserved heterogeneity and undereducation. Few studies have controlled for unobserved heterogeneity when estimating Duncan and Hoffman's wage equation. Using the German Socio-Economic Panel data from 1984 to 1998, Bauer (2002) has applied the fixed effects method. He has indicated that unobserved heterogeneity explains a large part of the difference between returns to required education and over/undereducation: in particular, the estimated differences become smaller for men and totally disappear for women. On the basis of the same estimation method, very different results are reported by Korpi and Tåhlin (2009) using data from Swedish Level of Living surveys from 1974, 1981, 1991 and 2000. They have found that returns to over/undereducation remain smaller in absolute value than the return to required education after controlling for fixed unobserved heterogeneity.[6]

In a fixed effects analysis, unobserved heterogeneity c_i becomes part of the disturbance term: $\epsilon_{it} = c_i + u_{it}$, where u_{it} is the independent, identically distributed error. The fixed effects method is attractive because it allows for c_i to be arbitrarily correlated with the explanatory variables. Indeed, its main assumption is strict exogeneity of the covariates conditional on c_i: $\mathbb{E}[u_{it}|\mathrm{x}_{i1}, \mathrm{x}_{i2}, ..., \mathrm{x}_{iT}, c_i] = 0$ for $t = 1, ..., T$, where x_{it} is the vector of regressors. However, the fixed effects method does not allow the estimation of time-invariant variables and can lead to imprecise estimates if the key variables in x_{it} do not vary much over time (cf Wooldridge, 2002). As noted by Nielsen (2007), a fixed effects approach would only identify the returns to educational mismatch from information on individuals who change their level of education within the sample period. In fact, recall that $S^a = S^r + S^o - S^u$. In case S^a is constant across time, the within-individual variation in S^r, S^o and S^u is characterized by perfect multicollinearity. Even with little longi-

[6] Korpi and Tåhlin (2009) have also applied the instrumental variables (IV) method in odrer to address the problem of omitted variable bias. While their IV results give support to the job competition hypothesis, the use of weak instruments is likely to cast doubt on the robustess of their estimates. As pointed by Leuven and Oosterbeek (2011), it is indeed very difficult to apply the IV method in the context of Duncan and Hoffman's wage equation, in particular to find convincing instruments for educational mismatch.

tudinal variation in S^a, the fixed effects estimation remains problematic. As the fixed effects estimator only makes use of the within-individual variation in the sample, this leads to inefficient estimates of the returns to over-, required and undereducation. According to Plümper and Troeger (2007), the inefficiency of the fixed effects model results from the fact that it disregards the between variation and, thus, does not take all the available information into account. While Korpi and Tåhlin (2009) do not present any information about between and within variations of actual education, Bauer (2002) displays this information. As expected, variation across individuals is much larger than variation within individuals: the ratio of the between standard deviation of actual education to the within standard deviation of the same variable is ranging between 7.35 (for women) and 8.68 (for men). The high values of this ratio cast doubt on whether the fixed effects estimates of the returns to educational mismatch could be identified.

Actual education is time-constant in our sample since we exclude individuals enrolled in education (i.e. $S^a_{it} = S^a_i$ for all t). How can we reconcile the variation need of the fixed effects estimator and the time-invariant nature of actual education? In order to address the aforementioned problem of identification, we use a simplified version of Duncan and Hoffman's model proposed by Rumberger (1987) in which years of overeducation and years of undereducation are restricted to have a symmetric effects on wages (i.e. $\alpha^u = -\alpha^o$):

$$\ln w_{it} = \delta + \alpha^r S^r_{it} + \alpha^o (S^a_{it} - S^r_{it}) + \beta_1 X_{it} + \beta_2 X^2_{it} + \gamma G_i + \epsilon_{it}.$$

The use of this specification is motivated by the two latest meta-analyses of studies estimating Duncan and Hoffman's model (Rubb, 2003; Leuven and Oosterbeek, 2011); they have demonstrated that the returns associated with overeducation and undereducation are in nature close to symmetry. Rubb's main message about the average returns to over-, required, undereducation computed on the basis of 85 wage estimates is stated as follows (p. 621):[7] "on average, the literature finds that the premium paid for overeducation is approximately equal to the penalty for undereducation, but lower than the

[7]Rubb (2003) does not provide any information about the estimation methods used in the selected studies; however, it is likely that the return estimates are mainly obtained from the ordinary least squares regression.

returns associated with an increase in required education." Based on approximately 150 studies,[8] Leuven and Oosterbeek's descriptive results reinforce Rubb's statement (p. 30): "the return to a year of required schooling is around 0.09, to a year of overschooling more or less half of that, and a year of underschooling results in a wage penalty of again around half of the return to a required year of schooling." It should be noted that the findings of Rubb (2003) and Leuven and Oosterbeek (2011) are probably biased since most studies included in their meta-analyses have neglected unobserved heterogeneity when estimating the Duncan and Hoffman specification. Nevertheless, the hypothesis of symmetric returns to overeducation and undereducation is crucial for our identification strategy, given that the use of Rumberger's wage equation allows the fixed effects estimator to exploit the within-individual variation in required education.

We can rearrange Rumberger's wage equation as

$$\ln w_{it} = \delta + \underbrace{(\alpha^r - \alpha^o)}_{\Delta\alpha} S_{it}^r + \alpha^o S_{it}^a + \beta_1 X_{it} + \beta_2 X_{it}^2 + \gamma G_i + \epsilon_{it} \qquad (1)$$

where $\Delta\alpha$ corresponds to the differential rate of return between required education and educational mismatch. The identification of this coefficient is possible with fixed effects since S^r varies over time. Preview hypotheses are restated according to this last equation: while the human capital hypothesis requires $\alpha^r - \alpha^o = 0$, the formal test of the job competition hypothesis simplifies to $\alpha^o = 0$. Equation 1 is estimated using [i] the Pooled Ordinary Least Squares with robust standard errors and [ii] different panel data estimation methods, where $\epsilon_{it} = c_i + u_{it}$. We consider two panel data models:

- *Random Effects*
  While this model allows coefficients on time-constant explanatory variables to be identified, its main shortcoming is the strong assumption that unobserved heterogeneity is independent from covariates. As with pooled ordinary least squares, the random effects regression model includes all the time-invariant and time-varying covariates, and three dummies for the second, third and fourth waves.

[8]Contrary to Rubb (2003), Leuven and Oosterbeek (2011) include a small number of studies using instrumental variables (4 estimation results) and fixed effects (5 estimation results).

- *Fixed Effects*
  This model is more appropriate to estimate the wage effects of educational mismatch since it allows for correlation between unobserved heterogeneity and covariates; but estimation of time-invariant explanatory variables is not possible as the fixed effects estimator needs individuals' variation over time. Thus, only the time-varying controls are considered (i.e. required education, potential work experience and potential work experience squared).[9]

All the explanatory variables are presented in Table 5, while descriptive sample statistics are presented in Table 6 (see the appendix). Since identification in fixed effects analysis relies on within-individual variation, sufficient variation in S^r within workers is needed in order to obtain precise estimates. Between and within standard deviations are displayed in Table 6. Even if most of the variance in the modal measure of S^r is due to differences across workers, there is also substantial variance within workers. In fact, the within standard deviation is close to 50 percent of the between standard deviation. Hence, our identification strategy based on sufficient variations in S^r appears valid.

4 Results

Regression results from equation 1 are displayed in Table 1; pooled ordinary least squares (POLS), fixed effects (FE) and random effects (RE) estimates are presented in the first, second and third columns, respectively.

POLS results show that the differential rate of return between required education and educational mismatch ($\alpha^r - \alpha^o$) and the return to educational mismatch (α^o) are both of them significantly positive: workers earn a return of 0.073 (= 0.017 + 0.056) for a year of required education and a positive return of 0.056 for a year of surplus education (i.e. a negative return of -0.056 for a year of deficit education). Hence, these estimates are fully consistent with most earlier studies according to which the human capital and job competition hypotheses are rejected, given that the return to overeducation is

[9]The wave dummies and the potential experience variable are perfectly linearly related, so the wave dummies are excluded in fixed effects analysis.

Table 1: Wage returns to educational mismatch: Regression results

	POLS	FE	RE
$\alpha^r - \alpha^o$	0.017**	0.001	0.013**
	(0.004)	(0.006)	(0.004)
α^o	0.056**	-	0.058**
	(0.003)		(0.004)
Observations	3,474	3,474	3,474
Number of i	1,152	1,152	1,152
Adjusted R^2	0.245		
Overall R^2		0.0785	0.247
Hausman test			14.34**
t test: $s_{i,t+1} = 0$		-0.80	

Standard errors in parentheses, POLS with robust standard errors.
** p<0.05, * p<0.10
Source: Swiss Household Panel, four waves from 1999 to 2002.
Notes: Data are unweighted. Individuals surveyed only once at the first wave are omitted in order to obtain comparable estimates with respect to the fixed effects analysis. Required education is measured with the *mode* procedure. Additional controls are unreported; full regression results are reported in Table 7 of the appendix.

positive and lower than the return to required education, i.e. $\alpha^r - \alpha^o > 0$ and $\alpha^o > 0$.

The rejection of the Hausman test implies that the RE model is inconsistent for estimating the wages returns to educational mismatch since unobserved heterogeneity is correlated with covariates. Therefore, we focus our attention on the FE estimates. This method indicates that the differential rate of return between required education and educational mismatch is no more significant, i.e. $\alpha^r - \alpha^o = 0$. Moreover, this result emphasizes that there is no evidence against the human capital hypothesis. Consequently, omitted variable bias seems to explain the entire difference between returns to required education and educational mismatch.

Given that our fixed effects estimates are based on an unbalanced panel data set, it is important to determine if sample selection due to attrition is present. As our longitudinal sample is set up in a way such that attrition is an absorbing state, one way to test for attrition bias is to add a lead of selection indicator $(s_{i,t+1})$[10] as an additional regressor in the fixed effects analysis and test for significance using a t test (Wooldridge, 2002). The result of this procedure (presented at the bottom of Table 1) show that the lead variable is not significant (t statistic corresponds to -0.80), meaning there is no evidence of attrition bias. Therefore, it is reasonable to conclude that sample selection is not an issue in our fixed effects regression.

5 Robustness checks

In this section, we wonder whether our findings rest on the specific measure of required education that we adopt when estimating equation 1. Recall that required education is measured with the *mode* procedure, on the basis of the CNEF conversion scale between levels and years of education (cf Table 4) and occupations disaggregated on a 2-digit level.

The most obvious alternative is the measure derived from the *mean* procedure. Accordingly, required education corresponds to the mean value of workers' educational attainment for their occupation. However, in order to satisfy $S^a = S^r + S^o - S^u$, required education needs to be equal to workers' actual education for those adequately educated (i.e. in case their actual education is within plus or minus one standard deviation of the mean education for their occupation) given that $S^o = 0$ and $S^u = 0$. The between and within standard deviations for this variable are reported in the last row of Table 6 (see the appendix). The identification of the parameter of interest $(\alpha^r - \alpha^o)$ should not be an issue since the mean-based measure of required education shows some variation across time: its within standard deviation represents 1/3 of its between standard deviation. All the estimates of Table 1 are re-derived in Table 2 by replacing the mode-based measure of required education by the mean-based measure in equation 1. The results are qualitatively similar and thus confirm our main conclusion: returns to required education and educational mismatch become equal after controlling for unobserved heterogeneity.

[10] Let s_{it} be a binary selection indicator for individual i at wave t: $s_{it} = 1$ if $(\mathrm{x}_{it}; \ln w_{it})$ is observed, and zero otherwise.

Table 2: Wage returns to educational mismatch: Additional results

	POLS	FE	RE
$\alpha^r - \alpha^o$	0.032**	0.003	0.025**
	(0.005)	(0.009)	(0.006)
α^o	0.047**	-	0.051**
	(0.004)		(0.005)
Observations	3,474	3,474	3,474
Number of i	1,152	1,152	1,152
Adjusted R^2	0.250		
Overall R^2		0.0813	0.251
Hausman test			17.89**
t test: $s_{i,t+1} = 0$		-0.80	

Standard errors in parentheses, POLS with robust standard errors.
** p<0.05, * p<0.10

Source: Swiss Household Panel, four waves from 1999 to 2002.

Notes: Data are unweighted. Individuals surveyed only once at the first wave are omitted in order to obtain comparable estimates with respect to the fixed effects analysis. Required education is measured with the *mean* procedure. Additional controls are unreported; full regression results are reported in Table 8 of the appendix.

Additional robustness checks consist in assessing the sensitivity of the estimates when we consider for the measurement of required education either a more detailed classification of occupations (i.e. disaggregated on a 3-digit level) or an alternative conversion scale between levels and years of education. For instance, Weber (2010) have proposed a conversion scale according to which an individual needs to spent 13 years (instead of 12 years) to get a high school degree while a certificate from a technical or vocational school is obtained after 14 years (instead of 15 years). We re-estimate equation 1 in which the mode- or mean-based measures of required education are computed using this alternative conversion scale or the 3-digit ISCO code. The estimation results are presented from Table 9 to Table 11 (see the appendix). All the fixed effects estimates of the differential rate of return between required education and educational mismatch remain statistically indifferent from zero. They are again qualitatively similar to previous estimates, indicating that our results are robust to various ways of deriving the statistical measure of required education.

6 Conclusion

The purpose of this paper has been to solve the problem of omitted heterogeneity bias when estimating the wage returns to required education, overeducation and undereducation. We have used data from the Swiss Household Panel whose longitudinal aspect allows us to control for unobserved heterogeneity by means of fixed effects. While a few studies have also applied this method (Bauer, 2002; Korpi and Tåhlin, 2009), they have neglected the fact that sufficient variations in actual education within workers are needed to identify the returns to required education, overeducation and undereducation. This paper distinguishes from these studies since our identification strategy exploits the within-individual variation in required education instead. After rearranging the wage equation proposed by Rumberger (1987) that implicitly recognizes a symmetric relationship between overeducation and undereducation, the parameter of interest is indeed associated with required education: it measures the differential rate of return between required education and educational mismatch.

Based on the strong assumption that educational mismatch is uncorrelated with unobserved heterogeneity, estimating the wage returns to required education, overeducation and undereducation by pooled ordinary least squares produces results in line with the literature: the return to overeducation is positive and lower than the return to required education. In other words, overeducated workers earn more than their adequately educated coworkers but less than those adequately educated with the same level of education. The reverse reasoning applies to undereducated workers, since years of overeducation and years of undereducation are supposed to have symmetric effects on wages. However, the fixed effects results show that pooled ordinary least squares method is consistent with omitted heterogeneity bias. Indeed, once the sources of time constant unobserved heterogeneity are controlled for, the wage returns associated with overeducation and undereducation are downward and upward biased, respectively, in pooled ordinary least squares.

Returns to required education, overeducation and undereducation are then equally rewarded as human capital hypothesis suggests. This interpretation of the Swiss labour market is consistent with the observed fact that Switzerland has a flexible labour market keeping the rate of human resource utilization high (OECD, 2000).

Two possible further modifications of this paper would be as follows. First, we have assumed that the required education variable was strictly exogenous conditional on unobserved heterogeneity; however if this variable is correlated with the i.i.d. error in some time period, fixed effects are inconsistent and may lead to biased estimates. Therefore, one possibility for improvement would be to apply first differencing and find convincing instruments for required education, the latter of which is far from simple. Second, the estimated specification hinges on the assumption that the returns to overeducation and undereducation are symmetrical. Even if the latest meta-analyses of studies on educational mismatch have agreed with this statement, it would be worth testing the robustness of our findings on the basis of a more flexible relationship between overeducation and undereducation.

References

Bauer, T. K. (2002). Educational mismatch and wages: a panel analysis. *Economics of Education Review*, 21(3):221–229.

Becker, G. S. (1964). *Human capital: a theoretical and empirical analysis, with special reference to education.* National Bureau of Economic Research, New York.

Chevalier, A. (2003). Measuring over-education. *Economica*, 70(279):509–531.

Duncan, G. J. and Hoffman, S. D. (1981). The incidence and wage effects of overeducation. *Economics of Education Review*, 1(1):75–86.

Hartog, J. (2000). Over-education and earnings: where are we, where should we go? *Economics of Education Review*, 19(2):131–147.

Kiker, B. F., Santos, M. C., and de Oliveira, M. M. (1997). Overeducation and undereducation: Evidence for Portugal. *Economics of Education Review*, 16(2):111–125.

Korpi, T. and Tåhlin, M. (2009). Educational mismatch, wages, and wage growth: Overeducation in Sweden, 1974-2000. *Labour Economics*, 16(2):183–193.

Leuven, E. and Oosterbeek, H. (2011). Overeducation and mismatch in the labor market. IZA Discussion Papers 5523, Institute for the Study of Labor (IZA).

McGuinness, S. (2006). Overeducation in the labour market. *Journal of Economic Surveys*, 20(3):387–418.

Mendes de Oliveira, M., Santos, M. C., and Kiker, B. F. (2000). The role of human capital and technological change in overeducation. *Economics of Education Review*, 19(2):199–206.

Nielsen, C. P. (2007). Immigrant overeducation: evidence from Denmark. Policy Research Working Paper Series 4234, The World Bank.

OECD (2000). *Economic Survey of Switzerland*. OECD, Paris.

Plümper, T. and Troeger, V. E. (2007). Efficient estimation of time-invariant and rarely changing variables in finite sample panel analyses with unit fixed effects. *Political Analysis*, 15(2):124–139.

Rubb, S. (2003). Overeducation in the labor market: a comment and re-analysis of a meta-analysis. *Economics of Education Review*, 22(6):621–629.

Rumberger, R. W. (1987). The impact of surplus schooling on productivity and earnings. *Journal of Human Resources*, 22(1):24–50.

Thurow, L. C. (1975). *Generating inequality: mechanisms of distribution in the U.S. economy*. Basic Books, New York.

Verdugo, R. R. and Verdugo, N. T. (1989). The impact of surplus schooling on earnings: Some additional findings. *The Journal of Human Resources*, 24(4):629–643.

Weber, S. (2010). Human capital depreciation and education level: Some evidence for Switzerland. Mimeo, Department of Economics, University of Geneva.

Wooldridge, J. M. (2002). *Econometric Analysis of Cross Section and Panel Data*. The MIT Press, Cambridge, Massachusetts.

A Appendix

Table 3: Individuals retained in the empirical analysis

	Wave/Year							
	1/1999		2/2000[a]		3/2001[b]		4/2002[c]	
Selection criteria	No. of i	%	No. of i	%	No. of i	%	No. of i	%
Individual interview completed	7,799	100.0	6,345	100.0	5,756	100.0	4,892	100.0
Working age population	6,297	80.7	5,072	79.9	4,412	76.7	3,680	75.2
Valid information on the national origin	6,182	79.3	5,005	78.9	4,361	75.8	3,649	74.6
Individuals of Swiss origin	4,978	63.8	4,129	65.1	3,604	62.6	3,060	62.6
Employed*	3,904	50.1	3,046	48.0	2,504	43.5	2,000	40.9
Valid information on employment status	3,863	49.5	3,017	47.5	2,485	43.2	1,978	40.4
Salaried employees**	2,764	35.4	1,791	28.2	1,238	21.5	883	18.0
Valid information on occupational category	2,712	34.8	1,741	27.4	1,197	20.8	853	17.4
Individuals in occupation with at least 10 observations	2,702	34.6	1,719	27.1	1,185	20.6	845	17.3
Valid information on potential experience	2,680	34.4	1,707	26.9	1,179	20.5	841	17.2
Valid information on gross hourly earnings	2,033	26.1	1,152	18.2	742	12.9	428	8.7
Final sample	2,033		1,152		742		428	
In %	100.0		56.7		36.5		21.1	

Source : Swiss Household Panel 1999-2002, data are unweighted.

[a] Jointly surveyed in 1/1999 and 2/2000.

[b] Jointly surveyed in 1/1999, 2/2000 and 3/2001.

[c] Jointly surveyed in 1/1999, 2/2000, 3/2001 and 4/2002.

* We exclude individuals who reported being unemployed or not in the labour force.

** We exclude individuals who reported being self-employed or enrolled in education.

Table 4: CNEF conversion scale between levels and years of education

Description	Years of schooling
Primary and lower secondary level	
Compulsory school, elementary vocational training	9
Domestic science course, 1 year school of commerce	10
Upper secondary level	
General training school	12
Apprenticeship	12
Full-time vocational school	12
Maturity (high school)	12
Tertiary level	
Technical or vocational school	15
Higher vocational college	15
University	18
PhD	21

Source: Codebook for CNEF variables in the SHP (2009).

Table 5: Explanatory variables included in the empirical analysis

Continuous variable	Dummy variable	Ref.
Years of actual education (S^a)	**Gender**	
	Men	×
Years of required education (S^r)	Femme	
Potential experience	**Wave/Year**	
(= year of interview - year since started	*1/1999*	×
working without prolonged interruption)	2/2000	
	3/2001	
Potential experience squared	4/2002	

Source: Swiss Household Panel.

Table 6: Individual characteristics: Summary statistics

Wave/Year	1/1999	2/2000	3/2001	4/2002	Total
G_i (women)					
Mean	0.479	0.479	0.491	0.479	0.482
Overall S.D.	0.500	0.500	0.500	0.500	0.500
Between S.D.					0.500
Within S.D.					0.000
X_i (experience)					
Mean	16.361	17.361	18.534	19.084	17.492
Overall S.D.	11.437	11.437	11.139	10.983	11.357
Between S.D.					11.446
Within S.D.					0.925
w_i (CHF/h)					
Mean	3.197	3.312	3.477	3.363	3.316
Overall S.D.	1.450	1.863	1.801	1.224	1.654
Between S.D.					1.386
Within S.D.					0.974
S^a					
Mean	13.160	13.160	13.305	13.453	13.227
Overall S.D.	2.577	2.577	2.612	2.721	2.604
Between S.D.					2.577
Within S.D.					0.000
S^r (mode)					
Mean	12.779	12.786	13.168	13.185	12.915
Overall S.D.	1.845	1.865	2.244	2.164	1.991
Between S.D.					1.803
Within S.D.					0.751
S^r (mean)					
Mean	12.936	12.942	13.292	13.351	13.065
Overall S.D.	1.677	1.698	2.032	2.066	1.823
Between S.D.					1.688
Within S.D.					0.554
Observations	1,152	1,152	742	428	3,474

Source: Swiss Household Panel 1999-2002, data are unweighted.

Table 7: Full regression results: S^r measured with the *mode* procedure

	POLS	FE	RE
$\alpha^r - \alpha^o$	0.017**	0.001	0.013**
	(0.004)	(0.006)	(0.004)
α^o	0.056**	-	0.058**
	(0.003)		(0.004)
β_1	0.020**	0.051**	0.022**
	(0.002)	(0.010)	(0.003)
β_2	-0.000**	-0.001**	-0.000**
	(0.000)	(0.000)	(0.000)
γ	-0.102**	-	-0.100**
	(0.014)		(0.021)
Constant	-0.065	0.521**	-0.053
	(0.055)	(0.122)	(0.070)
Wave dummies	yes	no	yes
Observations	3,474	3,474	3,474
Number of i	1,152	1,152	1,152
Adjusted R^2	0.245		
Overall R^2		0.0785	0.247
Hausman test			14.34**
t test: $s_{i,t+1} = 0$		-0.80	

Standard errors in parentheses, POLS with robust standard errors.

** p<0.05, * p<0.10

Source: Swiss Household Panel, four waves from 1999 to 2002.

Notes: Data are unweighted. Individuals surveyed only once at the first wave are omitted in order to obtain comparable estimates with respect to the fixed effects analysis.

Table 8: Full regression results: S^r measured with the *mean* procedure

	POLS	FE	RE
$\alpha^r - \alpha^o$	0.032**	0.003	0.025**
	(0.005)	(0.009)	(0.006)
α^o	0.047**	-	0.051**
	(0.004)		(0.005)
β_1	0.020**	0.051**	0.022**
	(0.002)	(0.010)	(0.003)
β_2	-0.000**	-0.001**	-0.000**
	(0.000)	(0.000)	(0.000)
γ	-0.101**	-	-0.100**
	(0.014)		(0.021)
Constant	-0.138**	0.501**	-0.114
	(0.055)	(0.140)	(0.072)
Wave dummies	yes	no	yes
Observations	3,474	3,474	3,474
Number of i	1,152	1,152	1,152
Adjusted R^2	0.250		
Overall R^2		0.0813	0.251
Hausman test			17.89**
t test: $s_{i,t+1} = 0$		-0.80	

Standard errors in parentheses, POLS with robust standard errors.
** p<0.05, * p<0.10

Source: Swiss Household Panel, four waves from 1999 to 2002.

Notes: Data are unweighted. Individuals surveyed only once at the first wave are omitted in order to obtain comparable estimates with respect to the fixed effects analysis.

Table 9: Robustness checks: In order to measure required education, occupations are disaggregated on a 2-digit level and another conversion scale between levels and years of education is used (cf Weber, 2010)

	Mode			Mean		
	POLS	FE	RE	POLS	FE	RE
$\alpha^r - \alpha^o$	0.016**	-0.007	0.012**	0.036**	0.015	0.031**
	(0.004)	(0.007)	(0.004)	(0.005)	(0.009)	(0.006)
α^o	0.060**	-	0.063**	0.050**	-	0.054**
	(0.003)		(0.004)	(0.004)		(0.005)
β_1	0.021**	0.051**	0.023**	0.021**	0.050**	0.022**
	(0.002)	(0.010)	(0.003)	(0.002)	(0.010)	(0.003)
β_2	-0.000**	-0.001**	-0.000**	-0.000**	-0.001**	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
γ	-0.109**	-	-0.107**	-0.110**	-	-0.108**
	(0.014)		(0.021)	(0.014)		(0.020)
Constant	-0.115**	0.623**	-0.109	-0.238**	0.362**	-0.236**
	(0.051)	(0.139)	(0.068)	(0.058)	(0.147)	(0.073)
Wave dummies	yes	no	yes	yes	no	yes
Observations	3,474	3,474	3,474	3,474	3,474	3,474
Number of i	1,152	1,152	1,152	1,152	1,152	1,152
Adjusted R^2	0.259			0.265		
Overall R^2		0.0687	0.260		0.0987	0.266
Hausman test			19.98**			12.71**
t test: $s_{i,t+1} = 0$		-0.78			-0.87	

Standard errors in parentheses, POLS with robust standard errors.
** p<0.05, * p<0.10

Source: Swiss Household Panel, four waves from 1999 to 2002.
Notes: Data are unweighted. Individuals surveyed only once at the first wave are omitted in order to obtain comparable estimates with respect to the fixed effects analysis.

Table 10: Robustness checks: In order to measure required education, occupations are disaggregated on a 3-digit level and the CNEF conversion scale between levels and years of education is used (cf Table 4)

	Mode			Mean		
	POLS	FE	RE	POLS	FE	RE
$\alpha^r - \alpha^o$	0.014**	-0.003	0.012**	0.031**	-0.008	0.026**
	(0.004)	(0.009)	(0.005)	(0.005)	(0.012)	(0.006)
α^o	0.058**	-	0.059**	0.047**	-	0.050**
	(0.003)		(0.004)	(0.004)		(0.005)
β_1	0.020**	0.054**	0.022**	0.020**	0.054**	0.021**
	(0.002)	(0.011)	(0.003)	(0.002)	(0.011)	(0.003)
β_2	-0.000**	-0.001**	-0.000**	-0.000**	-0.001**	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
γ	-0.112**	-	-0.110**	-0.110**	-	-0.108**
	(0.015)		(0.022)	(0.015)		(0.022)
Constant	-0.023	0.550**	-0.030	-0.112**	0.613**	-0.102
	(0.053)	(0.159)	(0.072)	(0.054)	(0.187)	(0.074)
Wave dummies	yes	no	yes	yes	no	yes
Observations	3,153	3,153	3,153	3,153	3,153	3,153
Number of i	1,054	1,054	1,054	1,054	1,054	1,054
Adjusted R^2	0.245			0.251		
Overall R^2		0.0727	0.246		0.0653	0.252
Hausman test			15.61**			21.90**
t test: $s_{i,t+1} = 0$		-0.80			-0.79	

Standard errors in parentheses, POLS with robust standard errors.
** p<0.05, * p<0.10
Source: Swiss Household Panel, four waves from 1999 to 2002.
Notes: Data are unweighted. Individuals surveyed only once at the first wave are omitted in order to obtain comparable estimates with respect to the fixed effects analysis.

Table 11: Robustness checks: In order to measure required education, occupations are disaggregated on a 3-digit level and another conversion scale between levels and years of education is used (cf Weber, 2010)

	Mode			Mean		
	POLS	FE	RE	POLS	FE	RE
$\alpha^r - \alpha^o$	0.010**	-0.000	0.009*	0.030**	0.002	0.025**
	(0.005)	(0.009)	(0.005)	(0.006)	(0.012)	(0.007)
α^o	0.065**	-	0.066**	0.053**	-	0.057**
	(0.004)		(0.005)	(0.005)		(0.006)
β_1	0.021**	0.054**	0.023**	0.020**	0.054**	0.022**
	(0.002)	(0.011)	(0.003)	(0.002)	(0.011)	(0.003)
β_2	-0.000**	-0.001**	-0.000**	-0.000**	-0.001**	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
γ	-0.116**	-	-0.114**	-0.117**	-	-0.114**
	(0.014)		(0.021)	(0.014)		(0.021)
Constant	-0.086	0.520**	-0.102	-0.180**	0.495**	-0.178**
	(0.054)	(0.156)	(0.072)	(0.057)	(0.187)	(0.075)
Wave dummies	yes	no	yes	yes	no	yes
Observations	3,153	3,153	3,153	3,153	3,153	3,153
Number of i	1,054	1,054	1,054	1,054	1,054	1,054
Adjusted R^2	0.259			0.264		
Overall R^2		0.0752	0.260		0.0777	0.265
Hausman test			12.77**			14.88**
t test: $s_{i,t+1} = 0$		-0.81			-0.76	

Standard errors in parentheses, POLS with robust standard errors.
** p<0.05, * p<0.10

Source: Swiss Household Panel, four waves from 1999 to 2002.

Notes: Data are unweighted. Individuals surveyed only once at the first wave are omitted in order to obtain comparable estimates with respect to the fixed effects analysis.

Institut de Recherches Économiques et Sociales
Université catholique de Louvain

Place Montesquieu, 3
1348 Louvain-la-Neuve, Belgique

ISSN 1379-244X D/2011/3082/036