DEPARTMENT OF ECONOMICS

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The Impact of Educational Mismatches on Wages: The Influence of Measurement Error and Unobserved Heterogeneity[1]

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Abstract

We investigate the differential impact of alternative combinations of horizontal and vertical educational mismatches on wages. By using panel data for Belgian graduates, we consider the role of unobserved worker heterogeneity. Random measurement error in both types of mismatches is accounted for by adopting instrumental variable techniques. We consistently find that overeducated individuals without field of study mismatch earn less than adequately educated workers with a similar educational background. However, for individuals who are working outside their field of study, such a wage penalty is not always observed once accounting for unobserved heterogeneity and random measurement error. In some cases, field of study mismatch even seems to be financially beneficial to the worker.

Keywords: returns to education, field of study mismatch, overeducation, underemployment, earnings inequality, ability bias

JEL: I24, J24, J31

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1. Introduction

Research shows that, in many countries, a substantial proportion of young people is overeducated at the start of the career – i.e. these young people are employed in jobs requiring less education than the level they attained (Groot & Maassen van den Brink, 2000; McGuinness, 2006). A substantial proportion of young people also fails to find a job matching their field of study (Wolbers, 2003; Robst, 2007a; Verhaest *et al.*, 2015). The relevance of the mismatch subject is indicated by a large number of studies finding that overeducated individuals receive lower earnings (Hartog, 2000; Rubb, 2003) than adequately educated individuals with similar levels of education. More recently, a number of studies showed that also the match with respect to the field of study matters for earnings. For US graduates, Robst (2008) found that the negative impact of overeducation on wages is stronger for those who are employed outside their field of study. Similar conclusions were reached by Støren and Arnesen (2007) on the basis of data for graduates from a large range of European countries. Béduwé and Giret (2011) found similar differences in effects for French vocational graduates on job satisfaction, but not on wages. Finally, Verhaest *et al.* (2011) found more negative wage effects of overeducation for Flemish school leavers when overeducation was combined with horizontal mismatch. As Béduwé and Giret, they did not find a wage effect of horizontal mismatch if not combined with overeducation.

In general, the aforementioned studies thus suggest that in particular a combination of overeducation and mismatch in terms of fields of study is problematic for individuals. This seems logical since both their level and their type of education are not fully utilized in their jobs (Robst, 2008). Within this interpretation, the combination of both types of mismatches is the consequence of labour market allocation problems, either temporary because of job search costs (Dolado *et al.*, 2009) and imperfect information (Jovanovic, 1979), or more permanently because of labour market rigidities such as downward wage rigidity (Thurow, 1975) and lack of mobility (Frank, 1978). While labour market allocation problems may also cause individuals to be overeducated without having a field-of-study mismatch, this may have less severe effects on productivity since only their level of education is not fully utilized. Hence the smaller wage penalty that is associated with this type of mismatch. Another striking finding is the insignificant wage effect of horizontal mismatches among those having the appropriate level of education. This suggests that this type of mismatch concerns no real problem in terms of productivity and is not a consequence of labour market allocation problems. In contrast to the findings on the other types of mismatch, this is in line with Human Capital Theory (Becker, 1964), which assumes that labour markets are able to fully reap the benefits of educational investments. For instance, these individuals may be employed in occupations

that face upward pressure on wages as a consequence of labour demand growth. Or both employers and employees may manage to reshape the jobs of these workers in order to utilize all of their skills.

The number of studies having already investigated the combined impact of horizontal and vertical mismatches yet remains limited. Moreover, each of the aforementioned studies relied on standard cross-sectional regression analysis. The extent to which their conclusions regarding the impact of educational mismatches on wages are correct largely depends on two prerequisites. First of all, individuals with a mismatch need to be similar to those without a mismatch in terms of unobservable characteristics that affect wages (Leuven & Oosterbeek, 2011). If not, some individuals may only be formally matched or mismatched (cf. Chevalier, 2003). For instance, because of lower innate ability or lower quality of education, workers may not be capable to occupy jobs that are formally in line with their level of education. Hence the lower wages of overeducated workers. That this problem of unobserved heterogeneity is real is at least illustrated by a few studies that found that overeducated workers score on average lower on cognitive ability tests (Green *et al.*, 2002; Sohn, 2010). Similarly, individuals may combine a lack of required field-of-study skills with higher levels of innate ability. Hence the finding that horizontally mismatched workers earn as much as their adequately educated counterparts, even if horizontal mismatch, ceteris paribus, affects productivity negatively. A second prerequisite is that the educational requirements of jobs are not measured with error. For instance, random measurement error may cause some individuals to be wrongly classified as mismatched while others may be apparently matched. This may cause the observed earnings differences between matched and mismatched individuals to be much smaller than they truly are. The problem of measurement error is obvious from the finding that different measures of mismatches are only loosely correlated to each other (Battu *et al.*, 2000; Van der Velden & van Smoorenburg, 1999; Verhaest & Omey, 2006; Sellami *et al.*, 2015).

Both unobserved worker heterogeneity and measurement error thus may substantially bias the results on the impact of both horizontal and vertical mismatches on wages. Several studies have already investigated the role of unobserved worker characteristics for the relationship between overeducation and wages by using panel data. Bauer (2002) and Frenette (2004) indeed found that unobserved heterogeneity explains almost the full earnings penalty of overeducation. More recent studies, however, found that unobserved factors explain only half or even less of the wage effect of overeducation (Dolton & Silles, 2008; Korpi & Tåhlin, 2009; Verhaest & Omey, 2012; Mavromaras *et al*, 2013; Pecoraro, 2014). Also cross-sectional studies that included skill-related indicators and test scores found that heterogeneous skills explain only part of the effect of educational mismatches (Chevalier & Lindley, 2009; Sohn, 2010; Levels *et al.*, 2014). With respect to the role of measure-

ment error in educational requirements, evidence is more scarce. A limited number of studies investigated the role of random measurement error for the estimated wage penalty to overeducation by using one overeducation measure as instrumental variable for another. Robst (1994) found a substantially higher wage penalty to overeducation if measurement error was accounted for. Similarly, Dolton and Silles (2008) and Verhaest and Omey (2012) concluded that upward ability bias found in their study was (more than) compensated by the downward bias resulting from measurement error. As far as we know, no studies have already investigated the role of unobserved heterogeneity and random measurement error for the wage impact of horizontal mismatches and its combination with overeducation.

In this paper, we investigate whether and to what extent unobserved heterogeneity and random measurement error explain the differential impact of alternative combinations of mismatches. We rely on data regarding the transition from school to work in Flanders (Belgium). These data are based on large-scale representative surveys for cohorts of 23-year olds with follow-up surveys at ages 26 and/or 29. The longitudinal character of these data enables to apply panel-data techniques to account for unobserved heterogeneity. Further, the data provide several measures on both horizontal and vertical mismatch, also allowing to account for measurement error by means of applying instrumental variable analysis.

The remainder of the paper is structured as follows. First, we give an overview of the used data and explain our empirical methodology. Next, the results are overviewed. We end with a discussion and formulate some final conclusions.

2. Data and methodology

2.1 Data

The analysis is based on the SONAR data regarding the transition from school to work in Flanders. This dataset contains data on three cohorts of about 3000 Flemish young people, which were born in the years 1976, 1978 and 1980 respectively. Each cohort was interviewed at the age of 23. Follow-up surveys were conducted at age 26 for the cohort 1976 and 1978. For the cohort 1976 and 1980, data are also available at the age of 29. The response rates for these follow-up surveys ranged from 60% to 70%. More information about the set-up of the survey can be found in SONAR (2003, 2004, 2005). We construct a panel data set that contains information on the following three time points: (i) the start of the first registered job, (ii) at the age of 26 and (iii) at the age of

29.[6] [7] Information on essential variables such as net wages, job satisfaction and mismatch status is available for these three time points, except if they were for less than one year in their first job at age 26 or 29. We focus in our analysis on individuals with a higher education degree. We exclude those who are self–employed, observations with missing values or extreme values of wages, respondents for which we have only one observations, and those with a change in their level of education or field of study between the observation points. This leaves a sample of 2235 individuals and 5066 observations.

2.2 Model specification

In our analysis, we focus on the effects of vertical and horizontal mismatch on the net wages. Two types of models are estimated in the literature on the impact of overeducation. While some studies estimate a specification that conditions on the level or years of attained educations, others rather condition on required years of education (see Hartog, 2000; Rubb, 2003). While the first specification compares the effects of mismatch for individuals with a similar educational background, the second specification compares individuals that occupy similar jobs. In the fixed-effects models, however, effects of time-constant educational background variables such as the level of education and the field of study are not identified. Essentially, the fixed-effects models investigate the impact of changes in the mismatch status for an individual without change in educational attainment over time. So, in order to have comparable estimates, we condition on years of education and field of study in the random effects model. Several specifications are estimated, with the most general models having the following specifications for the random effects (1.1) and fixed effects (1.2) analyses respectively.

$$\ln Y_{it} = \alpha_1 YOVER_{it} + \beta_1 YEDUC_i + \delta_1 HMM_{\boldsymbol{it}} + \gamma_1 YOVER_{it} * HMM_{\boldsymbol{it}} + \mathbf{X_i \varphi_1} + \mathbf{I_{it} \theta}_1 + \mu_{1i} + \varepsilon_{1it} \quad \textbf{(1.1)}$$

$$\ln Y_{it} = \alpha_2 YOVER_{it} + \beta_2 HMM_{\boldsymbol{it}} + \gamma_2 YOVER * HMM_{\boldsymbol{it}} + \mathbf{I_{it} \theta}_2 + \mu_{2i} + \varepsilon_{2it} \quad \textbf{(1.2)}$$

with Y_{it}= net wages of individual i at time t (with t=1 for start first job, t=2 for observation at age 26, t=3 for observation at age 29), $YOVER_{it}$= years of overeducation, HMM_{it}= horizontal mismatch, $YEDUC_i$= years of education, X_i= a vector of fixed observed individual characteristics, I_{it}= a vector of observed characteristics that vary within individuals, μ_{1i} or μ_{2i} = an individual random or fixed effect, and error terms ε_{1it} and ε_{2it}. By assuming fixed effects instead of random

6 For individuals without jobs at the time of the interview, information was gathered referring to the end of their last job.

7 The situation at age 23 is not included since information about the mismatch status and net wages are not available for all individuals and cohorts.

effects, we account for the eventual correlation of the mismatch and other observed variables with unobservable individual variables such as ability and motivation. For both the fixed-effects and random-effects model, we distinguish three specifications that differ with respect to the mismatch variables that are included: (i) in a first specification we only include $YOVER_{it}$, allowing to compare our results with other studies that did not account for horizontal mismatches[8]; (ii) in a second model we add HMM_{it} and (iii) in a last specification we also include the interaction term between the two types of mismatch (cf. equation (1.1) and (1.2)). To investigate the extent to which random measurement error in mismatch results in biased outcomes, equations (1.1) and (1.2) are estimated relying both on standard panel-data estimators and on instrumental-variable (IV) panel-data estimators. For our IV estimators, we use one or more measures of mismatch as instruments for a benchmark measure. In a first IV procedure, only years of overeducation is assumed to be measured with error. Hence, $YOVER_{it}$ and $YOVER*HMM_{it}$ are instrumented. A second procedure also accounts for measurement error in horizontal mismatch and thus assumes also HMM_{it} to be endogenous. Below, we discuss our measures of mismatch that are used as benchmark or as instrument in more detail.

2.3 Measurement of overeducation

The literature distinguishes several types of methods for the measurement of overeducation (see Hartog, 2000, for a discussion). A first method is based on a subjective approach where respondents are asked if they are over-, under- or adequately educated for their job. A second method is an indirect measure of overeducation. Respondents can be asked what is the required level to do or to get the job. Overeducation is then measured by comparing the required level of education with the attained educational level. A third measure is a more objective job analysis measure, which is based on the required educational level according to an occupational classification. A fourth method, realized matched, compares the educational level of the worker with the average or modal educational level of the workers within her occupation.

In our analysis, we rely on job analysis (JA) as main indicator for overeducation since it reflects the core idea of overeducation in the literature (Hartog, 2000). In the SONAR data, each occupation has been coded following the Standard Occupation Classification of Statistics Netherlands (CBS, 2001). The classification contains five functional levels: less than lower secondary (<LS), lower secondary (LS), higher secondary (HS), lower tertiary (LT) and higher tertiary education (HT). The equivalent years of attained and required education are based on the minimal years

8 Given that we focus on higher educated individuals, only a small part of the individuals are undereducated. Therefore, we do not account for years of undereducation.

that are needed to achieve each level: $<$ LS = 6 years, LS = 10 years, HS = 12 years, LT = 15 years and HT = 16 years. Hence, an individual is defined to be overeducated (*OVER=1*) if the educational level exceeds the functional level. Years of overeducation (*YOVER*) are computed by years of attained education minus years of required education for overeducated individuals; for other workers it is set to zero.

To account for random measurement error in our JA measure, we apply an instrumental variable approach, with a direct self-assessment measure (DSA) of overeducation and an indirect self-assessment measure (ISA) as instruments. The first measure is based on the following survey question: 'According to your opinion, do you have a level of education that is too high, too low or appropriate for your job?' We use a dummy for overeducation as instrumental variable. Individuals that indicate that their level of education is too high for the job are defined as overeducated (*OVER=1),* while those with an appropriate level or a lower level of education are defined as having a vertical match (*OVER=0).* The second subjective measure is based on the survey questions regarding the required level to get the job. First, the respondents were asked whether a qualification was required to get their job. If they confirmed this question, they got the following question: 'To get your job, what educational level were you required to have?' We use the same educational classification as for the JA measure to compute years of required education and years of overeducation. Those who answered that no qualification was required to get the job are classified in the functional category $<$LS. In comparison to the JA measure, these two subjective measures have some disadvantages. One of the limitations of the DSA measure is that it does not deliver information regarding the years of overeducation. Second, subjective overeducation measures are likely to be influenced by job satisfaction and probably reflect match satisfaction instead of overeducation. Further, ISA measures relying on a question regarding the required level 'to do' the job can be considered to be more appropriate than those relying on the required level 'to get' the job (see Hartog 2000; Verhaest & Omey, 2006). However, such a measure is not available in the SONAR data for all cohorts and jobs.[9] Nevertheless, both our DSA and ISA measure might serve as useful instrument. As a robustness check, we also execute some estimates with the DSA measure as benchmark and the JA and ISA measures as instrumental variables.[10]

In Table 1, we report descriptives on the incidence of overeducation relying on our three measures. Based on the JA measure, the incidence of overeducation is 52% at the start of the first job. At the age of 26 and 29, the incidence of overeducation is lower and amounts about

9 This measure is not available for the cohort of 1976. For the 1978 cohort, it is only available for first jobs before the age of 23.

10 For this analysis, we include a dummy for overeducation (OVER) instead of years of overeducation (YOVER). Both instruments (JA and ISA) are measured in years of overeducation.

42% and 46% respectively. For the subjective measures, we find at the start of the career an incidence of about 24% based on the DSA measure. The incidence of overeducation decreases to 14% at the age of 26 and to about 12% at the age of 29. This lower incidence of overeducation on the basis of more subjective measures are in line with other studies (see Verhaest & Omey, 2006, for an overview). Relying on the ISA measure, the incidence of overeducation is 28% at the start of the career. At the age of 26 and 29, the incidence of overeducation is lower and amounts 19% and 18% respectively. We also report correlations between the three mismatch indicators for both types of mismatches (see Table 2). We note a relatively low correlation of 0.429 between *YOVER* on the basis of JA and *OVER* on the basis of DSA. This low correlation is in line with other studies (see Battu *et al.*, 2000; Van der Velden & van Smoorenburg , 1999).

Table 1: Descriptive statistics on horizontal and vertical mismatch (average values)

	JA			DSA			ISA		
	Start Career	At age 26	At age 29	Start Career	At age 26	At age 29	Start Career	At age 26	At age 29
YOVER	1.676	1.144	1.218	-	-	-	1.293	0.988	1.082
OVER	0.522	0.422	0.455	0.235	0.140	0.116	0.281	0.193	0.182
HMM	0.253	0.245	0.263	0.161	0.107	0.100	0.466	0.201	0.264

Data source: SONAR 1976 (23), 1976 (26), 1976 (29), 1978 (23), 1978 (26) , 1980 (23) and 1980 (29), own calculations; number of individuals = 2235, number of observations= 5066.

2.4 Measurement of horizontal mismatch

Also for horizontal mismatch, we use a JA approach for our benchmark measure. The CBS classification also defines for each occupation a required field of study. Since the classification was originally developed for The Netherlands, we translated this to the Flemish educational context. Based on the additional information provided by the CBS (CBS, 2001) and information about the required learning outcomes of the various study programs in Flanders, we examined which fields of study match the competencies required for every occupation. We distinguished 177 fields of study and 65 professional disciplines. The 177 fields of study are derived from the question in the survey about the completed field of study. Each field of study was matched with one or more professional disciplines. Individuals with a field of study that is at least to some extent related with their professional discipline are defined as having a horizontal match (*HMM=0*). Individuals with a completely different field of study are defined as having a

horizontal mismatch (*HMM=1*). More detailed information on the construction of this job analysis measure can be found in Sellami *et al.* (2015).

Because random measurement is also unavoidable for this JA indicator, we also instrument horizontal mismatch. As instruments we also use a DSA and an ISA measure. The DSA measure is based on the following survey question: "Was the content of your first (last) (current) job in line with your education?". Here, respondents could choose among: (1) completely, (2) somewhat in line, and (3) not at all in line. The first two answers are defined as a horizontal match and the latter as a horizontal mismatch. The second subjective measure is based on the field of study asked by the employer at the time of hiring to get the job. The respondents who answered affirmatively on the question whether a qualification was required to get their job (cf. supra), also got the following question: 'Which field(s) of study were required by the employer?' We clustered both the reported required fields and the graduate's attained field into 11 broad categories. In the case of correspondence between the broad attained and required field, graduates were assumed to have a match on the basis of ISA. Apart from those reporting one or more required fields, a substantial group did not report any particular required field. While this is evidently not problematic for the employer, it is for the graduate since her field of study is not fully utilized in the job (see Sellami *et al.*, 2015, for a discussion). Therefore, also these individuals are defined to have a horizontal mismatch. Nevertheless, we execute a sensitivity analysis to assess whether this decision affects our conclusions. For this analysis, we keep 'horizontal mismatch' and 'no particular field required' as two separate categories and include separate dummies on each of these two categories as instrumental variables for our JA measure of horizontal mismatch.

As with overeducation, the incidence of horizontal mismatch depends on the applied measure (see Table 1). If measured by means of JA, we find that 25% of the sample is horizontally mismatched at the start of the career. Similar incidences are observed at the age of 26 and at the age of 29. Based on the direct subjective measure we note lower incidences of horizontal mismatches. At the start of the career, about 16% has a horizontal mismatch, while at a later age this incidence further drops to about 11%. The indirect subjective measure conversely delivers a much higher incidence of horizontal mismatch at the start of the career (47%). At the age of 26 and 29, this is clearly lower (20% and 26% respectively). One explanation for the differences between the JA measure and the ISA measure is the fact that 21% of the individuals reported that no field of study was required

As for overeducation, we also note relatively low correlations between the measures of horizontal mismatch; these correlations are around 0.3. Also the correlations between

overeducation and horizontal mismatch are positive, indicating that the those who are overeducated are also more likely to be horizontally mismatched. Given these correlations between the two types of mismatch, the risk on spillover effects when estimating the impact of mismatch on wages is clear, with one type of mismatch eventually picking up part of the effect of the other type of mismatch.

Table 2: Correlations between the different types and measures of mismatch

	OVER (JA)	YOVER (JA)	OVER (DSA)	OVER (ISA)	YOVER (ISA)	HMM (JA)	HMM (DSA)
YOVER (JA)	0.765						
OVER (DSA)	0.345	0.429					
OVER (ISA)	0.342	0.375	0.434				
YOVER (ISA)	0.250	0.377	0.373	0.758			
HMM (JA)	0.191	0.243	0.220	0.264	0.279		
HMM (DSA)	0.230	0.339	0.412	0.320	0.380	0.296	
HMM (ISA)	0.225	0.268	0.294	0.415	0.449	0.262	0.308

Data source: SONAR 1976 (23, 23, 29), 1978 (23, 26) , 1980 (23, 29), own calculations; number of individuals = 2235, number of observations= 5066.

2.5 Wages

The survey question regarding wages differs somehow between the waves and cohorts. For the initial interview (at the age of 23), of the 1976 cohort official net monthly wages were reported in intervals of 124 euro (BEF5000) for lower wages and intervals of 248 euro (BEF10000) for higher wages. In the other surveys, respondents were asked to report their exact net monthly wage. If the respondents refused to answer this question, they got the interval question. For these answers, we used the midpoint of the interval. All answers are converted to hourly wages and deflated on the basis of the consumer price index that is typically used for wage indexation.[11] [12]

2.6 Control variables

We include several control variables in each model. Time-fixed control variables (**X**) are dummies for gender (1 dummy), non–European descent (1), year of birth (2) and broad fields of study (10).

11 Conversion into hourly wages is based on the percentage of employment (full-time=100%), and equalizing full-time jobs to 40 hours/week.

12 Extreme values (two standard deviations above or below the average log) are excluded.

We also include a number of time-varying variables (**I**), namely work experience, work experience squared, tenure, tenure squared, percentage of employment (full-time=100%), cohabiting (1 dummy), having a child (1), observation at age 26 (1) and at age 29 (1), firm size (4), sector of employment (11), public sector (1), shift work (2) and night work (4). A detailed description of each of these variables and their measurement can be found in Appendix B.

Given the fact that some employers pay higher wages than others, some of the results may be responsive to the inclusion of firm characteristics as control variables. Whether these characteristics should be included is dubious since the choice for a particular firm and industry may be endogenous to one's ability to find a good match. Therefore, as a sensitivity analysis, we also estimate a model without controlling for the size of the firm, the sector of employment and for whether the individual is employed in the private or public sector.

3. Estimation results

3.1 Standard panel-data results

In table 3, we report the estimation results on the impact of mismatch on wages, relying on our benchmark measure of mismatch. As mentioned in the methodology section, we distinguish three specifications, depending on the type of mismatch that is accounted for. Further, different estimations are reported depending on whether random or fixed effects are assumed and on whether random measurement error in mismatch is accounted for. We first discuss the results for models that do not account for measurement error. Thereafter, we also turn to the IV estimates. At the end, we also report the results of a number of sensitivity and robustness analyses.

On the basis of specification A (Table 3, column 1 and 2), we find that overeducated workers earn less than adequately educated workers with similar years of education and more than adequately educated workers with similar years of required education.[13] This is consistent with the findings of other studies. Including fixed effects instead of random effects results in a somewhat lower estimated penalty for overeducation, suggesting that part of the penalty to overeducation may result from lower ability levels or lower quality levels of education among overeducated workers.

13 The estimated difference in ln(wages) between overeducated workers and adequately educated workers with similar years of required education can be derived from the random-effects model and is equal to 0.119-0.020=0.098.

Table 3: The impact of educational mismatch on the natural log of wages – benchmark analysis with JA measure

	Standard panel-data estimates				IV panel-data estimates (YOVER endogenous)				IV panel-data estimates (YOVER and HMM endogenous)			
	(1)		(2)		(3)		(4)		(5)		(6)	
	Random effects		Fixed effects		Random effects		Fixed effects		Random effects		Fixed effects	
Specification A												
YEDUC	0.118***	(0.007)			0.113***	(0.007)						
YOVER	-0.020***	(0.001)	-0.013***	(0.002)	-0.054***	(0.004)	-0.044***	(0.008)				
Overall R^2	0.343		0.207		0.300		0.213					
Within R^2	0.336		0.350		0.285		0.303					
Specification B												
YEDUC	0.123***	(0.006)			0.113***	(0.007)			0.113***	(0.007)		
YOVER	-0.019***	(0.001)	-0.013***	(0.002)	-0.056***	(0.004)	-0.049***	(0.008)	-0.061***	(0.007)	-0.069***	(0.016)
HMM	-0.012**	(0.006)	-0.001	(0.011)	0.015**	(0.007)	0.033**	(0.013)	0.050	(0.036)	0.200**	(0.100)
Overall R^2	0.344		0.207		0.292		0.204		0.278		0.133	
Within R^2	0.336		0.350		0.282		0.292		0.271		0.169	
Specification C												
YEDUC	0.119***	(0.006)			0.110***	(0.007)			0.111***	(0.007)		
YOVER	-0.016***	(0.002)	-0.015***	(0.003)	-0.067***	(0.007)	-0.070***	(0.014)	-0.066***	(0.012)	-0.080***	(0.021)
HMM	0.001	(0.007)	-0.006	(0.013)	-0.013	(0.013)	-0.030	(0.025)	-0.024	(0.044)	0.103	(0.117)
YOVER*HMM	-0.007***	(0.003)	0.003	(0.004)	0.018**	(0.008)	0.035**	(0.014)	0.011	(0.015)	0.027	(0.026)
Overall R^2	0.346		0.205		0.279		0.172		0.276		0.141	
Within R^2	0.335		0.350		0.277		0.273		0.273		0.237	

Time-invariant control variables included in the random effects models: dummies for gender, non–European descent, year of birth and fields of study; Control variables in every model: experience, experience squared, tenure, tenure squared, percentage of full employment, dummies for cohabiting, having a child, sector, firm size , night work, shift work , public sector, observation at age of 26, observation at age 29.

YEDUC = years of education, *YOVER* = years of overeducation, *HMM* = horizontal mismatch;

Number of individuals = 2235; number of observations= 5066; *p<0.10;**:p<0.05;***:p<0.01; Standard errors are in parentheses

If also horizontal mismatch is included in the model (specification B), we find similar results for overeducation. Further, we find a significantly negative effect of horizontal mismatch on net wages if random effects are assumed. This result is in line with other studies (e.g., Robst, 2008). If we account for fixed effects, however, we find a statistically insignificant effect for horizontal mismatch. This suggests that the penalty to horizontal mismatch is not real and simply results from adverse unobserved characteristics.

In the last specification (C), we additionally include the interaction term between overeducation and horizontal mismatch. For the overeducated individuals without horizontal mismatch, we now find a slightly larger earnings penalty than on the basis of the two other specifications. For individuals that are employed outside their own study field without overeducation, we do not find any wage effect. As other studies (e.g. Robst, 2008; Støren & Arnesen, 2011), we find a negative interaction effect between horizontal mismatch and overeducation if eventual correlation between mismatch and unobserved heterogeneity is not accounted for (see Table 3, column 1). After controlling for fixed effects, however, this interaction turns out to be statistically insignificant. This suggests that those who combine both types of mismatches have unobserved characteristics that have an adverse effect on wages, resulting in a spurious interaction effect if not adequately accounted for.

Table 4: Wage effects for mismatched workers with average years of overeducation[($)]

	Standard panel-data estimates		IV panel-data estimates - *YOVER* endogenous		IV panel-data estimates - *YOVER* and *HMM* endogenous	
	(1) Random effects	(2) Fixed effects	(3) Random effects	(4) Fixed effects	(5) Random effects	(6) Fixed effects
Overeducated	-0.047***	-0.044***	-0.197***	-0.206***	-0.194***	-0.235***
Horizontal mismatch	0.001	-0.006	-0.013	-0.030	-0.024	0.103
Horizonal mismatch and Overeducated	-0.067***	-0.041***	-0.157***	-0.133***	-0.185***	-0.053

[($)] Computed for workers with conditional average years of overeducation (=2.94 years); derived from estimates on specification C, reported in table 3.

*p<0.10;**:p<0.05;***:p<0.01.

It may be tempting to conclude on the basis of the standard fixed-effects results reported in Table 3 that, along with overeducated workers without horizontal mismatch, also overeducated workers with a horizontal mismatch face a statistically significant wage penalty in comparison to those

without any type of mismatch. However, the extent to which this is true may depend on the number of years of overeducation that is considered. Therefore, in Table 4, we additionally report the wage effects for the following three groups: those combining average conditional years of overeducation (2.94 years) with a horizontal match, those having only a horizontal mismatch and those combining average conditional years of overeducation with a horizontal mismatch. These results confirm that the estimated wage penalty on the basis of the standard fixed-effects estimator is similar for both types of overeducated workers, with wage penalties of 4.4% for average overeducated worker without horizontal mismatch and a wage penalty of 4.1% for those with a horizontal mismatch.

3.2 IV panel-data results

To account for measurement error, we also execute instrumental variable panel-data estimates. In a first model (Table 3; column 3 and 4), we only account for random measurement error in overeducation. On the basis of specification A, we find a much higher wage penalty for overeducation than on the basis of the standard panel-data model. While the fixed-effects model delivers a slightly lower estimated penalty of overeducation, the estimate of 4.4% is still substantially higher than the estimate of 2.0% that was found on the basis of the standard random-effects model. Also this result is in line with other studies (e.g. Dolton & Silles, 2008) and suggests that the downward bias of the estimated penalty resulting from measurement error is higher than the upward bias resulting from unobserved heterogeneity.

On the basis of specification B, we now find a significantly positive effect of HMM if measurement error in overeducation is accounted for. This means that horizontally mismatched individuals earn more than individuals with a similar educational background who work within their own field of study. On the basis of the fixed-effects model, this estimated bonus is more than 3%. This result suggests that the previously found penalty to HMM is spurious and results from inadequate measurement of overeducation. To the extent that overeducation and horizontal mismatch are correlated, the HMM variable thus seems to pick up part of the effect of overeducation because overeducation is imperfectly measured.

Finally, on the basis of specification C, we also investigate whether the effect of HMM differs depending on the degree of overeducation. Also these results differ from the ones previously found in the literature. On the basis of the fixed-effects model, we find an insignificant negative effect for horizontal mismatch and a statistically positive interaction effect between both types of mismatches. If we neglect the insignificance of the HMM coefficient, this would mean that the effect

of HMM is negative for individuals that are employed at their level of education. Further, those having only one year of overeducation seem to experience no HMM penalty (-0.030+0.035≈0), while those having more than one year of overeducation even seem to experience a wage bonus for their HMM. This last conclusion is also evident from Table 4, which reports results for workers with average conditional years of overeducation. While average overeducated workers without HMM are estimated to face a wage penalty of 20.6%, those with a HMM have a wage penalty of 13.3% only. Another remarkable finding is that the fixed-effects estimate of the wage penalty to overeducation with horizontal mismatch is lower than the random-effects estimate, while no such difference in the estimated wage penalty is found with respect to overeducation without horizontal mismatch. This suggests that in particular those who combine both types of mismatches have adverse unobserved characteristics.

The aforementioned estimates did only account for measurement error in overeducation. In a last series of estimates, we also account for measurement error in HMM. The absolute values of the coefficients for HMM are larger than those in the models that do not account for measurement error in HMM. This suggests that measurement error also biases the estimated effects of HMM towards zero. However, the estimated effect of HMM remains, in the case of Specification C, statistically insignificant. We thus do not find convincing evidence on the basis of this specification that those who are only horizontally mismatched earn more than those who face no mismatches. Moreover, as shown in Table 4, once accounting for measurement error in HMM, we also do not find statistically significant evidence that those who combine horizontal mismatch with average years of overeducation earn less than adequately educated workers. At least, neglecting statistical significance, our results suggest that those combining both types of mismatches should have more than 1.94 years of overeducation[14] to face a wage penalty in comparison to those not having any type of mismatch. The estimated wage penalty for those with average years of overeducation and no HMM, on the other hand, further increases to 23.5%. With this wage penalty, these overeducated workers without HMM also earn significantly less than their overeducated counterparts with a HMM. [15]

While IV estimators may deliver unbiased estimates, these estimates are in general less efficient. This problem is further attenuated by the application of fixed-effects estimators instead of random-effects estimators. This may contribute to the insignificance of the effects on HMM. However, additional test results, reported in Table 5, indicate that our IV fixed-effects procedure is both adequate and preferable to the other test procedure. First of all, Hausman tests in general

14 This threshold is calculated by solving for YOVER the following equation: 0.103+(0.027-0.080)*YOVER=0.

15 The difference in wage penalties between these types of workers (-23.5%-(-5.3%)=-18.2%) is statistically significant at the 10% level.

Table 5: Test statistics on the validity and reliability of the IV fixed-effects estimates

Estimation procedure (see Table 3)	YOVER endogenous (4)	YOVER and HMM endogenous (6)
Hausman test - null: IV RE estimates are unbiased		
Specification A	49.8 (0.040)	-
Specification B	56.0 (0.014)	45.7 (0.107)
Specification C	78.6 (0.000)	51.6 (0.045)
Hausman test - null: standard FE estimates are unbiased (#)		
Specification A	20.0 (0.000)	-
Specification B	20.8 (0.000)	11.4 (0.000)
Specification C	10.8 (0.000)	8.6 (0.000)
Sargan Chi² test of over-identifying restrictions		
Specification A	1.2 (0.204)	-
Specification B	1.0 (0.312)	0.4 (0.839)
Specification C	0.5 (0.771)	1.3 (0.740)
F test first stage equation on excluded IV's		
Specification A: *YOVER*	153.1 (0.000)	-
Specification B: *YOVER*	125.5 (0.000)	86.7 (0.000)
Specification B: *HMM*	-	43.7 (0.000)
Specification C: *YOVER*	80.1 (0.000)	70.0 (0.000)
Specification C: *HMM*	-	32.8 (0.000)
Specification C: *YOVER*HMM*	154.2 (0.000)	91.3 (0.000)
Anderson canonical correlations LR statistic (Under-identification)		
Specification A	279.4 (0.000)	-
Specification B	233.4 (0.000)	33.8 (0.000)
Specification C	168.9 (0.000)	35.2 (0.000)
Cragg-Donald Wald F statistic (Weak identification)		
Specification A	153.1	-
Specification B	125.5	8.4($)
Specification C	44.3	5.9(£)
Angrist-Pischke multivariate F test of excluded IV's		
Specification A: *YOVER*	153.1 (0.000)	-
Specification B: *YOVER*	125.5 (0.000)	27.2(0.000)
Specification B: *HMM*	-	13.7 (0.000)
Specification C: *YOVER*	31.7 (0.000)	10.7 (0.000)
Specification C: *HMM*	-	7.3 (0.000)
Specification C: *YOVER*HMM*	61.1 (0.000)	9.5 (0.000)

(#) Based on augmented regression; ($) Critical values for the CD test in the case of two endogenous variables and four IV's (see Stock and Yogo, 2005): 11.04 (5% bias), 7.56 (10% bias), 5.57 (20% bias), and 4.73 (30% bias); (£) Critical values for the CD test in the case of three endogenous variables and six IV's (see Stock and Yogo, 2005): 12.20 (5% bias), 7.77 (10% bias), 5.35 (20% bias), and 4.40 (30% bias); P-values in parentheses.

reject the null hypotheses that the random-effects estimators and the standard panel-data estimators deliver unbiased estimates. Only in the case of specification B and assuming that both YOVER and HMM are endogenous, we do not reject the null hypothesis that the random effects IV specification estimates are unbiased. However, the null hypothesis is rejected in this case if we restrict our test to the coefficients for the two mismatch variables[16]. Further, on the basis of the Sargan test of over-identifying restrictions, we can never reject the validity of the instruments even if evaluated at relatively high threshold p values. Finally, F tests on the first stage regressions reject the null hypothesis that the instruments have no impact on the endogenous variables and the test statistics largely exceed the rule of thumb value of 10 which is in general used to differentiate between weak and strong instruments (see Stock *et al.*, 2002).

Along with these often-used statistics, several other tests can be used to evaluate the strength of instruments. First, we report the Anderson canonical correlations likelihood ratio statistic, which tests the null that the model is under-identified. As shown in Table 5, no signs of under-identification are detected. Second, to test for weak identification, we also report the Cragg-Donald (CD) Wald statistics and Angrist-Pischke (AP) multivariate F test statistics. In contrast to the standard F statistic, these alternatives account for the fact that some of our models contain multiple endogenous variables. The CD statistic delivers an overall test of weak identification, while the AP test delivers separate statistics for each endogenous variable. No problems of weak identification can be detected if we only account for measurement error in YOVER. The same is true if we also account for measurement error in HMM but not include an interaction term between both types of mismatches (specification B). While the CD statistic is equal to 8.43 in this case, this is above the 7.77 critical value for 10% maximal bias of the IV estimator relative to the OLS estimator (see Stock and Yogo, 2005)[17]. Only in the interaction specification case (specification C) with both YOVER and HMM being considered to be endogenous, we cannot reject the hypothesis of 10% maximal relative bias. However, the CD statistic of 5.9 remains above the critical value for 20% maximal relative bias of 5.35. The additional AP tests suggest that this 20% maximal bias applies to the HMM coefficient and not to those of YOVER and the interaction effect between YOVER and HMM. Although this outcome requires some caution, we consider this to be only a small bias that is tolerable.

3.3 Sensitivity analyses

We end with a few sensitivity analyses. As a first sensitivity analysis, we also estimate a model

[16] This alternative Hausman test delivers a chi^2 value of 6.3, with a corresponding p value (2 d.f.) of 0.043.

[17] Also the Stock *et al.* (2002) rule of thumb of 10 in the case of single endogenous variables is based on a maximal relative bias of 10%.

without controlling for the sector of employment and the size of the firm. The results of this analysis are presented in Appendix A. While the coefficients differ slightly, our major conclusions remain unaffected. In a second sensitivity analysis, we re-estimate the full model with a dummy for 'no particular field required' in the case of the ISA measure as additional instrument. Appendix B presents the results obtained from this analysis. Also these results also largely consistent with those presented earlier.

Table 6: Wage effects for mismatched workers - DSA Measure[($)]

	Standard panel-data estimates		IV panel-data estimates - *YOVER* endogenous		IV panel-data estimates - *YOVER* and *HMM* endogenous	
	(1) Random effects	(2) Fixed effects	(3) Random effects	(4) Fixed effects	(5) Random effects	(6) Fixed effects
Overeducated	-0.061***	-0.045***	-0.276***	-0.157***	-0.236***	-0.140**
Horizontal mismatch	-0.004	-0.004	0.062***	0.056*	0.262***	0.403**
Horizonal mismatch and Overeducated	-0.113***	-0.068***	-0.232***	-0.156***	-0.261***	-0.145***

[($)] Derived from estimates on specification C, reported in Appendix C.

*: p<0.10;**: p<0.05;***: p<0.01.

We also investigate whether our results differ if we use the DSA measure as main indicator and the JA and ISA measures as instrumental variables. In Table 6, we report the results on the wage effects for the different types of workers (see Appendix C for more extended estimation results). Based on the standard fixed-effects panel estimates, we now find a wage penalty of 4.5% for overeducated workers without HMM and a wage penalty of 6.8% for those with a horizontal mismatch. These results are similar to the findings in our benchmark analysis, although the estimated effect for the latter is somewhat higher (cf. Table 4). The IV analysis delivers more pronounced differences in comparison to our benchmark analysis. If we only account for measurement error in overeducation, we find that both types of overeducated workers face a similar wage penalty of about 16%. In the benchmark analysis, the estimated penalties for average overeducated workers were 21% and 13% for those without and those with HMM respectively. Finally, if we also account for measurement error in HMM, we find a substantial and statistically significant wage bonus in the case of HMM without overeducation. While also our benchmark analysis delivered a wage bonus for this group of workers, the estimate was not statistically significantly different from

zero. Moreover, the estimated wage penalty to overeducation is again relatively similar for both types of workers (14% and 15%). In our benchmark analysis that relied on JA, the estimated penalty for those with HMM was not statistically significantly different from zero either.

As in our benchmark analysis, we also execute a number of tests on the reliability and validity of our instruments when using the DSA measure as main indicator (see Appendix D). The results on the Sargan test reveal that, in the case of specification C, we cannot reject the null hypothesis that the instruments are uncorrelated with the error term when accounting both for measurement error in overeducation and in field of study mismatch. However, evaluated at the 10% significance level, the null is rejected for specification B. Further, if we only account for measurement error in overeducation, the instruments pass the test of both no under-identification and no weak identification (see Anderson canonical correlations likelihood ratio statistic and Cragg-Donald Wald F statistic). However, if we also account for measurement error in HMM, both models (specification B and specification C) seem to suffer from weak instrument bias since the CD test statistics are below the critical values for 30% maximal relative bias.

4. Discussion

Earlier studies on the relationship between mismatch and wages have mainly focused on the wage effects of vertical educational mismatches (over- and undereducation). In line with some more recent studies, we investigated the impact of alternative combinations of vertical and horizontal mismatches. The main contribution of our study lied in investigating whether and to what extent unobserved heterogeneity and measurement error explain the differential impact of alternative combinations of mismatches. We investigated the role of unobserved worker characteristics by using panel data on Flemish highly educated graduates. By using an IV approach, with one mismatch measure as instrumental variable for another, we accounted for random measurement error. In our benchmark analysis, we used a JA approach for our main measure of mismatch and DSA and ISA measures as instruments. In an additional sensitivity analysis, we used DSA as main measurement instead.

Our results based on the standard random-effects estimates (cf. Table 3, column 1) are consistent with the findings of other studies relying on cross-sectional data (Robst, 2008; Støren & Arnesen, 2011). We found that the negative effects of overeducation are the strongest for those who are employed outside their field of study. Also the finding that those with a horizontal mis-

match but an appropriate level of education earn at least as much as adequately workers with a similar educational background was previously found in the literature.

However, once accounting for unobserved heterogeneity and measurement error in both types of educational mismatches, some of these conclusions substantially change. First of all, based on our fixed-effects IV estimates, we did not find that those with a field-of-study mismatch face a stronger wage penalty to overeducation than those without field-of-study mismatch. On the contrary, on the basis of our benchmark analysis, with JA as main measure for mismatch, we even found those combining field-of-study mismatch with moderate levels of overeducation earning no less than those without any type of mismatch. Secondly, on the basis of an additional analysis with DSA as main measure for mismatch, those having a field-of-study mismatch without being overeducated were found to earn more than those without any type of mismatch. Only those being overeducated without having a field-of-study mismatch were thus consistently found to face a wage penalty in comparison to those without any type of mismatch. Moreover, this wage penalty was found to be much higher than the one estimated on the basis of the standard random-effects model.

The difference in outcomes between the standard random-effects model and those on the basis of the IV fixed-effects specification seems to be attributed to three different types of biases. First of all, assuming fixed effects instead of random effects reduced the estimated wage penalty for those combining both types of mismatches. This suggests that those combining both types of mismatches are negatively selected in terms of unobservables. Secondly, measurement error in overeducation provides another source of bias. Once accounting for this type of error in our benchmark analysis, the estimated interaction effect between both types of mismatches turned positive. A likely explanation is that, if measurement error in overeducation is not accounted for, horizontal mismatch picks up part of the effect of overeducation because of the correlation between both types of mismatch (cf. Table 3.2). Thirdly, also measurement error in HMM matters. This is easiest illustrated by the results on our specification without interaction effect between overeducation and horizontal mismatch (specification B); accounting for measurement error in both types of mismatch resulted in a much more sizeable estimated HMM bonus in comparison to the estimates that only accounted for measurement error in overeducation.

Overall, we thus do not find that a HMM is associated with a wage penalty and it thus seems not to be problematic for those who are treated. On the contrary, in some cases, it even seems to be associated with a wage bonus. As explained in the introduction, a potential explanation may be that these individuals are employed in labour market segments that face labour shortages, resulting in upward wage pressure. This may compensate eventual negative effects resulting from skill

underutilization. Or, related to this, these individuals may accept a horizontal mismatch because these jobs offers them compensating wage advantages. In line with these arguments, earlier studies have indeed shown that better pay and promotion opportunities may be a reason to accept a job that is not related with their educational level (Bender & Heywood, 2006) or field of study (Robst, 2007b).

However, it seems that this result is measurement specific. The wage bonus that is associated with HMM for individuals without overeducation seems only significant in the sensitivity analysis that relied on DSA as main measure. Yet, there may be some doubts about the validity of the results based on the DSA. On the basis of the Sargan test, we rejected the null hypothesis that our instruments are exogenous if we accounted for measurement error in overeducation. Further, the C-D statistic showed that there may also be some problem of weak identification in our analysis relying on DSA as main measure. Conversely, the analyses relying on JA as main measure did not deliver indications of unreliable and invalid instruments. Hence, it seems preferable to rely primarily on the outcomes of our benchmark analysis. Nonetheless, some caution is recommended and more research on this issue would be welcome.

5. Conclusion

Overall, this study indicates that horizontal mismatch is less a problem than overeducation. In line with previous studies, we found that overeducated individuals without field-of-study mismatch earn much less than adequately educated workers with a similar educational background. This suggests that this type of mismatch is rather an involuntary choice, for instance because of a lack of jobs at their own educational level. However, for individuals who are working outside their field of study, this seems not to be the case; some of them even seem to earn a wage premium. This suggests that such a mismatch is much more often voluntary, for instance because employers face labour shortages for these jobs and therefore offer higher wages. Nevertheless, regarding the issue which individuals with a HMM face a wage premium, our results were not stable and depended on the choice of the method for measuring mismatches. Therefore, more research relying on other datasets would also be useful.

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Appendix A: The impact of educational mismatch on the natural log of wages - without firm characteristics as control variables

	Standard panel-data estimates				IV panel-data estimates (YOVER endogenous)				IV panel-data estimates (YOVER and HMM endogenous)			
	(1)		(2)		(3)		(4)		(5)		(6)	
	Random effects		Fixed effects		Random effects		Fixed effects		Random effects		Fixed effects	
Specification A												
YEDUC	0.118***	(0.006)			0.117***	(0.007)						
YOVER	-0.019***	(0.001)	-0.014***	(0.002)	-0.048***	(0.003)	-0.040***	(0.006)				
Overall R^2	0.335		0.209		0.283		0.166					
Within R^2	0.333		0.338		0.283		0.296					
Specification B												
YEDUC	0.124***	(0.007)			0.116***	(0.007)			0.115***	(0.007)		
YOVER	-0.019***	(0.001)	-0.014***	(0.002)	-0.052***	(0.004)	-0.045***	(0.007)	-0.060***	(0.007)	-0.068***	(0.016)
HMM	-0.009	(0.006)	-0.001	(0.011)	0.025***	(0.007)	0.037**	(0.014)	0.092**	(0.037)	0.237**	(0.105)
Overall R^2	0.320		0.182		0.276		0.160		0.246		0.096	
Within R^2	0.326		0.330		0.278		0.285		0.257		0.127	
Specification C												
YEDUC	0.125***	(0.007)			0.113***	(0.007)			0.112***	(0.008)		
YOVER	-0.016***	(0.002)	-0.016***	(0.003)	-0.065***	(0.007)	-0.067***	(0.013)	-0.070***	(0.012)	-0.082***	(0.022)
HMM	0.003	(0.007)	-0.006	(0.013)	-0.010	(0.013)	-0.029	(0.025)	0.051	(0.045)	0.123	(0.119)
YOVER*HMM	-0.007***	(0.003)	0.003	(0.004)	0.022***	(0.008)	0.037**	(0.014)	0.019	(0.016)	0.032	(0.026)
Overall R^2	0.322		0.180		0.259		0.131		0.239		0.101	
Within R^2	0.325		0.331		0.273		0.265		0.257		0.166	

Time-invariant control variables included in the random effects models: dummies for gender, non–European descent, year of birth and fields of study; Control variables in every model: experience, experience squared, tenure, tenure squared, percentage of full employment, dummies for cohabiting, having a child, night work, shift work, observation at age of 26, observation at age 29.

YEDUC = years of education, *YOVER* = years of overeducation, *HMM* = horizontal mismatch;

Number of individuals = 2235; number of observations= 5066; *p<0.10;**:p<0.05;***:p<0.01; Standard errors are in parentheses.

Appendix B: The impact of educational mismatch on the natural log of wages – with 'No Particular field required' dummy as additional instrument

	IV panel-data estimates (*YOVER* and *HMM* endogenous)			
	Random effects (5)		Fixed effects (6)	
Specification B				
YEDUC	0.113***	(0.007)	-	-
YOVER	-0.060***	(0.013)	-0.070***	(0.016)
HMM	0.043	(0.060)	0.205**	(0.100)
Overall R^2	0.282		0.131	
Within R^2	0.274		0.161	
Specification C				
YEDUC	0.111***	(0.007)	-	-
YOVER	-0.066***	(0.012)	-0.077***	(0.022)
HMM	0.016	(0.042)	0.119	(0.114)
*YOVER*HMM*	0.013	(0.015)	0.022	(0.025)
Overall R^2	0.277		0.141	
Within R^2	0.275		0.200	

YEDUC = years of education, *YOVER* = years of overeducation, *HMM* = horizontal mismatch.

Time-invariant control variables included in the random effects models: dummies for gender, non–European descent, year of birth and fields of study; Control variables in every model: experience, experience squared, tenure, tenure squared, percentage of full employment, dummies for cohabiting, having a child, sector, firm size , night work, shift work, public sector, observation at age of 26, observation at age 29.

number of individuals = 2235, number of observations= 5066

p<0.10;:p<0.05;****:p<0.01; Standard errors are in parentheses

Appendix C: The impact of educational mismatch on the natural log of wages – DSA measure of educational mismatch

	Standard panel-data estimates				IV panel-data estimates (OVER endogenous)				IV panel-data estimates (OVER and HMM endogenous)			
	(1)		(2)		(3)		(4)		(5)		(6)	
	Random effects		Fixed effects		Random effects		Fixed effects		Random effects		Fixed effects	
Specification A												
YEDUC	0.129***	(0.007)			0.145***	(0.007)						
OVER	-0.082***	(0.006)	-0.054***	(0.009)	-0.244***	(0.007)	-0.157***	(0.027)				
Overall R^2	0.338		0.188		0.289		0.179					
Within R^2	0.335		0.346		0.269		0.316					
Specification B												
YEDUC	0.128***	(0.007)			0.148***	(0.008)			0.154***	(0.009)		
OVER	-0.073***	(0.007)	-0.052***	(0.009)	-0.286***	(0.023)	-0.186***	(0.036)	-0.348***	(0.054)	-0.271***	(0.103)
HMM	-0.027***	(0.007)	-0.009	(0.010)	0.055***	(0.012)	0.035**	(0.016)	0.142**	(0.063)	0.167	(0.125)
Overall R^2	0.340		0.182		0.269		0.166		0.231		0.113	
Within R^2	0.335		0.330		0.250		0.298		0.214		0.194	
Specification C												
YEDUC	0.128***	(0.007)			0.148***	(0.008)			0.147***	(0.008)		
OVER	-0.061***	(0.008)	-0.045***	(0.011)	-0.276***	(0.031)	-0.157 ***	(0.045)	-0.236***	(0.049)	-0.140**	(0.036)
HMM	-0.004	(0.010)	0.004	(0.014)	0.062***	(0.018)	0.056**	(0.024)	0.262***	(0.089)	0.403**	(0.184)
OVER*HMM	-0.048***	(0.015)	-0.027	(0.019)	-0.018	(0.038)	-0.055	(0.048)	-0.287**	(0.114)	-0.408**	(0.204)
Overall R^2	0.342		0.193		0.272		0.174		0.240		0.098	
Within R^2	0.336		0.347		0.253		0.309		0.219		0.102	

Time-invariant control variables included in the random effects models: dummies for gender, non–European descent, year of birth and fields of study; Control variables in every model: experience, experience squared, tenure, tenure squared, percentage of full employment, dummies for cohabiting, having a child, sector, firm size , night work, shift work , public sector, observation at age of 26, observation at age 29.

YEDUC = years of education, *OVER* = overeducation, *HMM* = horizontal mismatch;

Number of individuals = 2235; number of observations= 5066; *p<0.10;**:p<0.05;***:p<0.01; Standard errors are in parentheses.

Appendix D: Test statistics on the validity and reliability of the IV fixed-effects estimates – DSA measure of educational mismatch

Estimation procedure (see Appendix C)	OVER endogenous (4)	OVER and HMM endogenous (6)
Hausman test - null: IV RE estimates are unbiased		
Specification A	80.2 (0.000)	-
Specification B	87.1 (0.000)	143.0 (0.000)
Specification C	92.0 (0.000)	86.6 (0.000)
Hausman test - null: standard FE estimates are unbiased (#)		
Specification A	16.5 (0.000)	-
Specification B	16.5 (0.000)	7.0 (0.001)
Specification C	8.4 (0.000)	6.2 (0.000)
Sargan Chi² test of over-identifying restrictions		
Specification A	2.7 (0.100)	-
Specification B	2.7 (0.104)	5.0 (0.082)
Specification C	4.6 (0.098)	3.0 (0.395)
F test first stage equation on excluded IV's		
Specification A: *OVER*	153.1 (0.000)	-
Specification B: *OVER*	125.5 (0.000)	86.7 (0.000)
Specification B: *HMM*	-	43.7 (0.000)
Specification C: *OVER*	80.1 (0.000)	70.0 (0.000)
Specification C: *HMM*	-	32.8 (0.000)
Specification C: *OVER*HMM*	154.2 (0.000)	91.3 (0.000)
Anderson canonical correlations LR statistic (Under-identification)		
Specification A	315.8 (0.000)	-
Specification B	279.0 (0.000)	17.6 (0.005)
Specification C	148.8 (0.000)	21.3 (0.003)
Cragg-Donald Wald F statistic (Weak identification)		
Specification A	175.5	-
Specification B	152.8	4.4($)
Specification C	38.7	3.5(£)
Angrist-Pischke multivariate F test of excluded IV's		
Specification A: *OVER*	175.5 (0.000)	-
Specification B: *OVER*	152.8 (0.000)	10.0 (0.000)
Specification B: *HMM*	-	8.2 (0.000)
Specification C: *OVER*	42.4 (0.000)	19.7 (0.000)
Specification C: *HMM*	-	3.2 (0.012)
Specification C: *OVER*HMM*	324.2 (0.000)	4.2 (0.002)

(#) Based on augmented regression; ($) Critical values for the CD test in the case of two endogenous variables and four IV's (see Stock and Yogo, 2005): 11.04 (5% bias), 7.56 (10% bias), 5.57 (20% bias), and 4.73 (30% bias); (£) Critical values for the CD test in the case of three endogenous variables and six IV's (see Stock and Yogo, 2005): 12.20 (5% bias), 7.77 (10% bias), 5.35 (20% bias), and 4.40 (30% bias); P-values in parentheses.