FACULTEIT ECONOMIE
EN BEDRIJFSKUNDE

TWEEKERKENSTRAAT 2
B-9000 GENT
Tel. : 32 - (0)9 – 264.34.61
Fax. : 32 - (0)9 – 264.35.92

WORKING PAPER

The Determinants of Overeducation: Different Measures, Different Outcomes?[1]

Dieter Verhaest[2]

Eddy Omey[3]

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[2] Hogeschool-Universiteit Brussel, Faculty of Economics and Management; Sherppa, Faculty of Economics and Business Administration, Ghent University; Corresponding author: Hogeschool-Universiteit Brussel, Stormstraat 2, B-1000 Brussel, e-mail: Dieter.Verhaest@HUBrussel.be.

[3] Sherppa, Faculty of Economics and Business Administration, Ghent University.

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The determinants of overeducation: different measures, different outcomes?

Dieter Verhaest and Eddy Omey

Abstract

Purpose of the paper - To assess the sensitivity of the estimated determinants of overeducation to the used overeducation measure.

Design/Methodology/Approach - We analyse the determinants of overeducation among Flemish school leavers in their first job by means of probit regression analysis. Overeducation is measured on the basis of job analysis, self-assessments and realised matches.

Findings - Our results demonstrate that the application of different overeducation measures sometimes leads to different outcomes. Only a few variables – for instance the student's academic grade in the final year – are consistently found to be important for the explanation of overeducation. Some outcomes are consistent with the supposition that some indicators actually measure other concepts.

Research limitations/Implications - Further research using job analysis measures that are based on alternative and more recent occupational classifications would be useful.

Practical Implications - Measuring overeducation in various ways is recommendable to make reliable conclusions. At least, a careful consideration of the extent to which the measure used really captures overeducation is needed.

Originality/Value - The application of different measures provides further insight into the overeducation measurement problem.

Keywords - mismatch, overqualification, underemployment, measurement error

INTRODUCTION

Over the last few decades, investment in education has steadily increased in most industrialised countries. Parallel to this evolution have been growing concerns about overeducation. Today, an extensive body of literature on overeducation – typically defined as a situation in which individuals have more education than their job requires for one to perform adequately – exists, with estimates for different countries ranging from 10% to more than 40% of the working population (Groot and Maassen van den Brink, 2000a). Although mostly focusing on the relationship between overeducation and earnings, many articles also pay attention to the determinants of overeducation at the micro level. These studies usually find large differences in overeducation probabilities across workers with alternative profiles. Some factors such as previous labour market experience or higher academic grades are consistently found to be important for the explanation of overeducation at the micro level. However, evidence regarding the importance of several other factors such as gender, social background or the business cycle is much more mixed.

A potential explanation for these divergent outcomes across studies is a lack of uniform operationalisation of the overeducation concept in the literature. Whereas several alternative methods have been applied for the measurement of overeducation (see Groot and Maassen van den Brink, 2000a), data availability usually precludes researchers from basing their within-study inferences on more than one measure. Rare exceptions, studies that analyse the determinants of overeducation on the basis of more than one measure, are the papers by McGoldrick and Robst (1996) and by Giret and Hatot (2001). Both studies clearly note different outcomes across measures for some of the explanatory variables in their model. McGoldrick and Robst, for example, evaluate the relationship between gender and overeducation on the basis of three measures. These measures provide respectively a positive, a negative and a statistically insignificant relationship between being female and the probability of overeducation. This lack of robustness suggests that the outcomes in the literature on the determinants of overeducation might tell more about the applied measurement techniques than about the real factors that explain overeducation for the individual worker.

This paper examines this measurement problem in greater detail. Earlier research already demonstrated that the impacts of over- and undereducation on variables such as earnings, job satisfaction or training participation can depend on the applied measure (see Verhaest and Omey, 2006a). We investigate whether such measurement sensitivity also shows up if overeducation is analysed as a dependent variable. More specifically, we examine whether and how the determinants of overeducation differ with the type of measurement we use. We base our analysis on data for higher educated Flemish school leavers in their first job. These data provide the opportunity to evaluate the determinants of overeducation on the basis of four types of measures that have been used regularly in the existing literature: (1) job analysis, (2) direct self-assessment, (3) the self-assessed required level to get the job, and (4) realised matches.

The article is structured as follows. We start with a discussion of the different measurement methods and their problems. We then present some theoretical explanations for differences in overeducation probabilities across alternative types of workers and formulate the hypotheses that will be tested in the empirical analysis. The data, methodology and results of our empirical analysis are outlined thereafter. We end the paper with some general conclusions.

THE MEASUREMENT OF OVEREDUCATION

An individual i can be defined as being overeducated (OV_i^*) if his or her educational level e_i^* exceeds the required level of education to do his or her job r_i^*:

$$(1.1) \qquad OV_i^* \equiv \begin{cases} 1 & \text{if } e_i^* > r_i^* \\ 0 & \text{otherwise} \end{cases}$$

A fundamental remark about this definition is that formal education is an incomplete measure of human capital. Individuals also differ with respect to informal types of skill acquisition such as experience and on-the-job training. Moreover, individuals might be heterogeneous in their innate ability and the quality of their education[1]. Thus, a clear distinction should be made between educational and skill mismatches (Allen and van der Velden, 2001). In this paper, we use the concept of overeducation in the sense of educational mismatches – that is, when the attained educational level exceeds the required level.

From equation (1.1), it follows that the operationalisation of overeducation requires both a measure for the attained educational level of the worker e^* and the required level of education to do the job r^*. Although observed educational levels may also be subject to some measurement error, the measurement of r^* is much more problematic. Several methods for this measurement have been adopted in the literature. A first method measures r^* by job analysis (JA) (e.g., Rumberger, 1987). This so-called objective approach is based on experts evaluating the level of the schooling required to perform an occupation. A more subjective approach is based on the worker's self-assessment (SA) of the minimal level of education to get (e.g., Duncan and Hoffman, 1981) or to do (e.g., Hartog and Oosterbeek, 1988) his or her job. This method is sometimes labelled indirect self-assessment (ISA) in difference to other methods that measure overeducation on the basis of more direct self-assessments (DSA). Respondents are then simply asked whether they are overeducated for their job (e.g., Groeneveld, 1997) or whether they have skills that are not fully utilised (Halaby, 1994). A last method derives r^* from realised matches (RM). Required education is measured by the average (e.g., Verdugo and Verdugo, 1989) or modal (e.g., Kiker et al., 1997) educational level in an occupation. Chevalier (2003) adopts another approach by defining the objectively overeducated who are not satisfied with their jobs as being genuinely overeducated. Objectively overeducated individuals who nonetheless report being satisfied with their match are classified as being apparently overeducated. Similarly, Büchel (2001) combines the outcomes of an ISA measure with information on occupational status to determine whether someone is actually overeducated.

It is clear that each measure has its shortcomings. First of all, none of the measures is likely to be free of classical random measurement error. The classification of jobs by JA, for example, is not straightforward. In addition, there might be substantial heterogeneity of requirements within a similar job if the classification is based on more aggregate occupations (Halaby, 1994). Alternatively, workers can draw on detailed information of the characteristics of their job in their assessment of the level of education required to do or get the job. However, there is a lack of uniform coding instructions (Hartog, 2000). Some individuals might report current hiring standards or their own skill or educational level (Chevalier, 2003), others the modal educational level of identical workers in the firm. Finally, since the RM measure is also based on an occupational classification, the problems are similar to JA.

Perhaps even more troublesome is that some methods might provide systematically biased estimates of over- and undereducation or might even measure a related but different concept. As Hartog (2000) states, a carefully conducted JA should not lead to any systematic bias. However, this requires a regular update of the classification scheme. If not, a general upgrade of skill requirements due to skill-biased technological change might lead to an overestimation of the incidence of overeducation. Also, methodologies that are based on SA might result in biased estimates. This is obvious for the measure that is based on the assessment of the required level to get the job. These measures provide an indication of what Livingstone (1998) calls the credentials gap. Although this concept is clearly related to overeducation, it is not exactly the same. Employers might, for example, increase hiring standards in response to a cyclical or structural oversupply of educated workers, possibly causing the level required to get the job to deviate from the level required to do the job. Conversely, if the SA measure is based on this last level, the bias is likely to be less severe. Nonetheless, there might still be a problem if individuals tend to inflate the status of their own position (Hartog, 2000) or if they adapt their answers to their personal ambitions and expectations. These biases might also be a problem for measures that are based on direct SA. Moreover, these indicators might measure skill mismatches instead of educational mismatches, particularly if they are based on questions regarding skill utilisation. Finally, the mean or modal educational level might be a good estimate of the educational requirements if mismatches only result from temporary search and information frictions (Verhaest and Omey, 2006b). However, more structural overeducation within occupations will not show up on the basis of RM measures.

That these problems are indeed prevalent in reality is illustrated by the results of the scarce number of studies that measure overeducation on the basis of more than one measure. These studies usually find that the different indicators of overeducation are only loosely correlated to each other (see, e.g., van der Velden and van Smoorenburg, 1999; Groot and Maassen van den Brink, 2000b; Verhaest and Omey, 2006b). A logical explanation for these low correlations is random measurement error, resulting in less precise estimates of the determinants of overeducation. Apart from who is defined to be over-educated, the estimated level of overeducation is also found to depend on the applied methodology. On the basis of a meta-analysis of the literature, Groot and Maassen van den Brink (2000a) note a significantly lower estimate of the incidence of overeducation if measured by RM; this is in line with the statement that these last measures only capture non-structural overeducation. Studies that make their comparisons on the basis of the same dataset also usually find, contrary to Groot and Maassen van den Brink, a substantially higher estimate on the basis of JA compared with SA (see, e.g., Rumberger, 1987; Verhaest and Omey, 2006b). This last finding might be explained both by a systematic underestimation of JA job levels and by a systematic overestimation of SA job levels (cf. supra). Finally, descriptive results by Verhaest and Omey (2006b) show that the socio-economic groups with the highest overeducation incidence also depend on the applied measure. This indicates that the bias of some of the measures is at least partly related to workers' individual characteristics. We can then expect that the estimated determinants of overeducation on the basis of multivariate analyses also depend on the applied methodology.

THEORETICAL EXPLANATIONS AND HYPOTHESES

Before we discuss how the application of different measures of overeducation might lead to different conclusions, we first formulate some hypotheses regarding the determinants of 'true' overeducation. These hypotheses will then serve as a reference for our discussion.

Several factors can explain why some individuals have a higher probability of being overeducated than do others. To begin with, individuals might face different informational, financial, time or spatial constraints in their search for a job. Search theories (Mortensen, 1986) show that these constraints lead to lower reservation wages. Given the positive relationship between wages and the level of required education, the wage maximising behaviour in search models is equivalent to a situation in which individuals seek to maximise the required educational level for their job (Hartog, 2000). Hence, these job search constraints are likely to result in a higher propensity to accept overeducation positions. On the basis of these arguments, we formulate the following hypothesis:

(H1) Search constraints result in a higher probability of being overeducated.

Temporary matching frictions are another potential source of overeducation disparities. Theories on wage rigidities predict that temporary discrepancies in demand and supply lead to adjustments in hiring requirements instead of wages (Thurow, 1975). This demand and supply context can differ along several dimensions: job seekers might offer labour for different segments; they might look for jobs at a different place; or they might enter the labour market at a different time. We test the following hypothesis:

(H2) A higher local unemployment rate leads to a higher probability of being overeducated.

Also, discrimination is likely to result in different overeducation probabilities across alternative types of workers. According to the theory of statistical discrimination (Phelps, 1972), employers use the descriptive profile of job seekers as a proxy for their future productivity. This leads to fewer opportunities for all those individuals with a disadvantageous profile, irrespective of the individual's productivity itself. Taste discrimination (Becker, 1957) might also lead to different labour market opportunities. Whereas standard discrimination theories usually predict differential wages within firms, anti-discrimination policies often prevent this. Still, as Renes and Ridder (1995) show theoretically, this legislation is likely to result in differential overeducation. We test whether:

(H3) Women and school leavers with a Non-Western background are more likely to be overeducated.

Finally, as human capital theories (Becker, 1964) predict, different overeducation probabilities might simply result from actual productivity differences caused by factors other than the level of education. The career mobility theory of Sicherman and Galor (1990) is built on this argument and states that acquired experience helps to escape from overeducation. In the context of imperfect information, employers might also use observable characteristics of the educational career as a signal for general ability (Spence, 1973) or trainability (Thurow, 1975). Thus, we also test whether:

(H4) Overeducation is lower among school leavers with more alternative human capital endowments.

Under the assumption that these hypotheses are correct, one can expect them to be less confirmed by indicators less strictly related to overeducation. Those most closely related to overeducation are the JA measures and the SA measures that are based on the required level to do the job. Although random measurement error might affect the precision of the estimates, it does not directly lead to different hypotheses regarding the determinants of overeducation. Moreover, although the outcomes might still be biased by technological change or by individuals' expectations, it is difficult to predict a priori how this will affect the conclusions regarding our hypotheses. Conversely, the other indicators more clearly represent something other than overeducation and so also have different expected determinants. As employers might adapt the minimal hiring requirements to the local labour market conditions, for example, we can expect that (H2) does not apply to ISA measures that are based on the required level to get the job. Moreover, although it is prohibited to apply differential hiring requirements according to race and sex, employers are still able to take the expected composition of the pool of job applicants into account. For female occupations, for example, they might apply higher hiring standards. Consequently, if there is substantial job segregation by sex and race, (H3) will be rejected on the basis of this measure as well. Additionally, DSA measures might rather provide an indication of skill mismatches, and hence, for these measures, (H4) might not be appropriate. Finally, RM measures only pick up the non-systemic part of overeducation within occupations, and so the impact of the local unemployment rate (H2) is likely to be even more important for the explanation of this concept than for overall overeducation. Conversely, occupational segregation makes RM measures not well-suited to detect differences in overeducation by gender or race (cf. Battu and Sloane, 2002). As a result, (H3) might be rejected on the basis of this measure as well.

DATA AND OVEREDUCATION MEASURES IN THIS STUDY

The empirical analysis is based on two cohorts of the SONAR database regarding the transition from school to work in Flanders. At the end of 1999, about 3000 randomly chosen Flemish 23 year olds (born in 1976) were questioned regarding their educational and early labour market career. Another cohort of 3000 23 year olds were questioned at the end of 2001 (born in 1978). For both cohorts, follow-up surveys were also executed at the age of 26 with respective response rates of 68.3 and 71.2%. By combining the information on both the original and the follow-up surveys, we observed the first standard job for 92.6% of all respondents[2]. A disadvantage of a focus on first jobs is that overeducation might be a rather transitory state for many of the graduates in our sample. Yet, as Dolton and Silles (2003) show, being overeducated for one's first job strongly influences the quality of the match later on. Moreover, there is also some evidence that the differences in the incidence of overeducation across measures in the first job persist in later jobs (see Verhaest and Omey, 2003). Finally, concentrating on the first standard job has the conceptual advantage that educational mismatches to a greater extent correspond to skill mismatches, since these young workers do not yet have any on-the-job training or experience in regular jobs. We base the analysis on those who have completed at least lower tertiary education[3]. This group entered the labour market during the period 1997–2004. Further exclusion of individuals who returned to school after their first job, the self-employed, those who worked less than half-time (i.e., at least 18 hours / week), and observations with missing values results in a final sample of 1938 respondents. A detailed breakdown of our sample can be found in Appendix A. For an extensive description of the survey set-up, see SONAR (2003).

While most studies only apply a single method for the measurement of educational mismatch, our data allow us to assess overeducation relying on JA, SA and RM. The JA measure (JA) is derived from the 1992 Standard Occupation Classification of the Dutch CBS (2001)[4]. This is a detailed classification, based on a five-digit code and five complexity types: elementary, lower, medium, higher and scientific. The equivalent educational levels are: less than lower secondary (<LS), lower secondary (LS), higher secondary (HS), lower tertiary (LT) and higher tertiary education (HT). A problem with this standard JA measure is the gap of 5 to 12 years between the publication of the CBS classification and the labour market entry of the individuals in our sample. Thus, skill-biased technological change might result in an overestimate of the incidence of overeducation. In order to account for this possible bias, we also compute an alternative JA measure (JA-A). For this, we make use of information regarding the computer usage of the 1976 cohort individuals in their first job. These youngsters were asked the following question: 'Do you totally agree, rather agree, rather disagree or totally disagree: my first job required the skills to work with a computer?' Those two-digit CBS occupations in which a majority of the school leavers born in 1976 totally agreed on this question are considered to be upgraded occupations[5]. For these occupations, individuals are defined to be overeducated on the basis of JA-A only if their educational level is at least two levels higher than the original JA complexity level. For the other occupations, the same criterion as that used for the standard JA measure (one level) is used.

For the measurement on the basis of self-assessments, both a direct and an indirect approach are applied. The direct SA (DSA) is based on the question: 'Do you have a level of education which is, according to your own opinion, too high, too low, or appropriate for your job?' As this question makes no direct reference to skill utilisation, our measure is likely to deliver a better indicator for educational mismatches than some of the other DSA measures in the literature. To test whether this indeed the case, we also compute an alternative direct self-assessment measure on the basis of the following question: 'Do you totally agree, rather agree, rather disagree or totally disagree: 'In my first job, I could demonstrate my abilities.'' Those who answered negative on this question are defined to be overeducated on the basis of DSA-A. The indirect SA measure (ISA) is derived from the following question: 'To get your job, what educational level were you required to have?' This question was only asked to those confirming that their job required a qualification. To consider the overeducation status on the basis of this measure, we use the same educational classification as for the JA measure.

Finally, the computation of our RM measure is based on two-digit CBS occupations. We define educational requirements as the modal educational level within an individual's occupation. For this derivation, we make use of all of the available observations in the original and follow-up surveys of first jobs for both cohorts[6]. Quite often, researchers base the derivation of their RM measure on data for the full labour force. As a result, the mean or modal educational level within an occupation does not correspond to the mean or modal human capital level due to the additional experience or training of older workers (Kiker et al., 1997). By only using information on first jobs, we avoid this type of bias. Oppositely, data on new workers are likely to be more sensitive to cyclical developments. However, as our school leavers enter the labour market spread over a period of eight years, this problem applies less to our data. Moreover, this bias should be further eliminated by measuring requirements on the basis of the modal instead of the average educational level. In order to induce a change in the modal educational level within an occupation, a more substantial shift in the composition of new hires would be required. The usage of more aggregate job titles for the derivation of RM requirements is not unusual in the literature,[7] and it guarantees that we keep enough observations within each occupation. This provides more accurate average or modal values and also attenuates biases that are related to the more detailed occupations but not to the aggregate. However, it comes at a cost of more requirement heterogeneity within job titles. As a result, we also compute an alternative measure RM-A that is based on the individual's most detailed occupational title for which we have at least twenty observations in our data[8].

The incidence of overeducation ranges, for the standard definitions, from about 22% on the basis of DSA and ISA to about 49% on the basis of JA for our sample of school leavers (cf. Table 1, column (A)). The relatively high JA overeducation estimate is consistent with the assumption that JA systematically underestimates requirements because of skill-biased technological change. This seems to be confirmed by the results on our alternative JA measure, which provides a substantially lower overeducation incidence of 30.0% (cf. column (B)). However, this incidence is still about 8 pp higher than the DSA or ISA overeducation incidence. Also, the alternative measure DSA-A provides a comparable estimate of about 22%. These relatively low incidence rates on the basis of SA might, amongst others, be explained by a systematic overestimation of requirements due to social desirability bias. Also, the RM and RM-A estimates (33.8% and 30.2%) are substantially lower than the JA estimate[9]. As stated previously, structural overeducation will not show up on the basis of these types of measures.

Table 1: Incidence of overeducation by measurement method

	(A) Standard definitions				(B) Alternative definitions		
	JA	DSA	ISA	RM	JA-A	DSA-A	RM-A
$OV_i^m = 1$	49.1%	21.7%	21.8%	33.8%	30.0%	21.7%	30.2%
$OV_i^m = 0$	50.9%	78.3%	78.2%	66.2%	70.0%	78.3%	69.8%

JA = Job Analysis, *DSA* = Direct Self-Assessment, *ISA* = Indirect Self-Assessment, *RM* = Realised Matches, *JA-A* = Job Analysis, adapted for skill-biased technological change; *DSA-A* = Alternative DSA measure based on question regarding skill utilisation; *RM-A* = Alternative Realised Matches measure based on most detailed job title with at least 20 observations.
Data source: SONAR c76(23), c76(26), c78(23), c78(26), own calculations; N = 1938.

The correlations between these measures, reported in Table 2, further illustrate the measurement problem. The correlations are particularly low between the JA or RM measure and the DSA or ISA measure. Also, the usage of the alternative measure JA-A does not result in much higher correlation with the SA measures. Finally, the alternative measure DSA-A is only weakly correlated with the JA, JA-A, ISA and RM measures. In contrast, and as expected, its association with the standard DSA measure is clearly higher. However, a correlation of 0.46 remains fairly low for indicators that are supposed to measure the same concept. On the whole, this largely confirms the findings of Allen and van der Velden (2001), who note only a weak association between educational and skill mismatches.

Table 2: Spearman rank correlations between the different measures

	JA	DSA	ISA	RM	JA-A	DSA-A
DSA	0.386					
ISA	0.369	0.568				
RM	0.727	0.423	0.400			
JA-A	0.666	0.426	0.381	0.730		
DSA-A	0.252	0.455	0.284	0.249	0.263	
RM-A	0.645	0.435	0.415	0.798	0.732	0.267

JA = Job Analysis, *DSA* = Direct Self-Assessment, *ISA* = Indirect Self-Assessment, *RM* = Realised Matches, *JA-A* = Job Analysis, adapted for skill-biased technological change; *DSA-A* = Alternative DSA measure based on question regarding skill utilisation; *RM-A* = Alternative Realised Matches measure based on most detailed job title with at least 20 observations.
Data source: SONAR c76(23), c76(26), c78(23), c78(26), own calculations; N = 1938.

EMPIRICAL SPECIFICATION

The determinants of being overeducated are estimated by means of a probit specification. The first hypothesis regarding the influence of search constraints (H1) is indirectly tested by the inclusion of six variables. First, we include a dummy for whether the school leaver got a study grant during tertiary education. As only students from low-income families are entitled to such a grant, this variable can be seen as a proxy for possible financial constraints. We further include a dummy for whether the school leaver had a driving licence at the school leaving date to account for spatial constraints (cf. Büchel and van Ham, 2003). Another factor that might be related to job search constraints is whether the individual is cohabiting with his or her partner. On the one hand, cohabiting reduces the financial constraints of young job seekers if they can rely on the partner's income. On the other hand, cohabiting individuals are more spatially constrained in their job search. However, as Frank (1978) states, this last effect will mostly come into play for women if the family residence is chosen as a result of the professional career of the man. Hence, we further include a dummy for whether individuals were cohabiting at the time of graduation as well as an interaction term between this variable and gender[10]. To account for differences in time constraints and search intensity, we include a dummy for whether s/he started seeking work before leaving school. Finally, years of education of the father[11] are also included. Highly educated fathers not only are likely to be better informed regarding the job opportunities for school leavers from tertiary education but also probably have more relevant networks that they can provide to their children. Hence, this variable can serve as a proxy for possible information constraints[12]. To account for regional labour market developments (H2), we include the natural logarithm of the average regional unemployment rate during the first six months after leaving school. We further include an extra dummy for gender and one for having a non-Western background to account for possible discrimination (H3). The hypothesis with respect to the influence of additional human capital endowments (H4) is tested on the basis of six variables. First, two dummies are included for the students' grades in the final year: one for graduating with distinction and one for graduating with great or greatest distinction[13]. Another ability-related indicator that is included in the model is the number of repeated years during tertiary education. We further include a dummy for those who obtained their higher tertiary degree at university[14]. University education in Flanders is generally perceived to be more demanding than non-university higher tertiary education, and so we can expect that such a degree results in a lower probability of being overeducated. Finally, dummies for having acquired student work experience and for having acquired work-placement experience during formal education are included to test whether previous work experience plays a role. For summary statistics on these explanatory variables, see Appendix B[15].

To account for further differences in preferences and labour market conditions within the sample, several control variables are included. These variables are dummies for having a higher tertiary education degree (1 dummy), educational subjects (12), sector of employment (12), the region of residence (15) and year of birth (1).

The first standard job is not observed for all tertiary education school leavers in our data, which might cause sample selection bias. More specifically, 81 individuals left tertiary education without having had a first job of at least 18 hours/week by the time of the last survey. To test for this type of selectivity, we estimated the probability of being overeducated simultaneously with the probability of having had a first standard job by the time of the last survey[16]. The estimated covariance of the two error terms was, however, never statistically significant. Hence, we base our discussion regarding measurement sensitivity on simple univariate probit model estimates. The results on the basis of the bivariate probit selection models can be found in Appendix C.

RESULTS

We start our overview with the estimation results on the four standard measures of overeducation (cf. Table 3, column (A))[17]. To make the results comparable across the different measures, we report average marginal effects instead of probit coefficients[18]. Some factors are clearly not relevant for the explanation of overeducation regardless of the standard measure being used. The dummy for student work experience, for example, is never statistically significant. This finding sharply contrasts with those of studies that investigate the influence of overall labour market experience and that usually identify a negative relationship between labour market experience in standard jobs and the probability of being overeducated (see, e.g., Sloane et al., 1996). Experience by means of student work most likely does not match with the content of future jobs. Another explanation is that those with student work experience have a lower valuation of leisure and thus a higher inclination to accept lower-level jobs. Also, having a driving licence and

Table 3: The determinants of overeducation in the first job: average probit marginal effects estimates and robust standard errors (in parentheses)

	(A) Standard definitions								(B) Alternative definitions					
	JA		DSA		ISA		RM		JA-A		DSA-A		RM-A	
Study grant during tertiary education	0.016	(.022)	0.050**	(.022)	0.017	(.017)	0.008	(.022)	0.007	(.021)	0.047*	(.027)	0.035	(.023)
Having a driving licence	-0.014	(.026)	0.006	(.025)	-0.027	(.024)	0.010	(.026)	-0.005	(.024)	-0.006	(.023)	0.001	(.024)
Cohabiting with partner	-0.044	(.049)	-0.053	(.052)	-0.013	(.059)	-0.038	(.044)	-0.048	(.051)	-0.015	(.055)	-0.115***	(.044)
Start job search before leaving school	-0.044**	(.018)	-0.024	(.014)	-0.013	(.016)	-0.058***	(.020)	-0.042*	(.022)	0.005	(.019)	-0.066***	(.019)
Years of education father	-0.009**	(.004)	-0.005	(.003)	-0.005*	(.003)	-0.009***	(.003)	-0.010***	(.003)	-0.001	(.003)	-0.010***	(.003)
LN (regional unemployment rate)	0.080	(.054)	0.034	(.049)	0.037	(.058)	0.109**	(.048)	0.159***	(.045)	0.051	(.067)	0.163***	(.053)
Female	0.043*	(.022)	-0.003	(.020)	0.005	(.020)	-0.035*	(.020)	0.007	(.022)	0.035	(.024)	-0.025	(.018)
Female*Cohabiting with partner	0.049	(.064)	0.031	(.067)	0.021	(.068)	0.037	(.059)	0.055	(.063)	-0.015	(.061)	0.109*	(.062)
Non-Western background	0.018	(.041)	0.092*	(.054)	0.017	(.053)	-0.087	(.053)	-0.089	(.055)	0.104*	(.061)	-0.052	(.052)
Graduated with distinction grade	-0.050**	(.025)	-0.043**	(.019)	-0.075***	(.014)	-0.070***	(.020)	-0.054**	(.023)	-0.044**	(.022)	-0.070***	(.019)
Graduated with great or greatest dist. g.	-0.106***	(.033)	-0.078***	(.027)	-0.089***	(.029)	-0.096***	(.032)	-0.101***	(.032)	-0.028	(.034)	-0.091**	(.038)
Repeating years	0.027	(.017)	0.041***	(.014)	0.034**	(.015)	0.022*	(.012)	0.044***	(.014)	0.045***	(.017)	0.033**	(.015)
University degree	-0.132***	(.041)	-0.036	(.036)	-0.063**	(.026)	-0.037	(.034)	-0.058*	(.033)	-0.106***	(.022)	-0.050*	(.028)
Student work experience	-0.002	(.038)	0.022	(.033)	-0.005	(.032)	0.008	(.033)	-0.022	(.034)	0.052	(.035)	0.022	(.033)
Work-placement experience	0.001	(.029)	-0.013	(.024)	-0.013	(.029)	-0.037*	(.022)	-0.017	(.025)	-0.019	(.028)	-0.051**	(.023)
Wald Chi² (56)	4957.53***		3508.40***		2737.67***		5500.38***		2998.50***		996.34***		2869.11***	

Also included but not reported: intercept, dummies for sector of employment (12 dummies), region of residence (15), educational subject (12), higher tertiary education level (1), and age cohort (1). Standard errors are adjusted for clustering on region – month of labour market entry pairs (113 clusters);
JA = Job Analysis, *DSA* = Direct Self-Assessment, *ISA* = Indirect Self-Assessment, *RM* = Realised Matches, *JA-A* = Job Analysis, adapted for skill-biased technological change; *DSA-A* = Alternative DSA measure based on question regarding skill utilisation; *RM-A* = Alternative Realised Matches measure based on most detailed job title with at least 20 observations;
For dummy variables, the effects represent a discrete change from 0 to 1;
*: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$; N = 1938.

cohabiting with a partner are not found to be decisive factors for the match status of the first job. Academic achievement equally is consistently found to be relevant for the explanation of overeducation; graduating with distinction or great distinction leads on the basis of every measure to a significantly lower probability to be overeducated. This outcome largely corroborates previous findings in the literature regarding the influence of grades (see, e.g., Battu et al., 1999; Büchel and Pollmann-Schult, 2004).

For the other factors, outcomes are sensitive to the type of measure. Those who start their search activity before graduation, for example, have a significantly lower probability of being overeducated if measured by JA and RM. For the DSA and ISA measure, however, the effect is statistically insignificant. Likewise, those with a study grant during tertiary education are only found to have a higher likelihood of being overeducated if measured by DSA. Then again, fathers' education has a statistically significant effect on overeducation if measured by JA, ISA or RM. These last outcomes are similar to those of Giret and Hatot (2001); they note a significantly lower probability of being overeducated five years after leaving school among those with a highly educated father if measured by JA, but an insignificant effect on the basis of DSA. With respect to the unemployment rate, the effect is only statistically significant on the basis of RM. Also, the literature provides only limited evidence on the influence of cyclical factors. Studies by Büchel and van Ham (2003), Dolton and Silles (2003) and Lassibille et al. (2001), which were based on JA or ISA measures, all note an insignificant effect of the unemployment rate on the probability of being overeducated. However, in line with our findings on the basis of RM, Devereux (2002) finds that the educational levels of new hires within occupations are higher during recessions. Another factor that provides inconsistent results is the gender of the school leaver. In line with our expectations, women are more likely to be overeducated if measured by JA. However, the opposite outcome is noted on the basis of RM. The fact that these outcomes are not necessarily accidental is illustrated by the studies of Giret and Hatot (2001) and McGoldrick and Robst (1996), who note a very similar relationship between the applied measurement method and the estimated gender effect. With respect to ethnicity, we find a similar unexpected outcome on the basis of RM, although the effect is barely not statistically significant at $p < 0.10$. On the other hand, graduates with non-Western backgrounds are more likely to be overeducated on the basis of DSA. Finally, the conclusions with respect to the university degree dummy, the number of repeating years and work-placement experience are also measurement-specific. The number of repeated years is found to be related to overeducation if measured by SA or RM but not if measured by JA.

However, the university degree coefficient is only statistically significant if overeducation is measured by JA or ISA, whereas work-placement experience only has an impact on RM overeducation.

These results clearly illustrate that the application of different measures does not necessarily lead to similar conclusions[19]. To what extent do our outcomes now fit with the supposition that the measures actually measure related but different concepts? First, we assumed that JA measures clearly reflect the overeducation concept. Several hypotheses regarding the ‘true’ determinants of overeducation are indeed confirmed. Moreover, most of the coefficients have the expected signs. Nevertheless, there remain some hypotheses such as the one with respect to the local unemployment rate that are rejected. There is perhaps still some bias in this measure, for example, as a consequence of skill-biased technological change. At first sight, this seems to be confirmed by the results with alternative measure JA-A, which provides a significantly positive relationship between the local unemployment rate and overeducation (cf. Table 3, column (B)). Moreover, the effect of the number of repeated years is also statistically significant. However, the gender effect is statistically insignificant. Of course, this outcome does not necessarily imply that the standard JA measure provides an unbiased estimate of overeducation. Nevertheless, there is no decisive evidence that our alternative JA-A measure provides a more accurate and unbiased measure for overeducation.

We further hypothesised that DSA measures reflect skill mismatches rather than educational mismatches. Contrary to the JA measure, this measure indeed provides no evidence on the impact of a university degree. However, this is not the case for the number of repeated years and for grades earned. A probable explanation for these last outcomes is the existence of labour market rigidities, which result in a lack of suitable job offers. Rather moderate differences in skills, reflected by different grades and repeating years, might then result in more pronounced differences in labour market opportunities, as they decide one’s place in the job queue. This seems to be confirmed by the results on our alternative measure DSA-A, which provides very similar effects for the number of repeated years and the distinction grade dummies. For some other variables, however, results diverge between the two DSA measures. We find, for example, that a great distinction grade results in a higher probability of being overeducated if measured by DSA but not if measured by DSA-A. However, having a university degree decreases the probability of overeducation on the basis of DSA-A, but not on the basis of DSA. Given these divergent outcomes, it seems that our standard DSA indicator measures something other than skill utilisation. An alternative explanation is that the DSA measure reflects the expectations and ambitions of the individuals. Female school leavers, for example, might have lower expectations and as a result might assess themselves less often as being overeducated. That this is not unlikely is illustrated by the literature on job satisfaction, which also regularly finds that women are more satisfied than men if they work in similar jobs (see, e.g., Clark, 1997). Similarly, those with a non-university degree might also have lower expectations regarding their labour market opportunities.

Finally, our other two standard indicators were also supposed to measure something other than overeducation. The ISA measure that is based on the required level to get the job, for example, measures the so-called credentials gap, whereas RM measures provide only an indication of the non-structural part of overeducation within occupations. As expected, the effect of the local unemployment rate is found to be insignificant on the basis of ISA and significantly positive on the basis of RM. Moreover, the rejection of (H3) with respect to the influence of discrimination on the basis of these measures might be the result of segregation by sex and race. Nevertheless, finding that women actually have a lower probability of being overeducated remains somehow strange. Perhaps, as McGoldrick and Robst (1996) state, this is the result of requirement heterogeneity within occupations and simply indicates that, even within occupations, men take the best positions. That this explanation is not unlikely is illustrated by the statistically insignificant gender effect on the basis of alternative measure RM-A, which is based on more detailed and thus less heterogeneous occupational groups. Moreover, on the basis of this alternative measure, it is also found that those with a university degree are less likely to be overeducated. Finally, this is the only measure that provides statistically significant effects for cohabiting and for the interaction term between this variable and gender[20].

CONCLUSION

The central aim of this paper was to assess the measurement sensitivity of the estimated determinants of overeducation. To do so, we analysed data on Flemish tertiary education school leavers in their first job, using four different measures of overeducation. For every measure, several individual characteristics were found to have a statistically significant impact on the probability of being overeducated. However, only a few of these variables – for instance, the student’s academic grade in the final year – provided robust results for all standard measures used in the analysis. Moreover, with respect to gender, different measures even resulted in opposite conclusions. From this we conclude that the various measures do not capture equally well overeducation or are likely to be indicators for related concepts.

Indicators that are most likely to measure other concepts are those based on RM, the SA of the required level to get the job or a DSA of the utilisation of the individual's skills. RM capture only the non-structural part of overeducation within occupations, whereas measures based on the level required to get the job neglect that part of overeducation that may result from employers' inflated hiring requirements. Although both operational definitions certainly provide interesting insights into how overeducation evolves, neither of them can be equated with overeducation itself. Therefore, it should not come as a surprise that their determinants can be quite different, as illustrated by our finding that the local unemployment rate was related to the first measure but not to the second. DSA indicators based on questions regarding skill utilisation measure skill mismatches rather than educational mismatches. Hence, some academic grades were found to reduce the likelihood of being overeducated but not the likelihood of having a surplus of skills.

Other measures based on JA, the SA of the required level to do the job or a DSA of the overeducation status can be considered to more adequately capture the overeducation concept. Nevertheless, using the objective and subjective assessment of someone's overeducation status can lead to quite different results. In the case of JA, a certain degree of measurement error is almost unavoidable. In the case of self-assessment, individuals are likely to take their expectations and disappointments into account when they assess the quality of their match. We found, for example, that women and school leavers with a more highly educated father had a higher likelihood of being objectively overeducated, but not of being subjectively overeducated. Another example is that those with a study grant during tertiary education were only found to be more overeducated from a subjective point of view. So, ideally, analyses based both on JA and on SA measures should be conducted.

Finally, it is important to note that even relatively small changes to a specific measurement procedure do matter. On the basis of our standard RM measure, we found that women have a lower probability of being overeducated. This unexpected result disappeared however, when using our alternative RM measure, based on more detailed occupational codes. Hence, it seems advisable to base RM measures on the most detailed occupational codes for which enough observations are available. With respect to the other operationalisations, however, further research is needed to get a clearer picture. Applying an alternative JA procedure to account for changes in requirements due to technological change, did not provide decisive evidence that this alternative procedure results in more reliable outcomes. An alternative might be to examine the extent to which the application of more recent occupational classifications changes the outcomes of the analysis. Finally, it might also be useful to examine in greater detail to what extent the outcomes on the basis of SA measures depend on the specific wording of the underlying survey questions.

In sum, this paper demonstrates that the choice of the overeducation measure clearly influences the outcomes of the analysis. This result has important implications for further research in this field as well as for manpower policy. At the very least, a careful consideration of the extent to which the operational measure used really captures overeducation is needed.

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Appendix A: Breakdown of the sample

All respondents	6009
First job observations	5564
Individuals with a tertiary education degree	2460
Permanent school leavers	2260
Self-employed excluded	2078
Jobs with < 18 hours/week excluded	2057
Observations with missing values excluded	1938

Appendix B: Summary statistics on covariates

	Mean	Std. dev.
Study grant during tertiary education	0.321	
Having a driving licence	0.828	
Cohabiting with partner	0.084	
Start job search before leaving school	0.390	-
Years of education father	12.331	2.867
LN (regional unemployment rate)	2.440	0.286
Female	0.602	-
Female*cohabiting with partner	0.060	
Non-Western background	0.023	-
Graduated with a distinction grade	0.396	-
Graduated with a great or greatest distinction grade	0.095	-
Repeating years	0.332	0.603
University degree	0.239	-
Student work experience	0.890	-
Work-placement experience	0.818	-

Data source: SONAR c76(23), c76(26), c78(23), c78(26), own calculations; N = 1938.

Appendix C: The determinants of overeducation in the first job: average probit marginal effects estimates and robust standard errors (in parentheses), full estimation results

	(A) Standard definitions								(B) Alternative definitions					
	JA		DSA		ISA		RM		JA-A		DSA-A		RM-A	
Study grant during tertiary education	0.016	(.022)	0.050**	(.022)	0.017	(.017)	0.008	(.022)	0.007	(.021)	0.047*	(.027)	0.035	(.023)
Having a driving licence	-0.014	(.026)	0.006	(.025)	-0.027	(.024)	0.010	(.026)	-0.005	(.024)	-0.006	(.023)	0.001	(.024)
Cohabiting with partner	-0.044	(.049)	-0.053	(.052)	-0.013	(.059)	-0.038	(.044)	-0.048	(.051)	-0.015	(.055)	-0.115***	(.044)
Start job search before leaving school	-0.044**	(.018)	-0.024	(.014)	-0.013	(.016)	-0.058***	(.020)	-0.042*	(.022)	0.005	(.019)	-0.066***	(.019)
Years of education father	-0.009**	(.004)	-0.005	(.003)	-0.005*	(.003)	-0.009***	(.003)	-0.010***	(.003)	-0.001	(.003)	-0.010***	(.003)
LN (regional unemployment rate)	0.080	(.054)	0.034	(.049)	0.037	(.058)	0.109**	(.048)	0.159***	(.045)	0.051	(.067)	0.163***	(.053)
Female	0.043*	(.022)	-0.003	(.020)	0.005	(.020)	-0.035*	(.020)	0.007	(.022)	0.035	(.024)	-0.025	(.018)
Female*Cohabiting with partner	0.049	(.064)	0.031	(.067)	0.021	(.068)	0.037	(.059)	0.055	(.063)	-0.015	(.061)	0.109*	(.062)
Non-Western background	0.018	(.041)	0.092*	(.054)	0.017	(.053)	-0.087	(.053)	-0.089	(.055)	0.104*	(.061)	-0.052	(.052)
Graduated with distinction grade	-0.050**	(.025)	-0.043**	(.019)	-0.075***	(.014)	-0.070***	(.020)	-0.054**	(.023)	-0.044**	(.022)	-0.070***	(.019)
Graduated with great or greatest dis. g.	-0.106***	(.033)	-0.078***	(.027)	-0.089***	(.029)	-0.096***	(.032)	-0.101***	(.032)	-0.028	(.034)	-0.091**	(.038)
Repeating years	0.027	(.017)	0.041***	(.014)	0.034**	(.015)	0.022*	(.012)	0.044***	(.014)	0.045***	(.017)	0.033**	(.015)
University degree	-0.132***	(.041)	-0.036	(.036)	-0.063**	(.026)	-0.037	(.034)	-0.058*	(.033)	-0.106***	(.022)	-0.050*	(.028)
Student work experience	-0.002	(.038)	0.022	(.033)	-0.005	(.032)	0.008	(.033)	-0.022	(.034)	0.052	(.035)	0.022	(.033)
Work-placement experience	0.001	(.029)	-0.013	(.024)	-0.013	(.029)	-0.037*	(.022)	-0.017	(.025)	-0.019	(.028)	-0.051**	(.023)
Born in 1980	0.029	(.021)	0.023	(.024)	0.010	(.020)	0.059***	(.017)	0.081***	(.020)	0.049**	(.024)	0.065***	(.020)
Higher tertiary education degree	0.187***	(.040)	0.066*	(.040)	0.082*	(.042)	0.311***	(.048)	0.041	(.033)	0.086***	(.030)	0.119***	(.037)
Subject: Philos. and humanities (ref.)														
Subject: Law and criminology	-0.159*	(.082)	-0.101	(.077)	-0.087	(.082)	-0.044	(.081)	-0.078	(.085)	-0.038	(.083)	0.049	(.078)
Subject: Economics and business	-0.204***	(.062)	-0.222***	(.042)	-0.178***	(.037)	-0.172***	(.051)	-0.301***	(.047)	-0.102**	(.050)	-0.168***	(.045)

Subject: Political and social sciences	0.041	(.049)	0.124	(.091)	0.182**	(.085)	0.151**	(.059)	0.096	(.076)	-0.039	(.061)	0.059	(.071)
Subject: Psychology and pedagogy	-0.023	(.053)	0.006	(.104)	0.147	(.098)	0.099	(.070)	0.158**	(.068)	-0.009	(.091)	0.019	(.091)
Subject: Health and (para)medicine	-0.388***	(.064)	-0.254***	(.044)	-0.184***	(.053)	-0.281***	(.051)	-0.318***	(.051)	-0.215***	(.041)	-0.287***	(.043)
Subject: Natural sc. and mathematics	-0.191**	(.090)	-0.083	(.093)	-0.102	(.075)	-0.088	(.073)	-0.152*	(.082)	-0.129*	(.072)	-0.034	(.083)
Subject: Engineering and technology	-0.339***	(.063)	-0.272***	(.030)	-0.193***	(.038)	-0.199***	(.050)	-0.288***	(.050)	-0.159***	(.046)	-0.190***	(.044)
Subject: Applied biological sciences	-0.121	(.087)	-0.032	(.089)	-0.069	(.079)	-0.026	(.094)	-0.120	(.085)	0.089	(.095)	0.028	(.087)
Subject: Architecture	-0.187*	(.095)	-0.036	(.087)	-0.050	(.088)	0.038	(.106)	-0.001	(.105)	-0.086	(.076)	0.010	(.115)
Subject: Arts	0.009	(.094)	-0.004	(.083)	0.178*	(.097)	0.071	(.070)	0.013	(.104)	-0.062	(.089)	0.056	(.110)
Subject: Education	-0.335***	(.062)	-0.218***	(.050)	-0.102*	(.061)	-0.105**	(.053)	-0.316***	(.048)	-0.130**	(.051)	-0.236***	(.046)
Subject: Social work	-0.309***	(.065)	-0.228***	(.051)	-0.175***	(.049)	-0.152**	(.063)	-0.273***	(.053)	-0.159***	(.058)	-0.233***	(.053)
Region: Antwerpen (ref.)														
Region: Mechelen	0.033	(.043)	-0.037	(.048)	-0.046	(.041)	0.014	(.029)	0.055	(.037)	0.034	(.063)	0.047	(.037)
Region: Turnhout	-0.055	(.042)	-0.072*	(.042)	-0.061	(.038)	-0.060	(.038)	-0.043	(.045)	-0.048	(.047)	-0.017	(.035)
Region: Vilvoorde	0.046	(.047)	0.006	(.050)	-0.022	(.051)	0.011	(.049)	0.105**	(.052)	0.035	(.064)	0.074	(.060)
Region: Leuven	0.030	(.035)	-0.038	(.039)	-0.040	(.041)	0.021	(.031)	0.045	(.031)	-0.002	(.057)	0.028	(.037)
Region: Brugge	-0.021	(.046)	-0.013	(.039)	0.047	(.049)	-0.033	(.047)	-0.087*	(.047)	0.006	(.061)	0.038	(.046)
Region: Oostende	-0.074**	(.029)	-0.054	(.040)	-0.013	(.043)	-0.029	(.030)	-0.064**	(.031)	-0.006	(.060)	-0.032	(.027)
Region: Ieper	0.002	(.035)	-0.045	(.077)	-0.144	(.097)	0.091	(.079)	0.045	(.099)	0.010	(.102)	0.189***	(.034)
Region: Kortrijk	-0.044	(.039)	-0.074*	(.044)	-0.057	(.042)	0.019	(.035)	0.018	(.036)	-0.017	(.065)	0.027	(.046)
Region: Roeselare	0.025	(.045)	-0.066	(.045)	0.024	(.055)	0.035	(.043)	0.089*	(.048)	-0.051	(.060)	0.056	(.054)
Region: Aalst	-0.080*	(.047)	-0.087*	(.051)	-0.090	(.061)	-0.032	(.059)	-0.065	(.052)	0.001	(.061)	0.030	(.047)
Region: Dendermonde	-0.006	(.030)	-0.071*	(.041)	-0.049	(.039)	-0.046	(.029)	-0.065**	(.031)	0.005	(.055)	-0.002	(.031)
Region: Gent	-0.010	(.041)	-0.047	(.039)	-0.043	(.028)	-0.029	(.041)	-0.081*	(.043)	-0.003	(.052)	-0.016	(.041)
Region: Oudenaarde	-0.104	(.109)	-0.033	(.098)	0.071	(.057)	-0.113	(.071)	-0.061	(.074)	0.016	(.069)	-0.054	(.087)

Region: Sint-Niklaas	-0.065	(.068)	-0.066	(.048)	-0.032	(.041)	-0.079**	(.035)	-0.032	(.059)	-0.002	(.056)	-0.058	(.043)
Region: Limburg	-0.027	(.023)	-0.024	(.045)	-0.028	(.038)	-0.048*	(.025)	-0.017	(.028)	-0.000	(.045)	-0.065**	(.027)
Sector: Industry (ref.)														
Sector: Primary sector	0.112	(.125)	0.199	(.154)	0.374**	(.157)	0.334***	(.108)	0.314**	(.143)	-0.116	(.117)	0.352***	(.130)
Sector: Construction	0.114*	(.061)	0.048	(.071)	0.080	(.064)	0.078	(.083)	0.136*	(.076)	-0.051	(.058)	0.097	(.080)
Sector: Commerce	0.063*	(.035)	0.100**	(.040)	0.155***	(.046)	0.076**	(.036)	0.057	(.040)	0.038	(.042)	0.107***	(.033)
Sector: Catering	0.150*	(.085)	-0.071	(.101)	0.289**	(.143)	0.381***	(.075)	0.339***	(.104)	0.076	(.119)	0.400***	(.094)
Sector: Transport and communication	-0.003	(.058)	0.036	(.062)	0.188***	(.059)	0.028	(.043)	-0.061	(.058)	0.023	(.048)	0.063	(.048)
Sector: Finance	-0.011	(.051)	-0.085**	(.039)	-0.058	(.037)	-0.065	(.047)	-0.198***	(.043)	-0.046	(.046)	-0.216***	(.038)
Sector: Professional services	-0.165***	(.040)	-0.073**	(.034)	-0.034	(.032)	-0.116***	(.031)	-0.185***	(.035)	-0.037	(.032)	-0.146***	(.028)
Sector: Government	-0.246***	(.062)	-0.039	(.046)	0.055	(.044)	-0.211***	(.041)	-0.182***	(.045)	-0.023	(.054)	-0.135***	(.048)
Sector: Education	-0.575***	(.021)	-0.222***	(.023)	-0.200***	(.018)	-0.391***	(.017)	-0.365***	(.020)	-0.164***	(.025)	-0.330***	(.021)
Sector: Health Care	-0.262***	(.057)	-0.148***	(.034)	-0.123***	(.031)	-0.165***	(.049)	-0.105*	(.056)	-0.091**	(.038)	-0.128***	(.043)
Sector: Other Services	-0.150*	(.080)	-0.108*	(.055)	0.012	(.052)	-0.054	(.075)	-0.074	(.072)	-0.086*	(.051)	-0.005	(.081)
Sector: Unknown	-0.070	(.144)	-0.101	(.113)	0.218	(.143)	0.006	(.157)	-0.046	(.160)	0.145	(.153)	0.097	(.153)
Subjects: Wald Chi² (12)	115.87***		95.94***		125.15***		75.81***		112.84***		36.44***		80.13***	
Regions: Wald Chi² (15)	31.76***		15.27		31.80***		36.78***		66.26***		10.64		88.39***	
Sectors: Wald Chi² (12)	335.69***		117.34***		234.19***		207.51***		186.04***		44.63***		188.13***	
Model: Wald Chi² (56)	4957.53***		3508.40***		2737.67***		5500.38***		2998.50***		996.34***		2869.11***	

JA = Job Analysis, *DSA* = Direct Self-Assessment, *ISA* = Indirect Self-Assessment, *RM* = Realised Matches, *JA-A* = Job Analysis, adapted for skill-biased technological change; *DSA-A* = Alternative DSA measure based on question regarding skill utilization; *RM-A* = Alternative Realised Matches measure based on most detailed job title with at least 20 observations;
Standard errors are adjusted for clustering on region – month of labour market entry pairs (113 clusters);
For dummy variables, the effects represent a discrete change from 0 to 1;
*: p < 0.10; **: p < 0.05; ***: p < 0.01; N = 1938.

Appendix D: Maximum-likelihood probit model with sample selection: estimated coefficients and robust standard errors (in parentheses)

	(A) Standard definitions				(B) Alternative definitions		
	JA	DSA	ISA	RM	JA-A	DSA-A	RM-A
P(Overeducated in first job)							
Study grant during tertiary education	0.057 (.079)	0.199** (.082)	0.072 (.068)	0.036 (.088)	0.027 (.075)	0.047* (.027)	0.035 (.023)
Having a driving licence	-0.052 (.094)	0.022 (.100)	-0.109 (.102)	0.040 (.102)	-0.018 (.085)	-0.006 (.023)	0.001 (.024)
Cohabiting with partner	-0.168 (.172)	-0.251 (.247)	-0.073 (.252)	-0.170 (.184)	-0.185 (.203)	-0.015 (.055)	-0.115*** (.044)
Start job search before leaving school	-0.163** (.163)	-0.109* (.064)	-0.067 (.070)	-0.246*** (.082)	-0.157* (.083)	0.005 (.019)	-0.066*** (.019)
Years of education father	-0.031** (.013)	-0.018 (.014)	-0.019 (.012)	-0.035*** (.011)	-0.036*** (.011)	-0.001 (.003)	-0.010*** (.003)
LN (regional unemployment rate)	0.282 (.194)	0.135 (.198)	0.151 (.243)	0.426** (.197)	0.567*** (.163)	0.051 (.067)	0.163*** (.053)
Female	0.152* (.080)	-0.016 (.084)	0.014 (.083)	-0.142* (.080)	0.024 (.078)	0.035 (.024)	-0.025 (.018)
Female*Cohabiting with partner	0.180 (.230)	0.145 (.302)	0.092 (.288)	0.149 (.237)	0.207 (.237)	-0.015 (.061)	0.109* (.062)
Non-Western background	0.064 (.145)	0.338* (.181)	0.065 (.208)	-0.371 (.244)	-0.346 (.236)	0.104* (.061)	-0.052 (.052)
Graduated with distinction grade	-0.177** (.088)	-0.175** (.079)	-0.310*** (.065)	-0.281*** (.086)	-0.192** (.084)	-0.044** (.022)	-0.070*** (.019)
Graduated with great or greatest dis. g.	-0.378*** (.117)	-0.338** (.133)	-0.379*** (.143)	-0.394*** (.144)	-0.376*** (.132)	-0.028 (.034)	-0.091** (.038)
Repeating years	0.101 (.063)	0.170*** (.059)	0.148** (.063)	0.091* (.048)	0.160*** (.051)	0.045*** (.017)	0.033** (.015)
University degree	-0.479*** (.151)	-0.147 (.158)	-0.270** (.139)	-0.144 (.142)	-0.212 (.129)	-0.106*** (.022)	-0.050* (.028)
Student work experience	-0.012 (.136)	0.083 (.131)	-0.029 (.133)	0.026 (.133)	-0.079 (.124)	0.052 (.035)	0.022 (.033)
Work-placement experience	-0.003 (.109)	-0.066 (.098)	-0.066 (.117)	-0.155* (.091)	-0.066 (.093)	-0.019 (.028)	-0.051** (.023)
Born in 1980	0.103 (.074)	0.095 (.093)	0.043 (.081)	0.237*** (.064)	0.289*** (.066)	0.049** (.024)	0.065*** (.020)
Higher tertiary education degree	0.663*** (.142)	0.263* (.140)	0.330** (.151)	1.089*** (.146)	0.147 (.110)	0.086*** (.030)	0.119*** (.037)
Subject: Philos. and humanities (ref.)							
Subject: Law and criminology	-0.595** (.278)	-0.298 (.247)	-0.280 (.300)	-0.146 (.281)	-0.231 (.255)	-0.038 (.083)	0.049 (.078)

Subject: Economics and business	-0.745***	(.200)	-0.732***	(.178)	-0.644***	(.175)	-0.603***	(.187)	-0.953***	(.171)	-0.102**	(.050)	-0.168***	(.045)
Subject: Political and social sciences	0.189	(.230)	0.360	(.263)	0.551**	(.257)	0.536**	(.218)	0.302	(.244)	-0.039	(.061)	0.059	(.071)
Subject: Psychology and pedagogy	-0.098	(.217)	0.015	(.301)	0.439	(.288)	0.340	(.248)	0.506**	(.237)	-0.009	(.091)	0.019	(.091)
Subject: Health and (para)medicine	-1.348***	(.224)	-0.864***	(.212)	-0.657**	(.261)	-1.050***	(.231)	-1.015***	(.202)	-0.215***	(.041)	-0.287***	(.043)
Subject: Natural sc. and mathematics	-0.693**	(.301)	-0.229	(.292)	-0.317	(.287)	-0.288	(.251)	-0.448*	(.252)	-0.129*	(.072)	-0.034	(.083)
Subject: Engineering and technology	-1.188***	(.202)	-0.957***	(.159)	-0.708***	(.198)	-0.709***	(.191)	-0.906***	(.178)	-0.159***	(.046)	-0.190***	(.044)
Subject: Applied biological sciences	-0.464	(.309)	-0.092	(.264)	-0.225	(.271)	-0.089	(.324)	-0.360	(.259)	0.089	(.095)	0.028	(.087)
Subject: Architecture	-0.671**	(.322)	-0.076	(.254)	-0.130	(.312)	0.159	(.368)	0.015	(.318)	-0.086	(.076)	0.010	(.115)
Subject: Arts	0.081	(.417)	-0.033	(.253)	0.582*	(.301)	0.299	(.245)	0.063	(.319)	-0.062	(.089)	0.056	(.110)
Subject: Education	-1.177***	(.200)	-0.715***	(.207)	-0.338	(.228)	-0.360*	(.184)	-1.012***	(.186)	-0.130**	(.051)	-0.236***	(.046)
Subject: Social work	-1.089***	(.213)	-0.752***	(.223)	-0.623***	(.239)	-0.526**	(.232)	-0.849***	(.187)	-0.159***	(.058)	-0.233***	(.053)
Region: Antwerpen (ref.)														
Region: Mechelen	0.125	(.153)	-0.126	(.193)	-0.182	(.175)	0.061	(.112)	0.189	(.123)	0.034	(.063)	0.047	(.037)
Region: Turnhout	-0.193	(.148)	-0.288	(.189)	-0.251	(.172)	-0.244	(.161)	-0.158	(.170)	-0.048	(.047)	-0.017	(.035)
Region: Vilvoorde	0.169	(.171)	0.029	(.180)	-0.084	(.208)	0.044	(.189)	0.352**	(.165)	0.035	(.064)	0.074	(.060)
Region: Leuven	0.110	(.126)	-0.141	(.160)	-0.154	(.176)	0.084	(.117)	0.158	(.105)	-0.002	(.057)	0.028	(.037)
Region: Brugge	-0.076	(.161)	-0.049	(.149)	0.170	(.178)	-0.132	(.193)	-0.335*	(.200)	0.006	(.061)	0.038	(.046)
Region: Oostende	-0.263**	(.104)	-0.215	(.166)	-0.051	(.171)	-0.116	(.121)	-0.239*	(.124)	-0.006	(.060)	-0.032	(.027)
Region: Ieper	0.011	(.125)	-0.175	(.320)	-0.725	(.679)	0.341	(.294)	0.153	(.331)	0.010	(.102)	0.189***	(.034)
Region: Kortrijk	-0.153	(.137)	-0.292	(.199)	-0.229	(.182)	0.080	(.133)	0.068	(.124)	-0.017	(.065)	0.027	(.046)
Region: Roeselare	0.086	(.163)	-0.266	(.194)	0.087	(.202)	0.131	(.163)	0.296*	(.153)	-0.051	(.060)	0.056	(.054)
Region: Aalst	-0.279*	(.168)	-0.355	(.240)	-0.386	(.309)	-0.120	(.237)	-0.239	(.207)	0.001	(.061)	0.030	(.047)
Region: Dendermonde	-0.020	(.107)	-0.281	(.184)	-0.195	(.170)	-0.184	(.118)	-0.241*	(.124)	0.005	(.055)	-0.002	(.031)
Region: Gent	-0.037	(.145)	-0.187	(.162)	-0.179	(.120)	-0.117	(.165)	-0.310*	(.178)	-0.003	(.052)	-0.016	(.041)

Region: Oudenaarde	-0.373	(.394)	-0.126	(.395)	0.251	(.198)	-0.482	(.336)	-0.228	(.293)	0.016	(.069)	-0.054	(.087)
Region: Sint-Niklaas	-0.216	(.242)	-0.245	(.215)	-0.112	(.165)	-0.306**	(.153)	-0.108	(.222)	-0.002	(.056)	-0.058	(.043)
Region: Limburg	-0.096	(.081)	-0.091	(.177)	-0.110	(.154)	-0.192*	(.106)	-0.062	(.101)	-0.000	(.045)	-0.065**	(.027)
Sector: Industry (ref.)														
Sector: Primary sector	0.393	(.490)	0.578	(.415)	1.080**	(.452)	1.140**	(.458)	0.919*	(.490)	-0.116	(.117)	0.352***	(.130)
Sector: Construction	0.395	(.242)	0.142	(.214)	0.247	(.194)	0.239	(.260)	0.373*	(.212)	-0.051	(.058)	0.097	(.080)
Sector: Commerce	0.212*	(.120)	0.301***	(.114)	0.473***	(.129)	0.243**	(.110)	0.161	(.110)	0.038	(.042)	0.107***	(.033)
Sector: Catering	0.553	(.377)	-0.232	(.370)	0.842**	(.387)	1.352***	(.365)	1.009***	(.370)	0.076	(.119)	0.400***	(.094)
Sector: Transport and communication	-0.010	(.183)	0.110	(.185)	0.559***	(.162)	0.086	(.135)	-0.176	(.169)	0.023	(.048)	0.063	(.048)
Sector: Finance	-0.033	(.159)	-0.295*	(.151)	-0.209	(.146)	-0.210	(.151)	-0.613***	(.154)	-0.046	(.046)	-0.216***	(.038)
Sector: Professional services	-0.484***	(.111)	-0.242*	(.127)	-0.114	(.116)	-0.377***	(.108)	-0.565***	(.119)	-0.037	(.032)	-0.146***	(.028)
Sector: Government	-0.722***	(.178)	-0.132	(.158)	0.176	(.136)	-0.733***	(.168)	-0.557***	(.155)	-0.023	(.054)	-0.135***	(.048)
Sector: Education	-1.977***	(.143)	-1.006***	(.185)	-1.021***	(.173)	-1.756***	(.159)	-1.462***	(.163)	-0.164***	(.025)	-0.330***	(.021)
Sector: Health Care	-0.764***	(.164)	-0.561***	(.164)	-0.501***	(.162)	-0.550***	(.181)	-0.303*	(.170)	-0.091**	(.038)	-0.128***	(.043)
Sector: Other Services	-0.443*	(.228)	-0.380*	(.224)	0.042	(.174)	-0.168	(.241)	-0.212	(.212)	-0.086*	(.051)	-0.005	(.081)
Sector: Unknown	-0.184	(.429)	-0.322	(.481)	0.683*	(.393)	0.054	(.493)	-0.111	(.455)	0.145	(.153)	0.097	(.153)
Intercept	1.090*	(.647)	0.014	(.557)	-0.046	(.709)	-0.211	(.640)	-0.199	(.612)	0.145	(.153)	0.097	(.153)
Selection equation:														
P(job observation of >=18 hours/week)														
Study grant during tertiary education	-0.190	(.167)	-0.195	(.167)	-0.201	(.168)	-0.179	(.166)	-0.182	(.168)	-0.190	(.167)	-0.190	(.167)
Having a driving licence	0.313*	(.174)	0.332*	(.181)	0.321*	(.177)	0.324*	(.174)	0.311*	(.176)	0.313*	(.174)	0.313*	(.174)
Cohabiting with partner	0.967**	(.492)	0.978**	(.486)	1.010**	(.508)	0.979**	(.489)	0.970**	(.490)	0.967**	(.492)	0.967**	(.492)
Start job search before leaving school	0.699***	(.222)	0.698***	(.225)	0.691***	(.222)	0.686***	(.222)	0.684***	(.221)	0.699***	(.222)	0.699***	(.222)
Years of education father	-0.071**	(.030)	-0.067**	(.029)	-0.069**	(.029)	-0.071**	(.030)	-0.071**	(.030)	-0.071**	(.030)	-0.071**	(.030)

LN (regional unemployment rate)	-0.119	(.328)	-0.162	(.352)	-0.144	(.352)	-0.135	(.332)	-0.093	(.335)	-0.119	(.328)	-0.119	(.328)
Female	0.101	(.138)	0.113	(.141)	0.100	(.136)	0.115	(.140)	0.118	(.141)	0.101	(.138)	0.101	(.138)
Female*Cohabiting with partner	0.380	(.663)	0.352	(.677)	0.316	(.685)	0.340	(.653)	0.360	(.661)	0.380	(.663)	0.380	(.663)
Non-Western background	0.260	(.412)	0.266	(.409)	0.244	(.420)	0.268	(.417)	0.261	(.417)	0.260	(.412)	0.260	(.412)
Graduated with distinction grade	0.126	(.141)	0.127	(.145)	0.143	(.139)	0.127	(.141)	0.131	(.141)	0.126	(.141)	0.126	(.141)
Graduated with great or greatest dis. g.	0.404	(.324)	0.441	(.335)	0.434	(.341)	0.414	(.322)	0.405	(.325)	0.404	(.324)	0.404	(.324)
Repeating years	0.047	(.105)	0.048	(.109)	0.053	(.110)	0.060	(.108)	0.046	(.104)	0.047	(.105)	0.047	(.105)
University degree	-0.232	(.196)	-0.241	(.198)	-0.228	(.200)	-0.224	(.197)	-0.228	(.198)	-0.232	(.196)	-0.232	(.196)
Student work experience	0.342	(.247)	0.348	(.248)	0.352	(.245)	0.351	(.242)	0.354	(.239)	0.342	(.247)	0.342	(.247)
Work-placement experience	0.535***	(.181)	0.533***	(.184)	0.539***	(.184)	0.551***	(.183)	0.542***	(.180)	0.535***	(.181)	0.535***	(.181)
Born in 1980	-0.002	(.183)	0.013	(.176)	-0.029	(.180)	-0.021	(.180)	0.011	(.179)	-0.002	(.183)	-0.002	(.183)
Higher tertiary education degree	-0.287	(.228)	-0.300	(.232)	-0.307	(.243)	-0.305	(.227)	-0.282	(.225)	-0.287	(.228)	-0.287	(.228)
Subject: Philos. and humanities (ref.)														
Subject: Law and criminology	-0.033	(.474)	-0.008	(.478)	-0.025	(.476)	-0.065	(.464)	-0.020	(.471)	-0.033	(.474)	-0.033	(.474)
Subject: Economics and business	-0.032	(.323)	-0.029	(.320)	-0.026	(.327)	-0.038	(.314)	-0.006	(.315)	-0.032	(.323)	-0.032	(.323)
Subject: Political and social sciences	-0.240	(.427)	-0.206	(.411)	-0.192	(.412)	-0.252	(.426)	-0.224	(.422)	-0.240	(.427)	-0.240	(.427)
Subject: Psychology and pedagogy	0.310	(.432)	0.306	(.425)	0.307	(.429)	0.286	(.432)	0.312	(.431)	0.310	(.432)	0.310	(.432)
Subject: Health and (para)medicine	-0.578*	(.345)	-0.603*	(.339)	-0.593*	(.349)	-0.606*	(.341)	-0.577*	(.344)	-0.578*	(.345)	-0.578*	(.345)
Subject: Natural sc. and mathematics	-0.378	(.375)	-0.393	(.351)	-0.401	(.352)	-0.390	(.370)	-0.392	(.360)	-0.378	(.375)	-0.378	(.375)
Subject: Engineering and technology	-0.321	(.336)	-0.320	(.333)	-0.324	(.335)	-0.339	(.335)	-0.310	(.335)	-0.320	(.333)	-0.320	(.333)
Subject: Applied biological sciences	-0.292	(.521)	-0.258	(.520)	-0.296	(.514)	-0.314	(.529)	-0.265	(.536)	-0.292	(.521)	-0.292	(.521)
Subject: Architecture	-1.487***	(.508)	-1.489***	(.513)	-1.476***	(.517)	-1.512***	(.502)	-1.461***	(.496)	-1.487***	(.508)	-1.487***	(.508)
Subject: Arts	-0.952***	(.363)	-0.950***	(.355)	-0.940***	(.357)	-0.983***	(.356)	-0.955***	(.359)	-0.952***	(.363)	-0.952***	(.363)
Subject: Education	-0.062	(.497)	-0.089	(.510)	-0.090	(.514)	-0.110	(.494)	-0.061	(.493)	-0.062	(.497)	-0.062	(.497)

Subject: Social work	-0.484	(.470)	-0.467	(.469)	-0.460	(.466)	-0.533	(.471)	-0.486	(.466)	-0.484	(.470)	-0.484	(.470)
Region: Antwerpen (ref.)														
Region: Mechelen	-0.622**	(.308)	-0.679**	(.312)	-0.669**	(.321)	-0.603**	(.304)	-0.598*	(.312)	-0.622**	(.308)	-0.622**	(.308)
Region: Turnhout	-0.509	(.373)	-0.505	(.366)	-0.514	(.379)	-0.494	(.369)	-0.502	(.360)	-0.509	(.373)	-0.509	(.373)
Region: Vilvoorde	-0.764**	(.323)	-0.782**	(.323)	-0.800**	(.335)	-0.764**	(.321)	-0.737**	(.329)	-0.764**	(.323)	-0.764**	(.323)
Region: Leuven	-0.496	(.331)	-0.518	(.332)	-0.515	(.335)	-0.485	(.330)	-0.464	(.328)	-0.496	(.331)	-0.496	(.331)
Region: Brugge	-0.574	(.501)	-0.596	(.496)	-0.584	(.511)	-0.558	(.503)	-0.551	(.513)	-0.574	(.501)	-0.574	(.501)
Region: Oostende-Ieper	-0.286	(.311)	-0.289	(.309)	-0.268	(.315)	-0.267	(.309)	-0.279	(.310)	-0.286	(.311)	-0.286	(.311)
Region: Kortrijk	-0.606*	(.340)	-0.639*	(.346)	-0.640*	(.356)	-0.579*	(.339)	-0.562	(.354)	-0.606*	(.340)	-0.606*	(.340)
Region: Roeselare	-0.107	(.324)	-0.161	(.343)	-0.122	(.336)	-0.129	(.325)	-0.093	(.330)	-0.107	(.324)	-0.107	(.324)
Region: Aalst	-0.698*	(.368)	-0.708*	(.357)	-0.715**	(.358)	-0.649*	(.390)	-0.690*	(.369)	-0.698*	(.368)	-0.698*	(.368)
Region: Dendermonde	-0.771**	(.305)	-0.802**	(.310)	-0.791**	(.311)	-0.790***	(.298)	-0.757**	(.300)	-0.771**	(.305)	-0.771**	(.305)
Region: Gent	-0.282	(.292)	-0.295	(.285)	-0.320	(.294)	-0.280	(.286)	-0.272	(.290)	-0.282	(.292)	-0.282	(.292)
Region: Oudenaarde	-0.807*	(.424)	-0.768*	(.419)	-0.790*	(.414)	-0.790*	(.421)	-0.761*	(.430)	-0.807*	(.424)	-0.807*	(.424)
Region: Sint-Niklaas	-0.750**	(.331)	-0.776**	(.339)	-0.770**	(.335)	-0.710**	(.330)	-0.712**	(.334)	-0.750**	(.331)	-0.750**	(.331)
Region: Limburg	-0.412	(.290)	-0.406	(.291)	-0.425	(.296)	-0.394	(.291)	-0.395	(.290)	-0.412	(.290)	-0.412	(.290)
LN(potential experience)[$]	1.055***	(.082)	1.054***	(.082)	1.056***	(.084)	1.054***	(.082)	1.053***	(.082)	1.055***	(.082)	1.055***	(.082)
Intercept	-0.366	(.907)	-0.312	(.972)	-0.341	(.992)	-0.340	(.928)	-0.486	(.933)	-0.366	(.907)	-0.366	(.907)
Rho	-0.170	(.234)	-0.239	(.402)	-0.257	(.388)	-0.240	(.212)	-0.128	(.234)	-0.170	(.234)	-0.170	(.234)
Model: Wald Chi² (56)	4957.53***		3508.40***		2737.67***		5500.38***		2998.50***		996.34***		2869.11***	

[$] Potential experience is measured as the number of months between the school leaving date and the time of the last interview (cf. footnote 16)
JA = Job Analysis, *DSA* = Direct Self-Assessment, *ISA* = Indirect Self-Assessment, *RM* = Realised Matches, *JA-A* = Job Analysis, adapted for skill-biased technological change; *DSA-A* = Alternative DSA measure based on question regarding skill utilization; *RM-A* = Alternative Realised Matches measure based on most detailed job title with at least 20 observations;
Standard errors are adjusted for clustering on region – month of labour market entry pairs (113 clusters); For dummy variables, effects represent a discrete change from 0 to 1; *: $p < 0.10$; **: $p < 0.05$; ***: $p < 0.01$; N = 1938.

NOTES

[1] Some evidence. for instance, shows that less-able workers are more likely to be overeducated (Green et al., 2002).

[2] A standard job is defined as a paid job with a temporary or permanent labour market contract or as self-employment. Explicitly excluded were employment with a student work contract, holiday work, apprenticeship contracts, employment as part of a work placement and informal work.

[3] We exclude the least and moderately educated from the analysis because none of these school leavers was assessed to be over-educated on the basis of RM. Owing to compulsory education laws, none of the occupations in our data was occupied by a majority of school leavers without a secondary education degree.

[4] Note that this classification was originally developed for a labour market other than the one analysed in this paper. Although similar jobs sometimes have different titles in Flanders and The Netherlands, this did not cause major problems, since the classification is built on tasks to be executed instead of job titles. The classification has regularly been used in the Netherlands to measure overeducation. Van der Meer (2006) compared, on the basis of wage-equations, the validity of this measure with another more dated JA measure (the so-called Huijgen measure) and concluded that the CBS-measure is more valid.

[5] The question was not asked to the respondents in the 1978 survey.

[6] In total, we have 5376 observations of individuals. As the CBS classification counts 43 two-digit occupations, this results in an average of about 125 individuals per occupation. For the two-digit occupations that counted fewer than 20 individuals, we base the job level on the modal level within the corresponding one-digit occupation.

[7] Groot and Maassen van den Brink (2000b), e.g., based their RM measures on three-digit CBS occupations.

[8] Hence, if fewer than 20 observations are available at the most detailed level, requirement measurement for that detailed occupation is based on the next more aggregate level with at least twenty observations.

[9] As previously stated, it is generally found that RM also provides a lower incidence of overeducation compared to SA measures. As shown in Verhaest and Omey (2006b), this is also the case on the basis of the SONAR-data if the analysis is not restricted to the more highly educated.

[10] Although the number of children might also influence the opportunities of woman, we did not test for this, as only 13 women in our sample had already had children at the start of their labour market career.

[11] In the case of missing values, this variable is proxied by the years of education of the mother.

[12] We focus on the father, as it is well documented that men generally have access to more and better networks (see, e.g., Ibarra, 1993). In theory, the occupational level of the father should be a better indicator for parental network. Still, we prefer to use the educational level because of the large number of missing values on the occupational information of the parents. Moreover, problems of measurement error are likely to be severe, as the coding of the CSB-occupations is based on information collected by open answer questions that are posed to the respondents and not to the parents themselves.

[13] For a “distinction grade”, students should have an overall exam result of at least 65%; for a “great distinction grade”, their exam result should be at least 75%.

[14] Before the introduction of the bachelor’s and master’s degrees, universities did not provide lower tertiary degrees. Although students got a so-called “candidate degree” after two years, this was never perceived as being a full lower tertiary education degree.

[15] For the study grant variable, years of education of the father, and student work experience, missing values were imputed by their expected values on the basis of OLS or probit regressions. This resulted in 28 additional observations.

[16] Except for the industry dummies, all the variables that are included in the overeducation equation were also included in the selection equation. For the identification of the selection effect, we additionally included the natural logarithm of the number of months between the school leaving date and the time of the last interview (at age 23 or age 26). We can expect that the probability to observe a job increases with the length of this duration. Moreover, this observation period largely depends on whether individuals participated in the follow-up survey and on the exact date of this last interview (the survey period was usually four months). Hence, it should not have an influence on the probability to be overeducated. That these two statements are correct is confirmed by additional tests on the basis of separate models for each step.

[17] For the full estimation results, see Appendix D.

[18] Except for the interaction effect between cohabiting and gender, these average marginal effects were computed in Stata by means of the 'margeff' command (Bartus, 2005). For the interaction effect, we made use of the 'inteff' command, which was implemented in Stata by Norton et al. (2004)

[19] Additional tests on the proportionality of the different coefficient vectors also showed that the type of measurement applied is important for the outcomes; the hypothesis that the estimated coefficient vectors of two measures are proportional was always rejected.

[20] Separate estimates for men and women provided similar results: a statistically significant ($p < 0.10$) negative impact of cohabiting for men and a statistically insignificant impact for women.