The Role of Ability in Estimating the Returns to College Choice: New Swedish Evidence

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Abstract

This paper examines the effect on earnings of graduating from five different college groups. The study is based on an administrative data set unusually rich in terms of school grades, parental characteristics and other attributes. Contrary to most previous Swedish research, we find no systematic differences in estimated earnings between the college categories. This finding holds for all college graduates, for men and women separately and for graduates in two specific fields of education. The results indicate that an estimator of the earnings effects of college choice that does not properly adjust for ability is likely to be substantially biased.

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1. Introduction

In the economic literature, there has been a long tradition of estimating the returns to education in terms of years of schooling completed or level of education attained.[1] More recently, there has been an increased focus on estimating the labor market effects of college choice and college quality. The effects of college choice on employment opportunities and earnings are of obvious interest for individuals about to invest in higher education. The relationship between school quality and labor market outcomes is also important from a societal perspective. Following the rapid expansion and geographical decentralization of higher education in Sweden in recent years, there has been a growing concern about the quality of education provided at the newly established colleges (Sörlin and Törnqvist, 2000; Öckert and Regnér, 2000).[2]

Most of the literature looking at labor market effects of college quality is based on data for the United States. Recent contributions include Black et al. (1995, 1997, 2005), Datcher Loury and Garman (1995), Behrman et al. (1996), Brewer and Ehrenberg (1996), Brewer et al. (1999), Monks (2000), Berg Dale and Krueger (2002) and Black and Smith (2004, 2006). The basic finding from this research is that college quality matters for labor market outcomes. Depending on estimation methods and college quality classifications, these studies show that attending high-quality colleges rather than low-quality colleges generally increases wages in the range of 5−15 percent. Exceptions are Brewer et al. (1999) who report premiums as high as 40 percent for some cohorts, and Berg Dale and Krueger (2002) who do not find any significant effects of college quality. There are also some indications in the papers from the United States that women receive smaller gains from college quality than do men. Using data for the United Kingdom, Chevalier and Conlon (2003) report an effect on wages in the range of 0−17 percent of attending a high-quality university as opposed to a low-quality university. In this case, the results point towards slightly higher returns to college quality for women than for men.

There are a few available studies that use Swedish data to estimate the labor market effects of college choice. Lindahl and Regnér (2005) find that college graduates from old universities receive earnings that are approximately 4 percent higher than college graduates from new universities. Using a similar college classification, Lundin (2006) focuses on students with a business or economics degree and reports an earnings premium of about 6 percent for college graduates from old universities. There are also a

[1] See Card (1999) for an overview of this research and Björklund (2000) for a discussion of Swedish evidence.

[2] For an overview of the historical development of the higher education sector in Sweden; see e.g. Öckert and Regnér (2000) and Lindahl and Regnér (2005).

few Swedish papers that look at the earnings premium of graduating from individual colleges (Wadensjö, 1991; Gustafsson, 1996; Gartell and Regnér, 2002, 2005; Lindahl and Regnér, 2005). These studies generally report quite large earnings effects as compared to those focusing on aggregated college classifications; often in the range of –20 to +20 percent (even wider intervals when looking at specific college majors). However, the estimated effects of graduating from individual colleges tend to be less robust and hence, less conclusive as compared to the estimates based on aggregated college divisions.

The papers that focus on labor market effects of college quality in the United States typically use rather explicit measures of quality. Average SAT scores, average faculty salaries and student rejection and retention rates are commonly used indicators in this literature. The Swedish studies do not focus on labor market outcomes of college quality as such. Instead, they attempt to estimate the earnings effects of graduating from different colleges or groups of colleges. Although the emphasis on quality is generally less pronounced in these papers, the applied classifications are often perceived to approximate various aspects of college quality. Regardless of whether a study is based on the former or the latter approach, it is important to emphasize that any translation from differences in post-college graduation earnings to differences in college quality is far from clear cut. From a theoretical point of view, any observed correlation between college type and earnings may be due to college quality influencing worker productivity. This is the human capital interpretation of college effects (Mincer, 1958; Schultz, 1961; Becker, 1962). However, earnings may differ between colleges, not because of any effect of quality but simply because employers use college type as a signal of workers' innate productivity. This is the explanation offered by the signaling/screening model (Spence, 1973). The difficulty in distinguishing between the two approaches is that both imply a positive correlation between earnings and college quality. In this paper, we make no attempt to discriminate between the two theories.[3]

The purpose of this study is to contribute to previous research on college choice and earnings in three specific respects. First of all, the paper is based on unusually rich data in terms of school grades, parental characteristics and other attributes. The school grades are used as indicators of unobserved ability and include grades in English, mathematics and Swedish, as well as grade point average, at both the compulsory school and the upper secondary school level. Introducing school grades into the analysis is important, since they are essential for explaining college selection and also have a significant impact on earnings after college graduation. Second, we focus on an

[3] For a survey and discussion of human capital vs. signaling explanations of earnings differences; see e.g. Weiss (1995), Riley (2001) and Chevalier et al. (2004).

aggregated college classification which hopefully offers reasonable support on school grades across the college groups and enough observations in each category to generate meaningful estimates. Third, in the estimations, we avoid conditioning on covariates that are determined after college graduation. If post-college graduation variables are affected by college choice, controlling for them will lead to biased estimates of the returns to college choice, and the estimated effects are therefore difficult to interpret.

The study is based on an administrative data set consisting of six cohorts of Swedes born in the years 1969−1974, who have completed at least a three-year college degree no later than 1998/1999, and who received positive earnings in 2003. We estimate the effect on earnings of graduating from five different categories of colleges: *first*, *second* and *third generation universities*; *university colleges with postgraduate education*; and *other university colleges*.[4]

In addition to securing support on ability across the college groups, this classification is chosen because it captures important differences between the colleges in terms of factors presumably related to quality. One example is formal qualifications of teachers. The percentage of faculty with doctoral degrees varies between roughly 77 percent for *first generation universities* down to about 29 percent for *other university colleges*.[5] The college groups also differ in terms of academic tradition. The majority of *first generation universities* have been around for centuries, whereas the other universities and colleges were established only a few decades ago. *First* and *second generation universities* also tend to be considerably larger in terms of sheer size, offering courses in most academic fields. Although it is likely that there are both advantages and disadvantages of tradition and size, both are presumably positively related to college quality up to some point.

The paper relies on what Heckman and Robb (1985) refers to as selection on observables to identify the earnings effect of college choice in the presence of non-random selection of students into different colleges. The data at hand and the institutional setting governing college selection lends some support to this identification strategy. The data set used is fairly rich in terms of variables likely to affect both college application and college admission, such as school grades and family background characteristics. Furthermore, the college admission procedure in Sweden is relatively transparent and to a large extent based on observable characteristics.

We present estimates for all college graduates, together with separate estimates for men and women and graduates with different college majors. Contrary to the majority

[4] See Appendix A for details of the college classification.

[5] The exact figures (averages for the period 1995−1999) are as follows: *first generation universities*, 76.9%; *second generation universities*, 60.4%; *third generation universities*, 35.9%; *university colleges with postgraduate education*, 33.4%; and *other university colleges*, 29.1%. Source: Statistics Sweden.

of previous Swedish studies, we do not find any systematic differences in estimated earnings between the college groups. At the outset, the results show that college graduates from *first generation universities* (the most prestigious group) on average receive earnings that are about 22 percent higher than college graduates from *other university colleges* (the least prestigious group). These unconditional earnings differentials are, to a large extent, explained by substantial ability sorting across the college groups. When controlling for ability and other background variables and comparing comparable treatments, nothing remains of what initially appeared to be rather large earnings differentials in favor of the more prestigious universities. This finding does not only hold when looking at all college graduates, but also when focusing on men and women separately as well as when looking at college graduates in two specific fields of education. The results suggest that an estimator of the earnings effects of college choice that does not properly adjust for ability is likely to be substantially biased.

The paper proceeds as follows. Section 2 presents the econometric strategy. Sections 3 and 4 describe the institutional setting for higher education in Sweden and present the data set available for the study. Section 5 examines support on ability and the extent of ability sorting across the college groups. Section 6 presents the empirical results and Section 7 concludes.

2. Econometric strategy

The principal econometric problem in estimating the effect of college choice on earnings follows from the non-random nature of college selection. Better students sort into more selective colleges. This paper relies on what Heckman and Robb (1985) refers to as selection on observables to identify the earnings effect of college choice in the presence of non-random selection of students into different colleges. Under this assumption, conditioning on a sufficiently rich set of observable characteristics of students removes bias resulting from non-random selection into colleges.

We follow Heckman and Hotz (1989) and sketch the idea behind the identification strategy using a linear outcome equation and a linear index function describing treatment selection. To begin, consider the following basic earnings equation:

$$\ln Y_i = X_i\beta + C_{ij}\alpha_j + \varepsilon_i \qquad (1)$$

where the log earnings of individual i, $\ln Y_i$, is a function of a standard vector of earnings regressors, X_i, a college type indicator, C_{ij}, and an error term, ε_i. The parameter of interest is α_j, which we interpret as a homogeneous treatment effect of graduating from a particular college j (= 1, …, K). Note that in this application, all individuals receive treatment in the literal sense. Therefore, α_j represents the effect of graduating from college j as compared to college $k \neq j$. We adopt the usual convention that the covariates in X_i are measured prior to treatment and are assumed to be exogenous in the traditional sense, so that $E(\varepsilon_i | X_i) = 0$.

When college selection is non-random, selection bias in the estimation of the treatment effect, α_j, can occur because of dependence between C_{ij} and ε_i, so that $E(\varepsilon_i | C_{ij}, X_i) \neq 0$. In this case, an ordinary least squares regression of $\ln Y_i$ on X_i and C_{ij} does not yield consistent estimates of the treatment effect, α_j. Let college selection be governed by the following linear index function:

$$C_{ij}^* = Z_i \gamma_j + v_{ij}, \qquad C_{ij} = j \text{ iff } C_{ij}^* > \max_{k \neq j} \left(C_{ik}^* \right) \tag{2}$$

Equation (2) states that individual i will graduate from college j if this maximizes the value of the latent variable, C_{ij}^*. The latent variable is a function of the observed vector, Z_i (which may include some of the variables in X_i), and unobserved variables reflected in v_{ij}.

The selection on observables strategy assumes that the dependence between C_{ij} and ε_i is due to observed variables, Z_i, which influence selection into treatment. In this case, controlling for the observed selection variables, Z_i, solves the problem with selection bias, so that $E(\varepsilon_i | C_{ij}, X_i, Z_i) = E(\varepsilon_i | X_i, Z_i)$. This suggests the following linear control function model:

$$\ln Y_i = X_i \beta + Z_i \delta + C_{ij} \alpha_j + \varepsilon_i \tag{3}$$

Equation (3) can be estimated by conventional linear regression techniques to obtain consistent estimates of the treatment effect, α_j.[6] This approach was first proposed by Barnow et al. (1980).

[6] Note that if some of the variables in Z_i only affect college selection but not earnings, an alternative strategy for identifying the treatment effect is to use an instrumental variable estimator or Heckman's (1979) classical selection model. Both these approaches require valid instruments for consistent and robust estimation, i.e. at least one variable in Z_i that fulfills the conditions $E(z_i|C_{ij}) \neq 0$ and $E(z_i|\varepsilon_i)=0$. In practice, it is almost impossible to test whether such a variable exists, and in this particular application there is no credible argument for a valid instrument. We therefore stick with the selection on observables strategy and try to present an adequate set of control variables.

Clearly, this is not an unproblematic identification strategy. The selection on observables assumption requires that all factors affecting both college selection and earnings are included in X_i and Z_i. Although this assumption is not directly testable, its plausibility critically depends on both the data available and the institutional setting governing college selection.[7] In Sections 3 and 4, we shall see that the college admission procedure in Sweden is fairly transparent and that the data at hand is relatively rich in terms of variables likely to affect both college selection and earnings. There are also a number of recent papers which lend some support to the proposed identification strategy (Heckman et al., 1997; Heckman et al., 1998; Dehejia and Wahba, 1999, 2002; Smith and Todd, 2005). Using experimental estimates as a benchmark, one important finding in these studies is that richer data on variables affecting both treatment and outcomes substantially reduces, but does not eliminate, the conventional measure of selection bias. In all, these papers highlight that the credibility of any particular estimator depends on both the features of the data at hand and the institutional setting present in a given context.

In practice, the selection on observables approach can be implemented either by regression or by matching. In this application, we try to estimate the treatment effect of graduating from five different groups of colleges. Therefore, we stick to the linear regression technique because it is easier to apply in a multinomial setting. With a few quite simple measures, it is also possible to make the conventional regression approach somewhat more robust. First of all, some of the parametric assumptions underlying standard regression techniques can be relaxed. As noted by Smith (2000) and others, selection bias due to functional form restrictions fades when using a more flexible specification of the regression model, including dummy variables, higher-order and interaction terms. Another step is to explicitly impose support on important variables by careful grouping or trimming of the data before running a regression. While matching estimators typically drop observations lacking sufficient support, conventional regression estimators instead achieve comparability between treated and nontreated individuals by imposing linearity and extrapolating over regions of no support. The latter approach is, however, sensitive with regard to potentially incorrect functional form assumptions. Rubin (1973, 1979) shows that regression adjustment performed on matched samples substantially reduce bias and sensitivity with regard to model specification. In this paper, special attention is devoted to the grouping of the data to obtain reasonable support on school grades, which are the most important variables for explaining college selection.

[7] Heckman and Hotz (1989) present an indirect test based on pre-treatment earnings, but unfortunately there exists no such data for this study.

One problem in implementing the proposed identification strategy is choosing which variables to control for. We wish to control for all variables affecting both the treatment and the outcome. This requires careful thought, guided by economic theory and previous empirical results, about which factors do and do not affect college selection and earnings. To make the estimated treatment effect clearly interpretable, it is particularly important to avoid conditioning on covariates that are determined by the treatment (Rosenbaum, 1984; Heckman et al., 1999; Imbens, 2004). In this particular application, controlling for post-college graduation variables (such as experience, region of work or sector of employment) can result in a biased estimate of the treatment effect, because these variables may have been affected by the treatment, and thereby carrying part of the effect. Therefore, we restrict X_i and Z_i in equation (3) to only include variables measured prior to treatment. With this approach, the estimated effect can be interpreted as the *net effect* of the treatment, i.e. the sum of the *direct effect* (or causal effect) of the treatment and the *indirect effect* which operates through the effect of the treatment on other covariates (c.f. Simonsen and Skipper, 2005).

3. Institutional setting

Higher education in Sweden is offered by universities and university colleges.[8] The main difference between a university and a university college is that university colleges are not generally allowed to provide postgraduate education and to award postgraduate degrees. However, university colleges entitled to conduct research in specific disciplines also have the right to award postgraduate degrees in these disciplines.

The higher education institutions are primarily funded by the government. They receive funding for undergraduate education based on the number of students enrolled and student performance. They also receive funding for postgraduate education and research.

In contrast to colleges in the United States and many European countries, there are no tuition fees at Swedish universities and university colleges. The government provides universal financial support for all students. The support consists of two parts: a study grant and a study loan, which together constitute the study allowance. At present, the study allowance amounts to about SEK 7,000 (875 USD) per month, a third of

[8] This description is primarily based on official documents at the website of the National Agency for Higher Education, wwweng.hsv.se. See Öckert and Regnér (2000) for further discussions on the institutional setting for higher education in Sweden.

which is grants. The study allowance is independent of social background and the parents' financial circumstances.

Historically, admission to higher education in Sweden has been unrestricted. With the 1977 Higher Education Act, the government decided that admission should be restricted and that one administrative authority, the National Swedish Board of Universities and Colleges, should handle admission to all universities and university colleges according to standardized rules of eligibility and admission. The admission requirements have changed somewhat over time, but there are no major differences between the current requirements as described below and those originally formulated in the 1977 Higher Education Act.

Applicants fulfill the general admission requirements if they have completed an upper secondary education in Sweden or abroad. The general requirements also can be attained by work experience, if the applicants are at least 25 years old and have at least four years of work experience and knowledge in Swedish and English equivalent to upper secondary school. In addition to the general requirements, most programs have specific admission requirements, such as sufficient knowledge in key subjects for a particular program.

Fulfilling the general and specific admission requirements does not guarantee admission to a given program. In practice, the number of applicants for a particular program typically exceeds the number of places available. Applicants are then grouped into various categories and ranked according to their entry credits from grade point average (GPA) in upper secondary school, scores on the Swedish Scholastic Aptitude Test (SAT), and work experience. Those applicants who have been ranked highest are admitted to the program. At least one-third of the places offered in a particular program must be allocated on the basis of upper secondary school GPA and at least one-third according to SAT scores.[9] In addition, no more than ten percent of the places can be allocated on the basis of specific proficiency or other objective grounds determined by the universities themselves.

In conclusion, the admission procedure for higher education in Sweden is fairly transparent and to a large extent based on observable qualifications, mainly upper secondary school GPA and SAT scores. Hence, Swedish colleges are not allowed to choose freely among eligible students. The combination of universal financial support for all students and the lack of tuition fees further imply that the students' financial circumstances are not likely to directly affect college selection. Altogether, the prospects for a successful selection on observables strategy are probably somewhat

[9] In practice, the share of places allocated on the basis of upper secondary school GPA dominates.

better in Sweden than for example in the United States, where college admission decisions to a larger extent are based on unobservable factors.[10]

4. Data

The data set used in this study comes from a number of administrative registers kept by Statistics Sweden.[11] The data consists of six cohorts of Swedes born in the years 1969−1974, who have completed at least a three-year college degree no later than 1998/1999, and who received positive earnings in 2003.[12,13] The focus is thus on fairly recent college graduates. The follow-up period is still long enough for most individuals to have become established in the labor market. The minimum potential post-college labor market experience is about four years, and the average around seven years. By focusing on earnings at a rather early stage in working life, we reduce the risk that any effect on earnings of graduating from different colleges becomes distorted by overwhelming noise. This could be the case if the time gap between college graduation and measured earnings becomes too long.

As previously mentioned, the identification strategy in the paper requires that we observe all variables affecting both the treatment and the outcome. On the basis of this condition and the guidance of economic theory and previous empirical research, we have constructed a data set including (1) basic individual information such as age, sex, country of birth and region of residence; (2) grades in compulsory school and upper secondary school; (3) parental characteristics such as age, country of birth, level of education and earnings of the mother and the father; (4) neighborhood attributes such as

[10] See Berg Dale and Krueger (2002) for a discussion of observable and unobservable college admission characteristics in the United States.

[11] The data sources used are the Register of the Total Population, the Register of the Population's Education, the Register of Universities and University Colleges, the Register of Grades from the Compulsory 9-Year Comprehensive School, the Register of Grades from Upper Secondary School, the Register of Income Statements and the Register of Income, Taxes and Allowances.

[12] College graduates from artistic colleges and colleges run by the county councils are excluded. 12 percent of the individuals in the data set completed more than one college degree. In this case, the degree corresponding to the highest number of credits is selected in a first stage and, if necessary, the most recent one is chosen in a second stage. Note that having two or more degrees does not necessarily imply having received more education. Typically, multiple degrees are at different levels (e.g. bachelor and master) within the same field/major. Multiple degrees at the same level are typically a professional and a general degree from the same college education. Also note that some students may begin their education at one college and graduate from another. A student's college classification is always based on the type of college he or she graduated from.

[13] Since the sample is restricted to students who have completed at least a three-year college degree, there is some potential risk for dropout bias in the reported results. This could be the case if dropouts vary systematically between the college groups. Unfortunately, the data does not allow us to pursue this issue. See Öckert (2001) for a general discussion and analyses of potential problems with dropout bias.

the level of education and average earnings in the parish of residence; (5) information on the identity of the degree awarding college, field/major and number of credits of the degree.[14] All family background and neighborhood attributes, as well as information on the individual's region of residence, refer to the situation at the age of seventeen.[15] This is roughly a year prior to the earliest possible age of college enrollment.

Compared to previous Swedish studies on college choice and earnings, the available data is unusually rich in terms of school grades. The data set includes grades in English, mathematics and Swedish, as well as GPA, at both the compulsory school and the upper secondary school level. In addition to the grades, we have information on whether the individual has taken a more advanced course in English or mathematics in compulsory school and information on the study program in upper secondary school. The latter is particularly important, since the school system at the upper secondary level is rather diverse and heterogeneous as compared to the school system at the compulsory level. As a consequence, there is no reason to assume that a given grade from different upper secondary school programs reflects similar student achievements. Upper secondary school grades are therefore combined with information on study program.

School grades or variables correlated with grades, such as SAT scores, are standard in most studies from the United States on college quality and earnings (see the papers cited in the introduction). Although school grades frequently appear in previous Swedish research on the returns to education, they have rarely been used in studies on college choice and earnings.[16] Introducing school grades into the analysis is important for two reasons. First of all, we know from the presentation of the institutional setting that upper secondary school grades are the primary determinant of college admission in Sweden. Second, previous studies on the returns to education show that school grades have a significant impact on earnings; see e.g. Kjellström (1999) and Öckert (2001). In this latter context, school grades are typically used as indicators of some type of unobserved ability that is valued in the labor market. The results in Kjellström (1999) indicate that grades from compulsory school are as good a proxy of ability in this sense as test scores from intelligence tests designed to measure verbal, spatial and reasoning skills at 12−13 years of age. Here, we follow the tradition in this literature and use the school grades as indicators of latent true ability.

[14] To save space, we refer to the papers cited in the introduction for empirical and theoretical motivations for the variables included in the analysis. Willis (1986) and Card (1999) are two excellent survey articles in the field.

[15] An exception is the total annual income from capital of the mother and the father, which for all individuals refers to the situation in 1991.

[16] Two exceptions are Gustafsson (1996) and Lundin (2006) who use upper secondary school GPA when estimating the earnings impact of college choice for students with a degree in business or economics.

The dependent variable in the analysis is the log of total annual earnings from employment and self-employment in 2003. Annual earnings are a function of both hourly wages and number of hours worked during a year. To reduce the effect of labor supply decisions and unemployment, an earnings restriction of SEK 100,000 (12,500 USD) is imposed. Antelius and Björklund (2000) show that the estimated effects of education on annual earnings with this restriction are similar to those obtained using hourly wages. This restriction has also been used in several previous Swedish studies on college choice and earnings (Gartell and Regnér, 2002, 2005; Lindahl and Regnér, 2005; Lundin, 2006). However, it is important to note that the labor supply effect on annual earnings is not necessarily exogenous with regard to college choice (and college quality). Therefore, we also use a more moderate restriction, allowing for all positive earnings. From an economic perspective, it is difficult to argue that one of the two applied restrictions is necessarily better than the other. With the higher earnings restriction, the focus is primarily on the productivity of individuals who are employed. With the more moderate restriction, the focus is both on employment opportunities and worker productivity.

In total, there are 69,220 individuals satisfying the conditions: born in the years 1969−1974; completed at least a three-year college degree no later than 1998/1999; and received positive total annual earnings from employment and self-employment in 2003.[17] Grades in compulsory school are available for 95.7 percent[18], grades in upper secondary school are available for 89.2 percent, parental characteristics are available for 93.6 percent and neighborhood attributes are available for 98.7 percent. In total, 16.1 percent of the observations have missing values for at least one variable. After deleting observations with missing values, the data set is reduced to 58,049 individuals.[19] Imposing the earnings restriction of SEK 100,000 reduces the data set further, leaving 50,563 individuals.

Table 1 reports basic descriptive statistics by college group and Table 2 presents a detailed description of the variables used in the analysis.[20] From Table 1, it is apparent that there are large earnings differentials between the college groups. College graduates

[17] Three percent of the observations in the data set fulfilled the first two conditions, but were deleted due to zero total annual earnings in 2003. There were no systematic differences between the college groups in this respect.

[18] The Register of Grades from the Compulsory 9-Year Comprehensive School only covers individuals born 1972 or later, so this figure refers to birth cohorts 1972−1974.

[19] A comparison between the initial data set of 69,220 individuals and the final data set of 58,049 individuals with complete information, reveals that the percentage of individuals born in Sweden is higher in the latter (98.6 as compared to 84.0). The bias towards individuals born in Sweden is expected, since information on previous school achievements and parental background is more likely to be missing for immigrants.

[20] Complete descriptive statistics are available from the author.

Table 1. Sample means by college group (total number of observations is 58,049)

	First generation universities	Second generation universities	Third generation universities	University colleges with postgraduate education	Other university colleges
Outcome variable					
Total annual earnings 2003	307.6	276.5	252.6	246.5	228.3
Background characteristics					
Age	31.8	31.6	31.5	31.4	31.2
Women, %	54.0	56.4	65.4	65.0	68.9
Born in Sweden, %	98.3	99.0	99.3	98.9	98.9
Compulsory school grades					
English	3.97	3.80	3.72	3.60	3.56
Mathematics	4.05	3.89	3.70	3.61	3.57
Swedish	4.16	4.00	3.95	3.84	3.82
Grade point average	4.08	3.94	3.88	3.78	3.76
Upper secondary school grades					
English	3.83	3.60	3.44	3.30	3.29
Mathematics	4.03	3.87	3.66	3.55	3.45
Swedish literature	4.01	3.77	3.66	3.54	3.52
Swedish language	3.87	3.62	3.56	3.43	3.41
Grade point average	3.97	3.78	3.63	3.52	3.49
Parental characteristics					
Dad 3-year college or higher, %	39.9	26.7	20.3	17.2	18.6
Dad total annual earnings	311.5	271.8	253.8	252.9	252.4
Mom 3-year college or higher, %	32.6	24.9	18.2	16.3	17.9
Mom total annual earnings	172.2	160.6	152.4	149.4	153.5
Neighborhood characteristics					
3-year college or higher, %	11.2	8.6	7.7	7.9	8.4
Total annual earnings	153.5	143.1	142.2	142.8	146.1
Degree level (semesters)	7.9	7.7	6.7	6.5	6.6
Number of observations	29,225	9,962	5,413	6,083	7,366

Note: Sample means for compulsory school grades are based on 26,816 individuals belonging to birth cohorts 1972−1974.

from *first generation universities* have on average about SEK 80,000 (10,000 USD) or 35 percent higher annual earnings than graduates from *other university colleges*. The share of female college graduates is lower at *first* and *second generation universities*. It is also evident from Table 1 that there are systematic differences between the college groups in terms of school grades and parental characteristics. The grades fall monotonically from *first generation universities* where we find the highest grades to

Table 2. Variable description

Outcome variable	
Total annual earnings 2003 (SEK 1,000)	Total annual earnings from employment and self-employment in 2003 (2003 SEK). Log is used.
Background characteristics	
Age	Age in 2003. Represented by dummy variables.
Women	Dummy variable indicating a woman.
Born in Sweden	Dummy variable indicating born in Sweden.
Region of residence	A set of 4 dummy variables indicating region of residence (Stockholm county, Skåne/Västra Götaland county, counties with universities, other counties).
Pre-college graduation labor market experience	Years of pre-college graduation labor market experience (coded as employed during a given year if total annual earnings from employment are ≥ SEK 100,000 (2003 SEK)). Quadratic is used.
Potential post-college graduation labor market experience	Defined as 2003 minus year/semester of college graduation. Quadratic is used.
School grades	
Compulsory school grades	1a). Grades in English, mathematics and Swedish according to a five-point number scale (1−5). Represented by dummy variables. Complemented by 2 dummy variables indicating a more advanced course in English or mathematics.
	1b). Grade point average based on all courses (about 18). Quadratic is used. Complemented by 2 dummy variables indicating a more advanced course in English or mathematics.
Upper secondary school grades	2a). Grades in English, mathematics, Swedish literature and Swedish language according to a five-point number scale (1−5). Represented by dummy variables. Complemented by a set of 6 dummy variables indicating study program.
	2b). Grade point average based on all courses (about 18). Quadratic is used. Complemented by a set of 6 dummy variables indicating study program.
Parental characteristics	
Dad/mom age	
Dad/mom born in Sweden	Dummy variable indicating born in Sweden.
Dad/mom level of education	A set of 5 dummy variables indicating level of education (primary and secondary, upper secondary, < 3 year college, ≥ 3 year college, graduate).
Dad/mom total annual earnings from employment (SEK 1,000)	Total annual earnings from employment (2003 SEK).
Dad/mom total annual income from capital (SEK 1,000)	Total annual income from capital (2003 SEK).
Neighborhood characteristics	
3-year college or higher	Percent of working age population (20−64) in the parish of residence with ≥ 3-year college education or graduate education.
Total annual earnings (SEK 1,000)	Average total annual earnings from employment of working age population (20−64) in the parish of residence (2003 SEK).
College education characteristics	
Degree level	Length of college degree in semesters. Represented by dummy variables.
Field/major	A set of 10 dummy variables indicating college field/major.

Note: All parental and neighborhood characteristics, as well as information on the individual's region of residence, refer to the situation at the age of seventeen. An exception is total annual income from capital of the mother and the father, which for all individuals refers to the situation in 1991.

other university colleges where we find the lowest grades. College graduates from *first generation universities* also have a more favorable background in terms of parental education and earnings. A similar pattern can be found for neighborhood attributes. Finally, a college degree at *first* and *second generation universities* is roughly one semester longer than the average degree in the other three college groups.

5. Ability support and ability sorting across college groups

A major difference between conventional linear regression methods and matching methods is that the latter explicitly address the so-called support problem. To illustrate this problem, consider the case where high ability individuals only graduate from high quality colleges and low ability individuals only graduate from low quality colleges. In this setting, it is impossible to identify the effect of college quality on earnings without making arbitrary assumptions about the functional form of the relationship between earnings, college quality and ability. While matching estimators typically solve the support problem by dropping observations lacking sufficient support, conventional regression estimators instead achieve comparability by imposing linearity and extrapolating over regions of no support. A problem with the latter approach is the reliance on potentially incorrect functional form assumptions, which can generate considerable extrapolation bias.[21] Rubin (1973, 1979) demonstrates that regression adjustment performed on matched samples substantially reduce bias and sensitivity with regard to model specification.

In this study, special attention is devoted to examining the extent and nature of sorting on ability into different college groups. We draw on the results of Rubin and use an aggregated classification of colleges which hopefully offers reasonable support on ability across the college groups, while still retaining enough observations in each category to generate meaningful estimates. Similar data grouping approaches can be found in, for instance, Cawley et al. (2001) and Heckman and Vytlacil (2001).

To simplify the presentation, we use GPA in compulsory school and upper secondary school as one-dimensional indicators of unobserved ability. Table 3 reports the percentage of college graduates by ability quartiles for the five college categories in question. Panel A is based on birth cohorts 1972−1974 for which compulsory school grades are available and Panel B is based on birth cohorts 1969−1974 for which only

[21] See King and Zeng (2006) for an interesting discussion and illustration of the problems of extrapolation bias in causal analysis.

Table 3. Percentage of college graduates by ability quartile

College group	Ability quartile				Total
	First	Second	Third	Fourth	
Panel A: Birth cohorts 1972−1974 (*N*=26,816)					
First generation universities	21.4	17.5	34.1	27.0	100.0 (*N*=12,056)
Second generation universities	31.0	22.3	30.2	16.4	100.0 (*N*=4,733)
Third generation universities	36.0	22.7	31.1	10.2	100.0 (*N*=2,718)
University colleges with postgraduate education	44.9	22.5	24.7	8.0	100.0 (*N*=3,205)
Other university colleges	47.0	22.3	23.7	7.0	100.0 (*N*=4,104)
Panel B: Birth cohorts 1969−1974 (*N*=58,049)					
First generation universities	16.3	20.6	27.9	35.2	100.0 (*N*=29,225)
Second generation universities	25.7	26.5	25.9	21.9	100.0 (*N*=9,962)
Third generation universities	32.1	31.0	26.3	10.6	100.0 (*N*=5,413)
University colleges with postgraduate education	40.9	31.0	20.7	7.4	100.0 (*N*=6,083)
Other university colleges	43.6	31.1	19.3	6.0	100.0 (*N*=7,366)

Note: Panel A is based on compulsory school GPA and Panel B on upper secondary school GPA.

upper secondary school grades are available. Note that the cut-off values for the quartiles in both cases are based on all observations in the respective samples.

Two main findings emerge from Table 3. First, based on our measures of ability, there is a distinct pattern of ability sorting into the different college groups. In both panels, there is a considerable overrepresentation of college graduates from the bottom quartile of the ability distribution at *university colleges with postgraduate education* and *other university colleges*, and a considerable underrepresentation of college graduates from the top quartile. For example, in Panel B the percentage of college graduates from the bottom quartile is 43.6 percent at *other university colleges* as compared to 6.0 percent from the top quartile. Random sorting would imply roughly 25 percent from each quartile. We find a similar pattern for *third generation universities*, although slightly less pronounced. For *first generation universities*, the situation seems to be the reverse. The fraction of college graduates from the top quartile exceeds what would be expected from random sorting, whereas the percentage from the bottom quartile is less than what would be expected. This pattern appears somewhat stronger in Panel B. Similar patterns of ability sorting into colleges of different types can be found in the

United States (Black and Smith, 2004) and the United Kingdom (Chevalier and Conlon, 2003).

A second finding in Table 3 is that when we look at the level of quartiles and use a college classification with five different categories of colleges, there seems to be sufficient support on ability and enough observations in each cell to generate meaningful estimates. It is possible to further examine the extent of ability support by looking at the percentage of observations in each college group that passes a condition of interior ability support (c.f. Tobias, 2003). The interior ability support is defined over the interval $[\underline{A}, \overline{A}]$, where $\underline{A}$ is simply the largest minimum value of ability in any of the college categories and $\overline{A}$ is correspondingly the smallest maximum value of ability in any of the groups. This condition guarantees that over the interval $[\underline{A}, \overline{A}]$, there is support on ability across all college categories. Using this approach, we find the percentage of observations within the interior ability support to vary between 99.4 and 100.0 percent across the college groups (once more, the analysis is based on GPA in compulsory school and upper secondary school as indicators of unobserved ability). Thus, when examining ability support at a finer level than quartiles, the college classification still seems to offer reasonable support on ability.

In conclusion, we may well admit that the distinct pattern of ability sorting across college groups found here is perhaps not surprising as such. This is what we would expect given the admission procedure for higher education in Sweden. But the results do highlight that an estimator of the labor market effects of college choice that does not properly adjust for ability is likely to be substantially biased.

6. Regression estimates of the effects of college choice

This section presents regression based estimates of the effect on earnings of graduating from the different college categories. In all estimations, we use college graduates from *other university colleges* as the reference category. Throughout the section, the dependent variable is the log of total annual earnings from employment and self-employment in 2003. The analysis begins with a very parsimonious specification of equation (3) and proceeds by including additional sets of control variables. We refer to Table 2 for a detailed description of the variables used in each step of the analysis. To make the estimated treatment effect clearly interpretable, we restrict out set of conditioning variables to only include factors determined prior to college graduation. We will try to comment on potential deviations from this principle.

Table 4 reports the estimated earnings effects of graduating from the different college groups. For comparability with previous Swedish studies, the analysis begins by focusing on individuals with total annual earnings above SEK 100,000. Column (1) shows that in a specification with only an intercept term, the estimated earnings premium ranges from about 6 to 22 percent. For instance, the coefficient 0.219 indicates that college graduates from *first generation universities* on average receive earnings that are approximately 22 percent higher than the earnings of graduates from *other university colleges* (the excluded reference category).[22]

In column (2), we add a set of basic conditioning variables which can be found in most earnings equations. They include controls for age, woman, born in Sweden, region of residence at age seventeen, pre-college graduation labor market experience and potential post-college graduation labor market experience.[23] Introducing the basic controls has a particularly large effect on the estimates for college graduates from *first generation universities*; it reduces the estimated earnings premium by about a quarter.[24] We can also see a significant drop in the estimated premium for college graduates from *second generation universities* and a small increase in the premium for graduates from *third generation universities*.

In column (3), the analysis proceeds by conditioning on ability. This has a major impact on the results.[25] The estimated earnings premium for college graduates from *first* and *second generation universities* drops by roughly 50 percent. The reduction in the estimated premium for college graduates from *third generation universities* is also considerable. These results are consistent with the substantial ability sorting across college groups shown in the previous section. In this specification, we use GPA in upper secondary school together with information on study program in upper secondary school as indicators of unobserved ability (the set of school grades referred to as 2b in Table 2). Estimations based on our alternative specifications of unobserved ability (referred to as 1a, 1b and 2a in Table 2) produce similar results (not reported).[26] These alternative specifications also show that as long as we control for study program, upper secondary school grades perform significantly better in terms of explanatory power than do compulsory school grades. They also indicate that GPA at both the compulsory

[22] The familiar calculation exp(coefficient)-1 transforms the coefficient to the exact percentage return.
[23] Controlling for potential post-college graduation labor market experience is unquestionably a departure from the principle of only using pre-treatment variables. However, we cannot ignore the strong empirical evidence on the importance of experience for earnings in early working life. Also note that by specifying post-college graduation experience as "potential" rather than "actual", we avoid conditioning on a possibly endogenous variable. For the same reason, we use region of residence at the age of seventeen as a control variable, rather than post-college graduation region of work.
[24] The basic controls are jointly significant at the 1 percent level.
[25] The ability controls are jointly significant at the 1 percent level.
[26] Complete results are available from the author.

Table 4. Estimated effects of college choice on annual earnings, with and without earnings restriction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Earnings restriction	yes	yes	yes	yes	yes	yes	no
Basic controls	no	yes	yes	yes	yes	yes	yes
Ability controls	no	no	yes	yes	yes	yes	yes
Parent controls	no	no	no	yes	yes	yes	yes
Neighborhood controls	no	no	no	no	yes	yes	yes
College major and degree level controls	no	no	no	no	no	yes	yes
First generation universities	0.219***	0.171***	0.085***	0.079***	0.077***	0.010*	−0.009
	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.013)
Second generation universities	0.135***	0.113***	0.052***	0.049***	0.048***	−0.010*	−0.020
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.014)
Third generation universities	0.058***	0.068***	0.048***	0.047***	0.047***	0.013**	0.051***
	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.015)
University colleges with postgraduate education	0.057***	0.053***	0.048***	0.046***	0.045***	0.000	0.014
	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.015)
Adjusted R^2	0.037	0.224	0.273	0.276	0.277	0.350	0.206
Number of observations	50,563	50,563	50,563	50,563	50,563	50,563	58,049

Notes: The dependent variable is the log of total annual earnings from employment and self-employment in 2003. The reference category is the *other university colleges* group. The White/Koenker test rejects the null of homoskedasticity in all specifications. We therefore report robust standard errors in parentheses. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level and * indicates significance at the 10 percent level. All regressions include a constant term. **Basic controls** include controls for age, woman, born in Sweden, region of residence at age seventeen, pre-college graduation labor market experience and potential post-college graduation labor market experience. **Ability controls** include grade point average in upper secondary school and indicators for study program in upper secondary school. **Parent controls** include controls for age, born in Sweden, level of education, total annual earnings from employment and total annual income from capital of the mother and the father. **Neighborhood controls** include the share of working age population with three-year college education or higher and the average total annual earnings from employment of working age population in the parish of residence. **College major and degree level controls** include indicators for field/major and length of college degree in semesters. See Table 2 for additional information and exact specification of the control variables.

school and the upper secondary school level performs somewhat better as compared to grades in English, mathematics and Swedish at the corresponding levels.

In column (4) we introduce parent controls. This reduce the estimated earnings effects somewhat further.[27] But the drop in the estimates is much smaller at this stage.[28] Adding neighborhood controls in column (5) essentially has no effect on the estimates.[29] At this point, the estimated earnings premium ranges from about 4 to 8 percent, with college graduates from *first generation universities* showing the highest premium.

In column (6) we proceed by conditioning on college field/major and degree level. The argument for adding these variables is that there is considerable heterogeneity in the treatment received at different universities in terms of majors offered and the length of college education. For instance, education in technology, natural sciences and medicine is much more common at *first* and *second generation universities* as compared to the other three groups of colleges. Previously, we also saw that a college degree at *first* and *second generation universities* typically is about one semester longer than the average degree in the other three college groups. If one wants to compare comparable treatments, this is something that must be taken into consideration. For the moment, the heterogeneity in the treatment is handled by introducing college field/major and degree level as exogenous control variables.[30] Looking at the results, we see that conditioning on college field/major and degree level has a major impact on the estimates.[31] The only remaining significant effect is a 1 percent earnings premium for college graduates from *first* and *third generation universities* and a 1 percent negative effect on earnings for graduates from *second generation universities*.

Up to this point, the analysis has been based on individuals with total annual earnings above SEK 100,000. This approach reduces the effect of labor supply decisions and unemployment on earnings. But as mentioned earlier, the labor supply effect on earnings is not necessarily exogenous with regard to college choice (and college quality). If labor supply decisions vary systematically among graduates from the

[27] The parent controls are jointly significant at the 1 percent level.
[28] Extending the parent controls to also include civil status, industry and sector of employment has no additional effect on the estimates.
[29] Still, the neighborhood controls are jointly significant at the 1 percent level.
[30] We cannot observe whether a student chose field of education and degree level before, simultaneously with or after college choice. However, the important thing to note is that both field of education and degree level are determined prior to college graduation and hence, can be regarded as pre-treatment variables. Similar approaches can be found in, for instance, Black et al. (1997), Chevalier and Conlon (2003) and Lindahl and Regnér (2005). Still, there has been some discussion in the literature as to whether length of education should treated as an exogenous variable as it might partly depend on college quality; see e.g. Black and Smith (2004).
[31] The college major and degree level controls are jointly significant at the 1 percent level.

different college groups, the earnings restriction can lead to biased estimates. The consequences of excluding individuals with a relatively weak position on the labor market are revealed in column (7), which presents estimates based on all college graduates with positive earnings. Comparing these results with those reported in column (6), there are two major differences. First, the precision of the estimates is much lower. Standard errors are typically two or three times as high. Second, the estimated earnings premium for college graduates from *third generation universities* increases to about 5 percent.

The latter result implies that graduates from *third generation universities* on average have a higher probability of receiving earnings above the restriction. To confirm this, we estimate the probability of having total annual earnings above SEK 100,000 in 2003 using a probit model. Apart from the definition of the dependent variable, the specification of this model is exactly the same as the one used in column (6) of Table 4. The results from this exercise (not reported) show that college graduates from *third generation universities* indeed have a higher probability of receiving earnings above SEK 100,000 as compared to graduates from *other university colleges* (the excluded reference category), whereas the probability is lower for college graduates from *first* and *second generation universities* (all estimates significant at the 10 percent level or better).[32] These results confirm those reported in Lundin (2006).[33]

Summing up the results from the analysis so far, we can conclude that, when controlling for ability and other background variables and comparing comparable treatments, nothing remains of what at the outset appeared to be rather large earnings differentials in favor of the more prestigious universities. In effect, this means that the initial earnings premiums of about 22 and 14 percent of graduating from *first* and *second generation universities* disappear in a specification with a full set of control variables. The only remaining significant effect is a small positive premium of graduating from *third generation universities*.

Separate estimates for men and women

The analysis so far has been based on all college graduates. We proceed by looking at the earnings effect of college choice for different subgroups in the sample. In addition to

[32] Complete results are available from the author.

[33] Lindahl and Regnér (2005) also focus on an earnings restriction of SEK 100,000, but find that the premium of graduating from old universities (as opposed to new universities) increases significantly in a specification without restriction on earnings, which suggests that college graduates from old universities have a higher probability of receiving earnings above the restriction.

providing further information on the effects of college choice, this should also give us some idea of the robustness of the results up to this point.

We begin by estimating the effect on earnings of graduating from the different college groups separately for men and women; see Table 5. As we previously found rather large differences in the results depending on the earnings restriction, we continue to report estimates both with and without restriction on earnings. Columns (1) and (5) show results from a specification with basic controls. For both men and women, the estimated earnings premium ranges from about 5 to 17 percent, with college graduates from *first* and *second generation universities* showing the highest premium. The estimated effects are very close to those from the corresponding specification in Table 4.

Turning to columns (2) and (6), we see that the effect of conditioning on ability is very similar for men and women. In both cases, the estimated earnings premium for college graduates from *first* and *second generation universities* drops by about 50 percent. The fall in the premium for graduates from *third generation universities* is also significant. Comparing these results with those reported in Table 4, the pattern is almost identical.

Columns (3) and (7) present results from a specification with a complete set of controls. Once more, the effect is very similar for men and women. In this specification, virtually all remaining differences in estimated earnings between the college groups disappear. This is essentially a replication of the results from the corresponding specification in Table 4. Finally, columns (4) and (8) report estimates which allow for all positive earnings. For both men and women, we find a significant increase in the estimated earnings premium for college graduates from *third generation universities*. Once more, the results from Table 4 are confirmed.

Separate estimates for different college fields/majors

We previously saw that there is considerable heterogeneity in the treatment received at different universities in terms of majors offered and the length of college education. So far, this heterogeneity has been handled by conditioning on college field/major and degree level. An alternative approach for comparing comparable treatments is to estimate separate models for graduates with different college majors.

In this section, we direct our attention towards two specific fields of education. The first is *law and social sciences*, the second is *technology*. The main argument for focusing on these fields of education is that they are well represented across the

Table 5. Estimated effects of college choice on annual earnings for men and women, with and without earnings restriction

	Men				Women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Earnings restriction	yes	yes	yes	no	yes	yes	yes	no
Basic controls	yes	yes	yes	yes	yes	yes	yes	yes
Ability controls	no	yes	yes	yes	no	yes	yes	yes
Parent controls	no	no	yes	yes	no	no	yes	yes
Neighborhood controls	no	no	yes	yes	no	no	yes	yes
College major and degree level controls	no	no	yes	yes	no	no	yes	yes
First generation universities	0.170***	0.090***	0.015**	−0.005	0.173***	0.082***	0.010	−0.005
	(0.008)	(0.008)	(0.008)	(0.014)	(0.007)	(0.007)	(0.007)	(0.018)
Second generation universities	0.127***	0.062***	−0.001	−0.000	0.103***	0.046***	−0.013	−0.030
	(0.009)	(0.009)	(0.009)	(0.015)	(0.009)	(0.009)	(0.008)	(0.020)
Third generation universities	0.044***	0.035***	0.015	0.030*	0.084***	0.055***	0.014	0.053**
	(0.011)	(0.011)	(0.010)	(0.017)	(0.009)	(0.009)	(0.009)	(0.021)
University colleges with postgraduate education	0.045***	0.052***	0.003	0.017	0.055***	0.040***	−0.001	0.005
	(0.010)	(0.010)	(0.009)	(0.016)	(0.009)	(0.009)	(0.008)	(0.021)
Adjusted R^2	0.082	0.146	0.268	0.146	0.042	0.102	0.182	0.076
Number of observations	23,314	23,314	23,314	24,074	27,249	27,249	27,249	33,975

Notes: The dependent variable is the log of total annual earnings from employment and self-employment in 2003. The reference category is the *other university colleges* group. The White/Koenker test rejects the null of homoskedasticity in all specifications. We therefore report robust standard errors in parentheses. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level and * indicates significance at the 10 percent level. All regressions include a constant term. See Table 4 for definition of controls.

different college categories. This will guarantee enough observations to generate meaningful estimates for each field/college group combination. Another motivation for focusing on these specific fields is that they include types of education demanded in both the private sector and the public sector. Law and social sciences is dominated by graduates in business and economics, whereas technology is dominated by graduates in engineering.

Table 6 shows the estimated effects of college choice for the two fields in question. Once more, the table presents estimates both with and without restriction on earnings. Columns (1) and (5) report results from a specification with basic controls. Comparing these with the corresponding specification in Table 4, there are several interesting differences. To begin with, the estimated effects are much smaller, which reflects the fact that already at the outset, we compare more homogeneous treatments. For law and social sciences, the estimated earnings premium ranges from roughly 4 to 11 percent, with college graduates from *first generation universities* showing the highest premium. For technology, we find graduates from *first* and *second generation universities* in top, with an estimated earnings premium of about 11 percent. In this field of education, we can also note a significant negative effect on earnings for college graduates from *third generation universities* and *university colleges with postgraduate education*.

Looking at columns (2) and (6), we see that the effect of adding controls for ability is very similar for the two fields in question. In both cases, the estimated earnings premium for college graduates from *first* and *second generation universities* is reduced by about 40 percent. This is more or less in accordance with the results reported in Table 4. From this we can infer that the substantial sorting on ability observed in Section 5 does not only reflect that students with different abilities are sorted into colleges offering different types of educations, but that the pattern of ability sorting between colleges also is apparent within specific fields of education.

In columns (3) and (7), we introduce a complete set of conditioning variables, including controls for college degree level. The effect is rather different if we compare with the results from the corresponding specification in Table 4. The conclusion then was that, in a specification with a full set of controls, all remaining differences in estimated earnings between the college categories disappeared. Here, we find that for law and social sciences, college graduates from all college groups receive earnings that are significantly higher than the earnings of graduates from *other university colleges* (the excluded reference category). The estimated premium ranges from about 3 to 6 percent. But we cannot conclude from this that there is any specific premium of graduating from the more prestigious universities. The estimates indicate that graduates from *second generation universities* receive earnings that are significantly lower than

Table 6. Estimated effects of college choice on annual earnings for different college fields/majors, with and without earnings restriction

	Law and social sciences				Technology			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Earnings restriction	yes	yes	yes	no	yes	yes	yes	no
Basic controls	yes	yes	yes	yes	yes	yes	yes	yes
Ability controls	no	yes	yes	yes	no	yes	yes	yes
Parent controls	no	no	yes	yes	no	no	yes	yes
Neighborhood controls	no	no	yes	yes	no	no	yes	yes
College degree level controls	no	no	yes	yes	no	no	yes	yes
First generation universities	0.109***	0.062***	0.065***	0.050*	0.117***	0.071***	−0.006	0.016
	(0.012)	(0.012)	(0.013)	(0.027)	(0.010)	(0.010)	(0.012)	(0.024)
Second generation universities	0.047***	0.029**	0.029**	0.013	0.111***	0.071***	−0.009	0.024
	(0.015)	(0.014)	(0.015)	(0.032)	(0.011)	(0.012)	(0.013)	(0.026)
Third generation universities	0.056***	0.052***	0.064***	0.098***	−0.041**	−0.033*	−0.037**	0.015
	(0.014)	(0.014)	(0.014)	(0.029)	(0.018)	(0.017)	(0.018)	(0.031)
University colleges with postgraduate education	0.035**	0.040***	0.060***	0.071**	−0.022*	−0.014	−0.022*	−0.033
	(0.014)	(0.014)	(0.014)	(0.031)	(0.012)	(0.012)	(0.012)	(0.027)
Adjusted R^2	0.162	0.197	0.228	0.152	0.126	0.136	0.148	0.101
Number of observations	14,799	14,799	14,799	16,525	13,465	13,465	13,465	14,015

Notes: The dependent variable is the log of total annual earnings from employment and self-employment in 2003. The reference category is the *other university colleges* group. The White/Koenker test rejects the null of homoskedasticity in all specifications. We therefore report robust standard errors in parentheses. *** indicate significance at the 1 percent level, ** indicate significance at the 5 percent level and * indicates significance at the 10 percent level. All regressions include a constant term. See Table 4 for definition of controls.

the earnings of graduates from *third generation universities* and *university colleges with postgraduate education* who, in turn, receive earnings roughly the same as the earnings of graduates from *first generation universities*.[34]

Looking at the reported results for the field of technology, we find a 2 to 4 percent negative effect on earnings of graduating from *third generation universities* and *university colleges with postgraduate education*. But as college graduates from *first* and *second generation universities* on average seem to receive the same earnings as graduates from *other university colleges* (the excluded category), we again arrive at the conclusion that there is no indication of any special earnings premium of graduating from the more prestigious universities.

Finally, columns (4) and (8) present estimates allowing for all positive earnings. For law and social sciences, we once more find a substantial increase in the estimated earnings premium for college graduates from *third generation universities*. The significant premium of graduating from *second generation universities* disappears, whereas the remaining significant estimates range from about 5 to 10 percent.[35] For the field of technology, we can observe that in a specification without restriction on earnings, there are no significant differences in estimated earnings between the college groups.

7. Summary and concluding remarks

The purpose of this paper has been to estimate the effect on earnings of graduating from five different groups of colleges. The study is based on a large administrative data set that is unusually rich in terms of school grades, parental characteristics and other attributes. Contrary to the majority of previous Swedish research, we do not find any systematic differences in estimated earnings between the college categories. At the outset, the results show that college graduates from *first generation universities* (the most prestigious group) on average receive earnings that are roughly 22 percent higher than college graduates from *other university colleges* (the least prestigious group). These unconditional earnings differentials are, to a large extent, explained by substantial ability sorting across the college categories. Low ability students are heavily

[34] Using a Wald test, we reject the null that the estimated premium for graduates from *second generation universities* is the same as the estimated premiums for graduates from the other three college groups (*p* value 0.006), but we cannot reject the null that the estimates for the latter three are the same (*p* value 0.896).

[35] In this case, the Wald test rejects the null that the remaining three significant estimates are the same (*p* value 0.072).

overrepresented in the two college groups, whereas high ability students are overrepresented in the most prestigious university group. When controlling for ability and other background variables and comparing comparable treatments, nothing remains of what initially appeared to be fairly large earnings differentials in favor of the more prestigious universities. This finding does not only hold when looking at all college graduates, but also when focusing on men and women separately as well as when looking at college graduates in two specific fields of education. The results indicate that an estimator of the earnings effects of college choice that does not properly adjust for ability is likely to be substantially biased.

Can we draw any conclusions about college quality on the basis of these findings? In the literature focusing on the effects of college choice, earnings differentials among students having graduated from different colleges are typically perceived to reflect differences in college quality. Following this reasoning, the lack of significant differences in estimated earnings in this study would suggest that the different groups of colleges produce education of more or less equal quality. A weak support in favor of this interpretation is the fact that we find no gender differences in the college estimates. This is what we would expect, since men and women with the same major and degree level from the same college are likely to have received education of a similar quality. Neither for men, nor for women do the results indicate any systematic differences in estimated earnings between the college groups.

On the other hand, we know that the more prestigious universities have a comparative advantage in terms of factors likely to be related to college quality, such as formal qualifications of teachers. An alternative interpretation is therefore that the more prestigious universities indeed produce education of higher quality, but that the relationship between educational quality and earnings is particularly weak in the Swedish labor market, with its strong unions and compressed wage structure. However, during the last 10 to 15 years, there has been a clear trend of decentralization towards individual wage bargaining, followed by a dramatic increase in wage differences among white-collar workers in Sweden (Lundborg, 2005). This suggests that traditional market forces after all have affected wage setting during the period in question here. To some degree, this challenges the argument of a particularly week relationship between educational quality and earnings in the Swedish labor market.

There are still only a few available studies using Swedish data to estimate the labor market effects of college choice and hence, a great deal of scope for further research. One useful topic is to introduce non-parametric estimation techniques to more explicitly handle the support issue and the linear conditioning issue discussed in this paper. Another important development is to use estimation methods that can eliminate the

effect of lingering selection on unobservables and dropout bias. Other relevant issues for future research are to introduce explicit measures of college quality into the analysis and to evaluate the effect of college choice on other outcome variables than earnings. In all, such improvements will contribute to shed further light on the robustness of the reported effects of college choice in Sweden and other countries.

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Appendix A. College grouping

First generation universities	Second generation universities	Third generation universities	University colleges with postgraduate education	Other university colleges
Chalmers University of Technology (1829)	Linköping University (1975)	Karlstad University (1977)	Blekinge Institute of Techonology (1989)	Dalarna University College (1977)
Göteborg University (1891)	Luleå University of Technology (1971)	Växjö University (1977)	Jönköping University College (1977)	Halmstad University College (1983)
Karolinska Institutet (1810)	Umeå University (1965)	Örebro University (1977)	Malmö University College (1998)	Kristianstad University College (1977)
KTH – Royal Institute of Technology (1826)			Mid Sweden University College (1977)	Stockholm Institute of Education (1956)
Lund University (1666)			Mälardalen University College (1977)	Stockholm University College of Physical Education and Sports (1966)
SLU – Swedish University of Agricultural Sciences (1848)			University College of Kalmar (1977)	University College of Borås (1977)
Stockholm School of Economics (1909)				University College of Gävle (1977)
Stockholm University (1878)				University College of Skövde (1983)
Uppsala University (1477)				University College of Trollhättan/Uddevalla (1990)

Notes: The division is based on the official status of the colleges in 1999. Year of establishment in parentheses. In some cases, the colleges began providing limited education a few years earlier than reported in the table. Karlstad, Växjö and Örebro were originally established as university colleges, but received official status as universities in 1999.