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Do university enrollment constraints affect education and earnings?

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Abstract

In most countries the number of places at the universities is restricted. This paper estimates the effect of university enrollment constraints in 1982 on years of education and earnings in Sweden 1981-96. The effect on educational attainment is related to labor market performance, to estimate the effect of education on earnings. The variation used is driven by discrete jumps in the admission selection to university. The results show that university enrollment constraints affect educational attainment over the entire period studied. In 1996, admitted applicants in 1982 have about one quarter of a year longer education than screened out applicants. The effect of enrollment constraints changes with time. In the end of the panel, admitted applicants in 1982 are no better off than screened out applicants. The estimated return to education in Sweden is very low, both with least square and instrumental variable techniques.

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1 Introduction

In most countries the number of places at the universities is restricted. Some countries like Finland, Greece, Portugal and Sweden have centrally decided numerus clauses in all or most fields of study, while others like Germany, the Netherlands, Spain, the United Kingdom and the US let the individual institutions set the enrollment constraints themselves (Kirstein, 1999). The degree of selectivity varies between different institutions and countries. In Finland, for example, only 30 per cent of the applications are accepted (Statistics Finland, 1999). The acceptance rate is about 75 per cent in the UK (Higher Education Statistics Agency, 2001). In the US, even the lowest ranked universities in practice reject more than 10 per cent of their applicants (Winston, 1999).

When enrollment is restricted, students are allocated to university on the basis of either prices or performance indicators. The price for higher education is often low in Europe, which is why test scores or grades are used to select which individuals to admit. In the US, on the other hand, both prices and performance indicators are used to allocate students to university (Fernández, 1998). Although most institutions of higher education select their students, little is known about the effect of restricted admission on educational attainment and earnings.[1] This study estimates the impact of university enrollment constraints in 1982 on educational attainment and labor market performance, as well as the effect of education on earnings, in Sweden 1981-96. The variation used is driven by discrete jumps in the admission selection to university.

The 1977 Higher Education Act introduced enrollment constraints to all university studies in Sweden as well as standardized rules of eligibility and admission. Roughly 80 per cent of all available places were allocated centrally by two admissions authorities, while the universities filled the remaining places locally. Since the admission selection was centralized, applicants could in principle only be admitted to one program at one university at the time. The degree of selectivity at all universities was - and still is - high, not at least since tuition is free of charge and since all students are offered rather generous student grants. As a consequence, no more than half the pool of applicants a given year has gained admission to

[1] To my knowledge only two studies are directly (or indirectly) devoted to labor market effects of university enrollment constraints and admission selection. Dale and Krueger (1999) use student application behavior and college admission decisions to control for unobserved characteristics when estimating the effect of attending a more selective college. Bowen and Bok (1998) analyze the effects in the education and labor markets of using race in college admissions.

university during the last two decades. Hence, the enrollment constraints probably constitute a binding restriction for many people in Sweden - not only at a given point of time - but also in the long run.

University enrollment is typically non-random. If there is enrollment constraints, the allocation of individuals to university involves both self-selection and admission selection. Both of these selection mechanisms must be accounted for to find the causal effects of enrollment constraints and the effect of education on earnings. The identifying strategy in this paper involves three steps. First, admitted applicants are compared to different groups of non-admitted individuals, to differ out common unobserved determinants of self-selection to university (as well as differences in eligibility status). The university enrollment constraints and the selection of students to university generate a number of candidate comparison groups - non-applicants, non-eligible applicants and screened out applicants - that resembles the admitted applicants to different extent. In particular non-admitted applicants are well suited as a comparison group for admitted applicants, since all those involved are self-selected for higher education.

Second, dummy variables for all combinations of educational alternatives and admission quota groups are added to the model. This is because individuals differ, not only in whether they apply to university or not, but also in which universities and programs they prefer as well as in their educational background. Conditional on these characteristics, the admission decision lies in the hands of the admissions authorities. This means that admitted applicants to a particular alternative in a given admission quota group have a comparison group "of their own" consisting of non-admitted applicants to the very same alternative and quota group.

Third, admission selection is controlled for using the same information and selection rules as did the admissions authorities. Eligible applicants were ranked with respect to their formal merits, and admission was given to applicants who were ranked higher than some threshold value (determined by the number of places available). All the admission selection determinants for eligible applicants are available in the data. Since the admission status is deterministic given the observed underlying variables, the effects of enrollment constraints can be identified using a regression-discontinuity design (Thistlethwaite and Campbell, 1960).

The discrete jumps in admission status can, apart from being used to find causal effects of enrollment constraints on educational attainment and labor market performance, also be used to estimate the effect of education on earnings. If years of education changes discontinuously at the break-

point of the entry credits, admission status in 1982 is a valid instrument for education given smooth functions of the admission selection variables. Since the instrument is dichotomous, the estimator is given by the ratio between the effect of university enrollment constraints on earnings and their effect on years of education.

The results show that university enrollment constraints in 1982 affected educational attainment in the short as well as in the long run. In 1996, admitted applicants in 1982 on average have one quarter of a year longer education than screened out applicants. The effects of enrollment constraints on earnings differ with time. In the years following the admission selection in 1982, there is an earnings dip for admitted applicants. They catch up in the late 1980s and get ahead until the late 1980s. In 1988 the effect of enrollment constraints on earnings is estimated at almost 11,000 SEK. The effect then peters out and is completely eliminated in the end of the panel. The observed pattern can probably be attributed to differences in university enrollment rates over time. Since the effect of university enrollment constraints on years of education has no parallel in earnings, the estimated return to education using admission status as an instrument for education is very low (although measured with low precision). However, if the sample is restricted to those who earn more than 100,000 SEK in 1996, the least squares estimates suggests that the return to education is no higher than 1.7 per cent.

The rest of the paper is organized as follows. In Section 2 the enrollment constraints and the admission selection to university in Sweden in 1982 is described. The empirical strategy and the econometric models are given in Section 3, while Section 4 describes the data and reports some descriptive statistics. The results are given in Section 5 and Section 6 concludes the paper.

2 University enrollment constraints and admission selection in Sweden

Higher education in Sweden is provided by public universities and university colleges.[2] The studies are free of charge,[3] and student grants are available for all students in the form of allowances and loans. At the undergraduate level, students can choose between programs and single-

[2]The main difference between them is that university colleges generally do not provide graduate education. I will use the term university throughout the paper.

[3]There is a small fee to the student union.

subject courses. The programs vary in length and students are awarded a degree on successful completion of a full program. The courses are usually shorter but can be combined to a full degree.

Historically, admission to higher education in Sweden has been unrestricted. As the high school system expanded and the size of the cohorts increased, the question of enrollment constraints was brought up in the late 1960s. The 1977 Higher Education Act introduced enrollment constraints to all university studies in Sweden as well as standardized rules of eligibility and admission.[4] The programs and courses available, their curriculum and length as well as the number of places at different programs and universities were set by the Government. The admission to university took place twice a year, with most places being allocated in the fall semester. To be eligible to study at the universities, the students had to complete at least two years of high school. Good knowledge in subjects of particular importance to the studies in question was generally also required. All individuals had to apply for admission to the admissions authorities or to the universities. The National Swedish Board of Universities and Colleges (NSBUC) (Universitets- och högskoleämbetet, UHÄ) administered the applications for most of the places available in higher education.[5]

Applicants to the NSBUC could apply for admission to a maximum of twelve different educational alternatives.[6] All applicants were assigned to one or more of nine admission quota groups, depending on their educational background. There were two quotas for applicants with short high school, two for individuals with long high school, one for those with a particular form of adult education (folkhögskola) two for returning adults, and three for other applicants. The intention of the quota group system was to give individuals with different backgrounds an equal opportunity to study at the universities. One particular goal was to counteract the

[4] With the 1993 Higher Education Act the selection of applicants has become somewhat more liberal.

[5] In the fall of 1982, the NSBUC filled about 60 per cent of all available places at Swedish universities. In addition, the NSBUC handled admissions to about 6 per cent of places subject to special admission rules. The other central admissions authority, the Association of County Councils' admissions board for higher education (Landstingsförbundets antagningsnämnd), handled admissions to municipally administered undergraduate programs covering around 13 per cent of available places. Finally, about 21 per cent of the places were in the form of programs or separate courses allocated by the universities themselves.

[6] An educational alternative means a particular program at a particular university. In the autumn of 1982 there were 136 different programs distributed over 35 universities, amounting altogether to 466 different educational alternatives.

skewed socio-economic distribution among students. Predetermined rules as well as the relative number of applicants settled the allocation of places among the quota groups.

If the number of applicants for an alternative in a given quota group exceeded the places available, applicants were ranked according to their entry credits. The entry credits consisted of different components in different quota groups, namely grade point averages from high school (GPA), scholastic aptitude test scores (SAT), evaluations of earlier performance, and credits for work experience. The higher the entry credits, the higher the ranking. Applicants with the same credits were separated by their ordering of the alternative in relation to the other alternatives for which they had applied. However, if two or more applicants had the same credits and the same ordering of an alternative, their priority in the competition for places was determined by drawing lots, see Öckert (2001). The admission rule was to admit applicants who were ranked higher than some threshold value (given by the places available).

The admission selection process consisted of four rounds. The first three were conducted by the NSBUC, and the last one by the universities themselves. In the first two rounds all eligible applicants competed for admission to a maximum of twelve alternatives. An applicant gained admission if he or she was ranked higher than the number of places available. Lower ordered alternatives, to which an individual had applied, were then automatically cancelled. In the third round, the non-admitted applicants could remain on the waiting list for (at most) two programs only. Unless the applicants wanted to reorder the alternatives, the originally two highest ranked alternatives were kept. The universities then performed the final round of admissions, in which vacant places were allocated according to a waiting list.

3 Econometric issues

Traditional human capital theory assumes that individuals can freely choose the number of years of education so as to maximize the present value of their net expected benefits (e.g. Becker, 1962). Individuals are, in this way, self-selected into different levels of education on the basis of a number of characteristics, such as outcome potential and the response to education. In reality, there are restrictions on individual choices at most levels of education (e.g. mandatory education or enrollment constraints), and the years of education is determined by yet another set of characteristics. Unless the whole selection mechanism is taken into account, estimates of

the effects of education may suffer from bias (Heckman and Robb, 1985). Thus, to estimate the effect of enrollment constraints of education and earnings, and to estimate the effect of education on earnings, both self-selection and admission selection to university must be taken into account. The identifying strategy in this paper involves three steps.

3.1 Using different comparison groups

The first step of the identification strategy is to use different comparison groups to differ out common unobserved determinants of self-selection to university (as well as differences in eligibility status). The problem is to find a group of individuals who are similar to admitted applicants, but who were not admitted. Studies of the return to higher education have often used individuals who have chosen to attend lower levels of education as comparison groups.[7] The main problem with this approach is that individuals with different levels of education may differ in unobserved characteristics as well. When the number of places at the universities is restricted, the admission process generates a number of candidate comparison groups. In particular non-admitted applicants are well suited as a comparison group for admitted applicants, since all those involved are self-selected for higher education.[8] This study makes use of three comparison groups for admitted applicants: non-applicants, non-eligible applicants (non-eligibles) and screened out applicants (screen-outs).

The information available in the data differs with respect to the comparison group. Non-applicants lack information on both educational preferences and entry credits, while non-eligibles lack data on entry credits. Therefore, the first empirical model focuses on the effect of using different comparison groups conditional on a few demographic characteristics only. Consider a regression model that relates education or earnings to a number of observed and unobserved characteristics. Assume for now that the effect of enrollment constraints is equal for all individuals. The outcome

[7] Kane and Rouse (1995) and Leigh and Gill (1997) analyze the return to two and four years of college compared to high school graduates. Blundell et al. (2000) use individuals with the highest secondary school qualification (A level) as the comparison group for individuals with different university qualifications.

[8] Non-participating program applicants have been used as comparison group also in other studies, e.g. when evaluating the effects of military service (Angrist, 1998), private school vouchers (Rouse, 1998), training programs (Bell et al., 1995), disability insurances (Bound, 1989), vocational rehabilitation programs (Dean and Dolan, 1991) and child care services (Berger and Black, 1992).

for the ith individual at time t, can then be written as

$$Y_{it} = X_i\beta + D_i\alpha + \varepsilon_{it}, \tag{1}$$

where Y_{it} is a measure of education or earnings, X_i is a vector of time invariant demographic variables, D_i is a dummy variable for admission status in the fall of 1982, and ε_{it} is an individual specific error component. In the empirical part of the paper I will denote this model OLS (ordinary least squares).

As a benchmark, admitted applicants are compared to non-applicants. This might be the only comparison group at hand, in the absence of information from the admission selection process. However, the study and comparison groups differ, not only in admission status, but also in application behavior and eligibility status. Non-eligibles are more similar to admitted applicants in that they have applied to university. On the other hand they have not fulfilled the eligibility requirements, which a fraction of the non-applicants have. The primary comparison group for admitted applicants is the screened-out applicants, since they satisfy the eligibility requirements and have applied for admission to university. However, they differ from admitted applicants in the admission selection. It is, thus, not possible to a priori determine how the total bias is affected by using screen-outs instead of non-applicants or non-eligibles as comparison group for admitted applicants.

3.2 Controlling for program preferences and educational background

Individuals differ, not only in whether they have applied to university or not, but also in their educational background and their preferences for different universities and programs. Dale and Krueger (1999) show that controlling for the set of colleges at which *enrolled* students were accepted and rejected has a big impact on the estimated effect of attending a more selective college. The empirical strategy in this study differs from theirs in (at least) two ways. First, admitted applicants are compared to non-admitted individuals (and not to other admitted applicants), to find the absolute (and not the relative) effect of enrollment constraints. Second, information of educational preferences is available not only for universities but also for different programs. There is also data on educational background.

The second step in the empirical strategy is to add dummy variables for all combinations of educational alternatives and admission quota groups

(alternative-quota groups) to model (1). I will call this model FE (fixed effects). The extension restricts the sample in two ways. First, information on program preferences and educational background is available for applicants only, why non-applicants cannot be used as a comparison group. Second, it is only possible to make these controls if there are more applicants than places available within an alternative-quota group, otherwise there will be no within-group variation in admission status. Therefore, applicants to alternative-quota groups with fewer applicants than places available (or with no applicants being admitted) are eliminated. The FE model is given by:

$$Y_{it} = X_i\beta + M_i\gamma + D_i\alpha + \varepsilon_{it}, \tag{2}$$

where M_i is a vector of dummy variables for all combinations of educational alternatives and admission quota groups. The implication of the FE model is that applicants to a particular alternative in a given admission quota group have a comparison group "of their own", consisting of non-admitted applicants to the very same alternative and in the very same quota group. Hence, there are as many pairs of study and comparison groups as there are alternative-quota groups.[9]

Comparing admitted applicants to different groups of non-admitted applicants in combination with controlling for educational preferences leaves no room for self-selection. Conditional on having applied to a particular educational alternative and having been placed in a particular admission quota group, the choice of admission lies in the hands of the admissions authority. Even though the applicants' willingness to pay for their university studies probably varies, this has no direct effect on who eventually gains admission. Admission selection to university might, however, create at least as big a bias in the estimates as self-selection might.

[9] When summing the admission-quota groups together, it is important that each comparison group gets the same weight as its corresponding study group. However, since the numbers of admitted and non-admitted applicants typically differ within an alternative-quota group, this need not to be the case. To make the study and the comparison group means comparable - and to find the effect of treatment on the treated - non-admitted applicants are related to the number of admitted applicants. This is done by weighting the non-admitted applicants with the relative number of admitted to non-admitted applicants in that alternative-quota group. Admitted applicants are left unweighted.

3.3 The regression-discontinuity design

The ideal way to solve the problem of admission selection would be to randomly admit applicants to university. The admitted applicants and the non-admitted applicants would then on average have equal characteristics, and taking the mean difference in education or earnings between them would provide an unbiased estimate of the effect of enrollment constraints. In reality, however, admission to university is typically non-random, and probably correlated with unobserved outcome potential or individual response to treatment.

The admission selection rules in Sweden in the early eighties were clear and simple. Eligible applicants were ranked with respect to their formal merits and educational preferences, and admission was given to applicants who were ranked higher than some threshold value (determined by the number of places available). All the admission selection determinants for eligible applicants are available in the data. Hence, admission status is a deterministic function of observed underlying variables. Under the assumption that economic behavior evolves smoothly, the effects of enrollment constraints can be identified using a (sharp) regression-discontinuity design (Thistlethwaite and Campbell, 1960).

Identification is attainable by noting that individuals just above the cut-off point are similar on average to the ones just below, since they have almost identical values on the admission selection variables. If the effect of the selection variables can be assumed to be continuous around the cut-off point, the treatment effect can be identified for individuals just above the threshold. The mean outcome difference for marginal individuals, will then provide an unbiased estimate of the mean effect of enrollment constraints (Hahn et al., 2001). Formally:

$$\lim_{z_i \to z_o^+} E\left[Y_{it} \mid X_i, M_i, D_i = 1\right] - \lim_{z_i \to z_o^-} E\left[Y_{it} \mid X_i, M_i, D_i = 0\right] = \alpha, \qquad (3)$$

where z_i is the admission selection variables and z_o is the cut-off value. Without any further assumptions the effect can only be identified for those at the cut-off point; the greater the distance from the threshold value, the more biased the estimate may be.

What about heterogeneous treatment effects? If the response to treatment is heterogeneous with respect to the admission selection variables, the effect is only valid for those around the breakpoint. Hence, comparing marginal applicants will identify the marginal effect of treatment (ME). However, if the response to treatment is heterogeneous only with respect

to (observed or unobserved) characteristics that are unrelated to the selection variables, the estimated effect of treatment is valid for all treated individuals. This is because such variables do not affect the allocation of treatment, why treated individuals do not systematically differ from untreated individuals in this respect.

It is possible to use all observations in the sample to estimate the effect of enrollment constraints, under the assumption that including smooth functions of the admission selection variables will remove their direct effect on the outcome. This can be viewed as a functional form assumption that extrapolates the treatment effect above and below the cut-off point. If smooth functions of the admission selection variables will capture all systematic difference in outcome potential between admitted and non-admitted applicants, the effect of treatment on the treated (TT) can be identified even under heterogeneous response to treatment. Also, using the entire sample will produce more precise estimates.

The third step of the empirical strategy in this paper is to control for admission selection to university by adding flexible specifications of the admission selection variables to model (2). The model is denoted FE-RD (fixed effects regression-discontinuity) and is given by:

$$Y_{it} = X_i\beta + M_i\gamma + Z_i\delta + D_i\alpha + \varepsilon_{it}, \tag{4}$$

where Z_i is a vector of admission determinants. Due to data limitations, non-eligibles cannot be used as a comparison group in this part of the analysis.

A weakness of the RD design is that identification is typically attainable around the breakpoint only, i.e. for a handful of marginal individuals (Hoxby, 2000). The admission selection system in Sweden in 1982 can, at least in part, overcome this problem. First, there is not only one threshold value, but many. All combinations of alternatives and admission quota groups, have a cut-off point of their own. It is, thus, possible to identify the treatment effect around almost 2,300 break-points. Second, applicants to some alternative-quota groups have been randomly admitted to university at the margin, due to having equal merits and the same educational preferences and background (Öckert, 2001). There is, thus, some pure randomness around the breakpoint in some cases. The effect of enrollment constraints is then attainable not only at the breakpoint, but also within its immediate range.

3.4 Estimating the return to education

The exogenous variation in admission status can, apart from being used to find causal effects of enrollment constraints on educational attainment and labor market performance, also be used to estimate the effect of education on earnings. The sharp regression-discontinuity design exploits the fact that admission status among eligible applicants is *deterministic* given the admission selection variables. The relation between admission status and educational attainment does not typically have this characteristic.[10] However, if the regressor of interest is partly determined by a *discontinuous* function of observed covariates, the effect can be identified using a *fuzzy* regression-discontinuity design. Admission status is, thus, a valid instrument for education, given smooth functions of the admission selection variables (van der Klauuw, 1997; Hahn et al., 2001).

As a benchmark, the return to education is estimated using different comparison groups and different sets of control variables. This follows the same plan as above, and can be viewed as a mixed selection-on-observables and selection-on-unobservables approach. Different comparison groups and information on performance indicators and educational preferences can probably help to reduce the potential bias in the education estimates, even without using instrumental variables. The relation between earnings and years of education for the ith individual at time t, is given by:

$$W_{it} = X_i\tau + M_i\varphi + Z_i\pi + S_{it}\lambda + \nu_{it}, \tag{5}$$

where W_{it} is earnings and S_{it} is years of education. Non-applicants, non-eligibles and screen-outs will be used as comparison groups for admitted applicants. The model is denoted OLS if it conditions on the demographic characteristics, X_i, FE if it also controls for alternative-quota groups, M_i, and FE-RD if it also conditions on the admission selection variables, Z_i.

The instrumental variable model uses admission status as an instrument for education, given both the alternative-quota groups and the admission selection variables. Only screen-outs are used as a comparison group. Since the instrument is dichotomous the IV estimator can be expressed as the ratio between the reduced form estimates of admission status on earnings and of admission status on years of education:

$$\lambda_{IV} = \frac{E\left[W_{it} \mid X_i, M_i, Z_i,\ D_i = 1\right] - E\left[W_{it} \mid X_i, M_i, Z_i,\ D_i = 0\right]}{E\left[S_{it} \mid X_i, M_i, Z_i,\ D_i = 1\right] - E\left[S_{it} \mid X_i, M_i, Z_i,\ D_i = 0\right]}. \tag{6}$$

[10] This would be the case only if all admitted applicants receive treatment, while none of the screen-outs do. In reality, admitted applicants can choose to drop out from a program, while screen-outs can substitute something else for it.

This is the Wald (1940) estimator, which relates the difference in earnings by admission status to the difference in years of education by admission status. Under the common treatment effect assumption this estimator identifies *the* effect of education on earnings.

The instrumental variable estimator can be given a causal interpretation also in a world of heterogeneous response to treatment. Imbens and Angrist (1994) have developed an estimator known as the local average treatment effect (LATE). The crucial assumption is that no individual counteract his or her assignment. In this case it means that screen-outs that attend university would have done so also if they had been admitted in the fall of 1982 (monotonicity). The Wald estimator (6) can then be interpreted as the treatment effect for those who change treatment status as a response to the instrument (compliers).

The main analysis is based on the entire sample of eligible applicants. However, if the model specification does not entirely account for all the direct effects of the admission selection variables on the earnings, the estimated effect of education on earnings might be biased. As a stability check, the model is therefore estimated for individuals with different absolute distance from the threshold value. The functional form assumptions have less importance the smaller the variation in the admission selection variables is. In the end, only those with equal formal merits, but who differ in educational preferences and in the randomization, are being studied.[11]

4 The data and descriptive statistics

The data has been collected from administrative records kept by the Swedish National Archives (Riksarkivet) and Statistics Sweden (Statistiska centralbyrån), and include information on educational and labor market characteristics over the 1981-96 period. It covers all 62,265 applicants for undergraduate programs with central admission at the NSBUC in the fall of 1982, and a sample of 12,416 non-applicants at the same point of time. The non-applicants are stratified with respect to seven age classes and three educational classes to fit the structure of the applicants.[12]

[11] This approach differs from reducing the sample with a fixed share of the most "extreme observations" for admitted and non-admitted applicants. Identification in the RD design is typically attainable for those close to the threshold only. Eliminating a fixed share of the sample would not reveal if there actually are any individuals around the break-point. Also, it would keep any unbalances in the sample sizes of admitted and non-admitted applicants.

[12] For a more detailed description of the data see Öckert (2001).

The NSBUC data covers the last round of the central admission process, i.e. when applicants compete for at the most two educational alternatives. There is information on educational background, eligibility status, the programs that the applicants have applied for, which quota groups they have been placed in, their entry credits, their position on the waiting list, etc. It does not include any data from the local admission at the universities. However, the Register of Universities and University Colleges at Statistics Sweden has detailed information on enrollment and graduation from all undergraduate courses and programs in Sweden in the period 1977-96.[13] Thus, by matching the two sources together, it is possible to track down the individuals on the waiting list that have enrolled to a particular alternative in the fall of 1982. Since the place offers were given to the applicants on the waiting list in order of priority, all applicants above the last enrolled applicant are treated as being admitted.

All individuals have been merged with a number of other registers from Statistics Sweden. The Register of the Population's Education includes completed education at all levels. Information on earnings and transfers for the period 1981-96 is derived from the Register of Income, Taxes and Allowances. It combines information from the tax assessment and statements of income and allowances. It also includes data on gender, age, residence, marital status and ethnicity. Information on the parents' education has been obtained by linking the individuals to the households in which they were living according to the 1960 and 1965 censuses.[14] About 8 per cent of the individuals in the sample lack information on family background.

The years of education variable is derived from a number of administrative records. The data include complete education at all educational levels, which has been converted to effective years of education (based on expected time of completion). Time spent in school or university that do not lead to a formal degree is not registered.[15] The earnings are defined as the sum of gross wage earnings and compensation during temporary

[13] There is also information on graduate school enrollment and graduation.

[14] This must not be their biological parents.

[15] Less than 80 per cent of the program entrants in the academic year of 1987/88 received a degree within eleven years. The corresponding figure for those who enrolled to a separate course is less than 30 per cent (Statistics Sweden, 1999). To account for incomplete university education, I have constructed an alternative measure on years of education based on proxy variables (registrations and student grants) for university studies. The least squares estimates on the return to education then becomes somewhat lower and more sensitive to the choice of model specification, while the IV-estimates are about the same. The results are available from the author on request.

work absence (basically due to illness or parental leave) in 1996 SEK.[16] No earnings restrictions are imposed. Both years of education and earnings are observed in the 1981-96 period, which makes it possible to analyze the dynamics of university enrollment constraints. Since there is information from the pre-application period (1981), any differences in unobserved outcome potential between the study and the comparison groups can be tested for.

The primary regressor is a dummy variable indicating admission status in the fall of 1982 (for programs with central admission at the NSBUC). Different sets of control variables are used in different model specifications. The OLS model controls for a number of demographic characteristics: year of birth, a dummy variable for being a woman, seven dummy variables for the father's highest educational level and seven dummy variables for the mother's highest educational level. The FE model also includes dummy variables for all combinations of alternatives and admission quota groups. The FE-RE model also controls for entry credits in the nine admission quota groups as well as the applicants' ranking of the alternative (an integer ranging between one and twelve).

As discussed in the econometric section, most empirical models require the number of applicants to exceed the number of places available. To make the results comparable between different model specifications, this restriction is set for the entire analysis. In the fall of 1982, there were on average 3.5 eligible applicants per study place. However, the distribution of applicants was skewed and the competition to some alternatives-quota groups low. In particular, there were few applicants in the admission quota groups for foreign students. Eliminating alternative-quota groups with fewer applicants than places reduces the number of alternative-quota groups with about one third, from 3,425 to 2,298. Still, all admission quota groups and almost all alternatives are represented in the sample.

Applicants compete for at the most two alternatives in up to five different quota groups. To facilitate the analysis, individuals have been duplicated the number of times they appear in different alternatives and admission quota groups. This means that the same individual can generate several observations and that every observation refers to one alternative and one admission quota group only. In this way the number of observations increases to 147,115.[17]

[16] The average USD/SEK exchange rate in 1996 was 6.70.

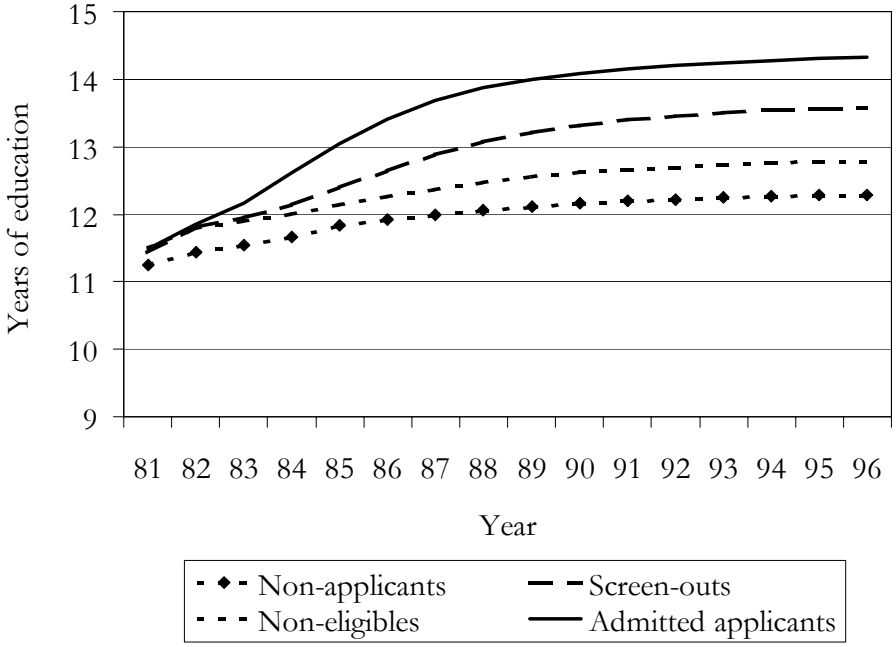
[17] The standard errors are adjusted for clustering (Moulton, 1986) in the analysis.

4.1 Years of education and earnings profiles

Weighted means and standard deviations for the demographic characteristics and the admission selection variables are reported in Table A1 in the Appendix. The typical person is born in the late 1950s or in the early 1960s. Roughly half the sample is females. Applicants are more likely than non-applicants to have a parent with at least a university degree. Also, admitted applicants have in general better family background than non-eligibles or screen-outs. As expected, the entry credits for admitted applicants are much higher than for screen-outs (almost two standard deviations).

Table A2 in Appendix and Figure 1 report years of education between 1981 and 1996. All groups experience a rapid increase in educational attainment the first seven or eight years of the panel.

Figure 1 Years of education 1981-96

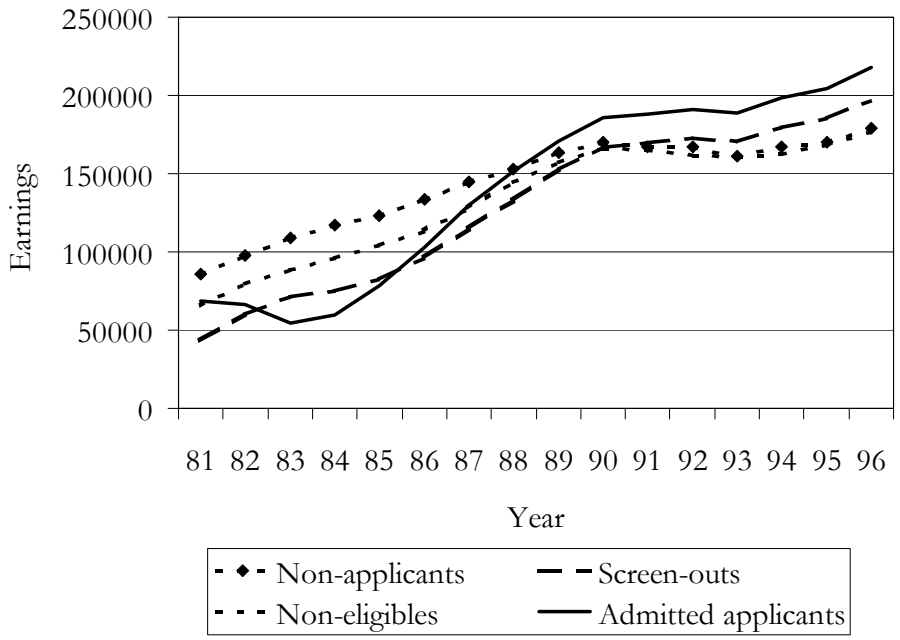


Notes: Non-admitted applicants are weighted by the relative number of admitted to non-admitted applicants. Admitted applicants are left unweighted.

In the 1990s, the growth in years of education is low and relatively stable. Admitted applicants spend more time in school or university than any other group. They are followed, in turn, by screen-outs, non-eligibles and non-applicants. Even though the non-eligibles did not fulfill the entrance requirements in the fall of 1982, they have longer education than non-applicants throughout the period. In 1996, there are big differences between the groups. Non-applicants have 12.3 years of education, non-eligibles have 12.8 years, screen-outs have 13.6 and admitted applicants have 14.3 years of education. Hence, enrollment constraints in combination with self-selection and admission selection to university in 1982 have generated substantial differences in educational attainment between the groups.

What about labor market performance? Average real earnings profiles for different groups of application are shown in Table A3 in Appendix and in Figure 2.

Figure 2 Earnings 1981-96



Notes: Non-admitted applicants are weighted by the relative number of admitted to non-admitted applicants. Admitted applicants are left unweighted. All numbers are in 1996 SEK.

Non-applicants clearly differ from other groups, not only in their initial earnings level, but also in how their earnings evolve over time. They are better off than other groups up to year 1988. From about 1990 their profile coincide with the one for non-eligibles. The earnings pattern for admitted applicants and screen-outs looks much like a text book illustration of the effects of human capital investments. However, there are some exceptions. First, the decline in earnings during the university studies is much smaller than what one might expect. This might be due to (1) a high share of dropouts among the admitted applicants, (2) a large share of university enrollment among the screen-outs and (3) students working while studying. Second, the earnings difference in subsequent years is perhaps somewhat smaller than the conventional picture. Note that all groups are affected by the recession in the early nineties, in particular non-applicants and non-eligibles.

4.2 Graphical analysis

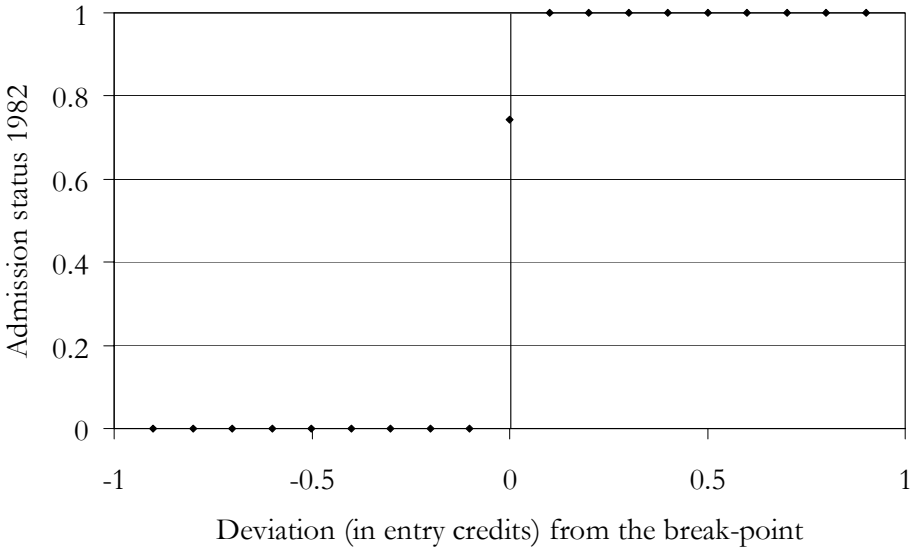
The defining characteristic of the regression-discontinuity design is that the probability of treatment is a discontinuous function of observed underlying (selection) variables. If it is assumed that the outcome of interest evolves smoothly with respect to the selection variables (or at least does not 'jump' when the probability of treatment does), the discontinuities in the probability of treatment can be used as a source of exogenous variation. To give some intuition about the empirical strategy, this section presents some graphs of the relation between the admission selection variables and different outcomes.

To identify the effect of enrollment constraints on education and earnings, the probability of admission in 1982 must change at the threshold value of the entry credits. Figure 3 reports the share of eligible applicants who gained admission in 1982 by their deviation from the breakpoint.[18] The relation between the entry credits and admission status is deterministic with one exception: some individuals at the breakpoint have not been admitted. This is due to applicants with equal merits having been separated by their ranking of the alternative or by the drawing of lots. The discontinuity in the probability of admission (along with the randomness around the breakpoint in some cases) implies that individuals differ in admission status even though they have (close to) identical formal merits. It

[18] Since the entry credits in the admission quota groups consist of different components, an encompassing measure, ranging between 1.0 and 5.0, has been constructed for this graph.

is, thus, possible to identify the effect of university enrollment constraints around the breakpoint.

Figure 3 Admission status 1982 and deviation from the breakpoint



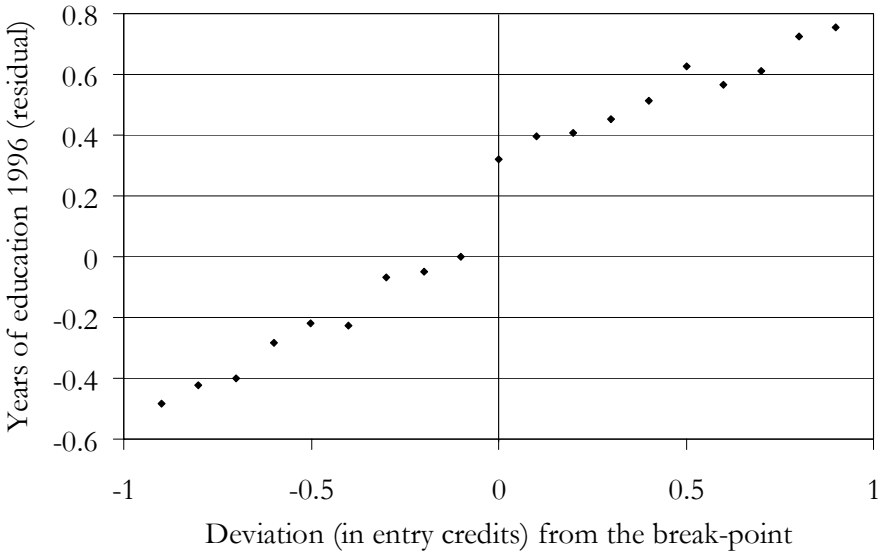
Notes: Admitted applicants are compared to screen-outs. Screen-outs are weighted by the relative number of admitted applicants to screened out applicants. Admitted applicants are left unweighted.

If years of education changes discontinuously at the breakpoint of the entry credits, admission status in 1982 is a valid instrument for education. Figure 4 shows the mean years of education in 1996 at different deviations from the breakpoint.[19] Clearly, the entry credits are positively related to the years of education. A one unit increase in the entry credits corresponds to an increase in education of about half a year. There is a clear discontinuity in years of education at the threshold value, with an increase in education of almost 0.3 years. Since the relation between the entry credits and the years of education is smooth (and close of being linear) at all other points, the sharp increase at the threshold value could only be explained by the enrollment constraints in 1982. Hence, individuals with equal entry

[19] Since all combinations of alternatives and admission quota groups have a threshold value of their own and because the entry credits consists of different components in different admission quota groups, I use the residual from a regression of years of education or earnings on demographic characteristics and alternatvie-quota groups in Figure 4 and Figure 5.

credits (and on average equal unobserved characteristics) differ in years of education due to luck (or bad luck) in the admission selection to university 15 years earlier. This variation is not a result of self-selection or admission selection to university, but rather of the enrollment constraints in 1982.

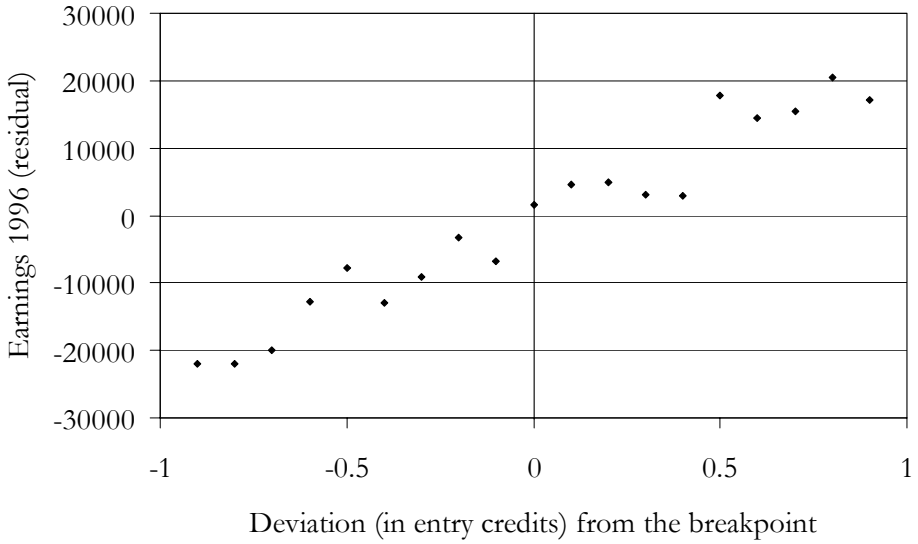
Figure 4 Years of education 1996 and deviation from the breakpoint



Notes: The residual is obtained from a weighted regression of years of education in 1996 on gender, year of birth, parental education, fixed effects for all combinations of alternatives and quota groups, admission status in 1982, entry credits and the applicants' ranking of the alternative. The coefficients and values for admission status in 1982 and the entry credits have been added back to the residual. Admitted applicants are compared to screen-outs. Screen-outs are weighted by the relative number of admitted applicants to screened out applicants. Admitted applicants are left unweighted.

What is the relation between the admission selection rules in 1982 and earnings? Figure 5 shows the average earnings in 1996 for different distance from the breakpoint. There is a positive and almost linear relation between the entry credits and earnings. However, there is no sharp change at the breakpoint. Hence, the extra years of education arising from the admission selection in 1982 have no parallel in earnings in 1996. To get a full picture of the effects of enrollment constraints on education and earnings and to estimate the return to education there is, however, need for regression analysis.

Figure 5 Earnings 1996 and deviation from the breakpoint



Notes: The residual is obtained from a weighted regression of earnings in 1996 on gender, year of birth, parental education, fixed effects for all combinations of alternatives and quota groups, admission status in 1982, entry credits and the applicants' ranking of the alternative. The coefficients and values for admission status in 1982 and the entry credits have been added back to the residual. Admitted applicants are compared to screen-outs. Screen-outs are weighted by the relative number of admitted applicants to screened out applicants. Admitted applicants are left unweighted. All numbers are in 1996 SEK

5 Results

5.1 Years of education

For university enrollment constraints to be effective, they should restrict admission to university for a cohort of applicants - not only at a given point of time - but also in the long run. It is, however, possible that enrollment constraints only lead to delayed enrollment and do not affect the amount of university education obtained. This is because non-admitted applicants may be admitted later on. Also, admitted applicants may choose to drop out.

Table 1 and Figure 6 report the effect of enrollment constraints in 1982 on years of education 1981-96.

Table 1 The effect of enrollment constraints on years of education

	Comparison group					
	Non-applicants	Non-eligibles		Screen-outs		
Year	OLS	OLS	FE	OLS	FE	FE-RD
1981	0.181	0.622	0.000	-0.007	-0.008	0.017
	(0.012)	(0.025)	(0.025)	(0.010)	(0.009)	(0.014)
1982	0.352	0.563	0.061	0.042	0.010	0.041
	(0.011)	(0.022)	(0.018)	(0.009)	(0.008)	(0.012)
1983	0.573	0.731	0.277	0.204	0.185	0.166
	(0.011)	(0.021)	(0.018)	(0.009)	(0.009)	(0.012)
1984	0.900	0.991	0.618	0.417	0.441	0.326
	(0.011)	(0.020)	(0.020)	(0.010)	(0.010)	(0.014)
1985	1.142	1.185	0.911	0.551	0.628	0.389
	(0.013)	(0.022)	(0.027)	(0.012)	(0.012)	(0.018)
1986	1.389	1.389	1.152	0.642	0.769	0.425
	(0.015)	(0.023)	(0.033)	(0.014)	(0.014)	(0.021)
1987	1.555	1.513	1.313	0.673	0.830	0.409
	(0.016)	(0.025)	(0.037)	(0.015)	(0.016)	(0.024)
1988	1.653	1.577	1.403	0.671	0.840	0.377
	(0.017)	(0.026)	(0.041)	(0.016)	(0.018)	(0.026)
1989	1.707	1.597	1.440	0.657	0.829	0.340
	(0.018)	(0.027)	(0.045)	(0.017)	(0.018)	(0.027)
1990	1.739	1.605	1.453	0.648	0.820	0.316
	(0.018)	(0.028)	(0.050)	(0.018)	(0.019)	(0.028)
1991	1.763	1.616	1.472	0.645	0.817	0.301
	(0.019)	(0.029)	(0.052)	(0.018)	(0.020)	(0.028)
1992	1.782	1.622	1.491	0.641	0.814	0.292
	(0.019)	(0.029)	(0.053)	(0.018)	(0.020)	(0.029)
1993	1.795	1.623	1.496	0.637	0.809	0.280
	(0.019)	(0.030)	(0.055)	(0.018)	(0.020)	(0.029)
1994	1.805	1.625	1.500	0.633	0.805	0.271
	(0.019)	(0.030)	(0.056)	(0.019)	(0.021)	(0.030)
1995	1.811	1.628	1.506	0.631	0.806	0.267
	(0.020)	(0.031)	(0.057)	(0.019)	(0.021)	(0.030)
1996	1.816	1.632	1.518	0.631	0.807	0.265
	(0.020)	(0.031)	(0.057)	(0.019)	(0.021)	(0.030)
N	42,207	42,614	42,614	121,876	121,876	121,876

Notes: Cluster-adjusted standard errors are in parentheses. The OLS estimates include controls for gender, year of birth and parental education. The FE estimates also include fixed effects for all combinations of alternatives and quota groups. The FE-RD estimates also include entry credits for nine quota groups and the applicants' ranking of the alternative. Non-admitted applicants are weighted by the relative number of admitted to non-admitted applicants. Admitted applicants are left unweighted.

In the first column, admitted applicants are compared to non-applicants using the OLS specification. Beginning in 1982, admitted applicants experience quite a rapid growth in years of education. The positive trend levels out in the 1990s. In the end of the panel, the university enrollment constraint effect is estimated at roughly 1.8 years of education. In the second column of Table 1, admitted applicants are compared to non-eligibles. The growth in years of education is somewhat lower in the 1980s when using this comparison group. In the third column of Table 1 the alternative-quota fixed effects are added, which leads to somewhat lower estimates.[20] The overall picture does, however, not change. In 1996, the effect of university enrollment constraints is estimated to be about 1.6 years of education.

Screen-outs are used as comparison group for admitted applicants in the fourth column of Table 1. The effect of enrollment constraints on educational attainment is much more moderate when using screen-outs as comparison group. Further, the screen-outs seem to compensate for the failure in the admission selection in 1982 rather quickly. The difference between the groups peaks in 1987. It then flattens out and stays around 0.6 years in all of the 1990s. In the fifth column of Table 1, the fixed effects are added which produces somewhat higher estimates.

Admission selection has not so far been considered in the analysis. The last column of Table 1 reports RD-FE estimates using screen-outs as comparison group for admitted applicants. The admission selection variables enter linearly in the model.[21] All estimates drop radically when adding the admission selection variables. Hence, much of the estimated effects in the previous specifications were due to differences in admission selection. The effect of university enrollment constraints on years of education settles around 0.27 years in the 1990s.

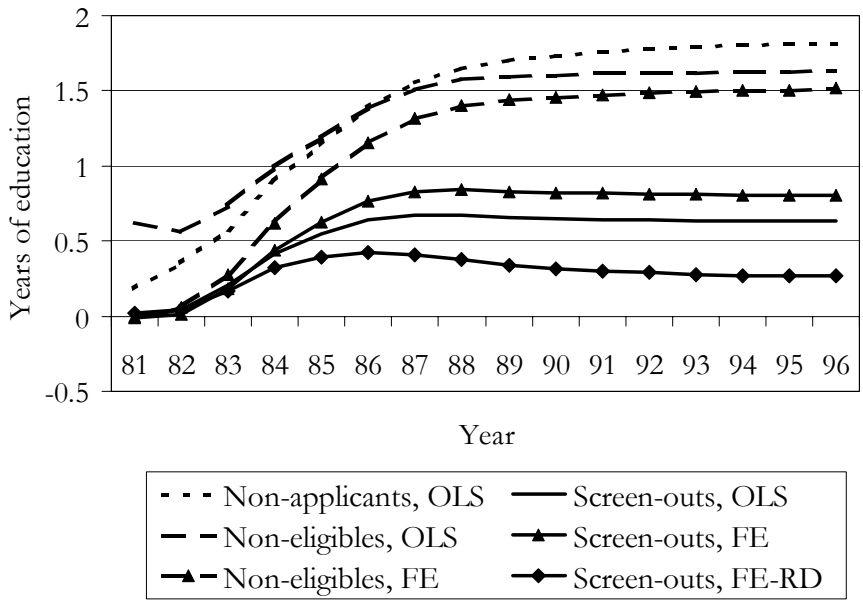
Years of education is an encompassing measure of educational attainment. To get a feeling for where the effect sets in, I have used alternative measures. First, I have estimated the effects of university enrollment constraints on the probability of attaining different educational levels in 1996. The results show that the admission in 1982 basically moves individuals from long high school (and to some extent from short university education)

[20] The pre-application difference in years of education vanishes when adding the fixed effects. This is because the admission quota groups are defined by educational background, and adding them as controls is analogous to conditioning on pre-application years of education.

[21] I have re-estimated the models with higher moments and interactions between admission status and the selection variables. Some of the moments and interactions emerge significantly different from zero, but the estimated effects do not change much.

to long university education (and to some extent to graduate education).

Figure 6 The effect of enrollment constraints on years of education



Notes: The OLS estimates include controls for gender, year of birth and parental education. The FE estimates also include fixed effects for all combinations of alternatives and quota groups. The FE-RD estimates also include entry credits for nine quota groups and the applicants' ranking of the alternative. Non-admitted applicants are weighted by the relative number of admitted to non-admitted applicants. Admitted applicants are left unweighted.

Second, I have estimated the probability of university enrollment at any time between 1977 and 1996. Admitted applicants in 1982 have about 7 per cent higher probability of university enrollment than screen-outs. Also the timing differs. Admitted applicants enroll to university on average two thirds of a year earlier than do the screen-outs (conditional on having enrolled). Third, the years of education effect has been decomposed into the effect arising from higher probability of attending university (participation effect) and the effect arising from longer university studies among those who enroll (dose effect).[22] Roughly 90 per cent of the effect is

[22] The years of education effect is given by $E[S_{it} \mid D_i = 1] - E[S_{it} \mid D_i = 0]$
$= E[S_{it} \mid R_{it} = 1, D_i = 0]\{P[R_{it} = 1 \mid D_i = 1] - P[R_{it} = 1 \mid D_i = 0]\}$
$+P[R_{it} = 1 \mid D_i = 1]\{E[S_{it} \mid R_{it} = 1, D_i = 1] - E[S_{it} \mid R_{it} = 1, D_i = 0]\}$,

due to higher probability of university enrollment for admitted applicants while only 10 per cent of the effect is due to longer education among those who enroll.

To sum up, the choice of comparison group as well as the empirical specification is crucial in finding causal effects of enrollment constraints. When admitted applicants are compared to non-applicants, the university enrollment effect in 1996 is estimated to be more than 1.8 years of education. The corresponding figure, when using screen-outs as comparison group and controlling for both alternative-quota group fixed effects and the admission selection variables, is 0.27 years of education. Hence, only 15 per cent of the gross difference is due to the enrollment constraints.

The university enrollment constraints in 1982 affect educational attainment even 15 years later. The effect is more pronounced in the years following the admission selection. Other groups find their way to university in subsequent years, making the overall difference smaller. However, they never catch up. So, enrollment constraints are effective in shutting individuals out from the universities also in the long run. However, the effect is not so big. The years of education effects also seem to be rather stable in the last seven or eight years of the panel.

5.2 Earnings

University enrollment constraints at a given point of time might affect earnings for a number of reasons. First, the constraints affect the timing of university studies. The study group members gain admission earlier than those in the comparison groups. Even though students quite often work while studying, the number of hours worked as well as the wage is probably low. It is, thus, reasonable to expect a negative earnings effect of enrollment constraints for a period following the year of admission. Since most comparison group members enter university later, their earnings probably will dip later on. Second, the enrollment constraints affect the amount of university education obtained. Thus, the earnings dip for the study group probably is larger than the later dip for the comparison groups. Third, a higher share of the study group receive the potential returns to university education.

Table 2 and Figure 7 present the effect of enrollment constraints on earnings.

where R_{it} is university registration and S_{it} is years of university education. The years of education conditional on registration, $E\left[S_{it} \mid R_{it}=1\right]$, is $E\left[S_{it}\right]/P\left[R_{it}=1\right]$.

Table 2 The effect of enrollment constraints on earnings

	Comparison group					
	Non-applicants	Non-eligibles		Screen-outs		
Year	OLS	OLS	FE	OLS	FE	FE-RD
1981	-13865.9	5850.2	5340.4	7309.7	16558.9	643.8
	(649.5)	(910.8)	(1551.8)	(461.3)	(492.8)	(653.6)
1982	-28179.1	-7597.1	-11263.7	-6696.8	-676.6	-9953.2
	(642.1)	(898.1)	(1628.4)	(445.2)	(530.9)	(706.5)
1983	-51518.1	-29858.0	-32833.4	-23931.8	-22112.0	-21377.6
	(652.5)	(951.3)	(1762.8)	(470.8)	(590.7)	(792.5)
1984	-54743.6	-28751.9	-34842.0	-19281.3	-20121.2	-15818.1
	(711.0)	(1031.0)	(1904.1)	(529.7)	(661.4)	(865.2)
1985	-40883.7	-17049.6	-24100.7	-7645.8	-9694.6	-4240.1
	(730.4)	(1103.4)	(2311.2)	(562.1)	(708.3)	(943.4)
1986	-25726.6	-2923.5	-7208.8	2874.6	2017.3	3799.4
	(788.3)	(1193.5)	(2620.0)	(622.6)	(787.7)	(1047.7)
1987	-10893.2	9179.0	4932.3	9837.5	11371.8	8579.8
	(822.0)	(1240.3)	(2707.9)	(678.3)	(822.9)	(1136.5)
1988	261.2	17884.6	11941.7	13450.4	16414.9	10730.3
	(850.8)	(1294.6)	(2889.4)	(704.5)	(881.2)	(1212.5)
1989	7299.8	22630.6	17604.8	12804.5	16334.1	7831.9
	(905.7)	(1368.8)	(3094.6)	(752.5)	(934.5)	(1284.2)
1990	14844.2	28608.7	23791.7	12433.5	17141.0	6076.3
	(954.8)	(1459.8)	(3411.2)	(799.1)	(985.1)	(1368.2)
1991	18109.2	31619.3	27578.5	12178.7	17813.1	4895.6
	(1120.0)	(1573.9)	(3652.1)	(891.8)	(1089.7)	(1460.9)
1992	21074.1	34262.3	34814.3	12188.9	18092.5	2649.2
	(1105.3)	(1669.6)	(3487.8)	(909.7)	(1125.8)	(1528.8)
1993	24238.6	35862.5	33111.0	12177.0	18635.3	1720.1
	(1221.6)	(1761.7)	(3934.1)	(1067.1)	(1249.9)	(1812.7)
1994	26926.4	38282.3	42693.3	12250.6	20319.8	852.9
	(1405.1)	(2217.6)	(3950.6)	(1306.5)	(1491.3)	(2143.5)
1995	28926.7	41452.1	43032.5	13717.8	21460.2	458.8
	(1354.4)	(1958.5)	(4065.2)	(1218.9)	(1403.8)	(1877.8)
1996	32718.8	46615.5	49050.7	15554.1	23781.9	-853.9
	(1453.6)	(2036.1)	(4193.2)	(1361.8)	(1640.7)	(2306.7)
N	42,207	42,614	42,614	121,876	121,876	121,876

Notes: Cluster-adjusted standard errors are in parentheses. The OLS estimates include controls for gender, year of birth and parental education. The FE estimates also include fixed effects for all combinations of alternatives and quota groups. The FE-RD estimates also include entry credits for nine quota groups and the applicants' ranking of the alternative. Non-admitted applicants are weighted by the number of admitted to non-admitted applicants. Admitted applicants are left unweighted. All numbers are in 1996 SEK.

The first column of Table 2 reports the OLS estimates when using non-applicants as the comparison group. Admitted applicants have significantly lower earnings than non-applicants in the pre-application year. This is somewhat surprising, but could in part be due to a higher share of admitted applicants than non-applicants being in school at the time (60 per cent to 45 per cent, respectively). The earnings for admitted applicants drop radically in comparison to the ones for non-applicants in the two years following the year of admission to university. In 1984 the earnings difference is almost -55,000 SEK. The negative earnings effects diminish in the subsequent years and vanish by 1988. The remaining period is characterized by a steady earnings growth for admitted applicants in comparison to non-applicants. In 1996, admitted applicants earn on average 30,000 SEK more than non-applicants. This corresponds to about 18 per cent of the average earnings of the non-applicants.

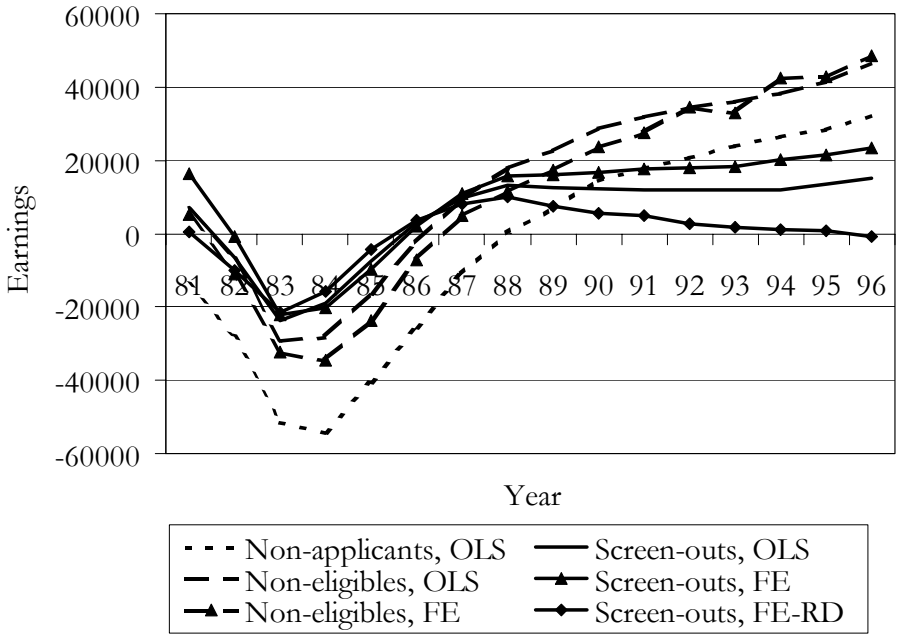
The second column of Table 2 presents the effects of enrollment constraints on earnings when comparing admitted applicants to non-eligibles. Admitted applicants seem to be better off even without receiving any treatment. In 1981 they earn almost 6,000 SEK more than non-eligibles. Differences in university enrollments generate negative earnings effects in the 1982-86 period. The earnings loss is about 30,000 at the most, which is less than when using non-applicants as comparison group. From 1987 and onwards, admitted applicants get ahead. The university enrollment constraint earnings effect is roughly 46,000 SEK in 1996, or 26 per cent of the average earnings for non-eligibles, which is much higher than when using non-applicants as comparison group. Adding the alternative-quota fixed effects does not change the result much.

The earnings of admitted applicants are compared to those of screen-outs in the three last columns of Table 4. In the OLS model, admitted applicants have higher earnings than the screen-outs even in the pre-application year. The earnings penalty from attending university is lower when using screen-outs as the comparison group than when using the other groups. Also, the period with positive earnings starts earlier. In the 1988-96 period, the earnings effects lie rather stable around 13,000 SEK. This corresponds to about 6.5 per cent of the average earnings for screen-outs. Adding the fixed effects to the model generates somewhat higher earnings effects for most of the period.

The last column of Table 4 reports the estimates from the FE-RD model. Most admitted applicants and screen-outs are in school in 1981. Earnings differences in the pre-application period may therefore not be very informative about unobserved outcome potential. Nevertheless, the

groups should not differ in this respect. Indeed, the first row of column six shows that the earnings of admitted applicants and screen-outs do not significantly differ in 1981. The admission to university leads to an earnings dip for admitted applicants in the 1982-85 period. The earnings effect is then almost 11,000 SEK. They then get ahead up to year 1988. In the subsequent years the earnings differences diminish, to be completely eliminated in 1996. Hence, there are no long term effects of university enrollment constraints on earnings.

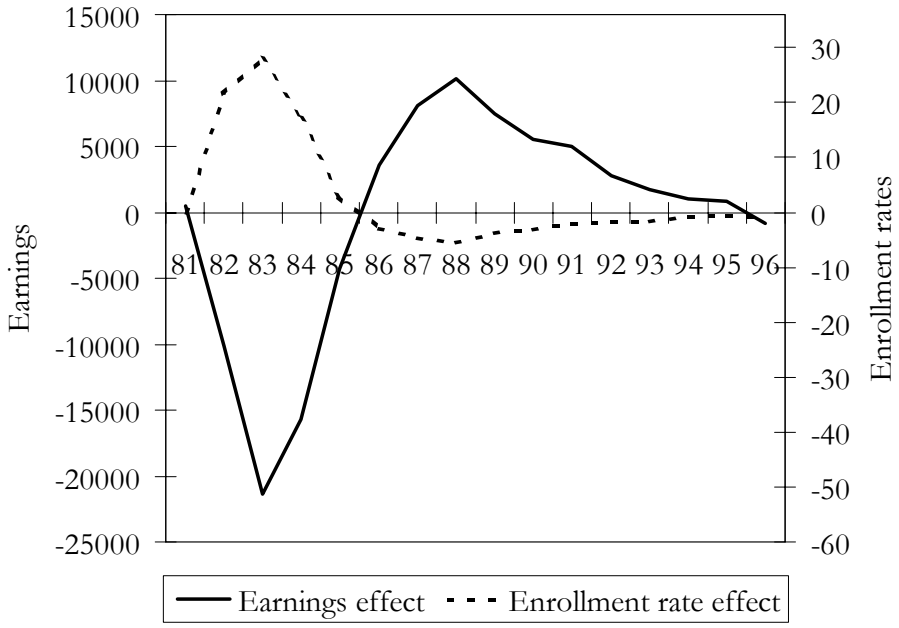
Figure 7 The effect of enrollment constraints on earnings



Notes: The OLS estimates include controls for gender, year of birth and parental education. The FE estimates also include fixed effects for all combinations of alternatives and quota groups. The FE-RD estimates also include entry credits for nine quota groups and the applicants' ranking of the alternative. Non-admitted applicants are weighted by the relative number of admitted to non-admitted applicants. Admitted applicants are left unweighted. All numbers are in 1996 SEK.

Why do the earnings effects vanish in the 1990s? The university enrollment constraints in 1982 affect both the amount and the timing of university education. Figure 8 depicts the effects of university enrollment constraints on earnings and university enrollment rates.

Figure 8 The effect of enrollment constraints on earnings and enrollment



Notes: The enrollment rate effects are estimated using a linear probability model. Admitted applicants are compared to screen-outs. The empirical specification includes controls for gender, year of birth, parental education, fixed effects for all combinations of alternatives and quota groups, entry credits for nine quota groups and the applicants' ranking of the alternative. Non-admitted applicants are weighted by the relative number of admitted to non-admitted applicants. Admitted applicants are left unweighted.

Clearly, the earnings pattern is negatively related to university enrollments. The earnings dip in the beginning of the period corresponds to an increase in the university enrollment rates. By the same token, the positive earnings effects in the late 1980s took place when a higher share of the comparison group members attended university. The differences in enrollment rates, however, seem to be associated with higher earnings differences in the late 1980s than in the early 1980s. This is to be expected since the value of forgone earnings increases with time, both due to experience and to education. The observed earnings and enrollment rate effect pattern is, thus, consistent with a scenario where forgone earnings while studying is the only monetary effect of university education.[23] To

[23] I have also estimated the effects of university enrollment constraints on childbearing,

sum up, even though the university enrollment constraints in 1982 affect educational attainment also in the long run, they are not associated with higher earnings. As expected, the admitted applicants fare worse on the labor market in the years following the admission. From 1986 they get ahead, but only for a couple of years. The biggest earnings effect is found in 1988. Thereafter the effect peters out. The earnings pattern is found to be a mirror image of the university enrollment rates. The dip and peak in the earnings pattern corresponds to a peak and a dip in the enrollment rates. One interpretation of the observed pattern is that the earnings effects vary around zero, driven by differences in university enrollment rates.

5.3 Return to education

University enrollment constraints affect both educational attainment and earnings in 1981-96. This section tries to relate the differences in years of education to the differences in earnings. First, the return to education is estimated using different comparison groups and adding more controls to the model. This is analogous to the approach in the previous sections. Second, discontinuities in the admission selection to university are used as an instrument for years of education.

Using a cohort of applicants (and non-applicants) makes it possible to study the dynamics of the returns to education. On the other hand, there is need for a long follow-up period. Even though the year of application is dated back to the early eighties, roughly 5 per cent of the sample is registered at a university course or program in 1996. Also, the earnings of the first years in the labor market might not be a very good measure on the long-run effects of university education. To avoid some of these problems, I will study the effect of education on earnings for the 1990s only.

Table 3 reports least squares estimates of the return to education using different comparison groups and model specifications. It reveals a number of interesting findings. First, the rate of return increases steadily over time. The reason for this pattern is probably not a rapid growth in the rate of return to education in general, but rather a reduction of the share of students in the sample and low initial earnings. This illustrates the problem of using a cohort of applicants to estimate the return to education. Second, the sample studied matters.[24] The return to education for

unemployment and migration/mortality. However, the effects are very small (and in most cases not significantly different from zero), and do not contribute to explaining the observed earnings pattern.

[24]The distinction between study and comparison groups is less clear in this part of

the pooled sample of admitted applicants and non-applicants is estimated at about 12,700 SEK in 1996. This corresponds to about 7 per cent of the average earnings for non-applicants.[25] The corresponding figure when studying admitted applicants and screen-outs is about 11,300 SEK. The difference between the estimates is significantly different from zero.

Table 3 Least squares estimates of the education earnings premium

	Comparison group					
	Non-applicants	Non-eligibles		Screen-outs		
Year	OLS	OLS	FE	OLS	FE	FE-RD
1990	7436.4	8460.5	8071.5	6618.4	6732.0	5932.2
	(234.1)	(274.0)	(790.6)	(229.4)	(275.6)	(283.5)
1991	8163.3	9264.8	8835.5	7460.3	6888.2	6028.4
	(266.7)	(301.0)	(792.7)	(243.8)	(293.3)	(301.7)
1992	8931.5	9894.0	9621.8	8239.3	7346.5	6461.7
	(261.5)	(315.7)	(757.5)	(249.8)	(301.2)	(307.9)
1993	9308.4	10097.4	9740.2	8633.5	7683.1	6758.2
	(277.7)	(333.2)	(848.6)	(268.7)	(332.9)	(344.0)
1994	10087.9	10763.2	10686.5	9445.6	7848.0	6780.6
	(344.1)	(433.6)	(910.7)	(340.6)	(399.0)	(409.4)
1995	11113.5	11782.5	11344.5	10053.1	8503.0	7382.1
	(318.4)	(394.7)	(943.3)	(310.4)	(365.5)	(372.0)
1996	12688.1	13690.6	13427.1	11300.8	9484.5	8227.8
	(337.8)	(376.2)	(944.1)	(349.9)	(434.2)	(440.5)
N	42,207	42,614	42,614	121,876	121,876	121,876

Notes: Cluster-adjusted standard errors are in parentheses. The OLS estimates include controls for gender, year of birth and parental education. The FE estimates also include fixed effects for all combinations of alternatives and quota groups. The FE-RD estimates also include entry credits for nine quota groups and the applicants' ranking of the alternative. Non-admitted applicants are weighted by the relative number of admitted to non-admitted applicants. Admitted applicants are left unweighted.

Third, much of the estimated returns to education can be explained

the analysis. As before, admitted applicants are pooled with different samples of non-admitted individuals. However, the focus of the analysis is not the differences between the study and the comparison groups, but instead the differences between groups with different amount of education.

[25] The return to education in Sweden is typically estimated at between four and five per cent, see Arai and Kjellstrom (1999) for a survey. The higher (gross) return to education in this study might be due to the fact that it (1) includes all workers, non-wage earners as well as wage earners, (2) uses a non-representative sample of the Swedish population, and (3) measures the return to effective (rather than to actual) years of education.

by observed characteristics from the admission selection process. In all, roughly 35 per cent of the estimated earnings effects is eliminated when pooling admitted applicants with screen-outs instead of non-applicants and when adding information from the admission selection process to the model. The best estimate using least squares suggests that the return to education is roughly 8,000 SEK in 1996. This corresponds to about 4 per cent of the average earnings for screen-outs. Controlling for observed variables from the admission selection process will probably not free the estimates from endogeniety bias. However, the discontinuity in the admission rules can be used as a source of exogenous variation in education. The Wald estimator using admission status as an instrument is simply the ratio between the effect of enrollment constraints on earnings to their effect on years of education.

Table 4 reports IV estimates using admission status as the instrument and controlling for alternative-quota groups and admission selection variables. In column one, the entire sample is used. Unfortunately, all estimates are rather imprecise. There is, nevertheless, a substantial effect of education on earnings in 1990: more than 19,000 SEK. However, the IV estimates drops radically over the period. Since the effect of the instrument on educational attainment is rather stable in the 1990s, this is mainly due to the effect of the instrument on earnings. In 1993-96 the return is not significantly different from zero. The point estimate is actually negative in 1996.

The fuzzy regression discontinuity design identifies the effect around the breakpoint. Smooth functions of the selection variables can be used to extrapolate the effect above and below the threshold. To check the effect of these functional form assumptions, the remaining columns of Table 4 present the effect of education on earnings when restricting the sample with different absolute deviations in entry credits from the breakpoint. In the last column only those with equal entry credits are studied. Unfortunately, the precision in the estimates is low, why they do not significantly differ from each other. If anything, the functional form assumptions seem to underestimate the true earnings effect of education.

Could the low returns to education be due to differences in labor supply? Table 5 reports the return to education for those earning more than 100,000 SEK at different points in time. The estimates are much lower with than without the earnings restriction. This is to be expected since years of education probably have a positive effect on both employment and hours worked. Still, the estimates follow the same pattern as do the unrestricted earnings premium.

Table 4 IV estimates of the education earnings premium. Restricting the sample with different absolute deviation in entry credits from the breakpoint.

		Absolute deviation from the breakpoint:			
Year	All	Max 1.0	Max 0.5	Max 0.2	Max 0.0
1990	19252.7	16630.1	14791.9	12344.4	14668.5
	(4457.3)	(4389.9)	(5237.1)	(6363.8)	(17294.4)
1991	16267.7	14844.7	11955.2	9272.4	-12073.8
	(4926.0)	(4857.3)	(5811.2)	(6956.6)	(17633.9)
1992	9080.0	7882.4	8186.0	11591.6	-11592.1
	(5221.9)	(5177.4)	(6104.9)	(7308.8)	(17686.3)
1993	6149.1	6890.8	4349.5	7852.3	-5245.1
	(6449.2)	(6231.3)	(7774.6)	(8172.4)	(18394.1)
1994	3151.2	2037.9	-3744.8	-4019.5	-32736.6
	(7899.4)	(7520.3)	(9327.0)	(11837.7)	(30088.6)
1995	1718.3	3176.8	-891.3	1471.6	-18593.1
	(7015.3)	(7003.3)	(8554.7)	(10281.5)	(26438.3)
1996	-3220.0	606.8	5917.5	-3239.8	-38085.2
	(8736.2)	(8815.3)	(9731.7)	(12685.8)	(33973.3)
N	121,876	90,124	52,057	24,106	4,022

Notes: Cluster-adjusted standard errors are in parentheses. The empirical specification includes controls for gender, year of birth, parental education, fixed effects for all combinations of alternatives and quota groups, entry credits for nine quota groups and the applicants' ranking of the alternative. Admitted applicants are compared to screen-outs. Screen-outs are weighted by the relative number of admitted applicants to screened out applicants. Admitted applicants are left unweighted. Alternative-quota groups without observations in both the study and the comparison group within the required range from the breakpoint have been eliminated.

In 1996 the return to education, when pooling admitted applicants with non-applicants in the OLS specification, is estimated at 3.8 per cent. If instead pooling admitted applicants with screen-outs, the estimated return drops to 2.8 per cent. Adding the fixed effects to the model, leads to radically lower returns. This is probably due to the fixed effects controlling for lower levels of education, which have been found to have high returns in the sample.[26] Adding the admission selection variables to the model

[26] I have decomposed years of education into lower education (compulsory school and high school) and higher education (undergraduate education and graduate education). When pooling admitted applicants with screen-outs and using the OLS specification, the estimated returns to lower education is as high as 9.0 per cent, while the return to

reduces the estimate from 1.8 to 1.5 per cent in 1996.

Table 5 Least squares estimates of the education earnings premium, restricting earnings to be greater than 100,000 SEK.

	Comparison group				
	Non-applicants	Screen-outs			
Year	OLS	OLS	FE	FE-RD	IV
1990	0.026	0.021	0.015	0.011	0.051
	(0.001)	(0.001)	(0.001)	(0.001)	(0.016)
1991	0.028	0.021	0.014	0.010	0.066
	(0.001)	(0.001)	(0.001)	(0.001)	(0.018)
1992	0.028	0.023	0.015	0.011	0.060
	(0.001)	(0.001)	(0.001)	(0.001)	(0.019)
1993	0.029	0.022	0.014	0.010	0.036
	(0.001)	(0.001)	(0.001)	(0.001)	(0.020)
1994	0.031	0.025	0.016	0.012	0.028
	(0.001)	(0.001)	(0.001)	(0.001)	(0.021)
1995	0.035	0.026	0.017	0.014	0.014
	(0.001)	(0.001)	(0.001)	(0.001)	(0.022)
1996	0.038	0.028	0.018	0.015	0.004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.024)

Notes: Cluster-adjusted standard errors are in parentheses. The OLS estimates include controls for gender, year of birth and parental education. The FE estimates also include fixed effects for all combinations of alternatives and quota groups. The FE-RD estimates also include entry credits for nine quota groups and the applicants' ranking of the alternative. Non-admitted applicants are weighted by the relative number of admitted to non-admitted applicants. Admitted applicants are left unweighted.

The IV point estimate is less than 0.5 per cent, although not significantly different from zero or the other estimates. However, since the earnings restriction increases the precision in the estimates, it is possible to rule out returns to education exceeding 1.7 per cent (using the FE-RD specification). Hence, the return to education is low in Sweden, both when using least squares and instrumental variable techniques.

higher education is only 2.2 per cent. The high return to lower education is probably due to the relatively small variation in years of lower education in the sample. Small differences in years of education might therefore be associated with big differences in unobserved outcome potential. Adding the fixed effects lead to a radical drop in the estimated return to lower education to about 1.8 per cent, while the return to higher education only drops to about 1.9 per cent.

5.4 Caveat

Bound et al. (1995) discuss problems with using instruments that are weakly correlated with the regressor of interest. Even weak correlations between the instruments and the error term in the outcome equation can then lead to inconsistency.[27] The inconsistency goes in the same direction as the correlation between the instruments and the error term. Bound et al. (1995) propose the partial R^2 of the excluded instruments as a rough test for the quality of the instrumental variables. Table 6 presents the partial R^2 for the admission status dummy in the first step equation of years of education.[28]

Table 6 Partial R^2 for the excluded instrument

Year	Partial $R^2 \times 100$
1990	0.271
1991	0.236
1992	0.216
1993	0.194
1994	0.179
1995	0.170
1996	0.165

Notes: Test statistics for admission status in 1982 in the first-step equation of years of education on gender, year of birth, parental education, fixed effects for all combinations of alternatives and quota groups, entry credits for nine quota groups and the applicants' ranking of the alternative. Admitted applicants are compared to screen-outs. Screen-outs are weighted by the relative number of admitted to screened out applicants. Admitted applicants are left unweighted.

The partial R^2 is relatively low, although substantially higher than what has been reported in many other studies e.g. Angrist and Krueger (1991).[29] For the low IV-estimates to be due to inconsistency, the instrument must be negatively correlated with the error term. In other words, the discontinuities in the admission selection must have a direct negative

[27] Bound et al. (1995) also discuss the problem of finite sample bias. However, exactly identified models (as this one) do not suffer from finite sample bias (Angrist and Krueger, 1999).

[28] The partial R^2 is given by $\frac{R^2_{yxz}-R^2_{yx}}{1-R^2_{yx}}$, where R^2_{yxz} is R^2 when including the instrument z, and R^2_{yx} is R^2 when excluding the instrument z.

[29] Despite the criticism of Bound et al. (1995), few studies report the Partial R^2 for the excluded instruments.

effect on earnings. To fit the pattern of the IV-estimates, the negative relation between the instrument and the earnings must also strengthen over time. It is, however, not clear why such a relationship should exist.

If the response to treatment differ between individuals the Wald estimator identifies LATE (Imbens and Angrist, 1994). This is the mean effect of treatment for those who receive more university education due to luck in the admission selection to university in 1982 (compliers). The question is why some of the unsuccessful applicants end up in university while others don't. In a human capital framework, individuals would keep on applying to university as long as the associated net expected benefits are higher than the costs. However, the admission to university took place only twice a year, with most of the places being allocated in the fall semester.[30] Hence, unsuccessful applicants typically had to wait another year until they could apply for admission again. As time goes by the period left on the labor market (with potentially higher earnings) decreases, while the value of forgone earnings (due to the return to labor market experience) increases. Also, the chance of receiving a lucky wage offer increases. All these factors make unsuccessful applicants less likely to apply again.[31] University enrollment constraints probably also affect the re-application behavior, both by discouraging unsuccessful applicants to apply again and by reducing the chance of admission for those who apply. The group of compliers might, thus, consist of individuals who would otherwise have applied to university again, but who would have failed to gain admission due to the enrollment constraints. It might also consist of individuals who would not have re-applied to university due to having been discouraged to do so or due to having found a good job.

To check if the low return to education (at least partly) can be due to a high share of unsuccessful applicants having found a good job, I have estimated the effect of lagged earnings (in 1982) on the probability to enroll to university (in 1983) among screened out applicants. The effect of lagged earnings is small but positive, implying that individuals who have found a good job (high lagged earnings) are *more* likely to apply

[30] About three quarters of the places available at the NSBUC for the 1982/83 academic year were allocated in the fall semester. The corresponding figure for all places available at Swedish universities was two thirds.

[31] The outside options might also change with time. There was a small recession in Sweden in the early eighties, with rising unemployment (from 2 to about 3 per cent) between 1980 and 1983. The labor market situation improved for the remaining part of the 1980s. Thus, unsuccessful applicants (who might have applied to university for labor market reasons) were probably more likely to get a job in the years after 1982 than before, which lowers the probability for them to apply again.

later. This indicates that the selection of students to university (lagged earnings is a proxy for outcome potential) is of greater importance than possible exogenous wage chocks. Hence, it is not likely that a high share of compliers with lucky wage offers is the main explanation for the very low returns.

6 Conclusions

This paper estimates the impact of university enrollment constraints on educational attainment and labor market performance, as well as the effect of education on earnings, in Sweden 1981-96. The years of education is clearly affected by the enrollment constraints. However, the effect is not so big as the raw difference between admitted applicants and a group of non-applicants indicates (about 1.8 years of education in 1996). The more the comparison group resembles the study group, the smaller is the effect of enrollment constraints on educational attainment. Adding controls for alternative-quota groups and smooth functions of the admission selection variables reduce the estimates further. My best estimate suggests the effect of university enrollment constraints to be about one quarter of a year of education in 1996. Hence, most of the raw difference is driven by differences in self-selection and admission selection to university. The university enrollment constraints still have been successful in shutting individuals out from the universities, even though the long-run effect is not very big.

The effect of university enrollment constraints on earnings varies over the period studied. Admitted applicants experience quite a substantial earnings loss in the few years following the admission to university. This is in line with traditional human capital theory, since few students work while studying. The earnings effects turn positive after about four years and increase up to year 1988. In the subsequent years the earnings differences diminish, to be completely eliminated in the mid 1990s. Hence, there are no long term effects of university enrollment constraints on earnings. The positive effect of enrollment constraints on educational attainment, thus, has no parallel in earnings. The earnings effect has also been shown to be a mirror image of the enrollment rate effects. One possible explanation for this pattern is that university education has no monetary effects other than forgone earnings while studying. The university enrollment constraints have, thus, not given rise to any marked earnings inequality between admitted and non-admitted applicants.

The return to education is estimated with both least squares and in-

strumental variables techniques over the 1990-96 period. The least squares estimates increase with time, and are sensitive for the choice of comparison group and model specification. When admitted applicants are pooled with non-applicants, the earnings effect is estimated at roughly 12,500 SEK in 1996. This corresponds to about 7 per cent of the average earnings for non-applicants. The more similar the study group is to the comparison group, the lower is the estimates. Three quarters of the raw estimate remains when pooling admitted applicants with screen-outs and controlling for the alternative quota fixed effects and the admission selection determinants. The best least squares estimate suggests the return to education to be roughly 8000 SEK in 1996. This is about 4 per cent of the average earnings for screen-outs.

The instrumental variable estimator exploits the discrete jumps in years of education, due to the admission selection, to identify the effect of education on earnings. In 1990 the return to education is estimated at 19,000 SEK, which is about 9.5 per cent of the average earnings for screen-outs. However, the estimates drop radically over the period. From 1993 the effect is not significantly different from zero. In 1996, the point estimate is actually negative. If restricting the sample to those who earn more than 100,000 SEK in 1996, the least squares estimates rule out returns to education in Sweden higher than 1.7 per cent.

Appendix

Table A1 Means and standard deviations of variables used in the analysis

	Comparison group			Study group
Variable	Non-applicants	Non-eligibles	Screen-outs	Admitted applicants
Year of birth	1958.33	1958.61	1960.79	1958.71
	(5.89)	(5.56)	(3.87)	(5.37)
Female	0.55	0.44	0.52	0.52
Fathers education:				
- Basic school	0.38	0.30	0.26	0.26
- Comprehensive school	0.04	0.04	0.04	0.04
- High school < 2 years	0.16	0.17	0.16	0.14
- High school > 2 years	0.14	0.17	0.18	0.16
- University < 2 years	0.04	0.05	0.05	0.05
- University > 2 years	0.08	0.10	0.16	0.18
- Graduate school	0.01	0.01	0.02	0.02
- Not available	0.16	0.16	0.12	0.15
Mothers education:				
- Basic school	0.44	0.37	0.32	0.31
- Comprehensive school	0.12	0.11	0.14	0.13
- High school < 2 years	0.18	0.19	0.21	0.18
- High school > 2 years	0.04	0.06	0.07	0.07
- University < 2 years	0.04	0.06	0.08	0.08
- University > 2 years	0.05	0.06	0.09	0.11
- Graduate school	0.00	0.00	0.00	0.00
- Not available	0.12	0.15	0.09	0.12
Admission status 1982	0.00	0.00	0.00	1.00
Entry credits	N/A	N/A	3.01	4.04
			(0.62)	(0.59)
Ranking of alternative	N/A	N/A	2.34	2.34
			(2.19)	(2.31)
N	12,416	12,823	92,085	29,791

Notes: Non-admitted applicants are weighted by the number of admitted to non-admitted applicants. Admitted applicants are left unweighted.

Table A2 Years of education 1981-96

	Comparison group			Study group
Year	Non-applicants	Non-eligibles	Screen-outs	Admitted applicants
1981	11.25	11.51	11.44	11.49
	(1.00)	(1.20)	(1.09)	(1.19)
1982	11.44	11.80	11.83	11.86
	(1.10)	(1.07)	(0.98)	(1.03)
1983	11.54	11.90	11.96	12.17
	(0.93)	(1.03)	(0.97)	(1.07)
1984	11.65	12.00	12.14	12.61
	(0.87)	(1.01)	(1.01)	(1.24)
1985	11.83	12.15	12.41	13.05
	(0.89)	(1.09)	(1.17)	(1.49)
1986	11.92	12.26	12.65	13.41
	(1.03)	(1.19)	(1.37)	(1.72)
1987	11.99	12.37	12.88	13.68
	(1.14)	(1.29)	(1.59)	(1.90)
1988	12.06	12.47	13.08	13.87
	(1.23)	(1.38)	(1.75)	(2.01)
1989	12.12	12.56	13.23	14.00
	(1.30)	(1.47)	(1.87)	(2.09)
1990	12.16	12.63	13.33	14.09
	(1.36)	(1.56)	(1.95)	(2.14)
1991	12.19	12.67	13.40	14.15
	(1.41)	(1.62)	(2.01)	(2.18)
1992	12.22	12.71	13.46	14.20
	(1.44)	(1.65)	(2.05)	(2.21)
1993	12.24	12.74	13.51	14.25
	(1.47)	(1.70)	(2.08)	(2.23)
1994	12.26	12.77	13.55	14.28
	(1.49)	(1.74)	(2.11)	(2.24)
1995	12.28	12.78	13.57	14.30
	(1.51)	(1.78)	(2.13)	(2.26)
1996	12.29	12.79	13.59	14.32
	(1.53)	(1.78)	(2.15)	(2.27)
N	12,416	12,823	92,085	29,791

Notes: Non-admitted applicants are weighted by the number of admitted to non-admitted applicants. Admitted applicants are left unweighted.

Table A3 Earnings 1981-96

	Comparison group			Study group
Year	Non-applicants	Non-eligibles	Screen-outs	Admitted applicants
1981	86041.5	65491.2	43500.5	68534.2
	(68844.0)	(61681.4)	(45616.0)	(61572.1)
1982	97546.5	79559.3	60693.9	66051.8
	(66619.3)	(57375.9)	(47196.4)	(53738.0)
1983	108935.2	88797.8	71521.1	54337.6
	(63248.6)	(59503.6)	(51898.3)	(54157.4)
1984	117418.8	96265.2	75047.6	59622.8
	(67828.0)	(62776.3)	(59709.8)	(58498.8)
1985	123340.6	104849.4	82959.9	78188.7
	(68554.7)	(70757.9)	(62353.6)	(65241.2)
1986	133703.5	113610.9	96548.3	103263.7
	(73170.7)	(76964.6)	(70249.2)	(72979.3)
1987	144405.4	128275.2	114602.0	129899.7
	(75532.4)	(79488.8)	(75312.1)	(79335.1)
1988	153312.1	144604.7	132923.1	151843.7
	(78440.0)	(84564.5)	(78951.4)	(83305.6)
1989	163488.8	157540.0	152682.4	170595.7
	(83560.9)	(87762.4)	(84940.0)	(89776.1)
1990	169818.4	166661.6	167445.4	185543.4
	(87763.9)	(98476.5)	(91065.9)	(96158.2)
1991	167473.0	165861.7	169987.7	187906.0
	(105157.7)	(105141.6)	(98387.3)	(111466.0)
1992	167112.6	161708.7	173075.5	190948.5
	(101067.4)	(108558.3)	(102979.4)	(111753.8)
1993	161154.9	161294.9	170927.1	188749.3
	(114076.6)	(117302.2)	(126297.8)	(121438.6)
1994	166947.9	162620.2	180152.8	198873.1
	(128355.3)	(121984.2)	(142568.1)	(152425.0)
1995	170313.5	168394.2	185451.4	204611.7
	(118433.5)	(124454.6)	(131655.8)	(148596.2)
1996	179109.4	176579.9	197368.1	218236.8
	(125242.8)	(126270.9)	(152934.2)	(164242.6)
N	12,416	12,823	92,085	29,791

Notes: Non-admitted applicants are weighted by the number of admitted to non-admitted applicants. Admitted applicants are left unweighted. All numbers are in 1996 SEK.

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