Long-Run Returns to Field of Study in Secondary School

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Abstract: This paper studies whether specialized academic fields of study in secondary school, which are common in many European countries, affect earnings as an adult. Non-random sorting of students into one field over another makes this a difficult question to answer. As pointed out by Kirkeboen et al. (2016), with multiple unordered alternatives, identification requires not just quasi-random variation into fields of study, but also an accounting of an individual's next-best alternative. Our setting is Sweden, where at the end of ninth grade students rank fields of study and admissions to oversubscribed fields is determined based on a student's GPA. We use a regression discontinuity design which allows for different labor market returns for each combination of preferred versus next-best choice, together with nationwide register data for school cohorts from 1977-1991 linked to their earnings as adults. Our analysis yields four main findings. First, Engineering, Natural Science, and Business generally yield higher earnings for compliers, while Social Science and Humanities result in sizable drops. The magnitudes can be as large in absolute value as estimates of the return to two years of additional education. Second, the returns to completing a field exhibit substantial heterogeneity as a function of next-best alternatives. Third, the pattern of returns is consistent with comparative advantage for some field choice combinations, while others exhibit either random sorting or comparative disadvantage (consistent with individuals choosing based on non-pecuniary factors). Fourth, most of the differences in adult earnings can be attributed to differences in adult occupations and postsecondary programs. Taken together, these long-lasting effects have important implications for policy, as students are relatively young and may have limited information about their skills and interests when choosing fields at age 16.

Keywords: Field of study, secondary education, comparative advantage

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1 Introduction

The structure of secondary education differs widely across countries, both in terms of duration and content. The U.S. system is characterized by a common and comprehensive focus in high school, where individuals do not generally choose specialized majors until their final years of university studies. In contrast, many countries throughout the world, including much of Europe, require students to specialize in secondary school, choosing fields of study which prepare them for specific fields of study at the university level or provide vocational training (for an overview, see OECD, 2019). While there is emerging evidence on the return to different college majors,[1] an equally important question is whether the returns to different academic secondary school fields have long-run effects on labor market outcomes. This is particularly relevant for policy, because students are still relatively young and may have limited information when making these field choices.

Despite the importance of the question, evidence on the returns to different academic fields in secondary school remains scarce. One challenge is that students endogeneously sort into one field of study over another. This problem is compounded by the fact that students choose between multiple unordered alternatives. As pointed out by Kirkeboen, Leuven, and Mogstad (2016), identification of a meaningful parameter requires not just quasi-random variation into fields of study, but also an accounting of individuals' next-best choices. On top of these identification challenges, the data requirements are formidable. One needs information not only on each individual's completed field of study, but also which academic program they were admitted to, and their preferred and next-best alternative choices. In addition, to examine long-run labor market impacts, one needs to be able to follow individuals several decades later and observe their occupation and earnings.

We overcome these challenges in the context of Sweden's secondary school system. There are five academic fields of study which take at least three years to complete: Engineering, Natural Science, Business, Social Science, and Humanities. In addition to these academic fields, which comprise the focus of our paper, there are between 17 and 21 non-academic two year programs. These can be split into general non-academic (3 fields) and vocational non-academic (14-18 fields). The reason we focus on academic fields is that admission into the non-academic programs was most often not limited, so we cannot use our research design for them. During the time period we study, individuals could only apply to their own school region, unless their preferred field of study was not offered.

At the end of ninth grade, students rank their preferred fields of study, and admission to oversubscribed fields is determined by the student's cumulative ninth grade GPA. Admission

[1] For a summary of the literature, see Altonji, Blom, and Meghir (2012) and Altonji, Arcidiacono, and Maurel (2016). Most of the existing work relies on OLS, with the recent exceptions of Andrews, Imberman, and Lovenheim (2017), Hastings, Neilson, and Zimmerman (2013), and Kirkeboen, Leuven, and Mogstad (2016), which each take advantage of admission discontinuities for college majors.

decisions are decided centrally, and the allocation mechanism is both Pareto efficient and strategy proof. Importantly, individuals just above and below the GPA cutoff should be roughly similar on all observable dimensions, allowing us to use a regression discontinuity (RD) design to estimate effects for marginal students. To deal with the unordered nature of field choices, we allow for separate jumps at the field-specific GPA cutoffs for each combination of preferred and next-best fields. This allows us to get separate estimates for the payoff to Engineering for those with a second-best choice of Natural Science versus Business, for example. Since we are primarily interested in the return to completing a field, we use the sharp jumps in admission at the GPA cutoffs as instruments for field completion in a fuzzy RD design. We also estimate sharp RDs for the policy-relevant question of the return to being admitted to a specific field.

This research design is possible because we were able to gain access to the field rankings, admission decisions, and completed fields of study for all students between the years 1977-1991. Using personal identification numbers, we are able to link this data to high-quality register data. In particular, we observe labor market outcomes decades later, when individuals are in the prime of their working careers.

Our first empirical finding is that the returns to completing different academic fields are often sizable, and can be both negative and positive. Earnings payoffs are generally positive or zero for Engineering, Natural Science, and Business. In contrast, the returns to Social Science and Humanities are mostly negative. For example, the returns to Engineering range from 1.9% to 8.6%, depending on an individual's next-best alternative field of study, while the returns to Social Science range from -10.3% to 0.5%.

Second, earnings payoffs to different fields depend on next-best alternatives. For example, there is a 9.3% return to Business relative to a second-best choice of Natural Science, but essentially no return (0.4%) for those who have Humanities as their next-best alternative. This pattern reveals that individuals choosing Humanities as their next-best field are a selected sample which are not directly comparable to those choosing Natural Science as their next-best field. Formal tests reject the null hypothesis that second-best choices do not matter for each of the field-specific returns.

Third, the pattern of returns is consistent with individuals pursuing comparative advantage in expected earnings for some field choice combinations but not others. Business/Natural Science, Engineering/Natural Science, Natural Science/Social Science, and Engineering/Business all show statistically significant evidence supporting comparative advantage for individuals who rank one field over the other. For the three combinations of Natural Science/Humanities, Business/Social Science, and Engineering/Social Science, random sorting into fields cannot be rejected. Finally, both Social Science/Humanities as well as Business/Humanities show strong evidence for comparative disadvantage.[2] Comparative advantage and comparative disadvantage are both consistent with generalized versions of the Roy model which include non-monetary gains. In contrast, such patterns are not consistent with other commonly used models such as the Ben Porath model which relies on efficiency units (Heckman and Guilherme, 1985).

Fourth, most of the differences in adult earnings across academic programs can be explained by differences in expected earnings across occupations and postsecondary programs linked to different secondary fields of study.

Our study is most closely related to the literature on the returns to college major choice, both conceptually and methodologically. Early work on this question relied on OLS (see the survey by Altonji et al. 2016). Recently, several papers have used admissions threshold as instruments for college major enrollment or completion. Hastings et al. (2013) is the earliest example, which takes advantage of a score-based threshold in Chile for admission to different degree programs (defined by a given university and major). They use high-quality administrative data to estimate long-term labor market outcomes using an RD design. They estimate the intention-to-treat parameters associated with being admitted to a program, which are useful for thinking about policies to marginally expand or contract admissions to certain programs. They do not have the required data to look at program completion. More recent work by Kirkeboen et al. (2016) makes the important point that with multiple unordered choices, instruments for each program are not enough to identify a meaningful IV parameter without accounting for next-best alternatives. They use administrative data for the entire country of Norway which also has a centralized system with an threshold for admission into different degree programs (again defined by a given university and major). They study the payoffs to different college majors and to attending more selective schools using IV, since they lack the power to use an RD design. Finally, Andrews et al. (2017) studies the impact of switching to a business major in college using GPA cutoffs and an RD design with data from Texas.[3]

More broadly, our paper is related to work which looks at the effects of school curricula or the completion of specific classes (Altonji 1995, Altonji et al. 2012, Deming and Noray 2018, Joensen and Nielsen 2009, Levine and Zimmerman 1995, Rose and Betts 2004), tracking in elementary and secondary school (Dustmann, Puhani, and Schönberg 2017, Pekkarinen, Uusitalo, and Kerr 2009), and general versus vocational training (Bertrand, Mogstad, and Mountjoy 2019, Brunello and Rocco 2017, Golsteyn and Stenberg 2017, Hall 2012, Hanushek, Schwerdt, Woessmann, and Zhang 2017, and Malamud and Pop-Eleches 2010).

[2]The remaining combination of Engineering/Humanities occurs so rarely that the standard error associated with our test is too large to draw a meaningful conclusion.

[3]See also Arcidiacono (2004) and Rodriguez, Urzúa, and Reyes (2016), which adopt a more structural approach to study college majors.

Relative to the existing literature, we make several novel contributions. As far as we know, we provide the first causal estimates of the returns to academic fields of study in secondary school. This is the primary contribution of the paper. While returns to college major and institution are important, returns to secondary school field are equally relevant for economic productivity, individual welfare, and policy debates on how to best structure secondary education. Our data and empirical design also have several advantages relative to existing work. We can estimate returns for both the intention-to-treat effect from being offered admission as well as the causal effect of program completion. The rich administrative data allow us to link fields of study to long-term labor market outcomes over two decades later, when individuals are in the prime of their working careers. Moreover, we can estimate returns to single academic fields of study, without having to combine them and without the confounding factor of institution choice to deal with. The returns are easy to interpret, as fields all follow the same centrally-decided curriculum. We also report results by gender and family background. A key empirical advance is that we are able to estimate returns for each first-second best choice combination using a convincing RD design. In their seminal paper, Kirkeboen et al. (2016) motivate their insights with a flexible RD design, but in the end need to use a more restrictive IV approach with few instruments because of small sample sizes.[4]

The magnitude and variability of our estimates are substantively important. The absolute value of the estimates often exceed the return to an additional two years of education, which has been estimated to be in the neighborhood of 3 to 5% per year in Sweden (Meghir and Palme 2005, Black et al. 2018). Individuals are making these field choices at the relatively young age of 16, when preferences are still in flux and individuals are still learning about their abilities. While this type of secondary education system arguably has some advantages, it also has potential downsides. These early field choices are linked to future earnings and occupations in a way which may limit future economic mobility. This is a significant consideration, both for individuals and for policymakers. It is important to recognize that our design estimates returns to field of study for compliers, rather than the general population. Fortunately, this is the relevant group from a policy perspective, as reforms which expand or contract different fields on the margin target exactly these individuals.

The remainder of the paper proceeds as follows. The next section describes Sweden's system of secondary education, the admission process into fields of study, and our unique data. Section 3 discusses identification using preferred and next-best alternative choices in an RD design. Section 4 presents our main findings and performs several robustness checks. Section 5 tests for comparative advantage and Section 6 explores possible mechanisms for our findings. The final section concludes.

[4]To control for selection, Kirkeboen et al. use instruments for each field (but not instruments for each preferred×next-best field combination) and the application score as a control variable in an IV rather than a RD framework.

2 Setting and Data

2.1 Fields of Study in Sweden

The Swedish educational system requires nine years of compulsory schooling, after which individuals can drop out or apply to a field of study in secondary school (i.e., high school).[5] During the years we study (1977-1991), there were five academic fields of study to choose from: Engineering, Natural Science, Business, Social Science, and Humanities. Throughout the paper, we refer to fields of study and programs interchangeably. These academic programs took three years to complete, with the exception of Engineering, which had the option of a fourth year of more practical technology-oriented courses. The five academic fields are preparatory for future studies at the university level.

As shown in Table 1, there are substantial curriculum differences across the academic fields. The two STEM fields (Engineering and Natural Science) require more math and natural science classes, and the math courses are taught at an advanced level. Engineering additionally requires a series of technology-related courses, at the cost of fewer art, language, and social science classes. The optional 4th year of Engineering further adds additional technical courses in a chosen specialty (machinery, chemistry, construction, or electronics). Natural Science adds more science classes and some general social studies and language classes. In contrast, Business only requires a single three hour class in the natural sciences, and instead has 25% of the curriculum devoted to business-related courses such as law and accounting. Both Social Science and Humanities devote time to extra social studies and liberal arts classes. Languages comprise 35% of the curriculum for Social Studies, and 43% for Humanities.

In addition to these five academic fields, which comprise the focus of our paper, there were between 17-21 non-academic programs offered. These non-academic programs took two years to complete. There were 14-18 vocational programs aimed at preparing students for a career, and 3 general programs which provided additional general education, but not at the level needed to qualify for university studies.[6] Appendix Figure A1 displays the number of students admitted to each of the five academic fields plus the two aggregated non-academic programs. Roughly half of students are admitted to an academic field, with Engineering and Business being the most popular. The focus of this paper is on the academic fields; the reason is that admission into most of the non-academic programs was not limited, and so we

[5]During the nine years of compulsory schooling there is little specialization. There are two tracks for math, two tracks for English, and the choice of one elective. All other courses are common across students during our time period.

[6]The two-year non-academic general programs were introduced in the 1960s as a middle ground between the academic and non-academic vocational programs. Completing a two-year non-academic program enabled a student to enroll in short education programs classified as tertiary school, such as pre-school teaching or nursing. All municipalities offer adult education, which allows students to complete a three-year secondary school degree and qualify for university education. For further details, see Stenberg (2011).

cannot use the research design we describe in Section 3 for them.

We focus on the period 1977-1991 because the academic fields of study and non-academic programs did not experience major changes over this time frame.After our sample period, there were two sets of reforms. In 1992, Business, Social Sciences, and Humanities were merged into one field, non-academic vocational fields were lengthened to three years, and non-academic general programs were abolished.[7] The 1992 education reform also provided funding to private schools at a similar level to public schools; the resulting expansion of private schools made it possible to apply to the same program offered by several schools, or in other municipalities, and substantially reduced the number of oversubscribed programs. In 2011, Business, Social Sciences, and Humanities re-emerged as separate fields in some, but not all, regions.

2.2 Admission Process

Students apply to be admitted to a field of study. During our sample period (1977-1991), individuals were only allowed to apply for fields in their region of residence unless a field was not offered in their home region. Depending on the year, there are between 115 and 135 secondary school regions, with a median number of 962 applicants per year and school region.

Students compete for slots based on their application GPA. This GPA is the average grade across 10-12 school subjects as of ninth grade. Grades range from a low of 1 to a high of 5 and are supposed to be normally distributed with a mean of roughly 3 in the entire population (including those who drop out of school, pursue a non-academic field, and pursue an academic field). Applicants received a bonus of 0.2 to their GPA for being a minority gender applicant, defined as applying to a field which in the prior year had accepted less than 30% of their gender nationally (e.g., females applying to Engineering). Unless otherwise specified, when we refer to GPA in the remainder of the paper, we are referring to adjusted GPA. Admission decisions only distinguish between GPAs to the first decimal.

The admission process works as follows. During the final semester of ninth grade, students rank their preferences on a standardized one-page application form. They can specify up to 6 fields. The forms are sent to a central administration office which then allocates students to classes based on their preference rankings and GPA. Admission decisions are made sequentially, with the highest-GPA applicant being admitted to their first-choice field, the second-highest GPA applicant being admitted to their highest-ranked field among the set of fields which still have space in them, and so forth. This mechanism of allocating slots is known as "serial dictatorship" and has been shown to be both Pareto efficient and strategy proof (Svensson 1999). In other words, with this allocation mechanism, there is no incentive for students to misreport their true ranking of preferences.[8]

[7] A few three-year non-academic pilot programs were tested between 1987-1991.

[8] In theory, it is possible that only allowing 6 fields causes individuals to put a safe option down as their

After admission decisions are sent out in July, there can be reallocations of students to different fields of study. This can happen for a variety of reasons. For example, a student admitted to Engineering may change their mind and transfer to another field, such as Humanities, that still has open slots. This move will also open up a slot in Engineering, which another student can take. While changes can happen at any time, it becomes more difficult to switch after the fall of the first year given curriculum differences.

These reallocations are not necessarily random, as they depend on individuals changing their minds and potentially discretion on the part of the local secondary school principal. Luckily, we observe the actual admission decision, which is a mechanical and binary function of the GPA cutoff. We can use the admission decision cutoff in an RD design to instrument for program completion. We can also use the sharp cutoff in admission decisions to estimate the policy-relevant effect of admission itself.

2.3 Data

Our analysis uses several different data sources that we link together using unique identifiers for each individual. The most novel data for this study is the ranking list applicants make when they apply for admission to secondary school fields. We observe all of the field choices submitted by a student. This is important, because it allows us not only to observe which field an applicant is admitted to, but also what their next-best alternative choice is. As discussed in Section 3.1, this information is vital for identifying an interpretable causal effect. This data had been reported as lost by the Swedish National Archives, but was miraculously saved by Hans Eric Ohlson at Statistics Sweden in response to a request we made.[9]

Over our sample period, the number of applications to secondary school increased. In 1977 only 60% of the ninth-grade cohort applied to secondary school, but by 1991 this had risen to 80%. Summed over all years, the population of first-time applicants between 1977-1991 is 1,330,453. We drop all observations which have a non-academic program as the preferred choice, because most of these programs are not space constrained and are therefore not useful given our empirical design. We also drop observations where GPA is missing or outside the range of 2.0 to 5.0. This leaves us with 598,265 observations. Our estimation sample is limited to (i) applications to fields which are competitive (i.e., oversubscribed), (ii) individuals who

6th choice, so as to make sure they get into at least one field. This seems unlikely in our setting, as only 0.2% of all applicants are admitted to their 6th choice (and only 1.57% even list a sixth choice). During the years 1982-84, individuals were given bonus GPA points for the first and second choices on their ranking lists. So for these years, individuals may have not revealed their true preferences in their rankings. In a robustness check we exclude these years, and the estimates hardly change (see Section 4.3).

[9]Summary statistics relating to the ranking lists appeared in a 1992 government report, so we knew the data existed at one time. But when we contacted the Swedish National Archives, they reported it as lost. In an unrelated conversation about a different dataset, we let Ohlson know our frustration that the data had been lost. It turns out that Ohlson was the person responsible for delivering the data over 25 years ago to the archives, and that he kept meticulous records. With his help and intervention, we were able to secure the data. Ohlson retired only weeks after the data was delivered.

list a next-best alternative, and (iii) a sample window of -1.0 to +1.5 GPA points around the cutoff, leaving us with a sample of 250,970 observations.[10] Our baseline sample is comprised of the 233,437 observations where we are able to use our preferred earnings variable, which is measured in logs.

For our purposes, we need to define an individual's preferred choice and their next-best alternative. For 96% of individuals, the preferred choice is their first choice on their ranking list and their next-best alternative is their second choice. For the 4% of individuals who are admitted to a third or lower ranked choice, the preferred choice is defined as the choice ranked immediately above their accepted choice, and the next-best alternative as their accepted choice. This gives us information on both preferred and next-best fields, and a quasi random source of variation for each combination of fields for individuals near the admission thresholds. For ease of exposition, we will refer to the preferred field as the first-best choice, even if it turns out that it was not the first choice on their list. Likewise, we will refer to the next-best alternative field as the second-best choice. In other words, we will refer to first- and second-best choices as the relevant preferred and next-best choices.

The number of individuals with each combination of first- and second-best choices in our baseline sample can be found in Table 2. Some combinations have many observations, such as a first choice of Engineering and a second choice of Natural Sciences (N=31,877) or a first choice of Business and a second choice of Social Science (N=29,849). The most sparsely populated combinations are those which include a STEM field and Humanities, with just a few hundred individuals. As Appendix Table A1 documents, the observations are spread across almost 3,500 competitive programs in different years and school regions. That same appendix table also details how many individuals list a non-competitive academic program (i.e., a program which admitted all applicants) as their first choice. Forty-five percent of individuals have a first choice academic field which is non-competitive.

Using personal identification numbers, we link individual's field choice rankings and GPAs to population register data. The Swedish register data is known for its high coverage and reliability. It contains information on long-term labor market outcomes, including annual earnings and occupation. We measure annual earnings between the ages of 37 to 39, taking the average over years with positive earnings.[11] Our main earnings measure takes the natural log of earnings, limiting the sample to individuals earning more than a minimal amount (roughly $12,000), as suggested by Antelius and Björklund (2000).[12] This restriction excludes

[10] We further exclude individuals with GPAs at the cutoff where this is a mix of accepted and non-accepted individuals at the cutoff (see the next section for details). We also exclude a small number of applications which involved school regions and years where the Engineering and Natural Sciences fields were combined.

[11] We use the ages 37 to 39 because this is the latest set of ages with consistent occupation codings for everyone in our sample. Earnings include income from self-employment, sick leave, and parental leave benefits since these are partly included in employer earnings via collective bargaining agreements.

[12] Antelius and Björklund use a SEK 100,000 threshold, which translates into roughly $12,000. We apply their threshold, accounting for wage growth and inflation, to other other years in our sample.

only 7 percent of observations, since our estimation sample is composed of relatively high earners with strong labor force attachment. The motivation for this approach is that Antelius and Björklund found it makes estimates of log annual earnings similar to estimates of log hourly wages in Sweden. To probe robustness, we also use earnings in levels (including zeros and low earnings) and earnings rank (including zeros and low earnings) as alternative outcome measures and find similar results.

Figure 1 provides an initial look at how GPAs and earnings vary by completed program for all individuals. There is not a simple correspondence between fields with high average GPAs and high average earnings. Students completing Natural Science have the highest GPAs, while those pursuing non-academic vocational programs have the lowest. Earnings are highest for Engineering and lowest for Humanities. While we plot GPA and log earnings for all program completers in Figure 1, we remind the reader that our analysis is focused on individuals in academic programs which are competitive.

The register data also includes information on socioeconomic background characteristics. We have information on several pre-determined variables which are observed before the admission decision: each parent's education, each parent's earnings, whether at least one parent is foreign born, and parent's age at the time of their child's birth. Summary statistics for these variables are found in Appendix Table A2. In the first column, we present means for our estimation sample. This sample is limited to fields of study in years and school regions where demand exceeded supply, so that there was a competition for slots. The third column shows means for the sample of individuals with a non-competitive program as their first choice. The means from the two samples are fairly similar. The table also reports on the characteristics of the students. If anything, average GPA is higher for the non-competitive sample. This is due to the fact that programs like Natural Science have applicants with relatively high GPAs, even though they are less likely to be oversubscribed and subject to a competition.

2.4 Determining GPA cutoffs

While we observe the choice rankings for each individual and the admission decision, the GPA cutoff is not recorded in the dataset. Instead, we must infer the GPA cutoff from the data ourselves. Fortunately, in most cases this is simple and transparent, as the rules appear to have been followed.

Each combination of year, region, and field has the potential to be a competition for slots. We refer to these as "cells." Our empirical design only applies to competitive cells. If there are more applicants than slots, the admission GPA cutoff is inferred from the data. We limit our sample to cells where there is evidence for a sharp discontinuity, that is, where everybody above the GPA cutoff is admitted to the program and everybody below the cutoff is not.[13]

[13]We allow for a small amount of noise in the data due to measurement error, which is likely during this

One wrinkle is that there can be a mix of accepted and non-accepted individuals at a cutoff GPA. For example, if the cutoff is 3.2 in a cell, there may only be slots for 3 out of the 5 applicants with a GPA of 3.2.[14] In this case, it is important to know how people at the cutoff with the same GPA were admitted. We found some documentation which indicated admission was random, but also documentation which said that sometimes secondary criteria such as math grades were used to break ties. Since we do not know the criteria used to break ties, we discard the observations at the cutoff GPA. This should not create a problem, as we are still able to identify a sharp discontinuity above and below this mixed-cutoff GPA. Continuing with the example of a mixed cutoff at 3.2, we would drop all individuals with a GPA exactly equal to 3.2 in the cell, but define the cutoff as 3.2 for the remaining observations in the cell.

When there is not a mix of accepted and non-accepted individuals at a cutoff, we simply define the cutoff GPA as the average between the two adjacent GPAs. So for example, if everyone with a GPA below 3.3 is not admitted and everyone with a GPA above 3.4 is admitted, we define the GPA cutoff as 3.35.

To allow us to pool the data across regions and years, we normalize the cutoff GPA to 0. The distribution of cutoff GPA values is plotted in the top panel of Figure 2 (white columns), with a comparison to the GPA distribution for our baseline sample (gray columns). This graph provides an indication of where individuals on the borderline of acceptance into a program are found in the skill distribution. The modal cutoff GPA of 3.2 corresponds to roughly the 15th percentile of GPAs in our baseline sample of applicants to competitive academic programs. To put this in perspective, the modal cutoff GPA also corresponds roughly to the median GPA of all ninth graders (including those applicants not in our sample which have a preferred non-academic choice and also those who don't apply to secondary school at all).[15] While the cutoffs vary substantially, they generally are only binding for applicants with GPAs in the bottom half of our estimation sample. Both of these comparisons will be important to keep in mind when interpreting the estimates, which will capture local average treatment effects for applicants around the cutoff.

One thing to note about the admission cutoffs is that programs which attract the highest GPA individuals do not necessarily have the highest cutoffs, or even a cutoff at all. This is because the cutoff is determined by supply versus demand for a field. For example, average GPAs are highest in Natural Science, but in many cases all students are admitted because there are fewer applicants than slots. The fields most likely to be oversubscribed in our data

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time period since most variables were transcribed and entered by hand. We drop obvious miscodes, which amounts to 0.43 percent of the data.

[14] As a reminder, GPA is only recorded to the first decimal, so the second decimal of GPA cannot be used to break ties.

[15] The median unadjusted GPA is 3.2 for the sample of all ninth graders in the years 1988-1991. This is the first set of years when registers of (unadjusted) GPA for the full population, including those not applying to secondary school, was first collected.

are Engineering, Business, and Social Science (see Appendix Table A1).

3 Identification

3.1 Using Preferred and Next-Best Choices in an RD Design

Our goal is to estimate the economic returns from being admitted to one field of study versus another. As pointed out by Kirkeboen et al. (2016), with multiple unordered alternatives, identification of returns requires more than just quasi-random variation into fields of study. One also needs to account for the fact that individuals have different second-best choices. OLS (which does not have any information on preferred and next-best fields) is biased both because individuals self-select into field choices and because individuals choosing the same preferred field can differ in their next-best fields. Even with no selection bias, OLS is difficult to interpret, because it is a weighted average of returns across individuals with different second-best choices, where the weights are unobserved.

Kirkeboen et al. go on to discuss what IV can and cannot identify when next-best alternatives are not observed. While their discussion and estimation approach center around traditional IV, the ideas are equally applicable to a fuzzy RD design. A randomly assigned cutoff for each field in a fuzzy RD design will eliminate selection bias, but without further assumptions, fuzzy RD will not estimate the return to any individual or group who choose one field over another.[16] One possibility is to impose "constant effects," where the returns to completing a field are the same for all individuals. This assumption is unpalatable because it rules out the possibility of comparative advantage. Another possibility is to impose "restrictive preferences" à la Behaghel et al. (2013). This assumption is also unattractive in the current setting. It implies that an individual who completes Social Science when they are just above the Business GPA cutoff would also have completed Social Science if they were just above the Engineering GPA cutoff.[17]

When next-best alternatives are available, however, RD can estimate LATEs for every preferred versus next-best field. The weak assumption needed in this case is what Kirkeboen et al. call an "irrelevance condition". This condition is best explained with an example. Consider an individual with a first choice of Engineering and a second choice of Business. The irrelevance condition says that if crossing the GPA threshold for admission to Engineering does not cause them to complete Engineering, then it does not cause them to complete another field like Social Science either.

In our paper, we allow for separate first stage and reduced form jumps at the cutoff for

[16]The example Kirkeboen et al. give in their study of college major choice is that "IV estimation would not tell us whether the gains in earnings to persons choosing engineering instead of business are larger or smaller than the gains in earnings to those choosing law instead of business. It is possible that persons choosing engineering gain while those choosing law lose."

[17]For this example, it is easiest to think of Social Science as being a field with unrestricted admission, but Business and Engineering as having binding cutoffs.

each combination of preferred and next-best fields. Our design deals with both selection and heterogeneity in next-best alternatives, under the standard assumptions needed for RD plus the relatively benign irrelevance condition. Our RD design is more flexible than Kirkeboen et al.'s IV approach, which controls for selection using instruments for each field (but not instruments for each preferred × next-best field combination) and the application score as a control variable. They use their more restrictive approach due to sample size considerations.

3.2 Regression Discontinuity Model

To estimate the returns to different fields, we exploit the discontinuity in admission decisions to different fields based on ninth grade cumulative GPA. Define dummy variables a_{jk} for $j = 1, ..., J$ and $k = 1, ..., K$ which equal 1 if an individual's preferred choice is j and next-best choice is k. The reduced form effect of the admission decision on log earnings for an individual with preferred field j and next-best alternative k, y_{jk}, can be modeled in an RD framework as follows:

$$y_{jk} = \sum_{jk} a_{jk} 1[x < c_j] g^l_{jk}(c_j - x) + \sum_{jk} a_{jk} 1[x > c_j] g^r_{jk}(x - c_j) + \sum_{jk} a_{jk} 1[x > c_j] \theta_{jk} + \alpha_{jk} + w'\gamma + e_{jk} \tag{1}$$

where we have omitted the individual subscript for convenience. The running variable x is an individual's GPA, c_j is the cutoff GPA for admission to program j, g^l_{jk} are unknown functions to the left of the cutoffs, g^r_{jk} are unknown functions to the right of the cutoffs, α_{jk} are dummy variables for each first-second best combination, w is a set of pre-determined controls (including parental background variables, year fixed effects, and school region fixed effects), and e_{jk} is an error term. The θ_{jk} coefficients capture the returns to individuals who are admitted to field j instead of their next-best alternative k. Since our dependent variable is measured in logs, these coefficients have the convenient interpretation of a percent increase in earnings.

In practice, admission cutoffs for a field vary by year and school region. To combine the data, we therefore normalize each cutoff to be 0, and adjust the GPA running variable accordingly. Note that in its most general form, equation (1) has separate functions to the left and right of the cutoffs for each combination of preferred and next-best alternatives. In our empirical analysis, we have a total of 5 preferred choices and 7 next-best alternatives, which means there are a potentially 30 functions to the left of the cutoff and 30 functions to the right of the cutoff. Estimating 60 unknown functions is very data demanding, so for efficiency, we impose some parametric functional forms.

For our baseline specification, we first impose that the functions g^l_{jk} and g^r_{jk} are linear. We also gain efficiency by imposing restrictions on the slopes to the left and the right of the cutoff. Our baseline, and most parsimonious, RD parameterization allows just 2 slopes: a common slope to the left and a common slope to the right. Another possibility is to impose common slopes to the right of the cutoff for each of the 5 preferred choices (regardless of the next-best choice), and common slopes to the left of the cutoff for each of the 7 next-best choices (regardless of the preferred choice). This parameterization links the normalized GPA slopes to the field an applicant was admitted to. We show the results for the 2 slope model are virtually identical compared to the 12 slope model (5+7 slopes), and similar to the 60 slope model (which has much larger standard errors). Our baseline model also parameterizes $\alpha_{jk} = \delta_j + \tau_k$, so that instead of 30 different intercept terms, we allow for 5 different intercepts based on first choices and 7 based on second choices. We remove this parametric assumption in a robustness check and find similar results, but with slightly larger standard errors.

Importantly, we always allow the jumps at the cutoffs, captured by θ_{jk}, to be both j and k specific, no matter what restrictions we impose on the functions g^l_{jk} and g^r_{jk} and the intercepts α_{jk}. Note that this differs from, and is more general than, the analysis done by Kirkeboen et al. in their study of college majors. They use IV, rather than RD, and estimate coefficients for each first choice j and coefficients for each second choice k, but not separate coefficients for each first-second choice combination jk.

While the reduced form coefficients are interesting in their own right (the returns to *field admission*), we are also interested in the returns to *field completion*. Let d_{jk} denote a dummy variable for an individual with a next-best alternative field k who completes their preferred field j. The first stage for this fuzzy RD design is:

$$d_{jk} = \sum_{jk} a_{jk} 1[x < c_j] h^l_{jk}(c_j - x) + \sum_{jk} a_{jk} 1[x > c_j] h^r_{jk}(x - c_j) + \sum_{jk} a_{jk} 1[x > c_j] \lambda_{jk} + \tau_{jk} + w'\gamma + u_{jk} \tag{2}$$

where h^l_{jk} are unknown functions to the left of the cutoffs, h^r_{jk} are unknown functions to the right of the cutoffs, τ_{jk} are dummy variables for each first-second best combination, w is the same set of pre-determined controls appearing in the reduced form equation, and u_{jk} is an error term. Whatever parametric functional form we impose in the reduced form we also impose in the first stage. This first stage RD identifies the jumps in completion probabilities, λ_{jk}, induced by the admission cutoffs. These jumps in completion probabilities can be used to scale the reduced form effects of equation 1. Importantly, we always allow the completion jumps at the cutoffs, captured by λ_{jk}, to be both j and k specific, no matter what parametric restrictions we impose.

If each jk margin were estimated as a separate regression and there were no control variables, the IV estimates would equal $\hat{\pi}_{jk} = \hat{\theta}_{jk} / \hat{\lambda}_{jk}$. We estimate all of the margins in a single regression to increase precision.

3.3 Threats to Validity

Manipulation. An important condition for a valid RD design is that the running variable cannot be perfectly manipulated. In our setting, the assumption is that students cannot adjust their GPA to be just to the right of the cutoff for their preferred program. While it is possible to study harder and get higher grades, there is little chance of a student being able to manipulate their GPA to be just over the cutoff. One reason is that the required GPA to get accepted into a program is not known in advance, and varies from year to year. The actual cutoff depends on the number of applicants to a program, since there are a fixed number of slots for each program. Panel B in Figure 2 illustrates the year-to-year variation in admission thresholds. It plots the distribution of first differences in admission cutoffs for fields in a school region. While the distribution is centered at 0, there is substantial variation. Indeed, for programs with a cutoff in successive years, the threshold differs over 80% of the time.

One way to test for manipulation is to check whether pre-determined characteristics are balanced around the admission cutoff. Appendix Figure A2 illustrates how various pre-determined characteristics of parents and children vary by distance to the cutoff. In these graphs, we combine all individuals, regardless of their preferred versus next-best alternative. There are no discernible jumps at any of the cutoffs. We test for discontinuities more formally using RD regressions using the 2 slope model in Appendix Table A3. All of the estimates are close to zero and not statistically significant.

Another common test for manipulation is to look at the distribution of observations around the cutoff. Unfortunately, it is not possible to do a standard McCrary (2008) test or the newer density test proposed by Cattaneo, Jansson, and Ma (2018). The reason is that pooling the data to a normalized cutoff of 0 creates a spurious density discontinuity when the cutoff is based on an order statistic. In ongoing research, Cattaneo, Dahl, and Ma are working on a proof for the spurious density discontinuity and ways to modify a density test to account for this.[18]

Exclusion, Monotonicity, and Irrelevance. With no manipulation, the RD design identifies the causal effects of admission to a program (i.e., the intention-to-treat effect). To identify the causal effects of completing a program, we additionally need exclusion restrictions, monotonicity, and irrelevance.

The exclusion restrictions require that crossing the admissions threshold for a program only affects outcomes through field completion. It is possible that being admitted to a program could have a direct impact on earnings if a person takes several specialized field classes before switching to another major. This not a major concern in our setting since

[18]We thank our econometrician colleagues Kaspar Wuthrich, Xinwei Ma, and Matias Cattaneo for helping us to think through these issues.

most switching takes place in the early fall of the first year of upper secondary school. Later switching is rare because the curriculum is specialized and most courses are taught once a year. And for individuals who are able to switch, it would have to be within fields that have very similar first year course requirements. For this reason, we do not think individuals granted admission obtain much in the way of specialized training if they do not complete a program. There is also the possibility that admission to a field alters the chances an individual drops out of school entirely. Since we are looking at individuals applying to the academic track, which is preparatory to further studies, this is not a common occurrence. We conclude that the exclusions restrictions do not pose major issues for our estimates.

The monotonicity assumption requires that crossing an admissions threshold does not make an individual less likely to complete that program. This assumption of no defiers seems likely to hold in our setting. Finally, we require the irrelevance condition previously discussed in Section 3.1. This condition also seems reasonable in our setting.

4 Results

This section presents our main empirical findings. We begin by reporting first stage estimates for how admission translates into program completion. We then present results for how field of study impacts future earnings. The section ends with a variety of robustness checks.

4.1 First Stage

We begin by documenting the relationship between admission to a program and program completion. To illustrate the idea of the first stage, consider individuals with a preferred choice of Engineering and a second choice of Natural Science. The top panel of Figure 3 plots the probability of completing the Engineering program in normalized GPA bins. Everyone to the right of the vertical line is (initially) admitted to the program, while everyone to the left is not (initially) admitted.

Completion of the program is not 100% to the right of the cutoff, because some people switch and complete other programs. This happens more often the closer an individual is to the right of the cutoff. This could be because those who barely gain admission have second thoughts about pursuing a field where they are the lowest-GPA students.

When an individual transfers out of Engineering, it opens up a slot for a student who was not initially admitted. This explains why individuals to the left of the admissions cutoff can complete the Engineering program as well. There is a positive slope to the left of the cutoff, which could be due to local schools offering any newly opened slots to the next-highest GPA student who preferred Engineering but did not get admitted. For example, suppose there are 65 applicants for 60 slots (corresponding to 2 classes of size 30). If 60 students are accepted, but then 2 individuals switch out of Engineering, it will open up 2 slots which can be filled by 2 of the 5 initially denied applicants. If these 2 individuals complete the program, the completion rate to the left of the cutoff will be 40%. These transfers into Engineering are not necessarily random, however, because who chooses to accept the offer is endogenous. Moreover, it is possible that local school principals use other criteria to allocate these newly opened slots which will induce selection bias. This is the reason we need to instrument for program completion (which is not random) with program admission (which is quasi-random near the cutoff).

The bottom panel of Figure 3 plots a similar first stage graph for individuals with a first-best choice of Engineering, but this time with a second-best choice of Business instead. While the first stage jumps in the top and bottom panels of Figure 3 are similar in magnitude, this is not generally the case for all first-second best margins.

The first stage regression for all first-second field combinations is modeled by equation 2. To begin, we use the baseline parameterization, which allows for one slope to the left and one slope to the right of the cutoff, but 30 jumps at the cutoffs (one for each first-second best margin) as explained in Section 3.2. Table 3 reports the jumps for each first-second choice margin. The estimated jumps are sizable, but there is some heterogeneity across different margins. For example, while the jump for the Engineering first-choice and Natural Science second-choice margin is 36%, it is only 25% for those with Engineering first-choice and Social Science second-choice. This makes some sense, as individuals who have a second-best choice of Social Science may not be as committed to a STEM field. The differential jumps based on next-best alternatives is a first hint that second-best choices are consequential, and need to be accounted for in estimation.

Similar estimates, while not shown, are found using the 12 slope model and the 60 slope model. The correlation between the thirty different estimates using the 2 slope model and the 12 slope model is 0.97, while for the 2 slope model versus the 60 slope model it is 0.98. No matter what parameterization we choose, the estimates are highly significant, indicating there will not be a weak instrument problem with our fuzzy RD. The reason to use the more parsimonious 2 slope model is for precision in the second stage.

4.2 Effect of Field of Study on Earnings

We now turn to estimates of the earnings return to different fields of study, which are allowed to be relative to each second-best choice. We first illustrate the idea graphically using two examples, and then turn to our regression based estimates for all possible first-second best combinations.

The top panel of Figure 4 considers the margin where Engineering is the first choice and Natural Science is the second choice. The graph plots the average of the natural log of earnings in 0.1 GPA bins, where earnings are measured between the ages of 37-39, as explained in Section 2.3. There are increasing slopes both to the right and the left of the cutoff, indicating that higher GPAs relative to the cutoff result in higher earnings. There is also a large jump at the cutoff of roughly .07 log points. In the bottom panel, we turn to individuals who have the same first-best choice of Engineering, but a different second-best choice of Business. Here, there is little evidence for a jump in earnings. This provides graphical evidence that the return to Engineering depends on an individual's second-best choice.

We chose to illustrate ideas using the Engineering first-choice and Natural Science second-choice margin and the Engineering first-choice and Business second-choice margin because they both have many applicants. Other choice margins are more sparsely populated, so we turn to our more parsimonious RD parameterization to gain precision. We start with the 2 slope model with 30 different returns (one for each first-second best margin) as described in Section 3.2. We focus on the fuzzy RD estimates; corresponding reduced form estimates can be found in Appendix Table A4.

Table 4 reports the fuzzy RD estimates, which are estimated via IV regression. All of the estimates appearing in the table are estimated at the same time in a single regression. The rows indicate an individual's first-best choice, while the columns indicate their second-best choice. Consider the entry Engineering first-choice and Natural Science second-choice, which is the IV estimate for the same margin shown in panel A of Figures 3 and 4. The estimate of 0.068 says that individuals who are admitted to their first-best choice of Engineering instead of their second-best choice of Natural Science experience an earnings premium of 6.8% as an adult. This is a sizable return. To put the magnitude into perspective, the return to an extra year of schooling in Sweden has been estimated to be around 3 to 5% per year in Sweden (Meghir and Palme 2005, Black et al. 2018).[19]

There are two initial takeaways from this table. First, the returns to different fields, while heterogeneous across second-best choices, are generally positive or zero for Engineering, Natural Science, and Business, whereas Social Science and Humanities mostly have negative returns. For example, the return to Engineering is positive relative to every second-best choice and ranges from 1.9% to 8.6%. In contrast, 10 out of 12 estimates for the returns to Social Science and Humanities are negative. For example, individuals who complete Social Science with a second-best choice of Business experience a 6.9% drop in earnings. This decrease shows up even when the next-best choice is non-academic: the return to completing Social Science when the next-best alternative is a non-academic vocational program is -10.3%.

Second, returns to different fields depend on next-best choices. For example, there is a 9.3% return to Business relative to a second-best choice of Natural Science, but no return to Business for those who choose Humanities as their second choice. We test whether second-best choices matter more formally by testing whether the IV estimates for each first-choice field (i.e., each row in the table) are jointly equal to each other. For example, for Engineering the

[19] Both of these studies use a schooling reform in Sweden to arrive at causal estimates.

test is $\hat{\pi}_{EN} = \hat{\pi}_{EB} = \hat{\pi}_{ES} = \hat{\pi}_{EH} = \hat{\pi}_{EG} = \hat{\pi}_{EV}$, where the subscripts indicate the first-second best margin using the starting initial for each program. The resulting F-statistics and p-values are reported in the last column of Table 4. We can easily reject that next-best alternatives do not matter for each of the fields.

Appendix Table A5 reports results by gender and parental education. The first column repeats our baseline results for comparison. In the second and third columns, we show results for males and females. We use our baseline specification, using a single regression which combines both genders, but which allows for separate cutoff jumps and separate slopes as a function of the running variable for each gender. The returns for completing one field over another are postively correlated for males and females, but there are important differences. In particular, the earnings penalty for completing Social Science or Humanities is larger for men compared to women relative to every possible second-best choice. Turning to separate estimates for children with high versus low educated parents (defined as at least one parent completing 12 years of education), we find that these are remarkably similar.

4.3 Specification Checks

In this section we provide a variety of robustness checks. To enable easier comparisons of coefficients, and to fit many results into a single table, we present estimates for the different specifications in tabular form. These robustness checks appear in Table 5, with the first column presenting our baseline estimates for comparison. The last row of each column reports the correlation of the estimates using the different specifications with the baseline estimates.[20]

We begin by exploring different parametric models for the RD regression. We first add in quadratic terms in the running variable to our baseline model. As column 2 shows, this pushes the estimates up slightly, mostly due to somewhat smaller first stage estimates. We next try adding in first-second choice specific intercept terms (i.e., 30 intercepts) to the baseline model. This likewise does not appreciably change the estimates, although the standard errors increase, especially for sparsely populated choice margins. Both sets of alternative estimates have a high correlation with the baseline estimates.

Our next set of specification checks relax the parametric assumption of a two slope model. We first allow for common slopes to the right of the cutoff for each of the 5 preferred choices (regardless of the next-best best choice), and common slopes to the left of the cutoff for each of the 7 next-best choices (regardless of the preferred choice). Before turning to these estimates, we present the raw data in graphical form in Figure 5. The top figure plots averages of log annual earnings in 0.1 bins, allowing for separate slopes for each of the five first-best choices to the right of the cutoff. While the graph makes clear the intercepts for the various first-best

[20]We weight the correlation by the inverse of the sum of the squared standard errors of the two estimates. While the estimates are consistently estimated, they are measured with error, and so the correlation coefficient could be biased.

choices differ, the slopes are remarkably similar to one another. The bottom figure conducts a similar exercise, plotting the averages separately for each of the 7 next-best choices.[21] Again, the intercepts for the various second-best choices differ, but not as much as they did for first-best choices in the top graph. And while the data are noisier to the left of the cutoff due to smaller sample sizes, the slopes are again similar to each other.

For comparison, we have also plotted the averages within a bin for a common slope to the left of the cutoff in the top graph and for a common slope to the right of the cutoff in the bottom graph. Comparing the top and bottom graphs, it becomes apparent that the 2 slope model is a reasonable parameterization relative to the 12 slope model.[22] This is confirmed in the estimates for the 12 slope model, in column 4 of Table 5. Note that Figure 5 is for illustrative purposes only; we never mix the 2 slope and 12 slope models in estimation.

We also estimate the 60 slope model, which allows for unrestricted slopes for each first-second best combination to the left and the right of the cutoffs. These estimates are found in column 5. The sets of estimates from both the 12 and 60 slope RD models yield similar results compared to our baseline. To see this visually, we plot the estimates for each of the first-second best combinations for the 12 and 60 slope models against the 2 slope model in Figure 6. Most of the dots are clustered around the 45 degree line in the figure. If anything, the 60 slope model estimates are slightly larger. The correlation of the 12 slope model with the 2 slope model is 0.97, while the correlation of the 60 slope model with the 2 slope model is lower at 0.79. The advantage of the 2 slope model, particularly relative to the 60 slope model, is that the estimates are substantially more precise for many of the combinations.

We next estimate our baseline model, but exclude the years 1982-84. During these three years, individuals were given a 0.5 GPA bonus for the first field on their ranking list and a 0.2 GPA bonus for the second field on their ranking list. This means that for these two years, the allocation mechanism was not strategy-proof. Instead, individuals could have been strategic about not putting their most preferred field first if they thought they wouldn't get in even with the GPA bonus. This could change the interpretation of our estimates. However, it turns out that excluding 1982-84 does not appreciably change the coefficient estimates, as shown in column 6.

Our next set of specification checks examine alternative definitions for the earnings variable. Our baseline model uses log earnings, as described earlier, and excludes roughly 7 percent of the sample who have zero or low earnings between the ages of 37 and 39. A first way to see whether excluding individuals with zero or low earnings matters is to estimate whether the probability of being in this restricted sample jumps at the GPA cutoff in a reduced form

[21] As a reminder, there are only five first-best choices, because we do not study non-academic first-best choices. This is because the non-academic two year programs are not oversubscribed very often.

[22] As a reminder, both models allow for different jumps at the cutoff for each first-second best combination, i.e., 30 different jumps at the cutoff.

RD regression. Appendix Table A6 presents these estimates. Out of 30 estimates, four are significant the 5% level and one is significant at the 10% level. This is slightly more than would be expected by chance, and could be indicative of a small extensive labor market response to field of study. While any bias is likely to be small, we probe the robustness of our log earnings variable by using two alternative earnings measures which do not exclude any observations.

Our first alternative earnings measure uses earnings in levels as the outcome, including low earnings and zeros. These results appear in column 7 of Table 5. Earnings are measured in real terms relative to 2016, and are converted to U.S. dollars using an exchange rate of 8.50 Swedish crowns per dollar. The pattern of estimates is similar to the baseline estimates appearing in the first column. The magnitudes are also roughly comparable. For example, individuals choosing Engineering over Natural Science experience an earnings increase of $4,806 per year. Since the average earnings for this group is $51,973, this translates into a 9.2% increase in average earnings. This compares to the estimate of a 6.8% increase in earnings for the baseline log specification column 1. The estimates using this earnings variable have a correlation of 0.97 with the log earnings estimates.

As another alternative earnings variable, we use earnings rank as the outcome. We calculate each individual's rank in the year-specific population earnings distribution for all individuals in Sweden between the ages of 16 to 64. The results using this as the outcome measure appear in the last column of Table 5. Roughly the same number of estimates are statistically significant using this measure compared to our log earnings measure. The correlation of estimates using rank earnings versus log earnings is 0.96.[23]

The conclusion from the battery of tests in Table 5 is that our results are robust to a variety of alternative specifications, both in terms of the parametric form of the RD regression and for how we measure earnings.

5 Tests for Comparative Advantage

Given the pattern of estimates in Table 4, a natural question is whether the findings are consistent with a model of comparative advantage in field choice. Consider a case with just two individuals, A and B, and two fields, j and k. A standard definition (Sattinger 1993) is that individual A has a comparative advantage in field j over k if the ratio of their earnings in field j versus k is larger than the corresponding ratio for individual B. Taking natural logs of the ratios implies that individual A has a comparative advantage in field j over k if the difference in log earnings between j versus k for individual A is larger than the corresponding

[23]While not shown in the table, we also explored 3 other modifications of our log earnings measure: (i) we excluded publicly provided parental leave and sickness benefits from our earnings measure, (ii) we adjusted the earnings threshold to account for inflation, but not wage growth, and (iii) we used earnings between the ages of 39-41 instead of 37-39 (the oldest ages for which we observe occupation). All three of these modifications result in estimates which are similar to the baseline estimates.

difference for individual B. In other words, individual A has a comparative advantage in field j over k if the percent increase in earnings for field j relative to k is larger for individual A compared to individual B.

This standard definition of comparative advantage has implications for first- and second-best choices. Comparative advantage in field choice preferences implies the expected earnings gain in percent terms for field j for individuals who rank j over k should exceed the negative of the expected earnings gain in percent terms for field k for individuals who rank k over j. Stated in terms of our model parameters, there are three possible cases:

$$\begin{aligned} &\text{Case (a):} \quad \pi_{jk} + \pi_{kj} > 0 \quad \textit{comparative advantage} \\ &\text{Case (b):} \quad \pi_{jk} + \pi_{kj} = 0 \quad \textit{random sorting} \\ &\text{Case (c):} \quad \pi_{jk} + \pi_{kj} < 0 \quad \textit{comparative disadvantage} \end{aligned}$$

where as a reminder, π_{jk} is the percent return to completing first choice j for individuals with second choice k. These parameters correspond to the IV estimates appearing in Table 4.

Case (a) is consistent with comparative advantage in field choice, as individuals are choosing the field within a pair of choices that results in higher earnings for them. Case (b) occurs when there is random sorting into fields for individuals on the margin of choosing j versus k. In this case, the return to completing field j with second choice k is equal but opposite in sign to the return to completing field k with second choice j. Finally, case (c) is consistent with individuals choosing based on comparative disadvantage. This could happen if individuals value non-pecuniary factors associated with different fields of study, where the non-pecuniary factors are negatively correlated with their potential earnings. Comparative disadvantage can occur with full information, but it can also be the result of imperfect knowledge about relative payoffs across fields.

In Table 6 we present estimates of $\pi_{jk} + \pi_{kj}$ for each pair of field choices. Consider first the example of individuals on the margin of Natural Science or Business. Students who complete their first-best choice of Business when their second-best choice was Natural Science earn a 9.3% premium (see Table 4). Looking at the reverse ordering of preferences, the return is 5.7% for those completing Natural Science when their second-best choice was Business. Random sorting would have predicted the two returns had opposite signs and were equal in absolute value. As the first row of Table 6 shows, the sum of the two estimates is 15.0, a number which is statistically different from zero. So this example is consistent with individuals pursuing comparative advantage in field choice.

The other rows in Table 6 display $\hat{\pi}_{jk} + \hat{\pi}_{kj}$ for the other nine margins, ordered from high to low. The field combinations which show statistically significant evidence of comparative advantage are Business/Natural Science, Engineering/Natural Science, Natural Science/Social Science, and Engineering/Business. Some field combinations have relatively small sums, and random sorting cannot be rejected: Natural Science/Humanities, Business/Social Science, and Engineering/Social Science. Two field combinations show strong evidence for comparative disadvantage: Social Science/Humanities and Business/Humanities. One field combination, Engineering/Humanities, occurs so rarely that although the estimated sum is large, it is not statistically different from zero.

In summary, 4 field combinations show evidence of comparative advantage, 3 of random sorting, and 2 of comparative disadvantage. Many of these sums are large, indicating an important role for comparative advantage and disadvantage in field choice. Field combinations involving Business, Engineering, or Natural Science have the most evidence for comparative advantage, whereas the combinations showing comparative disadvantage include Humanities.

Note that we cannot provide a characterization of selection into different fields for the entire population, but only for compliers on the margin of gaining entry into one field versus another. However, this is arguably the most interesting group from a policy perspective, because these are the individuals that could be shifted to one program from another, for example, by increasing slots in oversubscribed fields.

6 Mechanisms

Section 4 provides clear evidence of highly variable, and often sizable, returns to field of study in secondary school. A natural question is what drives these results. In this section, we explore three possible mechanisms: differences in years of schooling, differences in postsecondary programs, and differences in occupations.

First, if completing a field of study (for a given next-best alternative) induces individuals to get more or fewer years of schooling, this could have an effect on future earnings. For example, since the Engineering degree has an optional 4th year of studies, that could result in more years of education for individuals who complete the Engineering field. It is also possible that different fields of study have a different impact on the likelihood of attending college.

Second, since the fields of study in secondary school are preparatory to fields of study in college, it could also be the pattern of earnings we observe in Table 4 is explained by individuals being induced to complete different types of postsecondary programs. For example, if a student completes Business in secondary school, it could affect whether they complete a Business degree in college. In this case, earnings could increase because there is a positive return to a college Business degree. It could also be that individuals complete a postsecondary certification program to get licensed for an occupation.

Third, if entry into different occupations requires, or is eased by, having a specific secondary school degree, then differences in earnings across different occupations could explain our findings. For example, it may be harder to get a job as an accountant for individuals who completed Humanities versus Business in secondary school. The differential earnings of accountants versus other occupations could therefore be a third possible mechanism.

To perform this analysis, we create three variables which reflect expected earnings for each of the possible mechanisms. For expected earnings due to occupation, we assign each individual the mean log earnings of all individuals with the same occupation as of age 38 from the same school cohort. There are 355 different occupations. We construct similar expected earnings measures based on 315 different postsecondary programs and imputed years of schooling.[24] We use each of these measures as the outcome variable in an RD model which parallels our baseline specification in Table 4. This yields 30 different estimates, one for each first-second field choice combination, of the expected returns associated with each of the three mechanisms.

In Figure 7, we plot the 30 different baseline estimates against the 30 different years of schooling estimates (top panel), the 30 different postsecondary program estimates (middle panel), and the 30 different occupation estimates (bottom panel). There are labels for each first-second choice margin to aid in interpretation. There is a positive slope in all three panels, suggesting a contribution from each of these mechanisms. When the expected return due to extra years of schooling rises by 1%, the return to earnings we estimated in Table 4 rise by 0.5%. Likewise, when the expected returns due to postsecondary program or occupation rises by 1%, the returns rise by 0.95% and 1.4%, respectively. Appendix Table A7 reports these regression results.

The three mechanisms are not necessarily independent or mutually exclusive. As a final specification, we regress the baseline estimates on the three measures simultaneously in the final column of Appendix Table A7. The coefficient on the years of schooling estimates shrinks towards zero. The postsecondary program type coefficient falls by a third, but remains statistically significant. Likewise, the occupation coefficient falls by 20%, but also remains significant. The regression R^2 from this combined regression is 0.95. We caution, however, that the slope estimates and hence also the regression R^2 could be biased, as both the baseline estimates and the other estimates are measured with error. With this caveat in mind, the general conclusion is that both secondary school type and occupation play a sizable role in explaining the pattern of returns we observe for individuals on the margin of completing a field of study in secondary school.

7 Conclusion

There is emerging evidence that the return to different college majors can rival that of the return to going to college. An equally important question is whether the returns to different

[24] For the occupation measure we use 3 digit codes, but collapse to 2 digit codes if the number of observations is less than 100. For the postsecondary program measure, we require an individual to complete at least 3 years of postsecondary schooling (requiring at least 2 years yields similar results), and create a single category for individuals who do not complete a postsecondary program. We impute years of schooling based on highest education level, including any specialized education courses individuals take as adults. By using cohort-specific means, we do not need to assume anything about how the returns to schooling, postsecondary program, or occupation have changed over time.

academic secondary school majors have long-run effects on labor market outcomes. Our analysis yields four main results. First, the returns to completing different academic fields are often sizable, and can be both negative and positive. Second, earnings payoffs to different fields depend on next-best alternatives, consistent with self-selection based on field preferences. Third, the pattern of returns is consistent with individuals pursuing comparative advantage for some field combinations. For other field combinations, the results indicate random sorting and even comparative disadvantage. Fourth, most of the differences in adult earnings can be attributed to differences in adult occupations and postsecondary programs.

These findings are useful information for students making field decisions, as well as for school counselors and parents who could use this information to help students make a more informed choice. They are also valuable for policymakers choosing how to structure and reshape secondary education, including whether to relax enrollment limits on oversubscribed fields or to provide incentives to study one field over another.

While this paper makes important progress on estimating long-term payoffs to secondary field choices, several questions remain unanswered. The parameters we estimate are ex-post payoffs to fields of study. An interesting question for future research is whether these ex-post payoffs line up with ex-ante predicted payoffs. If they do, it suggests that students understand the monetary tradeoffs associated with different fields of study, and that some students are willing to trade off higher earnings for non-pecuniary returns. However, it is also possible that at age 16, when these students need to choose, they do not yet know what occupation will be the best fit for them and they may not be knowledgeable about earnings differences across fields. The parameters we estimate are also for compliers on the margin of gaining entry into a field. For these marginal individuals with relatively low GPAs, the effects can be sizable. It would be interesting to know if similar patterns hold for other individuals.

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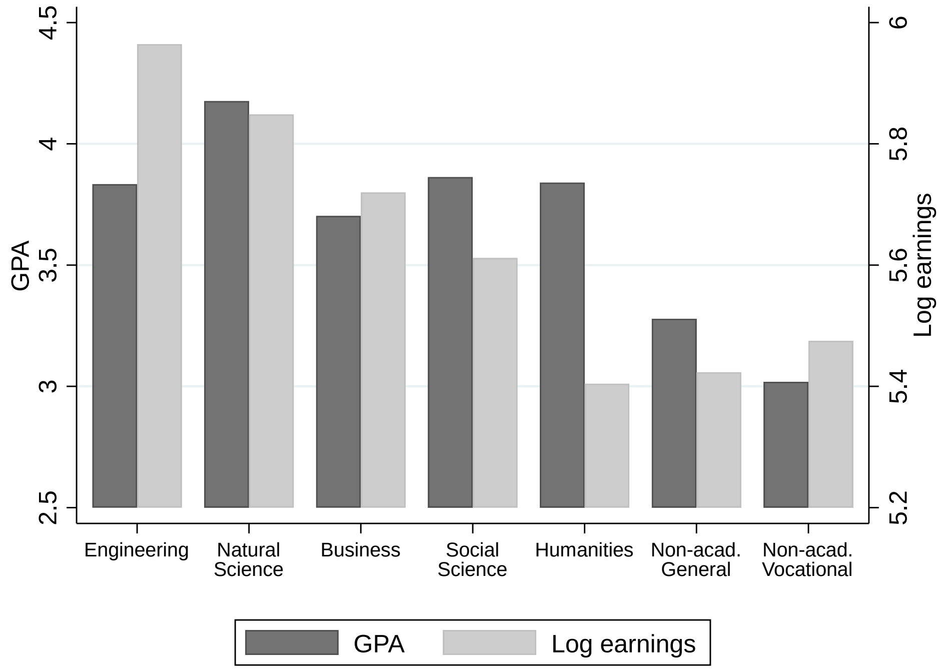
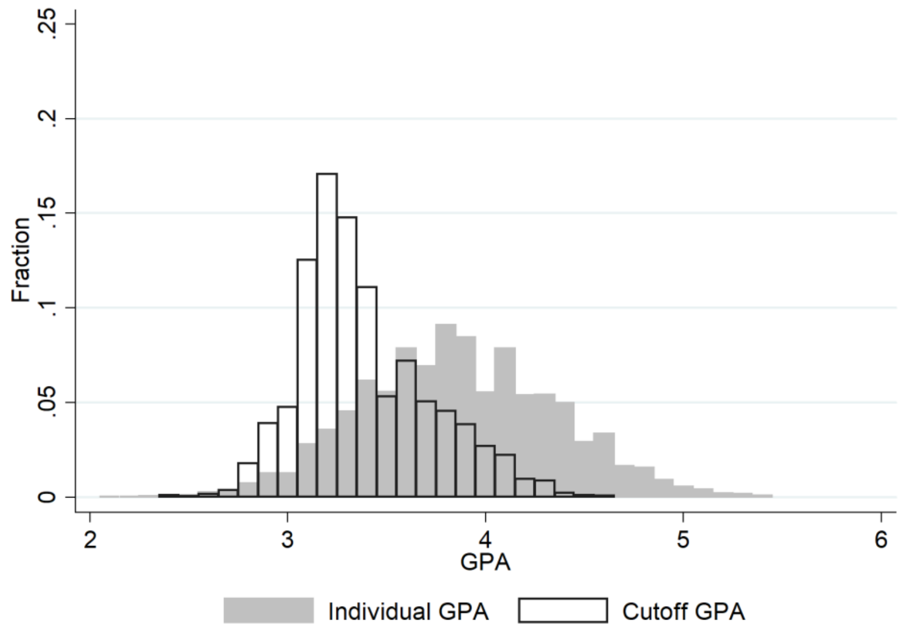
Figure 1. Ninth grade unadjusted GPA and adult earnings for program completers.



Notes: Sample of program completers who applied between 1977-1991. Adult earnings measured between the ages of 37-39. N=1,208,269 for GPA, N= 1,132,945 for log earnings.

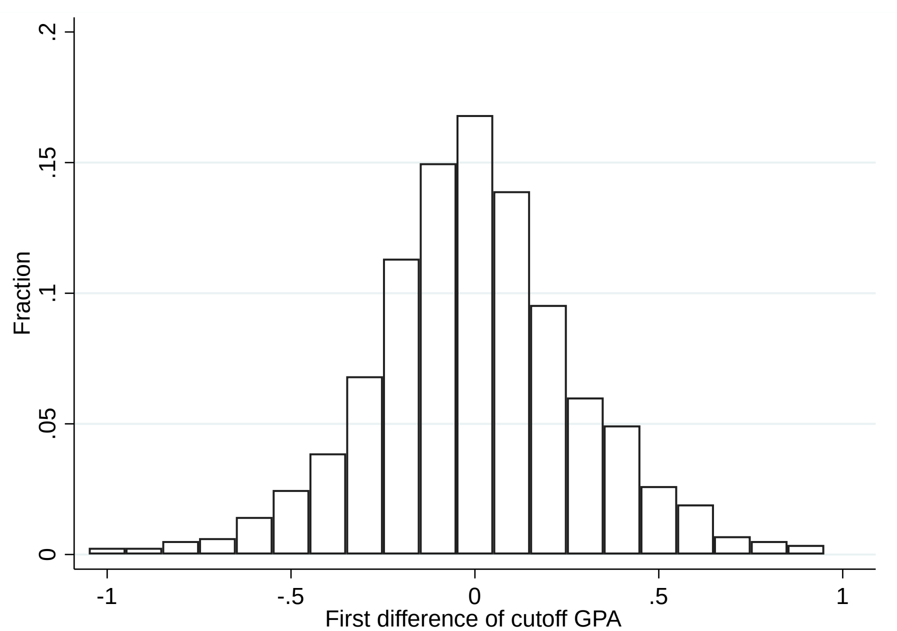
Figure 2: GPA, cutoff GPA, and first-differenced cutoff GPA distributions.

Panel A: Cutoff GPA and individual GPA



Notes: The white bars plot the distribution of cutoff GPAs for competitive programs, which vary by field of study, year, and school region. There are 3,487 competitive programs in our estimation sample. The grey bars plot the distribution of GPA for individuals in our estimation sample of 233,437 observations.

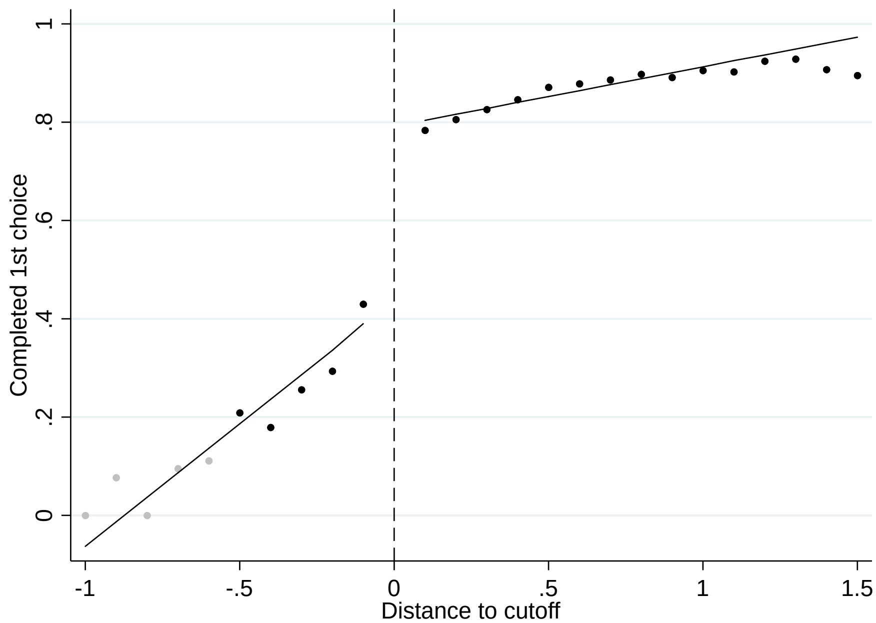
Panel B: Current minus lagged cutoff GPA



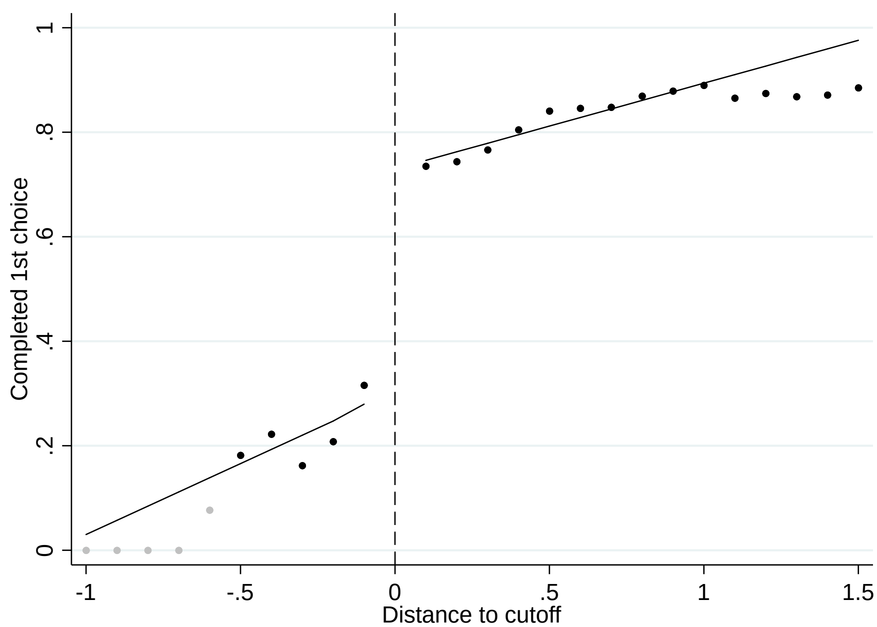
Notes: Sample limited to fields of study which are competitive two years in a row in a school region.

Figure 3. First stage graphs for Engineering vs. Natural Science and Business.

Panel A: Engineering vs. Natural Science



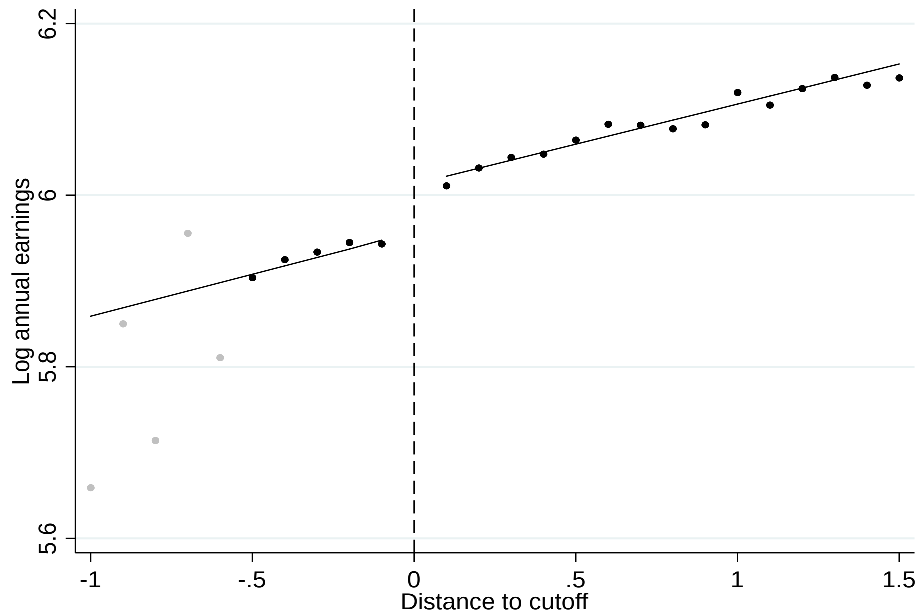
Panel B: Engineering vs. Business



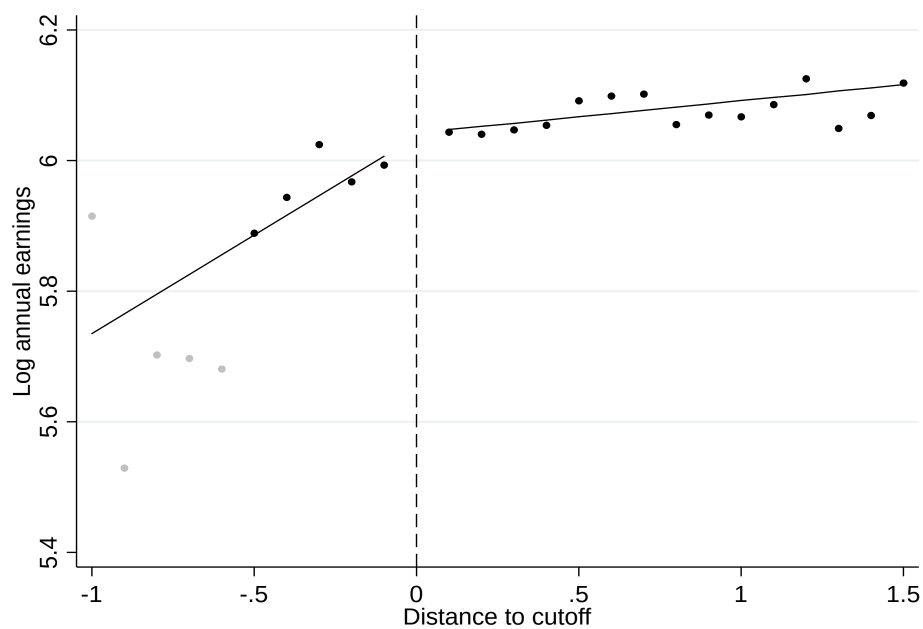
Notes: N = 31,877 (panel A) and N=11,997 (panel B). Each dot is the average completion rate in a 0.1 GPA bin, where GPA is measured relative to a normalized cutoff of 0. The dots furthest to the left of the cutoff appear in grey to emphasize that they contain only a small number of observations; the gray dots account for just 6% of the observations to the left of the cutoff in both panel A and B. The trend lines are RD estimates using the underlying data, no covariates, and triangular weights.

Figure 4. Reduced form graphs for Engineering versus Natural Science and Business.

Panel A: Engineering vs. Natural Science



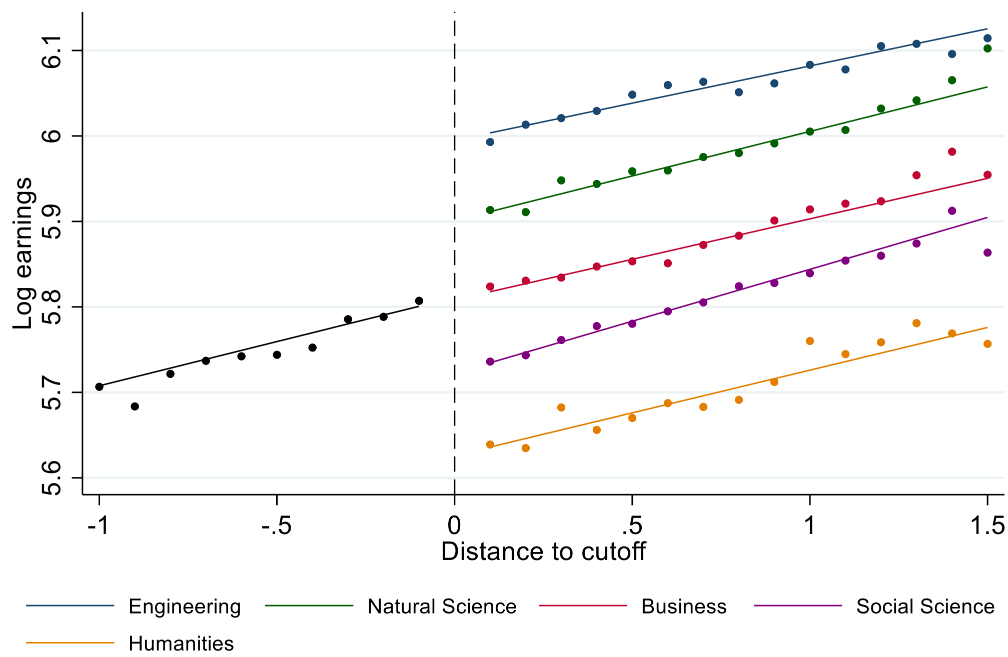
Panel B: Engineering vs. Business



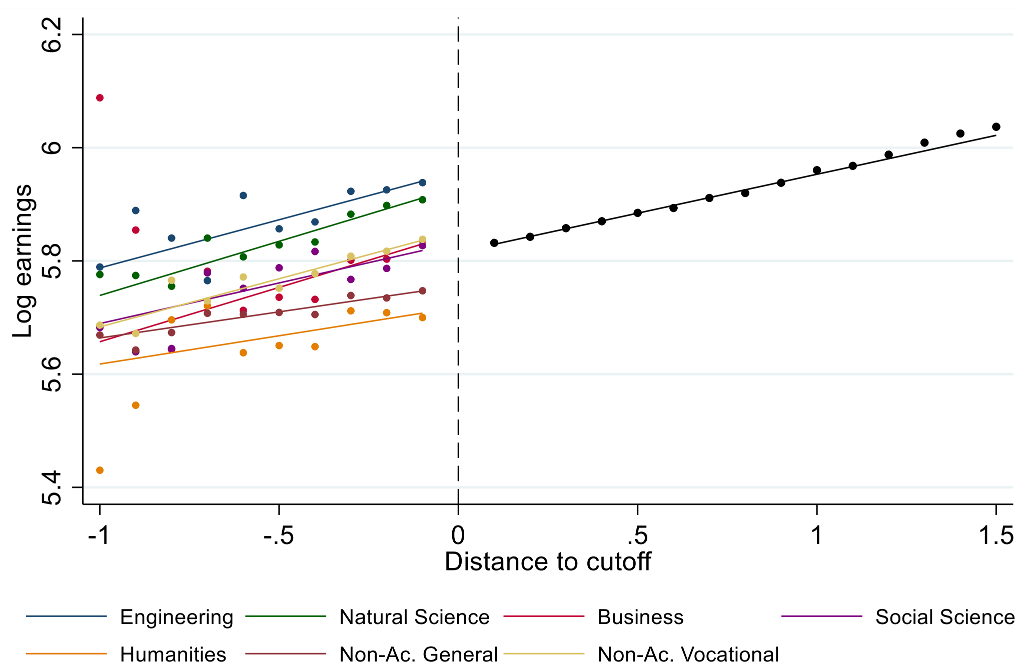
Notes: N = 31,877 (panel A) and N=11,997 (panel B). Each dot is the average completion rate in a 0.1 GPA bin, where GPA is measured relative to a normalized cutoff of 0. The dots furthest to the left of the cutoff appear in grey to emphasize that they contain only a small number of observations; the gray dots account for just 6% of the observations to the left of the cutoff in both panel A and B. The trend lines are RD estimates using the underlying data, no covariates, and triangular weights.

Figure 5. Comparison of 2 versus 12 slope models.

Panel A: Single slope below the cutoff, 5 separate slopes above the cutoff

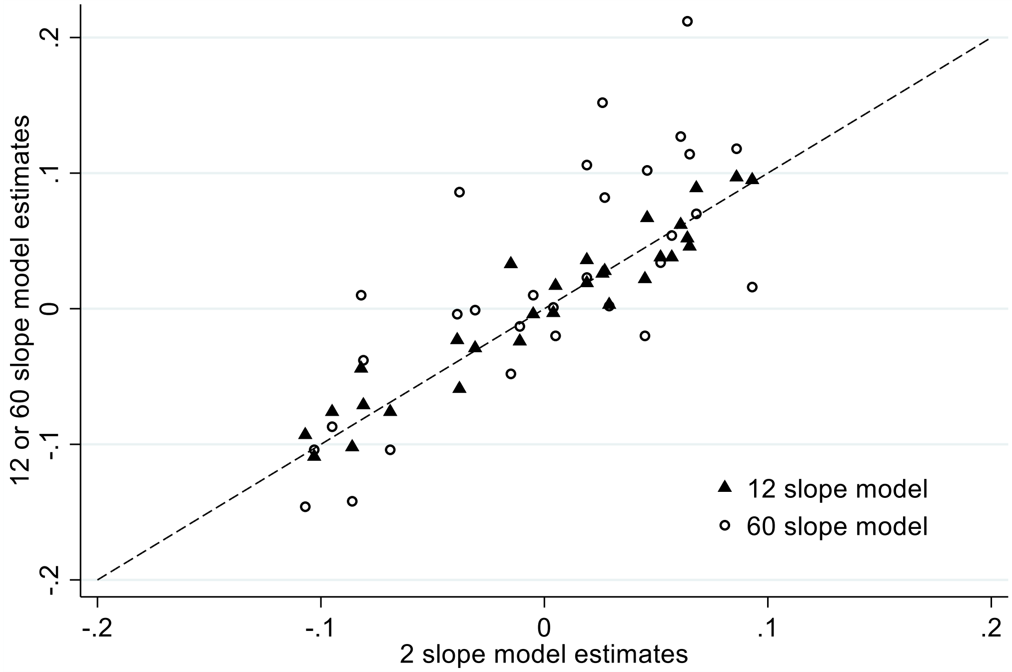


Panel B: 7 separate slopes below the cutoff, single slope above the cutoff



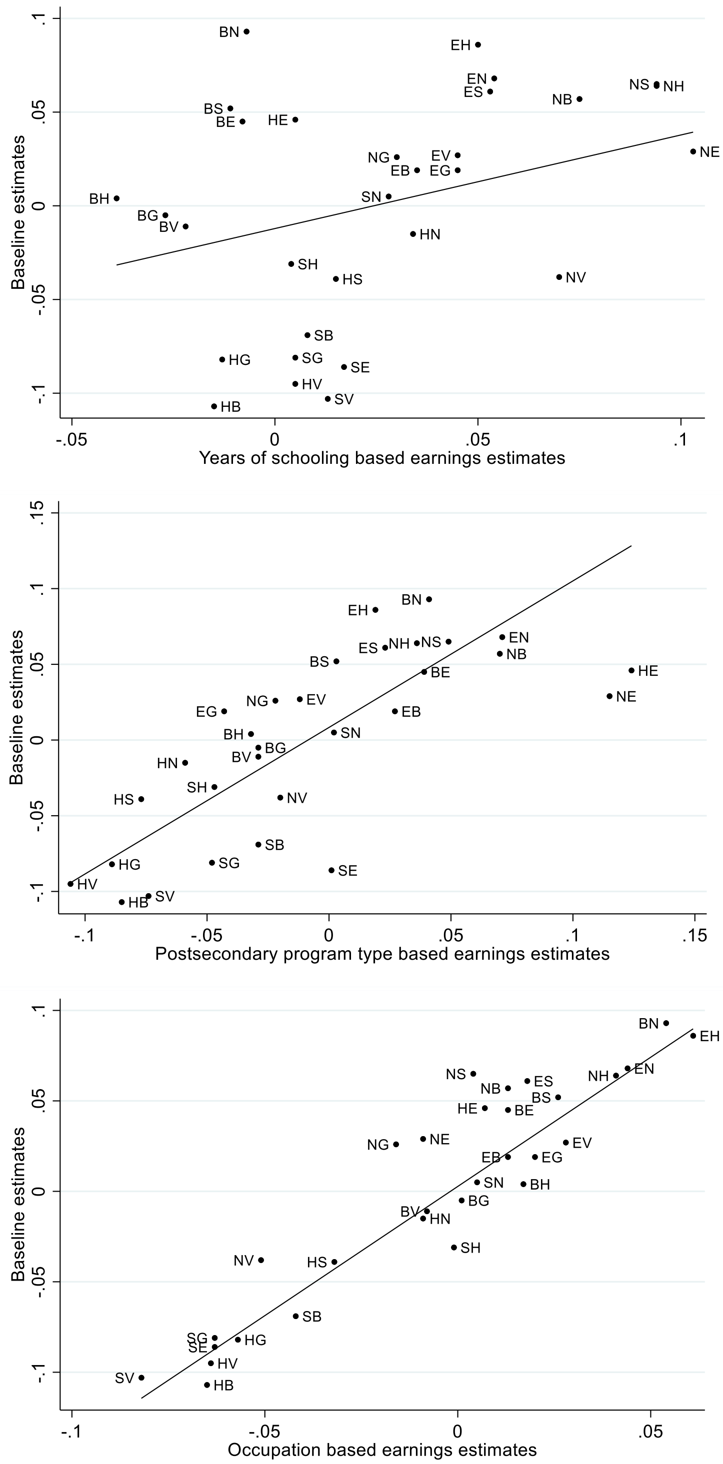
Notes: These graphs are for illustrative purposes; we never mix the 2 slope and 12 slope models in estimation. Each marker is the average of log earnings in a 0.1 GPA bin, where GPA is measured relative to a normalized cutoff of 0. The trend lines are RD estimates using the underlying data, no covariates, and triangular weights.

Figure 6. Comparison of IV estimates using the 60 slope, 12 slope, and 2 slope models.



Notes: There are 30 estimates for each model, one for each first–second best choice combination (see Table 4). The dashed line is the 45° line.

Figure 7. Mechanisms: Years of schooling, postsecondary program, and occupation.



Notes: Estimates for each margin are labeled by first–second best choice combination. E, N, B, S, H, G, V stand for Engineering, Natural Science, Business, Social Science, Humanities, General non-academic, and Vocational non-academic, respectively. The solid line is the regression slope, using the inverse of the squared standard errors of the baseline estimates as weights. See Table 7 and the text for details.

Table 1. Course requirements for each of the five academic programs.

	Weekly hours of course instruction				
Classes	Engineering	Natural Science	Business	Social Science	Humanities
Math	15^{adv}	15^{adv}	11	11	5
Natural science	17	22.5	3	9	7
Social science	11	16	16.5	25.5	25.5
Swedish	8	9	9	10	10
English	6	7	7	8	9
Additional languages	6	11	14	17	24
Art and music	-	4	-	4	4
Physical education	7	8	7	8	8
Technology related	22.5	-	-	-	-
Business related	-	-	25	-	-
Other	3.5	3.5	3.5	3.5	3.5
Total hours	96	96	96	96	96

Notes: The total amount of 96 hours consists of 34, 32, and 30 hours per week during the first, second, and third years, respectively. Engineering has an optional fourth year of 35 hours per week of mostly technology related courses. The superscript "adv" indicates that advanced math is required for Engineering and Natural Science. Business allows the possibility to exchange 3 hours of math with business-related courses. Natural science classes include physics, chemistry, and biology, while Social science classes include history, religion, philosophy, psychology, and social studies. These curricula are mandated by law and laid out in Lgy70 (Läroplan för gymnasieskolan); they remained unchanged during our sample period (1977-1991) but were modified in 1994.

Table 2. Number of observations by first-second choice combination.

	Second choice						
First choice	Engineering	Natural Science	Business	Social Science	Humanities	Non-acad. General	Non-acad. Vocational
Engineering	--	31,877	11,997	3,374	550	4,481	11,187
Natural Science	8,844	--	2,379	5,631	681	493	954
Business	7,624	6,694	--	29,849	8,123	18,127	13,462
Social Science	1,763	8,437	15,732	--	15,322	8,294	3,591
Humanities	412	582	2,316	7,206	--	2,191	1,264

Notes: Baseline sample of 233,437 individuals.

Table 3. First stage RD estimates for program completion.

	Second choice						
First choice	Engineering	Natural Science	Business	Social Science	Humanities	Non-acad. General	Non-acad. Vocational
Engineering	--	.362***	.406***	.249***	.260***	.398***	.409***
		(.010)	(.011)	(.015)	(.029)	(.011)	(.009)
Natural Science	.423***	--	.444***	.355***	.339***	.319***	.338***
	(.015)		(.017)	(.017)	(.025)	(.032)	(.024)
Business	.484***	.480***	--	.473***	.442***	.546***	.530***
	(.014)	(.011)		(.011)	(.012)	(.007)	(.008)
Social Science	.398***	.428***	.519***	--	.396***	.472***	.452***
	(.017)	(.012)	(.010)		(.011)	(.009)	(.012)
Humanities	-.065**	.255***	.464***	.396***	--	.324***	.308***
	(.027)	(.025)	(.015)	(.014)		(.016)	(.019)

Notes: N=233,437. The RD specification uses the 2 slope model; linear functions of the running variable of normalized GPA; a window of -1.0 to 1.5; triangular weights; fixed effects for year, school region, preferred field, and next-best alternative field; and controls for the parent and child characteristics listed in Table A3 (except for the GPA variables). Standard errors in parentheses.

** p<0.10, ** p<0.05, *** p<0.01*

Table 4. Returns to different fields of study: IV estimates of program completion on log earnings.

	Second choice							F-test for
First choice	Engineering	Natural Science	Business	Social Science	Humanities	Non-Acad. General	Non-Acad. Vocational	2^{nd} choice does not matter
Engineering	--	.068***	.019	.061**	.086**	.019	.027*	14.42
		(.016)	(.018)	(.025)	(.038)	(.017)	(.014)	[.013]
Natural Science	.029	--	.057**	.065**	.064*	.026	-.038	12.25
	(.024)		(.026)	(.026)	(.035)	(.050)	(.038)	[.032]
Business	.045**	.093***	--	.052***	.004	-.005	-.011	62.02
	(.020)	(.017)		(.016)	(.017)	(.010)	(.011)	[.000]
Social Science	-.086***	.005	-.069***	--	-.031*	-.081***	-.103***	48.01
	(.025)	(.017)	(.014)		(.017)	(.013)	(.016)	[.000]
Humanities	.046	-.015	-.107***	-.039*	--	-.082***	-.095***	14.65
	(.135)	(.037)	(.020)	(.020)		(.026)	(.030)	[.012]

Notes: N = 233,437. The RD specification uses the 2 slope model; linear functions of the running variable of normalized GPA; a window of -1.0 to 1.5; triangular weights; fixed effects for year, school region, preferred field, and next-best alternative field; and controls for the parent and child characteristics listed in Table A3 (except for GPA, which, when normalized is the running variable). Earnings are the average between ages 37-39 above a minimum threshold, and include income from self-employment, sick-leave, and parental leave benefits (see Section 2.3). The F-test in the last column tests whether the estimates in each row are equal to each other. Standard errors in parentheses, p-values in brackets.

** p<0.10, ** p<0.05, *** p<0.01*

Table 5. Robustness checks.

Margin	Baseline	Quadratic	1^{st}-2^{nd} intercepts	12 slopes	60 slopes	Excluding 1982-84	Earnings in levels	Earnings rank
E vs. N	.068***	.075***	.077***	.089***	.070**	.066***	4.806***	.035***
	(.016)	(.023)	(.019)	(.031)	(.035)	(.019)	(1.070)	(.012)
E vs. B	.019	.026	.029	.019	.023	.015	.925	.013
	(.018)	(.023)	(.032)	(.027)	(.038)	(.019)	(1.244)	(.012)
E vs. S	.061**	.069**	.042	.062	.127**	.049*	3.685**	.033*
	(.025)	(.031)	(.056)	(.040)	(.058)	(.028)	(1.603)	(.017)
E vs. H	.086**	.094**	.186	.097**	.118	.082*	5.380**	.054*
	(.038)	(.042)	(.222)	(.049)	(.080)	(.045)	(2.381)	(.028)
E vs. G	.019	.026	-.015	.036*	.106***	.029	-.709	.013
	(.017)	(.023)	(.025)	(.020)	(.036)	(.019)	(1.071)	(.012)
E vs. V	.027*	.034	.035**	.028	.082***	.024	.845	.033***
	(.014)	(.021)	(.016)	(.018)	(.029)	(.016)	(.997)	(.010)
N vs. E	.029	.035	.021	.003	.002	.039	2.912*	.012
	(.024)	(.028)	(.033)	(.045)	(.056)	(.026)	(1.614)	(.016)
N vs. B	.057**	.063**	.030	.038	.054	.045	2.948	.038**
	(.026)	(.030)	(.053)	(.037)	(.062)	(.028)	(1.934)	(.018)
N vs. S	.065**	.072**	.104**	.046	.114*	.051*	4.417***	.049***
	(.026)	(.031)	(.048)	(.045)	(.065)	(.029)	(1.705)	(.019)
N vs. H	.064*	.070*	.164*	.052	.212**	.063*	2.311	.059**
	(.035)	(.039)	(.087)	(.048)	(.102)	(.038)	(2.121)	(.025)
N vs. G	.026	.033	.046	.026	.152	-.012	-.367	.021
	(.050)	(.053)	(.080)	(.056)	(.121)	(.055)	(2.949)	(.036)
N vs. V	-.038	-.031	-.057	-.059	.086	-.050	-3.385	-.012
	(.038)	(.041)	(.052)	(.045)	(.114)	(.040)	(2.339)	(.027)
B vs. E	.045**	.051**	.065**	.022	-.020	.050**	5.370***	.021
	(.020)	(.025)	(.029)	(.035)	(.042)	(.022)	(1.502)	(.013)
B vs. N	.093***	.099***	.102***	.095***	.016	.088***	7.730***	.046***
	(.017)	(.022)	(.027)	(.028)	(.036)	(.019)	(1.171)	(.011)
B vs. S	.052***	.059***	.060***	.038	.034	.044**	4.059***	.043***
	(.016)	(.021)	(.019)	(.030)	(.034)	(.018)	(1.058)	(.011)
B vs. H	.004	.010	-.017	-.003	.001	.004	1.190	.027**
	(.017)	(.022)	(.028)	(.028)	(.035)	(.019)	(.998)	(.012)
B vs. G	-.005	-.001	-.010	-.004	.010	-.004	-.506	.001
	(.010)	(.016)	(.011)	(.012)	(.017)	(.011)	(.669)	(.007)
B vs. V	-.011	-.006	-.009	-.024*	-.013	-.014	-.409	.007
	(.011)	(.017)	(.012)	(.014)	(.020)	(.012)	(.822)	(.008)

Table 5. Robustness checks, continued.

Margin	Baseline	Quadratic	1st-2nd intercepts	12 slopes	60 slopes	Excluding 1982-84	Earnings in levels	Earnings rank
S vs. E	-.086***	-.079***	-.108***	-.102**	-.142***	-.081***	-2.871*	-.037**
	(.025)	(.030)	(.037)	(.040)	(.054)	(.028)	(1.698)	(.017)
S vs. N	.005	.011	-.025	.017	-.020	-.003	.446	-.002
	(.017)	(.023)	(.031)	(.030)	(.038)	(.020)	(1.144)	(.012)
S vs. B	-.069***	-.063***	-.071***	-.076***	-.104***	-.078***	-4.674***	-.031***
	(.014)	(.019)	(.016)	(.023)	(.026)	(.015)	(1.061)	(.009)
S vs. H	-.031*	-.024	-.028	-.029	-.001	-.028	-1.200	.006
	(.017)	(.023)	(.019)	(.029)	(.033)	(.018)	(.973)	(.012)
S vs. G	-.081***	-.075***	-.070***	-.071***	-.038	-.081***	-5.256***	-.051***
	(.013)	(.019)	(.014)	(.015)	(.023)	(.014)	(.787)	(.009)
S vs. V	-.103***	-.097***	-.114***	-.109***	-.104***	-.106***	-5.508***	-.050***
	(.016)	(.021)	(.020)	(.019)	(.031)	(.017)	(1.033)	(.011)
H vs. S	-.039*	-.032	-.077**	-.023	-.004	-.046**	-2.784**	-.007
	(.020)	(.026)	(.034)	(.036)	(.045)	(.023)	(1.225)	(.015)
H vs. G	-.082***	-.075**	-.040	-.044	.010	-.076**	-6.159***	-.057***
	(.026)	(.032)	(.044)	(.031)	(.059)	(.030)	(1.485)	(.020)
H vs. V	-.095***	-.088**	-.110**	-.076**	-.087	-.087***	-5.874***	-.047**
	(.030)	(.035)	(.049)	(.035)	(.067)	(.032)	(1.652)	(.022)
Correlation w/ baseline	1.00	0.99	0.96	0.97	0.79	0.99	0.97	0.96
Obs.	233,437	233,437	233,437	233,437	233,437	187,143	250,970	250,970

Notes: See notes to Table 4. Each row presents estimates for a specific first versus second choice combination using different RD specifications. E, N, B, S, H, G, N stand for Engineering, Natural Science, Business, Social Science, Humanities, General non-academic, and Vocational non-academic, respectively. The baseline estimates correspond to those reported in Table 3. Column two adds in quadratic terms in the running variable, while column three includes first–second choice specific intercept terms. The next two columns use the 12 slope model (one slope for each of the 5 first choices and the 7 second choices) and the 60 slope model (separate slopes to the left and right of the cutoff for each first–second choice combination. Column 6 excludes the years 1982-84; these years added GPA bonuses for the first and second choices on an individual's ranking list. The remaining columns use different earnings measures. Earnings in levels and earnings rank both include earnings below the threshold discussed in the text, and the final column uses an alternative measure of earnings which excludes publicly provided parental leave and sickness benefits. Standard errors in parentheses.

** p<0.10, ** p<0.05, *** p<0.01*

Table 6. Tests for comparative advantage.

Choice combinations	Sum of returns
Natural Science 1st – Business 2nd	.150***
and Business 1st – Natural Science 2nd	(.034)
Engineering 1st - Humanities 2nd	.132
and Humanities 1st – Engineering 2nd	(.143)
Engineering 1st – Natural Science 2nd	.097***
and Natural Science 1st – Engineering 2nd	(.032)
Natural Science 1st – Social Science 2nd	.070**
and Social Science 1st – Natural Science 2nd	(.035)
Engineering 1st – Business 2nd	.064**
and Business 1st – Engineering 2nd	(.029)
Natural Science 1st – Humanities 2nd	.049
and Humanities 1st – Natural Science 2nd	(.054)
Business 1st – Social Science 2nd	-.017
and Social Science 1st – Business 2nd	(.024)
Engineering 1st – Social Science 2nd	-.025
and Social Science 1st – Engineering 2nd	(.038)
Social Science 1st – Humanities 2nd	-.070**
and Humanities 1st – Social Science 2nd	(.029)
Business 1st – Humanities 2nd	-.103***
and Humanities 1st – Business 2nd	(.029)

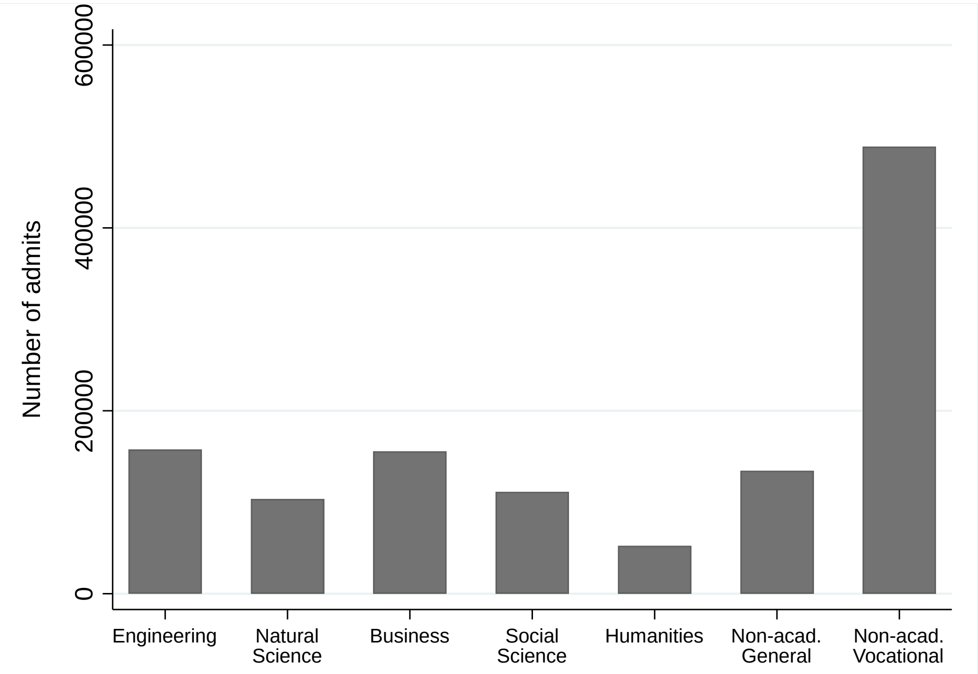
Notes: N = 233,437. See text for details on the tests. A positive sum is consistent with comparative advantage, a zero with random sorting, and a negative with comparative disadvantage. Standard errors in parentheses.
** p<0.10, ** p<0.05, *** p<0.01*

APPENDIX FIGURES AND TABLES

"Long-Run Returns to Field of Study in Secondary School"

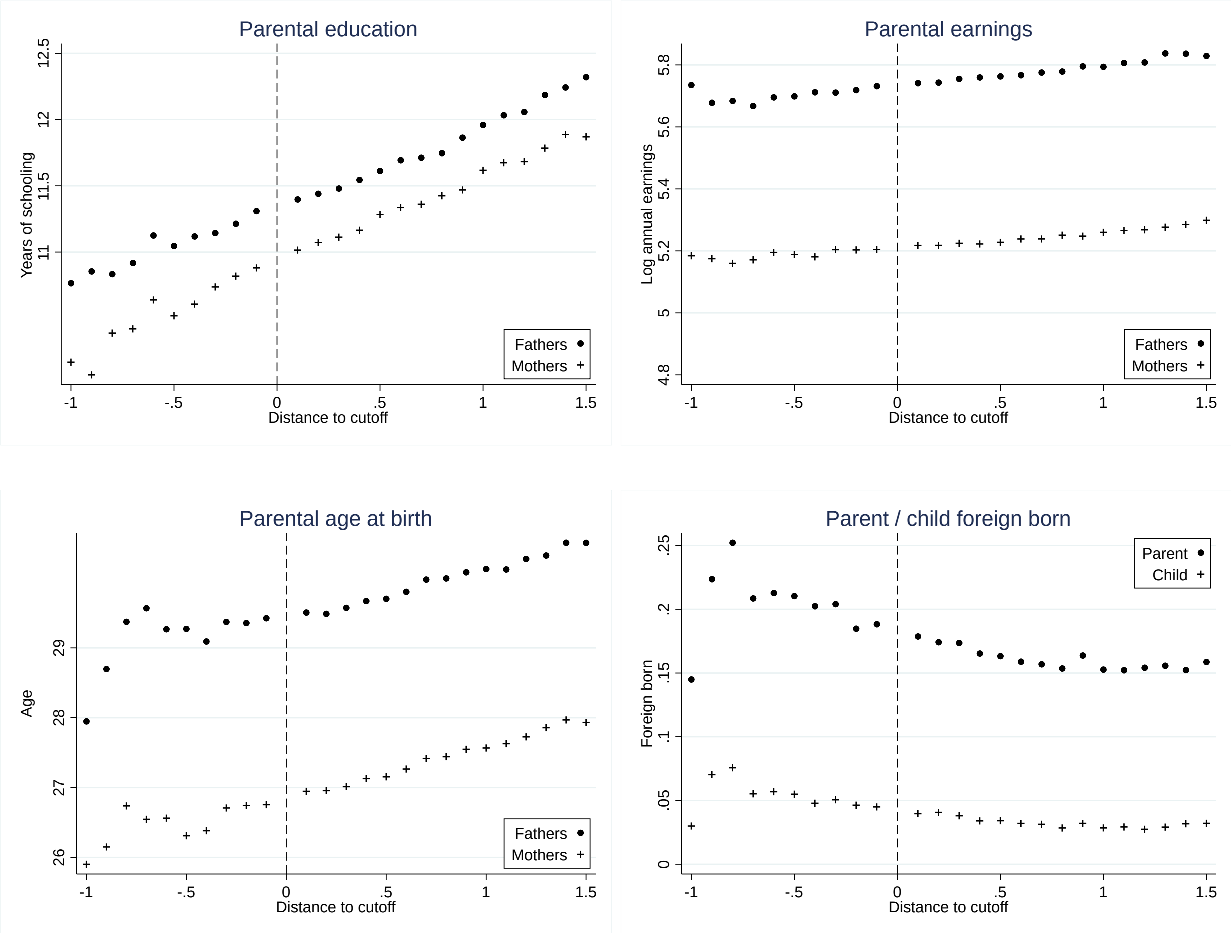
Gordon B. Dahl, Dan-Olof Rooth, and Anders Stenberg

Figure A1. Number of students admitted to each program.



Notes: Admission to secondary school programs between 1977-1991. N=1,208,269.

Figure A2: Smoothness of predetermined variables at the cutoff.



Notes: Each dot is the average for the relevant outcome in a 0.1 GPA bin, where GPA is measured relative to a normalized cutoff of 0. Parent foreign born is a dummy for whether at least one parent is foreign born. See Appendix Table A3.

Table A1. Competitive and noncompetitive program sample sizes.

	Baseline Sample:			
	Competitive programs		Non-competitive programs	
First choice	Individuals	Programs	Individuals	Programs
Engineering	63,466	793	52,171	1,079
Natural Science	18,982	395	50,583	1,457
Business	83,879	1,030	35,469	815
Social Science	53,139	873	32,120	970
Humanities	13,971	396	23,681	1,467
Total	233,437	3,487	194,024	5,788

Notes: Programs are defined by field of study, year, and school region. Non-competitive programs do not have an excess supply of applicants, and so have unrestricted entry.

Table A2: Summary statistics for applicants with a first-choice academic program.

Variables	Competitive programs	Share missing	Non-competitive programs	Share missing
Parent characteristics:				
Father age	29.74	0.05	29.99	0.07
Mother age	27.20	0.02	27.33	0.02
Father schooling	11.60	0.05	11.29	0.06
Mother schooling	11.23	0.02	10.82	0.02
Father earnings	5.76	0.18	5.75	0.20
Mother earnings	5.23	0.25	5.20	0.29
Foreign born parent	0.16	0	0.16	0
Child characteristics:				
Foreign born	0.03	0	0.03	0
Female	0.51	0	0.50	0
Age at application	15.99	0	15.99	0
GPA	3.86	0	3.94	0
Unadjusted GPA	3.74	0.02	3.81	0.03
Child outcomes:				
Log earnings	5.84	0	5.81	0
Observations	233,437		194,024	

Note: Years span 1977-1991. Parent and child characteristics are measured in the year of application (when the child is roughly 16 years old). Parent age refers to age at the time of the child's birth. Years of schooling inferred from highest education level. Earnings are measured between the ages of 37-39 and are converted to year 2016 US dollars using an exchange rate of 8.5 SEK to 1 USD. GPA is standardized to be mean 0 and variance 1 in the entire population, including those who do not apply to secondary school. Unadjusted GPA does not include any bonuses, e.g., the 0.30 bonus for being a minority gender applicant.

Table A3. Balancing tests for pre-determined characteristics.

Years schooling Father	Years schooling mother	Log earnings father	Log earnings mother	Age at birth father	Age at birth mother	Foreign born parent	Child foreign born
-.016	-.002	-.001	-.003	-.115	.013	-.005	-.003
(.058)	(.053)	(.010)	(.008)	(.133)	(.115)	(.008)	(.005)
249,860	259,892	214,708	197,345	249,613	259,336	264,324	264,324

Notes: Each column is an estimate from a separate RD regression which uses the 2 slope model; linear functions of the running variable of normalized GPA; a window of -1.0 to 1.5; triangular weights; fixed effects for year, school region, and program. There is a common jump for all first best – second best field choice combinations. Standard errors in parentheses.

** p<0.10, ** p<0.05, *** p<0.01*

Table A4. Reduced form estimates of program assignment on log earnings.

	Engineering	Natural Science	Business	Social Science	Humanities	Non-acad. General	Non-acad. Vocational
Engineering	--	.036***	.011	.026**	.040*	.009	.017**
		(.008)	(.010)	(.012)	(.020)	(.009)	(.007)
Natural Science	.017	--	.044***	.037***	.041*	.012	-.023
	(.014)		(.016)	(.014)	(.021)	(.025)	(.019)
Business	.033***	.068***	--	.035***	.000	-.004	-.008
	(.013)	(.011)		(.009)	(.010)	(.006)	(.007)
Social Science	-.066***	.001	-.046***	--	-.016*	-.050***	-.064***
	(.015)	(.010)	(.009)		(.009)	(.007)	(.009)
Humanities	.009	-.014	-.071***	-.028***	--	-.033***	-.039***
	(.024)	(.022)	(.012)	(.011)		(.012)	(.014)

Notes: N = 233,437. See notes to Table 4. Standard errors in parentheses.
** p<0.10, ** p<0.05, *** p<0.01*

Table A5. Heterogeneity by gender and parental education.

	Baseline	Males	Females	Low parental education	High parental education
E vs. N	.068***	.053***	.023	.064***	.069***
	(.016)	(.018)	(.022)	(.017)	(.017)
E vs. B	.019	.009	-.015	.010	.024
	(.018)	(.020)	(.023)	(.019)	(.018)
E vs. S	.061**	.051*	.036	.069**	.054**
	(.025)	(.026)	(.032)	(.028)	(.026)
E vs. H	.086**	.072	.054	.082	.089**
	(.038)	(.044)	(.054)	(.054)	(.041)
E vs. G	.019	.006	.002	.016	.023
	(.017)	(.019)	(.038)	(.019)	(.020)
E vs. V	.027*	.011	.005	.029*	.025
	(.014)	(.017)	(.029)	(.015)	(.015)
N vs. E	.029	.002	.039	.002	.040
	(.024)	(.026)	(.027)	(.027)	(.024)
N vs. B	.057**	.056*	.042	.028	.068**
	(.026)	(.032)	(.029)	(.035)	(.027)
N vs. S	.065**	.035	.073**	.045	.074***
	(.026)	(.031)	(.029)	(.030)	(.027)
N vs. H	.064*	-.016	.081**	.112**	.039
	(.035)	(.062)	(.036)	(.044)	(.040)
N vs. G	.026	.093	-.060	.091	-.068
	(.050)	(.072)	(.059)	(.060)	(.071)
N vs. V	-.038	-.085	-.020	-.017	-.046
	(.038)	(.053)	(.042)	(.048)	(.043)
B vs. E	.045**	.041*	.026	.019	.063***
	(.020)	(.023)	(.023)	(.022)	(.021)
B vs. N	.093***	.099***	.073***	.070***	.106***
	(.017)	(.020)	(.018)	(.018)	(.018)
B vs. S	.052***	.071***	.040**	.039**	.061***
	(.016)	(.019)	(.017)	(.016)	(.016)
B vs. H	.004	.009	.002	.005	.005
	(.017)	(.025)	(.018)	(.018)	(.018)
B vs. G	-.005	.022	-.022*	-.011	.003
	(.010)	(.015)	(.012)	(.010)	(.011)
B vs. V	-.011	-.008	-.016	-.016	-.003
	(.011)	(.015)	(.013)	(.012)	(.012)
S vs. E	-.086***	-.113***	-.075***	-.068**	-.092***
	(.025)	(.031)	(.028)	(.031)	(.027)
S vs. N	.005	-.015	.008	.005	.006
	(.017)	(.023)	(.018)	(.019)	(.018)
S vs. B	-.069***	-.092***	-.057***	-.074***	-.066***
	(.014)	(.019)	(.015)	(.015)	(.014)
S vs. H	-.031*	-.093***	-.020	-.021	-.036**
	(.017)	(.023)	(.018)	(.018)	(.017)
S vs. G	-.081***	-.110***	-.068***	-.075***	-.085***
	(.013)	(.020)	(.014)	(.014)	(.014)
S vs. V	-.103***	-.219***	-.075***	-.080***	-.120***
	(.016)	(.029)	(.017)	(.019)	(.017)
H vs. E	.046	-.073	.050	.024	.073
	(.135)	(.536)	(.111)	(.161)	(.174)
H vs. N	-.015	-.256**	.039	-.009	-.018
	(.037)	(.102)	(.036)	(.054)	(.042)
H vs. B	-.107***	-.184***	-.091***	-.107***	-.108***
	(.020)	(.042)	(.021)	(.022)	(.022)
H vs. S	-.039*	-.160***	-.019	-.027	-.049**
	(.020)	(.030)	(.021)	(.021)	(.021)
H vs. G	-.082***	-.137*	-.065**	-.031	-.130***
	(.026)	(.075)	(.027)	(.034)	(.028)
H vs. V	-.095***	-.390***	-.067**	-.120***	-.069**
	(.030)	(.147)	(.030)	(.035)	(.035)
Corr. btwn. groups		0.79		0.91	
Obs.	233,437	233,437		232,882	

*Notes: See notes to Table 4 and text for details. Standard errors in parentheses. Notes: N males = 114,036, females = 119,401, low-skilled parents = 95,995 and high-skilled parents = 136,887. * p<0.10, ** p<0.05, *** p<0.01*

Table A6. Probability of being included in the log earnings sample.

	Engineering	Natural Science	Business	Social Science	Humanities	Non-acad. General	Non-acad. Vocational
Engineering	--	.021*	.021	.027	-.018	.024**	.011
		(.012)	(.013)	(.019)	(.030)	(.012)	(.010)
Natural Science	.009	--	.002	-.014	-.024	.002	.023
	(.018)		(.020)	(.021)	(.030)	(.038)	(.031)
Business	.000	-.005	--	-.015	-.038**	.005	-.008
	(.014)	(.012)		(.013)	(.015)	(.008)	(.008)
Social Science	-.009	.015	.003	--	-.031**	.023**	.006
	(.017)	(.013)	(.010)		(.015)	(.011)	(.013)
Humanities	-.059	-.015	.007	.005	--	.034	.022
	(.096)	(.031)	(.018)	(.019)		(.026)	(.028)

Notes: N = 250,656. Sample includes all individuals with earnings, including zeros and low values. The dependent variable is equal to 1 if an observation is included in the log earnings sample. These are reduced form estimates. Standard errors in parentheses.
** p<0.10, ** p<0.05, *** p<0.01*

Table A7. Mechanisms: Years of schooling, postsecondary program, and occupation.

	Dependent variable: Baseline estimates			
Expected return due to:				
Years of schooling	.511*	-	-	0.063
	(.290)			(.086)
Postsecondary program	-	.953***	-	.310***
		(.144)		(.084)
Occupation	-	-	1.427***	1.168***
			(.088)	(.089)
R^2	0.100	0.611	0.904	0.951

Notes: We regress the thirty baseline estimates from Table 4 on thirty estimates of the expected returns due to three different mechanisms, which are also estimated using our baseline RD model. See text for details. The regression is weighted by the inverse of the squared standard error for the baseline model estimates. Standard errors in parentheses.
** p<0.10, ** p<0.05, *** p<0.01*