High School Majors, Comparative (Dis)Advantage, and Future Earnings

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Abstract: This paper studies whether specialized academic fields of study in secondary school (i.e., high school majors), which are common in many countries, affect earnings as an adult. Identification is challenging, because it requires not just quasi-random variation into majors, but also an accounting of individuals' next-best alternatives. Our setting is Sweden, where at the end of ninth grade students rank fields of study and admission 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 several key results. First, Engineering, Natural Science, and Business yield higher earnings relative to most second-best choices, while Social Science and Humanities result in sizable drops, even relative to non-academic vocational programs. The magnitudes are often as large as the return to two years of additional education. Second, the return to completing a major varies substantially as a function of a student's next-best alternative. Third, the pattern of returns for individuals with different first and second best choices is consistent with comparative advantage for many field choice combinations, while others exhibit comparative disadvantage or random sorting. Fourth, most of the differences in adult earnings can be attributed to differences in occupation, and to a lesser extent, college major. Taken together, these results highlight that the high school majors students choose at age 16, when they have limited information about their skills and the labor market, have sizable effects which persist into adulthood.

Keywords: Field of study, secondary education, comparative advantage

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1 Introduction

Many countries throughout the world, including much of Europe, Latin America, and Asia, require specialization in secondary school, with students choosing specific fields of study at age 15 or 16 which prepare them for college and direct entry into the workplace.[1] Understanding whether there are long-run labor market returns to early field specializations (i.e., "high school majors") is of central importance for education policy and models of human capital accumulation.[2] On the supply side, years of schooling have been highlighted as a key determinant of a nation's growth rate (Krueger and Lindahl 2001; Hanushek et al. 2008). Schooling majors could play an equally important role, with returns providing useful guidance on how to allocate resources across fields. On the demand side, students may be making decisions with little information, and providing guidance on long-run wage premiums could help them better plan for their future. A related issue is whether students recognize their own comparative advantage when choosing one major over another at such a young age.

Despite its importance, evidence on the returns to different academic majors in high school remains scarce, and is limited to observational studies (for a summary, see Altonji et al. 2012; Altonji et al. 2016). One challenge is that students endogeneously sort into majors. The problem is compounded by the fact that students have different next-best alternatives, which makes the counterfactual outcome different for individuals completing the same major. In such a setting, identification of meaningful parameters requires not just quasi-random variation into majors, but also an accounting of individuals' next-best choices (Kirkeboen et al. 2016). On top of these identification challenges, the data requirements are formidable. One needs information on each individual's preferred and next-best alternative choices and which major they were admitted to. To examine long-run impacts, one also needs to follow individuals several decades later and observe their earnings.

We overcome these challenges in the context of Sweden's secondary school system. We use a regression discontinuity design (RD) to compare individuals just above versus just below GPA admission cutoffs for different majors. We can account for different preferred and

[1] Countries requiring students to choose fields in secondary school in Europe include the Czech Republic, Denmark, France, Italy, Norway, Poland, Spain, Sweden, and the United Kingdom; in Latin America include Argentina, Chile, Colombia, Cuba, Mexico, Paraguay, and Venezuela; and in Asia include Indonesia, Iran, Malaysia, Pakistan, the Philippines, and Saudi Arabia.

[2] We use the terms high school/secondary school and the terms major/field/program interchangeably.

next-best alternatives because we were able to gain access to the field rankings, admission decisions, and completed majors for all students between the years 1977-1991. Using personal identification numbers, we link this data to labor market outcomes more than two decades later, when individuals are in the prime of their working careers.

During the time period of our study, students choose between five academic majors which take at least three years to complete: Engineering, Natural Science, Business, Social Science, and Humanities. In addition to these majors, which comprise the focus of our paper and roughly half of applicants, there are also non-academic two year programs. We focus on gaining admission to academic programs since admission into non-academic programs was most often not limited. We can, however, use our research design to study non-academic programs as possible second-best choices.

At the end of ninth grade, students rank their preferred majors, and admission to oversubscribed majors is determined by the student's cumulative ninth grade GPA.[3] Admission decisions are made 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 students on the margin of admission. We allow for separate jumps at the major-specific GPA cutoffs for each combination of preferred and next-best fields. For example, the payoff to Engineering is estimated separately for those with a second-best choice of Natural Science versus Business. We use the sharp jumps in admission at the GPA cutoffs as instruments for completing a specific major in a fuzzy RD design. We also estimate sharp RDs for the policy-relevant question of the return to being admitted to a major.

Our empirical analysis reveals that the high school major choices made early in life have long-lasting effects on earnings. The pattern of major-specific returns provides insights on (i) the returns to different academic majors, (ii) the role of next-best choices, (iii) the benefits of academic versus vocational majors, (iv) comparative advantage and disadvantage, and (v) mechanisms related to future college major and occupation.

Our first empirical finding is that the earnings returns to completing different academic

[3] There is not a simple correspondence between oversubscription, average GPA, and future earnings. Business and Engineering top the list for the most oversubscribed majors, while Natural Science and Humanities are the least likely to be oversubscribed. Students in Natural Science have the highest GPAs and those in Business the lowest, while earnings are highest for Engineering and lowest for Humanities.

majors are often sizable, and can be both positive and negative. For example, the returns to Engineering range from 0.7% to 7.0%, depending on an individual's next-best alternative field of study, while the returns to Social Science range from -9.4% to 1.6%. 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, even when compared to next-best non-academic programs where the earnings losses exceed 7%.

Second, earnings payoffs vary substantially based on next-best alternatives. For example, there is a 9.1% return to completing Business relative to a second-best choice of Natural Science, but essentially no return to completing Business (-0.8%) for those who have Humanities as their next-best alternative. Formal tests reject the null hypothesis that second-best choices do not matter for each set of major-specific returns. The estimates are robust to alternative RD parameterizations, earnings measures which include zeros, and corrections for multiple inference.

Third, we find evidence that academic majors are not better than non-academic majors for marginal students. The estimated return to completing a 3-year academic program when the next best alternative is a 2-year non-academic program is either close to zero or negative. One possible explanation is that marginal students are choosing or being encouraged to pursue an academic major based on average earnings differences, when in fact they will benefit less given their backgrounds.

Fourth, the pattern of returns is consistent with individuals pursuing comparative advantage in expected earnings for many field choice combinations, but not all. For example, individuals who complete Natural Science with a second-best choice of Business earn a 5.6% premium, while those who complete Business with a second-best choice of Natural Science earn a 9.1% premium. Random sorting would have predicted the two estimates were equal in magnitude, but opposite in sign. Five field choice combinations show evidence for comparative advantage, two for comparative disadvantage, and three for random sorting.[4] Comparative advantage is more common when first and second best choices include Engineering, Business, or Natural Science, while comparative disadvantage occurs with Humanities.

Fifth, most of the differences in adult earnings across high school majors can be explained

[4]Comparative disadvantage can be explained by students either having poor information or taking into account non-monetary returns.

by differences in occupation and, to a lesser extent, in college majors. For example, our evidence indicates that individuals who complete Business instead of Social Studies in high school earn more as adults in part because they pursue higher-paying college majors (e.g., accounting versus psychology) and even more so because they end up in higher paying occupations (e.g., sales manager versus social worker). These two mechanisms appear to be in play simultaneously, with occupation being roughly three times as influential as college major. In contrast, years of schooling is not an explanation once these other two mechanisms are accounted for.

Methodologically, our study is related to designs which use score-based admissions thresholds to study the returns to institution and college major choice. Hastings et al. (2013) uses data from Chile and a RD design to estimate the intention-to-treat effects of being admitted to a degree program (defined by the combination of a given university and major) on long-term labor market outcomes. Subsequent 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 parameter without accounting for next-best alternatives. Using data for Norway, they study the effect of degree program completion (again defined by a given university and major) on short-run earnings using IV. Finally, Andrews et al. (2017) studies the impact of switching to a business major in college using data from Texas and a RD design.[5] These papers find large earnings differences for different college major choices.

Our paper contributes to this nascent literature by providing the first causal estimates of the returns to academic majors in high school. The high school and college margins are conceptually distinct, and each important in their own right. More students go to high school than attend college; in our sample, even among those who pursue an academic degree in high school, less than half continue on to college. We show that early field specialization has long-lasting wage effects, with evidence that many individuals recognize their comparative advantage even at the relatively young age of 16. Moreover, we find the returns to different high school majors is not primarily due to the pursuit of different college degrees, but rather individuals ending up in higher or lower paying occupations.

Our setting additionally allows us to explore the benefits of choosing an academic major over a nonacademic or vocational track. Another key distinction is that our setting does not

[5]Other work has adopted more structural approaches; see, for example, Arcidiacono (2004).

have a systematic ordering where some majors always require higher GPAs for admission, either within or across school regions (see Section 2.4). In contrast, college major returns are likely to in part reflect match effects based on a general ordering of which majors and universities consistently have higher admission cutoffs. Finally, our setting is simpler in that students are choosing majors only, and not making the combined choice of a college major plus institution choice.

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, 2016; Levine and Zimmerman 1995; Rose and Betts 2004), ability tracking in elementary and secondary school (Argys et al. 1996; Card and Giuliano 2016; Dustmann et al. 2017; Pekkarinen et al. 2009), and general versus vocational training (Bertrand et al. 2019; Brunello and Rocco 2017; Golsteyn and Stenberg 2017; Hall 2012; Hanushek et al. 2017; Malamud and Pop-Eleches 2010).

Our research design rules out the possibility that the major-specific returns we estimate simply reflect a sorting of higher-ability individuals into higher-paying majors. The findings speak to the question of whether high school majors primarily capture sheepskin effects (Spence 1973) versus human capital accumulation (Becker 1964; Mincer 1974). The estimates are inconsistent with degree-signaling effects as the dominant explanation, as individuals with the same major but different second-best choices experience different earnings returns. Moreover, comparative advantage and disadvantage argue against a common ranking of majors, and in favor of a generalized Roy model (which includes non-monetary gains) and specific human capital accumulation.

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). Hence, productivity differences across high school majors have the potential to nontrivially impact both individual earnings and national GDP growth. While we cannot directly evaluate whether the benefits associated with this type of secondary education system exceed the costs, the long-lasting labor market effects we estimate are an important consideration. Individuals make these field choices at the relatively young age of 16, when preferences are in flux and they are still learning about their abilities. From a purely fiscal policy standpoint, our results argue for an expansion of Business and the two STEM fields (Engineering and Natural Science), and a contraction of Social Science and Humanities, although we recognize that non-pecuniary factors are also an important consideration.[6]

The remainder of the paper proceeds as follows. The next section describes Sweden's secondary education system, the admission process, and our unique data. Section 3 discusses identification using preferred and next-best choices in a 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. The final section concludes.

2 Setting and Data

2.1 High School Majors in Sweden

The Swedish educational system requires nine years of compulsory schooling, after which individuals can apply to a high school major.[7] During the years we study (1977-1991), there were five academic majors to choose from: Engineering, Natural Science, Business, Social Science, and Humanities. These academic programs took three years to complete, with the exception of Engineering, which had the option of a fourth year of more technology-oriented courses. The five academic majors are preparatory for future studies at the university level, as well as preparatory for direct entry into the labor market. Approximately half of students with an academic major continue on to college.

As shown in Table 1, there are substantial curriculum differences across the academic majors. 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 fourth year of Engineering further adds 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

[6]Our design estimates returns for students on the margin of admission, rather than the general population. This is a relevant group from a policy perspective, as reforms which expand or contract different fields target exactly these individuals.

[7]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.

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.[8]

Figure 1 provides an initial look at how GPAs and earnings vary by completed major for all individuals. There is not a simple correspondence between majors with higher average GPAs and higher 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.

In addition to these five academic programs, 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. Students in the nonacademic programs take a completely separate curriculum and are in a completely separate set of classes.[9] Appendix Figure A1 displays the number of students admitted to each of the five academic majors plus the two aggregated non-academic programs. Roughly half of the students are admitted to an academic major, with Engineering and Business being the most popular. The focus of this paper is on the academic majors; the reason is that admission into most of the non-academic programs was not limited, and so we cannot use the research design we describe in Section 3 for non-academic preferred choices.

We focus on the period 1977-1991 because the academic and non-academic programs remained stable 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 major, non-academic vocational programs were lengthened to three years, and non-academic general programs were abolished. 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

[8]While we focus on differences in curriculum, it is also possible that different majors expose individuals to a different set of peers or a different set of teachers, both of which could also influence future earnings (e.g., Sacerdote 2011; Chetty et al. 2014).

[9]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).

to the same major offered by different schools, or in other municipalities, and substantially reduced the number of oversubscribed programs. In 2011, Business, Social Sciences, and Humanities re-emerged as separate majors in some, but not all, regions.

2.2 Admission Process

Students apply to be admitted to a high school major. During our sample period (1977-1991), individuals were only allowed to apply for majors in their region of residence unless a field was not offered in their home region. Depending on the year, there are between 115 and 137 high school regions, with a median number of 927 applicants per year and school region.

Slots are allocated based on application GPA if a major is oversubscribed. 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 or pursue a non-academic program). Applicants received a bonus of 0.2 to their GPA for being a minority gender applicant, defined as applying to a major which in the prior year had accepted less than 30% of their gender nationally (e.g., females applying to Engineering). This bonus means that some individuals can have an adjusted GPA above 5. 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 majors. The forms are sent to a central administration office which then allocates students to majors based on their preference rankings and GPA. Admission decisions are made sequentially, with the highest-GPA applicant being admitted to their first-choice major, the second-highest GPA applicant being admitted to their highest-ranked major among the set of majors 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.[10]

[10] In theory, it is possible that only allowing 6 choices causes individuals to put a safe option down as their 6th choice, so as to make sure they get into at least one program. 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

The determining factor for whether a specific major will be oversubscribed has to do with the lumpiness of class sizes. Classes, and therefore majors, are often capped at multiples of 30 students. If there is only one class for a given major and 33 students list the major as their first choice, it will be impacted. In contrast, if only 27 students list it as their first choice, everyone will be admitted. Depending on expected demand for a major, there could be two or even three classes for a given major. Because of natural variation in demand, a major may be oversubscribed in one region, but not another. Moreover, a major may be oversubscribed in a given school region in one year, but not the next.

In our setting, it is important not to confuse "oversubscription" with "highly competitive." There is not a universal or persistent ordering in which majors have higher cutoffs or are more likely to be oversubscribed, either across or within school regions. Moreover, average cutoffs (conditional on having a cutoff) are broadly similar across majors. After we introduce our data, we will empirically document the variation in relative cutoffs within the same school region over time in Section 2.4.

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 major, 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 high 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 a RD design to instrument for program completion. We can also use the sharp cutoff in admission decisions to estimate the effect of admission itself.

years 1982-84, individuals were given 0.5 and 0.2 bonus GPA points, respectively, for the first and second choices on their ranking lists. So for these years, individuals may have not revealed their true preferences. In a robustness check we exclude these years, and the estimates hardly change (see Section 4.3).

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 high school majors. We observe all of the field choices submitted by a student. This is important, because it allows us not only to observe which major 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 was used in a government report from 1992 but had been reported as lost by the Swedish National Archives, and was only miraculously saved by Hans Eric Ohlson at Statistics Sweden in response to a request we made.[11]

During our sample period, the number of applications to high school increased. In 1977 only 60% of the ninth-grade cohort applied to high 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. Roughly half of applicants have an academic first choice (611,837 observations), of which 326,211 apply to an oversubscribed major. Our sample is further limited to individuals who list a next-best alternative, are still observed in the administrative registers at age 38, and have an observed GPA within a sample window of -1.0 to +1.5 points around the cutoff, leaving us with 250,522 observations.[12] Our baseline sample is comprised of the 233,034 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 lowest-GPA

[11] In an unrelated conversation about a different dataset, we told Ohlson about our frustration with the data being lost. It turns out that Ohlson was the person responsible for delivering the data over 25 years ago from the archives, and that he kept meticulous records. With his help and intervention, we were able to secure the data just weeks before his retirement.

[12] 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. We also drop observations where GPA is outside the range of 2.0 to 5.0, as few individuals are found in these regions. For 1982-84 we use a GPA range of 2.5-5.5 (since those years had extra bonus points for first and second-best choices; see footnote 10).

choice above their accepted choice, and the next-best alternative as their accepted choice.[13] This gives us information on both preferred and next-best majors, and a quasi random source of variation for each combination of majors for individuals near the admission thresholds. For ease of exposition, we will refer to the preferred major 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 major 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 Appendix Table A1. Some combinations have many observations, such as a first choice of Engineering and a second choice of Natural Sciences (N=31,910) or a first choice of Business and a second choice of Social Science (N=29,850). The most sparsely populated combinations are those which include a STEM field and Humanities. As Table 2 documents, the observations are spread across almost 3,500 oversubscribed major programs in different years and school regions. That same table also details how many individuals list a non-impacted academic major (i.e., a major which admitted all applicants) as their first choice. Forty-five percent of individuals have a first choice academic major which is non-impacted. Each field has a sizable mix of oversubscribed versus non-impacted programs, as shown in Table 2. The fraction of programs which are oversubscribed by major are 42% (Engineering), 21% (Natural Science), 56% (Business), 47% (Social Science), and 21% (Humanities).

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.[14] 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).[15] This restriction excludes

[13] An alternative definition for those admitted to a third or lower ranked choice is to define their preferred choice as the one immediately above their accepted choice on their ranking list, even if it is not the lowest-GPA choice above their accepted choice. Using this alternative does not materially affect any of our results.

[14] 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.

[15] Antelius and Björklund use a SEK 100,000 threshold, which translates into roughly $12,000. We apply

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.

The register data also includes information on socioeconomic background characteristics. Summary statistics for these pre-determined parent and child characteristics are found in Appendix Table A2, broken down by whether a major was oversubscribed or non-impacted. The means for both parental and child characteristics across the two samples are broadly similar. Appendix Figure A2 further shows that the GPA and log earnings distributions for oversubscribed and non-impacted majors are quite similar. The small differences are due to the mix of majors which have a higher or lower probability of being oversubscribed. For example, Engineering is more likely to be oversubscribed compared to Natural Science, and while earnings are higher in Engineering, grades are higher in Natural Science. We conclude that the set of majors which are oversubscribed in a given year and location are only modestly different from those which are non-impacted.

2.4 Determining GPA Cutoffs

We observe the choice rankings for each individual and the associated admission decision, but 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 major has the potential to be a competition for slots. We refer to these as "cells." Our empirical design only applies to oversubscribed 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.[16]

their threshold, accounting for wage growth and inflation, to other other years in our sample.

[16]We allow for a small amount of noise in the data due to measurement error, which is possible during this time period since most variables were transcribed and entered by hand. For example, if one observation with a GPA of 3.8 is recorded as not admitted while all of the remaining observations higher than 3.3 are recorded

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 (as a reminder, GPA is only recorded to the first decimal). 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 of 3.3 or below is not admitted and everyone with a GPA of 3.4 or above 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 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 major are found in the skill distribution. The mean cutoff GPA of 3.44 corresponds to the 18th percentile of GPAs in our baseline sample of students applying to oversubscribed academic majors. For further context, a GPA of 3.44 corresponds to roughly the 60th percentile of GPAs in the sample of all ninth graders, including those who do not apply to high school. The cutoffs are therefore generally binding only for applicants with GPAs in the bottom half of our estimation sample. While not shown in the figure, the average cutoffs are fairly similar across the different academic majors, with mean cutoffs differing across majors by less than 0.2 GPA points, a small amount relative to the distribution of students' GPAs.[17]

as admitted, it is likely that either GPA or major was erroneously recorded. Our rule is to retain the cell if the "miscoded" observations represent less than ten percent of the observations at the given side of the cutoff. If the condition is met, we retain the cell, but drop the "miscoded" observations. This procedure drops just 0.3 percent of the data, and the observations which are dropped are evenly spread across GPA, consistent with the measurement error not being systematic. We also require there be at least 25 applicants and 3 observations to the left of the cutoff.

[17]We further note that most individuals have GPAs above the admissions threshold for their second-best

There is not a universal ordering of which majors are more likely to have higher admission cutoffs. For example, Engineering has a higher cutoff than Natural Science in 37% of years within the same school region on average, while the reverse is true in 25% of years. In 38% of years both programs either have open enrollment, or less commonly, identical cutoffs. Similar patterns are found for the other major combinations as reported in Appendix Table A3.[18]

These facts regarding the major cutoffs are useful to to keep in mind when interpreting the estimates, which will capture local average treatment effects for applicants around the cutoffs. Given the nature of our cutoff variation, these marginal students have roughly similar GPAs, regardless of which major is their first-best choice.

3 Identification

3.1 Using Preferred and Next-Best Choices in a 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 majors. 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 majors and because individuals choosing the same preferred major can differ in their next-best majors. 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 major in a fuzzy RD design will eliminate selection bias, but without further assumptions, it will not estimate the return to any individual or group who choose one major over another.[19] One possibility is to impose "constant effects," where the returns

choice. The fraction of students with GPAs above the cutoff for their second-best choice, by first-choice major, are 95% (Engineering), 97% (Natural Science), 92% (Business), 96% (Social Science), and 90% (Humanities).

[18] Appendix Figure A3 graphs the entire distribution of the within school region variation over time in relative major cutoffs.

[19] 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

to completing a major 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.[20]

When next-best alternatives are available, however, RD can estimate the local average treatment effect (LATE) for each 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 major like Social Science either.

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

3.2 Regression Discontinuity Model

To estimate the returns to different majors, we exploit the discontinuity in admission decisions to different majors 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 major j and next-best alternative k, y_{jk}, can be modeled in a RD framework as follows:

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

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."

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

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 major 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 major 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 major 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 potentially 30 functions to the left of the cutoff and 30 functions to the right of the cutoff. Estimating 60 unknown functions is data demanding, so for efficiency, we impose some parametric functional forms. At the same time, we point out that we are at least as flexible as existing specifications in the literature, which sometimes do not account for second-best choices at all or use IV instead of RD.

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}.

While the reduced form coefficients are interesting in their own right (the returns to major admission), we are also interested in the returns to major 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 fuzzy RD 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. We note that our estimates will capture the LATE for individuals on the margin of admission to one field versus another, rather than the average treatment effect for the entire population.

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 major. 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 since there are a fixed number of slots for each major. Figure 3 illustrates the year-to-year variation in admission thresholds. It plots the distribution of first differences in admission cutoffs for majors in a school region. While the distribution is centered at 0, there is substantial variation. Indeed, for major 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 A4 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 A4. 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.[21]

Exclusion, Monotonicity, and Irrelevance. With no manipulation, the RD design identifies the causal effects of admission to a major (i.e., the reduced form effect). To identify the causal effects of completing a major, we additionally need monotonicity, exclusion restrictions, and irrelevance. The monotonicity assumption requires that crossing an admissions threshold does not make an individual less likely to complete that major. This assumption of no defiers seems likely to hold in our setting.

The exclusion restrictions require that crossing the admissions threshold for a major only affects outcomes through major completion. It is possible that being admitted to a major

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

could have a direct impact on earnings if a person takes several specialized major classes before switching to another major. This is not a primary concern in our setting since most switching takes place in the early fall of the first year of high 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 majors that have 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 major.[22]

There is also the possibility that admission to a major alters the chances an individual drops out of school entirely. Since we are looking at a positively selected set of individuals applying to the academic track, this is not a common occurrence (5% of students). Similarly, only a small fraction applying to the academic track switch to the nonacademic track (5%). We do find small effects of getting into a first-best choice on dropping out or switching to the non-academic track, but which are not large enough to have a sizable impact on our estimates.[23] When we re-run our analysis excluding those who drop out or switch to the non-academic track, none of the resulting estimates are statistically different from the baseline.

Finally, we require the irrelevance condition discussed in Section 3.1. While this condition seems plausible in our setting, it is possible that it does not hold for *completion* of a major. In contrast, we note that the irrelevance condition holds by construction for *admission* to a major. This is because we have a sharp discontinuity for admissions, where everybody above the GPA cutoff is admitted to the major and everybody below the cutoff is not. It is only when we use program completion to scale our reduced form estimates using a fuzzy RD that this issue arises. It is reassuring that the correlation between the reduced form and fuzzy RD estimates is 0.98.

[22] About 9% of individuals switch from the major they are initially admitted to and complete another major. Switching rates vary somewhat by major: 11% (Engineering), 13% (Natural Science), 6% (Business), 9% (Social Science), and 15% (Humanities). Using our data, we can ascertain that roughly half of this switching occurs before the school year has started in earnest (we have field enrollment data collected 2-3 weeks into the start of the school year). We believe most of the remaining switching happens before the first semester is over in December, although our data does not allow us to verify this. Moreover, we note that even if an individual finishes the first semester in one major and then switches to another, there will be no record of this, as in Sweden only the final grades in one's completed major appear on the transcript during our time period.

[23] Using our baseline specification, we find a 0.7 percentage point increase (s.e.=.3) in dropping out of high school and a 2.8 percentage point decrease (s.e.=.6) in the probability of switching to the non-academic track.

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 before turning to a variety of robustness checks.

4.1 First Stage

As a reminder, we have a sharp discontinuity for admissions, where everybody above the GPA cutoff is admitted to the program and everybody below the cutoff is not. This is illustrated for the entire sample in Figure 4. We use program completion to scale our reduced form estimates using a fuzzy RD.

We begin by documenting the relationship between admission and major 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 5 plots the probability of completing the Engineering major in normalized GPA bins. Everyone to the right of the vertical line is (initially) admitted to the major, while everyone to the left is not (initially) admitted. Completion of the major is not 100% to the right of the cutoff, because some people switch and complete other majors. 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 major 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 major, 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 major completion (which is not random) with major admission (which is quasi-random near the cutoff).

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 35%, 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. 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 as our baseline is for precision in the reduced form and second stage; we will report results for the 12- and 60-slope models in a robustness check.

4.2 High School Major and Future Earnings

We now turn to estimates of the earnings return to different majors, which are allowed to be relative to each second-best choice. We first illustrate the idea graphically with an example, and then turn to our regression based estimates for all possible first-second best combinations.

The bottom panel of Figure 5 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 (except for the leftmost dot which is a 0.5 bin due to sparsity), where earnings are measured between the ages of 37-39, as explained in Section 2.3. There are positive slopes both to the right and the left of the cutoff, indicating that higher GPAs result in higher earnings. There is also a large jump at the cutoff of roughly 0.06 log points.

We chose to illustrate identification using the Engineering first-choice and Natural Science second-choice margin because there are many applicants with this combination. 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. The sharp RD reduced form estimates for field admission can be found in Table 4. The fuzzy RD estimates for field completion, which are estimated via two stage least squares, are reported in Table 5 and illustrated in Figure 6.

Since the reduced form and fuzzy RD estimates show similar patterns, we focus on the latter. All of the estimates appearing in Table 5 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 fuzzy RD estimate for the same margin shown in Figure 5. The estimate of 0.064 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 6.4% 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, Devereux, Lundborg, and Majlesi 2018).[24]

There are three key takeaways from this table and the corresponding graphs in Figure 6. First, the returns to different academic majors, 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 0.7% to 7.0%. In contrast, 10 out of 12 estimates for the returns to Social Science and Humanities are negative. This decrease shows up even when the next-best choice is non-academic: the return to completing Social Science or Humanities when the next-best alternative is a non-academic program exceeds -7%.

Second, returns to different fields depend on next-best choices. For example, there is a 9.1% 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. This illustrates the importance of accounting for selection as a function of second best choices, and indicates that

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

returns are not uniform across student types. It also provides evidence against sheepskin effects being the dominant force, as future employers are likely to observe an individual's completed degree, but not their second-best choice.

Third, the estimated returns to completing a 3-year academic program when the next best alternative is a 2-year non-academic program are either close to zero or negative. One possible explanation for this pattern is that marginal students are choosing or being encouraged to pursue an academic major based on average earnings differences. Indeed, Figure 1 shows that earnings are higher on average in every academic major compared to non-academic majors (except for Humanities, where they are roughly equal). But it may be that these marginal students benefit less from pursing an academic major given their academic background. In terms of GPA, these marginal students will by construction be among the worst if they are accepted to their first-best academic major but, as Figure 1 implies, among the best students in their second-best non-academic major.

We examine whether second-best choices matter more formally by testing whether the fuzzy RD estimates for each first-choice major (i.e., each row in the table) are jointly equal to each other. For example, for Engineering the 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 major. The resulting F-statistics and p-values are reported in the third to last column of Table 5. For each of the majors, we reject that next-best alternatives do not matter at standard levels of significance.

In the second to last column of Table 5, we test whether there is significant variation in returns across second-best academic choices (ignoring the non-academic choices). For example, for Engineering the test is $\hat{\pi}_{EN} = \hat{\pi}_{EB} = \hat{\pi}_{ES} = \hat{\pi}_{EH}$. For Engineering, Business, Social Science, and Humanities formal tests reject equality of returns. Only for Natural Science are second-best academic choices not important.

In the last column of Table 5 we provide a formal test for whether there are average differences in returns for academically inclined students versus non-academically inclined students, where the two groups are defined by having an academic versus non-academic second-best choice. For example, for Engineering the test is $(\hat{\pi}_{EN}+\hat{\pi}_{EB}+\hat{\pi}_{ES}+\hat{\pi}_{E})/4 = (\hat{\pi}_{EG}+\hat{\pi}_{EV})/2$. For each of the five academic first-choice majors, we reject that the average difference is the same for academic and non-academic second-choices.

Appendix Table A5 reports earnings returns 10 years earlier, when individuals are age 27-29 and still on the upward-sloping portion of their age-earnings profile. To enable easier comparisons of coefficients, and to fit more results into a single table, we present estimates for the different specifications in tabular form. The returns to Engineering, Natural Science, and Business, which are generally positive at age 37-39, are smaller and sometimes even negative at age 27-29. In contrast, the returns to Social Science and Humanities, which generally are negative at age 37-39, are less negative at age 27-29. We view the age 37-39 estimates as a better measure of labor market returns, as they reflect earnings during the prime of an individual's working career.

Appendix Table A5 also 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 apply 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 to completing one field over another are broadly similar, but not identical, for males and females. One interesting pattern is that 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 similar to each other.

4.3 Specification Checks

In this section we provide a variety of robustness checks. These appear in Table 6, 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.[25]

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 reduce the bandwidth to be +/- 0.75 around the cutoff, which has little effect on

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

most estimates. We then 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. All three 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 7. The top figure plots averages of log annual earnings in GPA 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 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.[26] 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.[27] This is confirmed in the estimates for the 12-slope model, in column 5 of Table 6. Note that Figure 7 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 6. 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 8. Most of the dots are clustered around the 45 degree line in the figure. If anything,

[26]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.

[27]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.

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 0.76. 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 7.[28]

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 RD regression. Appendix Table A6 presents these estimates. Out of 30 estimates, four are significant at 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.

Turning back to Table 6, our first alternative earnings measure uses earnings in levels as the outcome, including low earnings and zeros. These results appear in column 8. 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

[28]While not shown in the table, we also tried including a proxy measure of average class size (number of students divided by 30) as an additional regressor. This has virtually no effect on the estimates.

increase of \$4,565 per year. Since the average earnings for this group is \$54,668, this translates into an 8.4% increase in average earnings. This compares to the estimate of a 6.4% increase in earnings for the baseline log specification in column 1.

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 6. Roughly the same number of estimates are statistically significant using this measure compared to our log earnings measure.[29]

The 30 estimates of Tables 4 and 5 are further robust to multiple inference adjustments using the False Discovery Rate (FDR) control (see, e.g., Anderson 2008). For the reduced form results in Table 4, 15 out of 17 estimates remain statistically significant, and for the fuzzy RD results in Table 5, 15 out of 17 estimates likewise remain significant (see Appendix Table A7).

The conclusion from the battery of tests reported in this subsection is that our results are robust to the parametric form of the RD regression, how we measure earnings, and multiple hypothesis testing.

4.4 Comparisons to OLS and KLM

To highlight the two problems of endogeneity and unknown counterfactuals, and therefore the benefits of instrumenting and controlling for second best choices, we compare our RD estimates to OLS. Appendix Table A8 reports OLS estimates which do not take into account an individual's next best choice.[30] We first estimate a model which also does not include a student's GPA, as that information is often not observed in a dataset. The estimates differ markedly compared to our baseline RD estimates, with 23 out of 30 comparisons being statistically different at the 10% level.

[29]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 baseline.

[30]For the OLS estimates, we regress log earnings on dummy variables for completing each of the possible majors, using the same set of school region fixed effects, year fixed effects, and demographic controls as in our baseline specification. We do not include any information on choice sets or admissions. Using different combinations of the estimated coefficients for completed majors, we can calculate the returns for each of the 30 pairs of majors.

One might naturally wonder if controlling for GPA in the OLS specification would eliminate some of these differences, as GPA is a proxy for ability. However, even with this addition, OLS yields substantially different estimates compared to the baseline RD estimates, with 23 out of 30 comparisons being statistically different in Appendix Table A8. One contributing factor for these discrepancies is that by ignoring second-best choices, OLS forces the relative returns between two majors to be symmetric but opposite in sign. For example, the OLS estimate for the return to Engineering relative to Natural Science is 4.1% and the return to Natural Science relative to Engineering is -4.1%. In contrast, our RD estimates are positive for both of these margins. In summary, OLS yields misleading and incorrect conclusions.

Our paper leverages KLM's methodology to account for second-best choices. We have more data and therefore the ability to implement a fuzzy RD design (allowing different slopes in the running variable of GPA on each side of the cutoff and using triangular weights) instead of their more parsimonious IV (including GPA as a single control variable). In the last column of Appendix Table A8, we explore what happens if we use the IV specification of KLM. When Engineering or Natural Science is a second choice, the estimates are all larger when using fuzzy RD. When Engineering is a second choice, all 4 estimates are statistically different at the 10% level and when Natural Science is a second choice, 2 out of 4 estimates are statistically different. For the remaining margins, the results are fairly similar.

Due to the nature of our data, our setting has several additional practical advantages compared to KLM's study of college major returns. First, individuals attend their local high school and are therefore only choosing majors, so we do not have the confounding factor of institution choice. Second, we do not have to combine academic majors, whereas KLM needs to collapse college majors into 10 broad categories. Third, we observe earnings over two decades later, while KLM looks at earnings 8 years after admission (i.e., roughly 4 years after completing college).

5 Tests for Comparative Advantage

Given the pattern of estimates in Table 5, a natural question is whether the findings are consistent with a model of comparative advantage in major choice. Consider a case with just two individuals, A and B, and two majors, j and k. A standard definition (Sattinger 1993) is that individual A has a comparative advantage in major j over k if the ratio of their earnings in major 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 if their difference in log earnings is larger than the corresponding difference for individual B. In other words, individual A has a comparative advantage in major j over k if the percent increase in earnings for major 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 major preferences, ignoring costs, implies the expected earnings gain in percent terms for major j for individuals who rank j over k should exceed the negative of the expected earnings gain in percent terms for major 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 fuzzy RD estimates appearing in Table 5.

Case (a) is consistent with comparative advantage in major choice, as individuals are choosing the major within a pair of choices that results in higher earnings for them. Case (b) occurs when there is random sorting into majors for individuals on the margin of choosing j versus k. In this case, the return to completing major j with second choice k is equal but opposite in sign to the return to completing major 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 majors, 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 majors.

In Table 7 we present estimates of $\pi_{jk} + \pi_{kj}$ for each pair of major 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 9.1% premium (see Table 5). Looking at the reverse ordering of preferences, the return is 5.6% 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 7 shows, the sum of the two estimates is 14.7, and this sum is statistically different from zero. So this example is consistent with individuals pursuing comparative advantage in field choice.

As a second example, consider the combination of Business and Humanities. When a student completes a first-best choice of Humanities and has a next-best alternative of Business, their earnings drop by 12.4%. This could be explained by students who pursue Humanities because they enjoy the subject better and are told to "follow their passion," despite this advice leading to lower wages. In contrast, when an individual completes a first-best choice of Business and has a next-best alternative of Humanities, earnings are essentially unchanged (-.8%). This could be explained by students who observe that Business majors earn more on average, but in fact they are not very good at Business classes. The sum of these two estimates is strongly negative, which indicates comparative disadvantage.

The other rows in Table 7 display $\hat{\pi}_{jk} + \hat{\pi}_{kj}$ for the other eight margins, ordered from high to low. The major choice 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, 5 major choice 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. These findings provide further evidence against sheepskin effects being the dominant mechanism behind earnings differences. The results also argue against models relying on efficiency units such as the Ben Porath model (Heckman and Sedlacek, 1985) and in favor of a generalized Roy model which includes non-monetary gains (Roy, 1951).

By way of comparison, Kirkeboen et al. (2016) find evidence for sorting based on comparative advantage in the choice of college majors. Presumably, there should be less sorting at earlier ages, as students have less information and high school would allow a student to learn more about their abilities. It is therefore especially interesting that we find evidence of substantial sorting already after grade nine.

6 Mechanisms

Section 4 provides clear evidence of highly variable, and often sizable, returns to high school majors. A natural question is what drives these results. In this section, we explore three possible mechanisms: years of schooling, college major, and occupation.[31]

First, if completing a major (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 fourth year of studies, that could result in more years of education for individuals who complete the Engineering major. It is also possible that different majors impact the probability of college attendance.

Second, since the high school majors are preparatory for college, the pattern of earnings we observe in Table 5 could also be explained by individuals choosing different college majors. For example, if a student completes the Business major in high school, it could affect whether they pursue a Business-related major in college. In this case, earnings could increase if there is a positive return to a college Business-related major. This channel could impact the 45% of individuals in our baseline sample who complete college, but cannot explain differential returns for the remaining 55%.

Third, if entry into different occupations requires, or is eased by, having a specific high school major, then differences in earnings across different occupations could explain our findings. For example, it may be easier to get a job as a bookkeeper for individuals who complete Business versus Humanities in high school. The differential earnings of bookkeepers versus other occupations could therefore be a third possible mechanism.

As a first pass, we conduct a conventional mediation analysis, where we add dummy variables for years of schooling, college degree type, and occupation to see how the estimates are affected. We do this in Appendix Table A9, adding each set of variables one at a time

[31] Lemieux (2015) asks the related question of how occupation, field of study, and the returns to education are connected using correlational data from Canada.

and then all three jointly. We split the table into two panels: the top for baseline estimates which are statistically significant, and the bottom for insignificant estimates.

Starting with the top panel, adding in years of schooling has relatively little effect on the coefficient estimates, with none of the estimated effects changing by more than 50%. Adding in college major dummies as mediating variables explains some of the variation, with 5 out of 17 estimates falling by more than 50%. The addition of occupation dummies shrinks many of the coefficients, with 10 out of 17 estimates falling by over 50%. In the final specification, we add all three sets of mediating variables at once. The estimates shrink by between 28% to 85%, with 12 out of 17 estimates falling by more than 50%. The bottom panel for insignificant estimates is not very revealing, as the estimates are generally close to zero to begin with.

One issue with this conventional mediation analysis is that the mediating variables are themselves outcomes, and hence endogenous. So as an alternative, we perform an exercise which does not suffer from this problem. To perform this analysis we use data for the entire Swedish population and create variables which reflect mean earnings associated with each of the three mechanisms. For example, for mean earnings due to occupation, we assign each individual in our sample the mean log earnings of all individuals in the population with the same occupation as of age 38 from the same school cohort. There are 319 different occupations. We then use this as the outcome variable in a RD model which parallels our baseline specification. This yields 30 different estimates, one for each first-second major choice combination, of the average return associated with different occupations. To understand what these RD estimates capture, consider an example. If individuals who are barely admitted to Business over a second-best choice of Humanities end up in higher paying occupations in general, the coefficient estimate will be positive.

We construct similar mean earnings measures based on 205 different college majors and the 10 categories that make up the years of schooling variable (from 9 to 18 years of schooling).[32] We similarly use these measures as the outcome variables in a RD model which parallels our baseline specification.

[32]For occupation and college major we use 4 digit codes, but collapse to 3 digits if the number of observations is less than 100 for a given cohort. For the college major measure, we create a single "no-college" category for all individuals without at least a three year college education (the standard length of a bachelor's program in Sweden). 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, college field of study, or occupation have changed over time.

To assess the importance of the different mechanisms, we compare each set of estimates against our baseline estimates. In Figure 9, we plot the 30 different baseline estimates against the 30 different years of schooling estimates (top panel), the 30 different college major estimates (middle panel), and the 30 different occupation estimates (bottom panel). To help with interpretation, suppose that each of the dots in the bottom panel was on the 45 degree line. This would imply the returns we estimated in Table 5 could be entirely explained by individuals choosing different occupations with higher or lower mean earnings. In contrast, if the slope was flat, occupational mean earnings would have no explanatory power.

There is a positive slope in all three panels in Figure 9, suggesting a contribution from each of these mechanisms. The steepness of the slope in the top panel implies that when the expected return due to extra years of schooling rises by 1%, the return to earnings we estimated in Table 5 rises by 0.5%. Likewise, when the expected returns due to college major or occupation rises by 1%, the returns rise by 1.0% and 1.4%, respectively. Appendix Table A10 reports these regression results.[33]

The three mechanisms are not necessarily independent or mutually exclusive. In the final column of Appendix Table A10, we regress the baseline estimates on the three measures simultaneously. The coefficient on years of schooling shrinks to zero. The college major coefficient falls by two-thirds, but remains statistically significant. Likewise, the occupation coefficient falls by roughly 20%, but also remains significant. The R-squared from this combined regression is 0.95. The contribution of occupation is roughly three times as large as college major, which is perhaps not surprising given that over half of individuals do not complete a college degree.

The general conclusion from both the traditional mediation analysis and from the more causal exercise is that occupation, and to a lesser extent college major (but not years of schooling), play important roles in explaining the pattern of returns we observe.

7 Conclusion

Secondary school systems requiring field specialization are prevalent in many countries, yet little is known about long-term labor market consequences. We provide the first causal

[33] We note the standard errors in these regressions could be biased, since the right hand side variables are measured with error.

evidence on how high school majors affect future earnings. Using unique data from Sweden, our analysis yields five main results. First, the returns to completing different academic majors are often sizable, and can be both negative and positive. Second, earnings payoffs to different majors depend on next-best alternatives. Third, academic majors do not result in higher earning relative to the non-academic track for marginal students. Fourth, the pattern of returns is consistent with individuals pursuing comparative advantage for many field combinations and comparative disadvantage for others. Finally, most of the differences in adult earnings can be attributed to differences in adult occupations, and to a lesser extent, college majors.

These findings are valuable for policymakers choosing how to structure and reshape secondary education, including whether to relax enrollment limits on oversubscribed majors or to provide incentives to study one major over another. Years of schooling have been highlighted as a key determinant of a nation's growth rate, and the magnitudes of our estimates suggest schooling majors could play an equally important role. These findings are useful for students making field decisions, as well as for the school counselors and parents who provide advice to them. From a theoretical perspective, our findings indicate that earnings differences across majors are not simply due to the sorting of high-ability individuals into high-paying majors. Moreover, our results argue against models relying on efficiency units (e.g., the Ben Porath model) and sheepskin effects being the dominant force, and in favor of a generalized Roy model and specific human capital accumulation.

While this paper makes important progress on estimating long-term payoffs to high school majors, several questions remain unanswered. The parameters we estimate are ex-post payoffs to majors. An interesting question for future research is whether these ex-post payoffs line up with ex-ante predicted payoffs.[34] If they do, it suggests that students understand the monetary tradeoffs associated with different majors, and that some students are willing to trade off higher earnings for non-pecuniary returns. However, it is also possible that at age 16, students do not yet know what occupation will be the best fit for them and they may not be knowledgeable about earnings differences across fields. In future work, it would be interesting to explore the factors influencing an individual's major choice, including the impact of parents, friends, and teachers. The parameters we estimate are also for compliers

[34]This question has been studied for college by, for example, Wiswall and Zafar (2015) and Zafar (2011).

on the margin of gaining entry into a major. For these marginal individuals, the effects can be as large in absolute value as the returns to 2 years of additional schooling. It would be interesting to know if similar patterns hold for other individuals.

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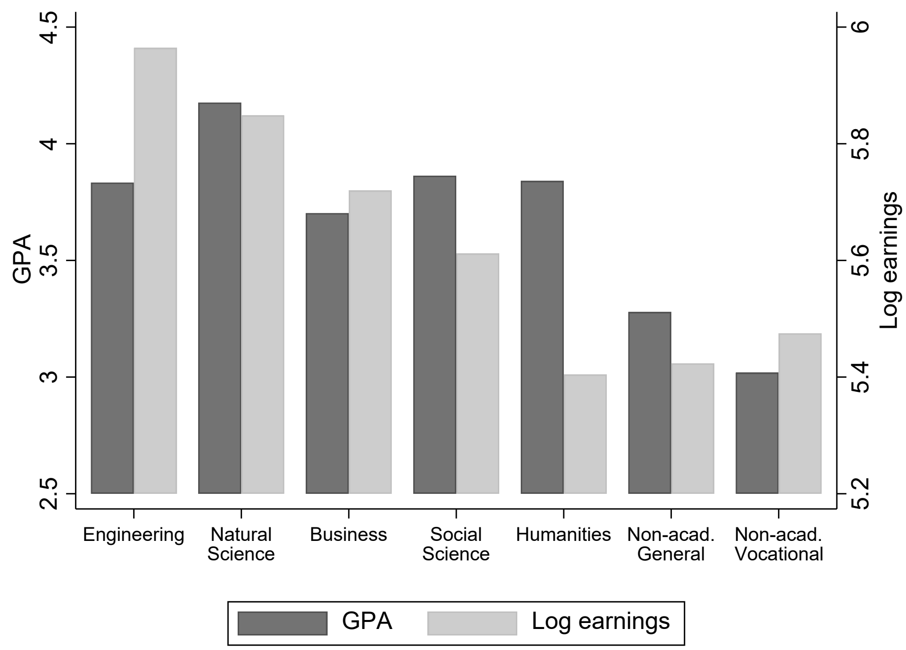
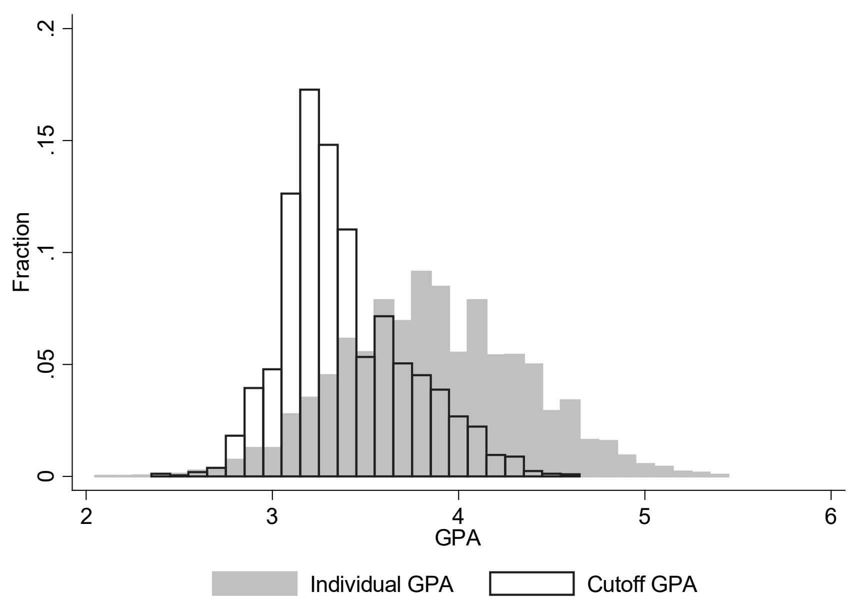
Figure 1. Ninth grade unadjusted GPA and adult earnings for program completers.



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.

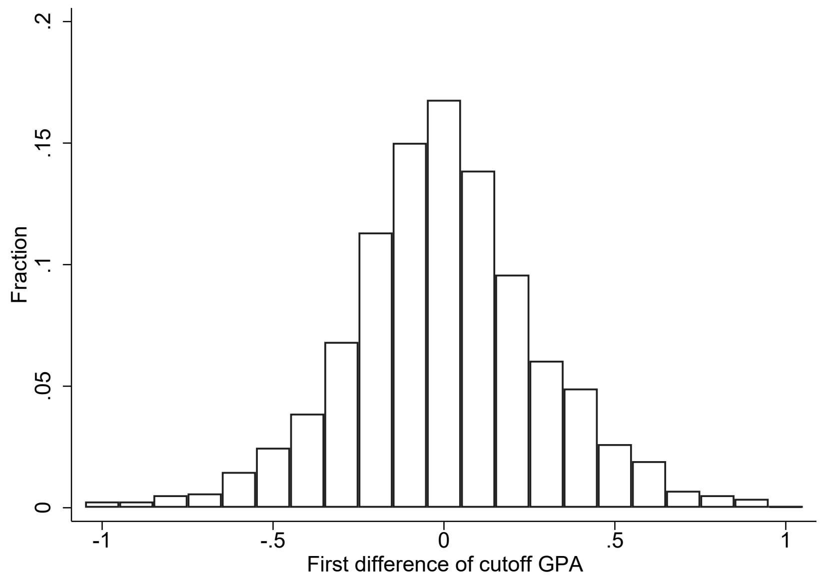
Figure 2: Cutoff GPA versus individual GPA distributions.



Notes: The white bars plot the distribution of cutoff GPAs for competitive programs, which vary by major, 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 233,034 observations.

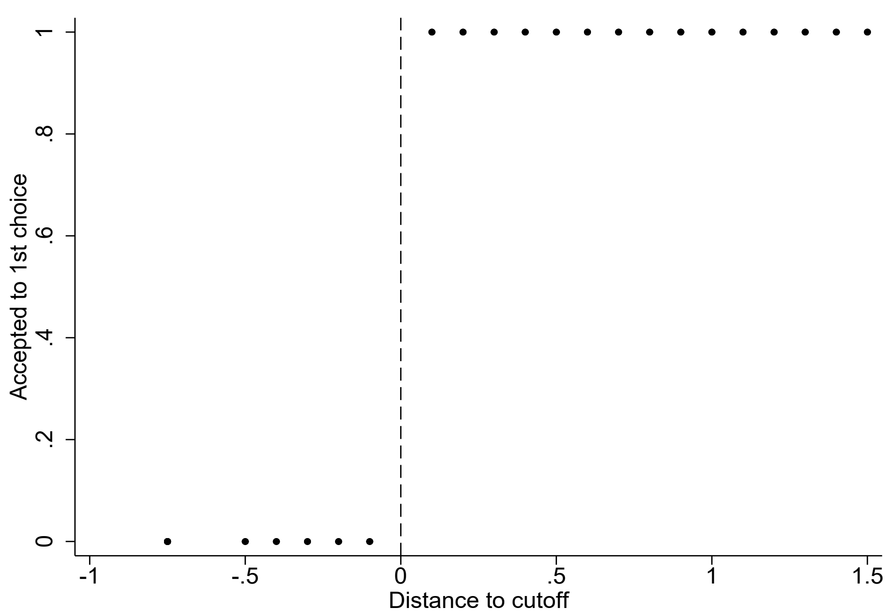
Figure 3: First-differenced cutoff GPA distributions.

Panel B: Current minus lagged cutoff GPA



Notes: Current minus lagged cutoff GPA, where the sample is limited to majors which are competitive two years in a row in a school region.

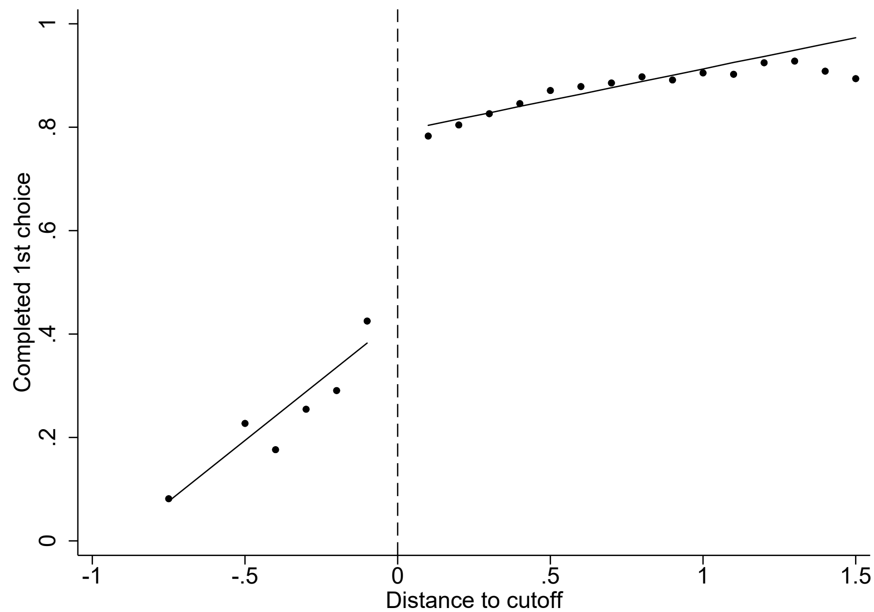
Figure 4: Discontinuity in admissions as a function of GPA.



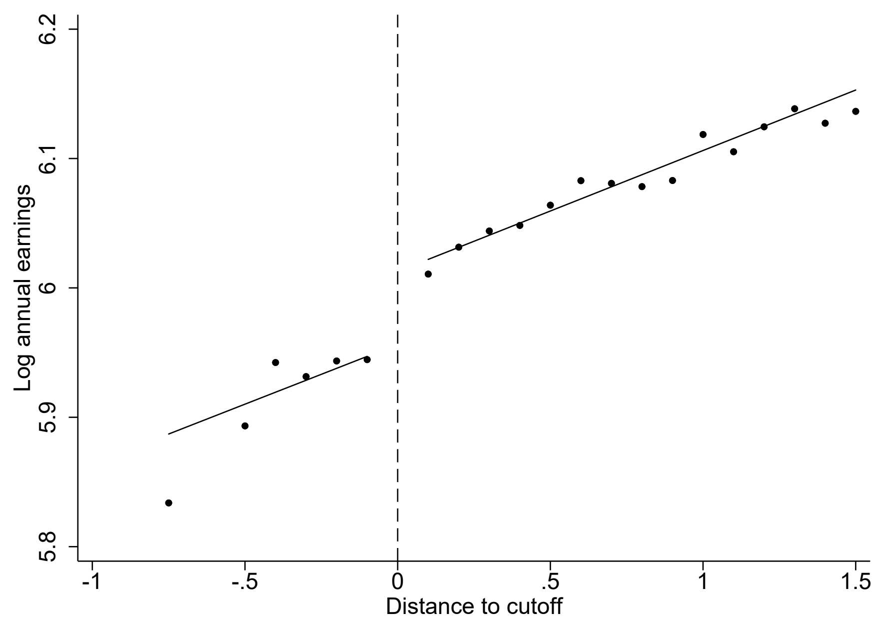
Notes: Each dot is the average acceptance rate in a .1 GPA bin, except for the leftmost dot which is a .5 bin due to sparsity. GPA is measured relative to a normalized cutoff of 0. N = 233,034.

Figure 5. Example of Engineering first choice vs. Natural Science second choice.

Panel A: RD first stage

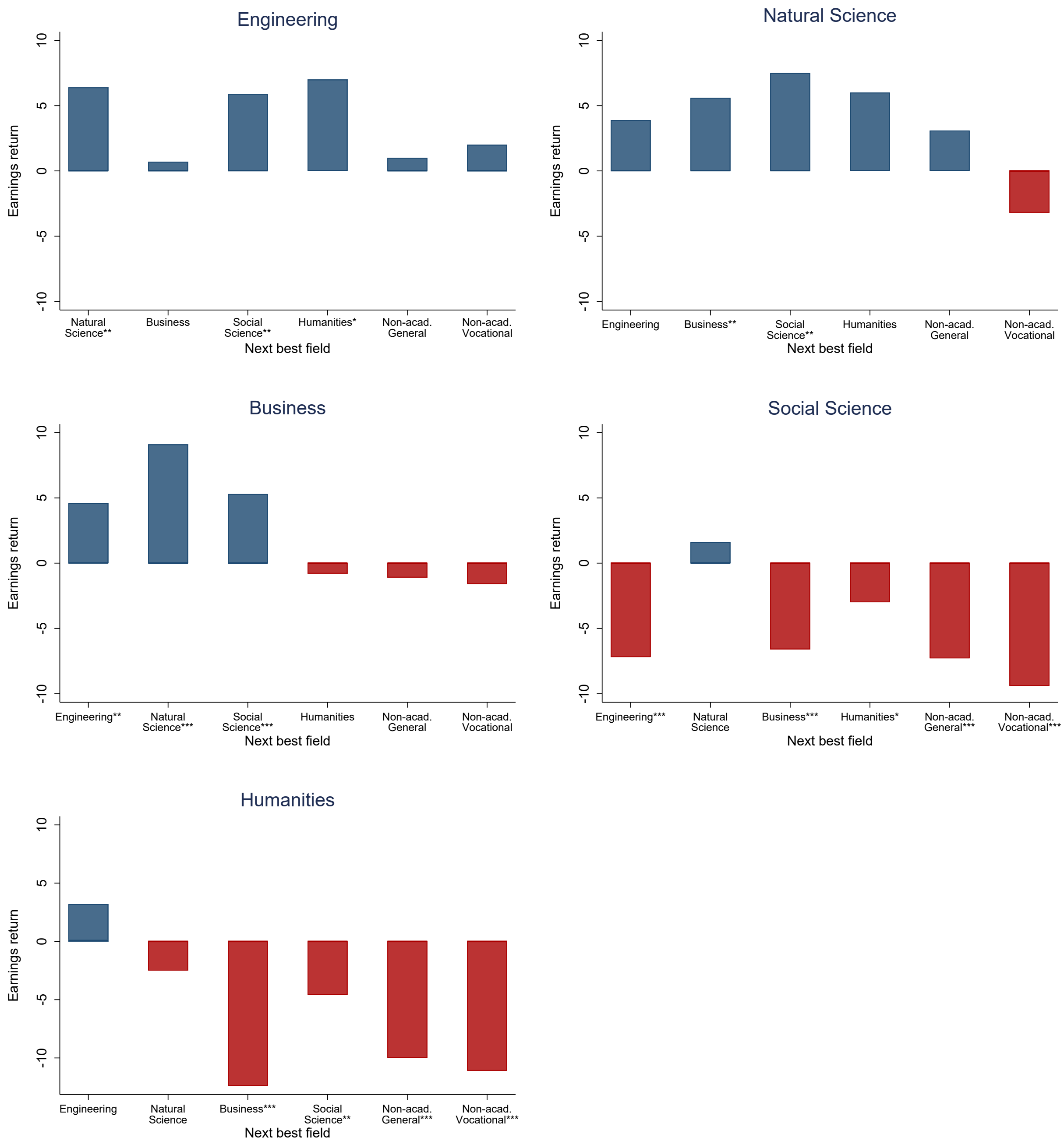


Panel B: RD reduced form



Notes: Each dot is the average acceptance rate in a .1 GPA bin, except for the leftmost dot which is a .5 bin due to sparsity. 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. N = 31,910.

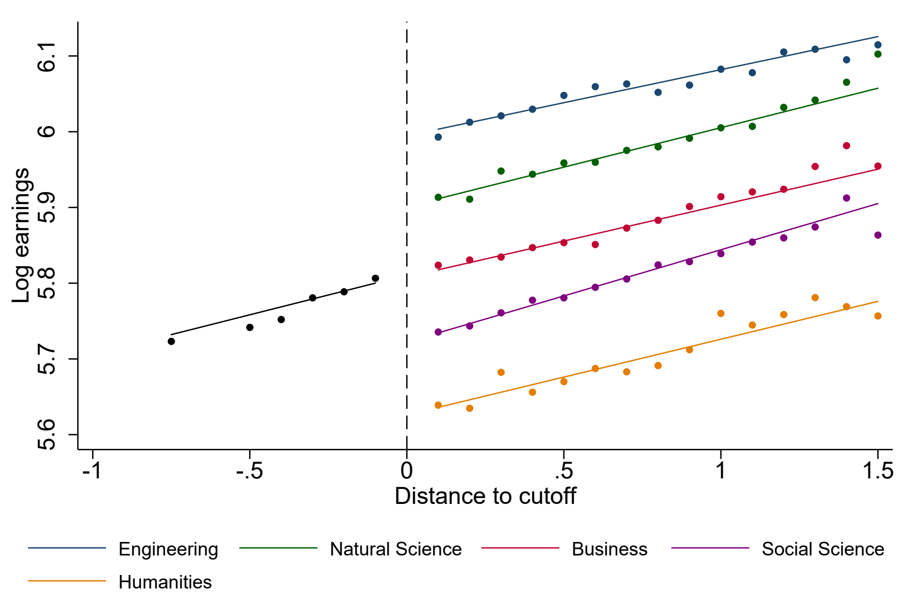
Figure 6: Earnings return by first-second choice combination.



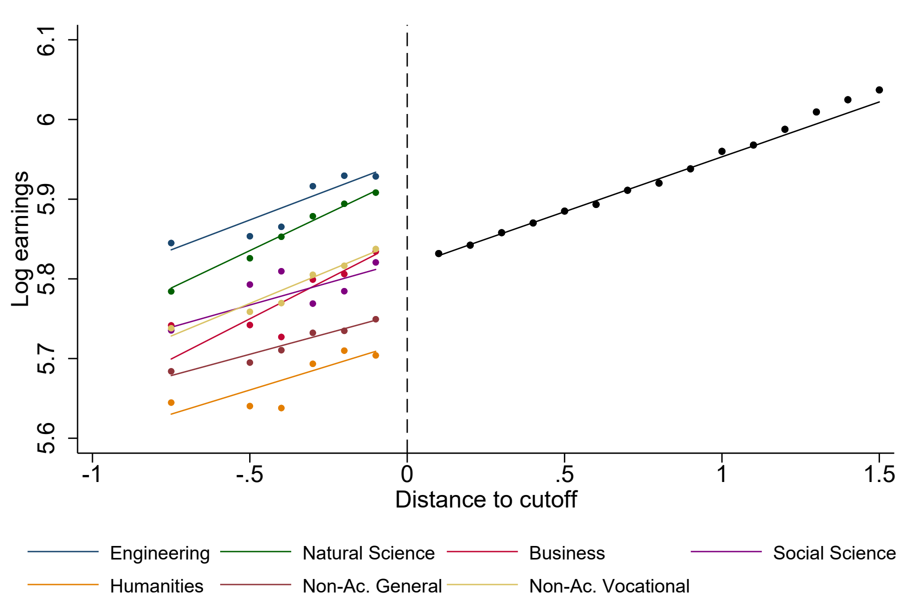
Notes: Baseline sample of 233,034 individuals.
** p<.10, ** p<.05, *** p<.01*

Figure 7. Comparison of 2 versus 12 slope models.

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

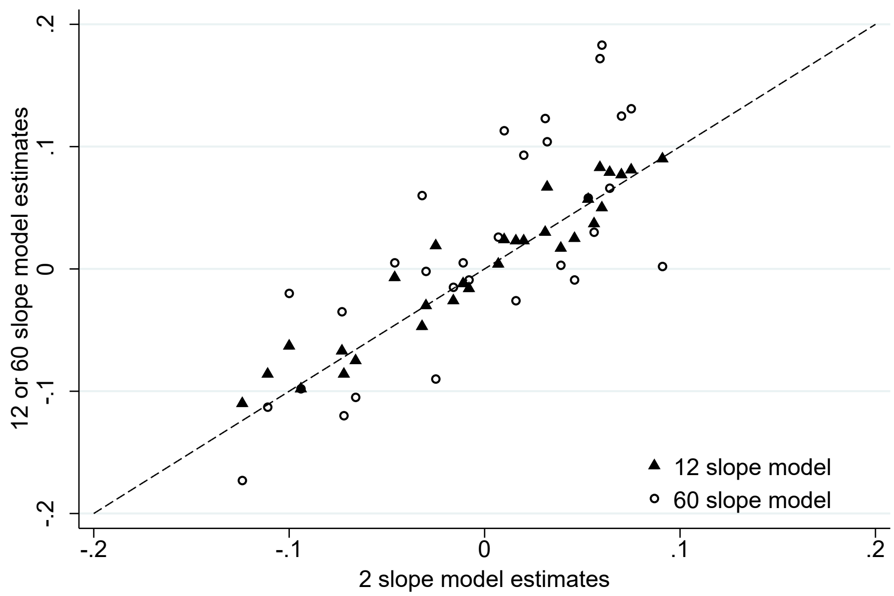


Panel B: 7 separate slopes below the cutoff, single slope above the cutoff



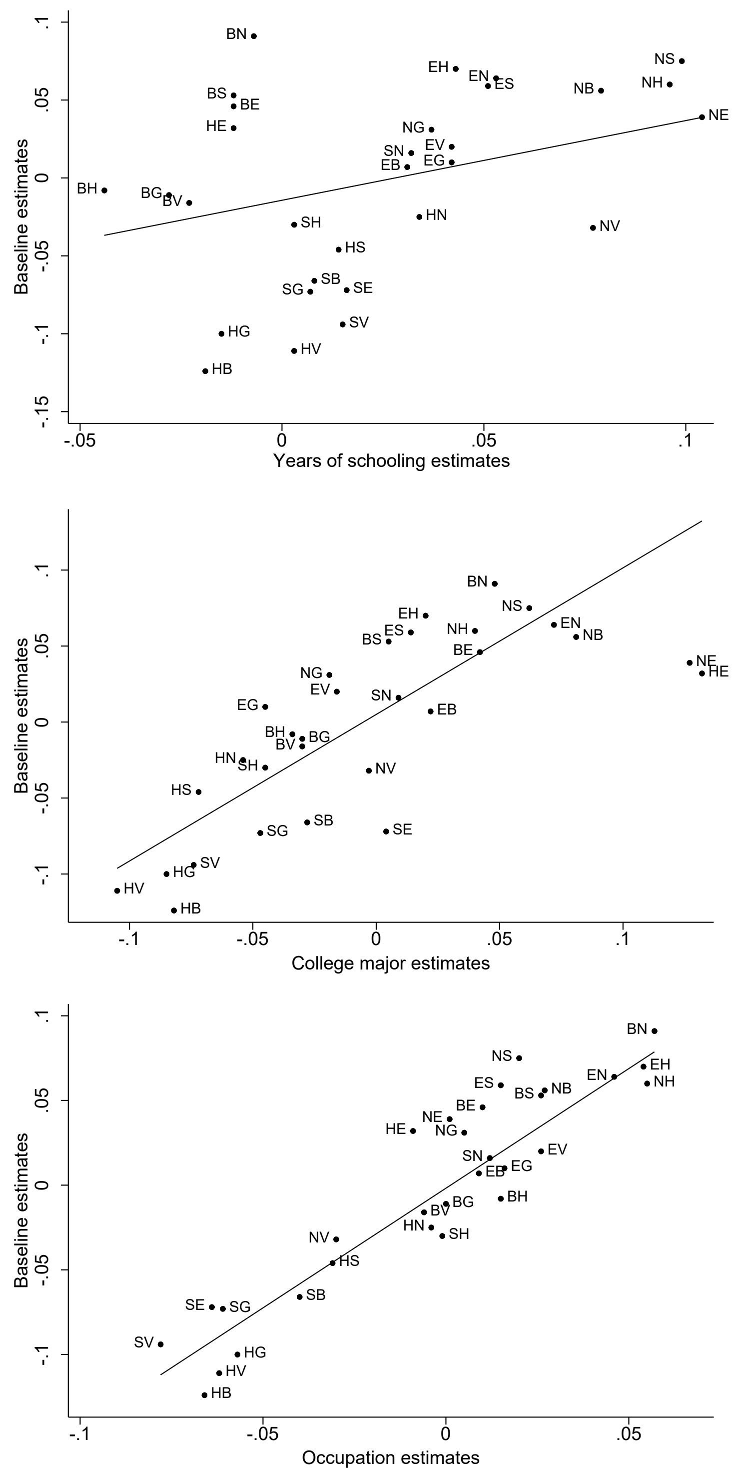
Notes: These graphs are for illustrative purposes; we never mix the 2 slope and 12 slope models in estimation. Each dot is the average acceptance rate in a .1 GPA bin, except for the leftmost dot which is a .5 bin due to sparsity. GPA is measured relative to a normalized cutoff of 0. The trend lines are RD estimates using the underlying data, no covariates, and triangular weights.

Figure 8. Comparison of fuzzy RD 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 5). The dashed line is the 45° line.

Figure 9. Mechanisms: Years of schooling, college major, 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 Appendix Table A10 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. Oversubscribed and non-impacted program sample sizes.

	Baseline Sample: Oversubscribed programs		Non-impacted programs		Share impacted	
First choice	Individuals	Programs	Individuals	Programs	Individuals	Programs
Engineering	63,610	793	52,171	1,079	.55	.42
Natural Science	18,830	395	50,583	1,457	.27	.21
Business	84,141	1,030	35,469	815	.70	.56
Social Science	52,465	873	32,120	970	.62	.47
Humanities	13,988	396	23,681	1,467	.37	.21
Total	233,034	3,487	194,024	5,788	.55	.38

Notes: Programs are defined by major, year, and school region. "Individuals" refers to the number/share of students applying to either an oversubscribed or non-impacted program. Non-impacted programs do not have an excess supply of applicants, and so have unrestricted entry.

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	--	.345***	.401***	.248***	.255***	.386***	.395***
		(.010)	(.011)	(.015)	(.029)	(.011)	(.009)
Natural Science	.397***	--	.424***	.338***	.317***	.292***	.309***
	(.016)		(.018)	(.017)	(.026)	(.032)	(.025)
Business	.469***	.458***	--	.468***	.431***	.530***	.512***
	(.014)	(.012)		(.012)	(.012)	(.007)	(.008)
Social Science	.376***	.399***	.503***	--	.377***	.448***	.426***
	(.017)	(.012)	(.010)		(.011)	(.009)	(.012)
Humanities	-.098***	.212***	.434***	.369***	--	.287***	.270***
	(.027)	(.025)	(.015)	(.014)		(.016)	(.019)

Notes: N=233,034. 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 major, and next-best alternative major; and controls for the parent and child characteristics listed in Appendix Table A2 (except for GPA, which, when normalized is the running variable). Standard errors in parentheses.
** p<.10, ** p<.05, *** p<.01*

Table 4. Reduced form sharp RD estimates of program admission on log earnings.

	Second choice						
First choice	Engineering	Natural Science	Business	Social Science	Humanities	Non-acad. General	Non-acad. Vocational
Engineering	--	.033***	.004	.026**	.031	.005	.014*
		(.008)	(.010)	(.012)	(.021)	(.009)	(.007)
Natural Science	.022	--	.043***	.043***	.039*	.014	-.020
	(.014)		(.016)	(.015)	(.022)	(.025)	(.019)
Business	.034***	.066***	--	.035***	-.008	-.008	-.010
	(.013)	(.011)		(.009)	(.010)	(.006)	(.007)
Social Science	-.056***	.008	-.043***	--	-.014	-.043***	-.057***
	(.016)	(.010)	(.009)		(.009)	(.007)	(.009)
Humanities	.008	-.018	-.079***	-.030***	--	-.037***	-.043***
	(.024)	(.022)	(.012)	(.011)		(.012)	(.014)

Notes: N = 233,034. 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 major, and next-best alternative major; and controls for the parent and child characteristics listed in Appendix Table A2 (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 for details). Standard errors in parentheses.
** p<.10, * p<.05, *** p<.01*

Table 5. Returns to different high school majors: Fuzzy RD estimates of program completion on log earnings.

	Second choice							F-tests for equality across 2nd choices		
First choice	Engineering	Natural Science	Business	Social Science	Humanities	Non-Acad. General	Non-Acad. Vocational	All	Acad.	Acad vs. non-ac.
Engineering	--	.064***	.007	.059**	.070*	.010	.020	16.22	10.49	6.81
		(.017)	(.018)	(.025)	(.039)	(.017)	(.015)	[.006]	[.015]	[.009]
Natural Science	.039	--	.056**	.075***	.060	.031	-.032	11.48	1.75	4.10
	(.025)		(.028)	(.028)	(.037)	(.052)	(.040)	[.043]	[.625]	[.043]
Business	.046**	.091***	--	.053***	-.008	-.011	-.016	65.17	21.98	38.19
	(.021)	(.017)		(.016)	(.018)	(.010)	(.011)	[.000]	[.000]	[.000]
Social Science	-.072***	.016	-.066***	--	-.030*	-.073***	-.094***	44.66	20.86	16.06
	(.026)	(.018)	(.014)		(.017)	(.013)	(.016)	[.000]	[.000]	[.000]
Humanities	.032	-.025	-.124***	-.046**	--	-.100***	-.111***	17.82	15.22	2.92
	(.141)	(.039)	(.021)	(.021)		(.028)	(.031)	[.003]	[.002]	[.087]

Notes: N = 233,034. See notes to Table 4. Standard errors in parentheses. 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<.10, ** p<.05, *** p<.01*

Table 6. Robustness checks.

Margin	Baseline	Quadratic	Smaller bandwidth	1st-2nd intercepts	12 slopes	60 slopes	Excluding 1982-84	Earnings in levels	Earnings rank
E vs. N	.064***	.071***	.065***	.072***	.079**	.066*	.060***	4.565***	.032***
	(.017)	(.024)	(.021)	(.019)	(.032)	(.036)	(.020)	(1.095)	(.012)
E vs. B	.007	.014	.013	.026	.004	.026	.001	.182	.005
	(.018)	(.024)	(.022)	(.032)	(.028)	(.039)	(.020)	(1.279)	(.012)
E vs. S	.059**	.067**	.083***	.080	.083**	.172***	.048*	3.340**	.030*
	(.025)	(.032)	(.031)	(.059)	(.041)	(.060)	(.028)	(1.634)	(.018)
E vs. H	.070*	.077*	.066	.231	.077	.125	.065	4.379*	.043
	(.039)	(.043)	(.051)	(.206)	(.050)	(.082)	(.046)	(2.417)	(.028)
E vs. G	.010	.017	.029	-.024	.024	.113***	.020	-1.361	.006
	(.017)	(.024)	(.022)	(.026)	(.020)	(.038)	(.020)	(1.122)	(.012)
E vs. V	.020	.027	.036*	.025	.023	.093***	.017	.272	.029***
	(.015)	(.022)	(.019)	(.017)	(.019)	(.030)	(.016)	(1.023)	(.010)
N vs. E	.039	.045	.0294	.013	.017	.003	.047*	3.432**	.016
	(.025)	(.030)	(.031)	(.033)	(.047)	(.059)	(.027)	(1.680)	(.017)
N vs. B	.056**	.062*	.055	.043	.037	.030	.044	3.006	.037*
	(.028)	(.032)	(.035)	(.063)	(.040)	(.067)	(.030)	(2.055)	(.019)
N vs. S	.075***	.082**	.088**	.080	.081*	.131*	.063**	4.934***	.053***
	(.028)	(.032)	(.036)	(.051)	(.047)	(.069)	(.030)	(1.800)	(.020)
N vs. H	.060	.067*	.102*	.246**	.050	.183*	.061	2.267	.056**
	(.037)	(.040)	(.056)	(.105)	(.050)	(.106)	(.040)	(2.222)	(.026)
N vs. G	.031	.038	.054	.071	.030	.123	-.006	-.064	.021
	(.052)	(.055)	(.068)	(.084)	(.058)	(.127)	(.056)	(3.064)	(.037)
N vs. V	-.032	-.025	.001	-.020	-.047	.060	-.043	-3.060	-.009
	(.040)	(.044)	(.058)	(.058)	(.048)	(.122)	(.043)	(2.476)	(.029)
B vs. E	.046**	.052**	.037	.067**	.025	-.009	.049**	5.435***	.021
	(.021)	(.026)	(.025)	(.028)	(.036)	(.044)	(.023)	(1.564)	(.014)
B vs. N	.091***	.097***	.067***	.075**	.090***	.002	.084***	7.786***	.045***
	(.017)	(.023)	(.021)	(.030)	(.029)	(.038)	(.020)	(1.223)	(.012)
B vs. S	.053***	.059***	.052***	.062***	.057*	.058	.045**	4.073***	.043***
	(.016)	(.022)	(.019)	(.019)	(.031)	(.035)	(.018)	(1.074)	(.011)
B vs. H	-.008	-.001	-.007	-.021	-.016	-.009	-.008	.696	.020
	(.018)	(.023)	(.021)	(.028)	(.029)	(.037)	(.020)	(1.029)	(.013)
B vs. G	-.011	-.006	-.009	-.014	-.012	.005	-.010	-.773	-.004
	(.010)	(.016)	(.012)	(.011)	(.013)	(.017)	(.012)	(.681)	(.007)
B vs. V	-.016	-.010	-.013	-.013	-.026*	-.015	-.018	-.636	.004
	(.011)	(.017)	(.014)	(.012)	(.015)	(.020)	(.012)	(.845)	(.008)

Table 6. Robustness checks, continued.

Margin	Baseline	Quadratic	Smaller bandwidth	1st-2nd intercepts	12 slopes	60 slopes	Excluding 1982-84	Earnings in levels	Earnings rank
S vs. E	-.072***	-.065**	-.081**	-.069*	-.086**	-.120**	-.068**	-2.283	-.030*
	(.026)	(.031)	(.032)	(.041)	(.042)	(.057)	(.029)	(1.775)	(.018)
S vs. N	.016	.022	.009	.018	.023	-.026	.007	.993	.004
	(.018)	(.024)	(.023)	(.035)	(.031)	(.040)	(.021)	(1.202)	(.013)
S vs. B	-.066***	-.060***	-.071***	-.073***	-.075***	-.105***	-.075***	-4.568***	-.029***
	(.014)	(.019)	(.017)	(.016)	(.024)	(.027)	(.016)	(1.095)	(.010)
S vs. H	-.030*	-.024	-.021	-.031	-.030	-.002	-.026	-1.188	.006
	(.017)	(.024)	(.021)	(.020)	(.030)	(.034)	(.019)	(1.001)	(.013)
S vs. G	-.073***	-.068***	-.065***	-.063***	-.067***	-.035	-.073***	-4.995***	-.048***
	(.013)	(.020)	(.016)	(.015)	(.016)	(.025)	(.015)	(.825)	(.010)
S vs. V	-.094***	-.088***	-.095***	-.105***	-.098***	-.098***	-.096***	-5.195***	-.045***
	(.016)	(.022)	(.020)	(.022)	(.020)	(.033)	(.018)	(1.079)	(.012)
H vs. E	.032	.055	.116	1.329	.067	.104	.027	1.299	.085
	(.141)	(.152)	(.210)	(3.550)	(.196)	(.412)	(.157)	(8.461)	(.097)
H vs. N	-.025	-.018	-.024	-.039	.019	-.090	-.044	-.493	-.000
	(.039)	(.044)	(.054)	(.076)	(.052)	(.093)	(.045)	(2.474)	(.028)
H vs. B	-.124***	-.118***	-.132***	-.108***	-.110***	-.173***	-.127***	-8.127***	-.055***
	(.021)	(.026)	(.025)	(.031)	(.030)	(.047)	(.024)	(1.269)	(.016)
H vs. S	-.046**	-.039	-.031	-.086**	-.007	.005	-.055**	-3.026**	-.009
	(.021)	(.027)	(.025)	(.035)	(.037)	(.048)	(.024)	(1.268)	(.016)
H vs. G	-.100***	-.092***	-.079**	-.082*	-.063*	-.020	-.098***	-6.873***	-.066***
	(.028)	(.034)	(.034)	(.044)	(.033)	(.067)	(.032)	(1.579)	(.021)
H vs. V	-.111***	-.103***	-.103***	-.142***	-.086**	-.113	-.106***	-6.517***	-.053**
	(.031)	(.037)	(.039)	(.051)	(.037)	(.073)	(.033)	(1.728)	(.023)
Corr. w/ baseline	1.00	.99	.98	.95	.97	.76	.99	.97	.95
Obs.	233,034	233,034	169,403	233,034	233,034	233,034	186,796	250,522	250,522

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 5. Column 2 adds in quadratic terms in the running variable, column 3 reduces the bandwidth to + or - .75, and column 4 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 two columns use earnings in levels and earnings rank instead of log earnings; both of these alternatives include zero and below-threshold earnings. Standard errors in parentheses.

** p<.10, ** p<.05, *** p<.01*

Table 7. Tests for comparative advantage and disadvantage.

Choice combinations	Sum of returns
Natural Science 1st – Business 2nd and Business 1st – Natural Science 2nd	$.147^{***}$ (.036)
Engineering 1st – Natural Science 2nd and Natural Science 1st – Engineering 2nd	$.103^{***}$ (.034)
Engineering 1st - Humanities 2nd and Humanities 1st – Engineering 2nd	.102 (.148)
Natural Science 1st – Social Science 2nd and Social Science 1st – Natural Science 2nd	$.091^{**}$ (.037)
Engineering 1st – Business 2nd and Business 1st – Engineering 2nd	$.053^{*}$ (.030)
Natural Science 1st – Humanities 2nd and Humanities 1st – Natural Science 2nd	.035 (.056)
Business 1st – Social Science 2nd and Social Science 1st – Business 2nd	-.013 (.024)
Engineering 1st – Social Science 2nd and Social Science 1st – Engineering 2nd	-.013 (.040)
Social Science 1st – Humanities 2nd and Humanities 1st – Social Science 2nd	$-.076^{**}$ (.030)
Business 1st – Humanities 2nd and Humanities 1st – Business 2nd	$-.131^{***}$ (.030)

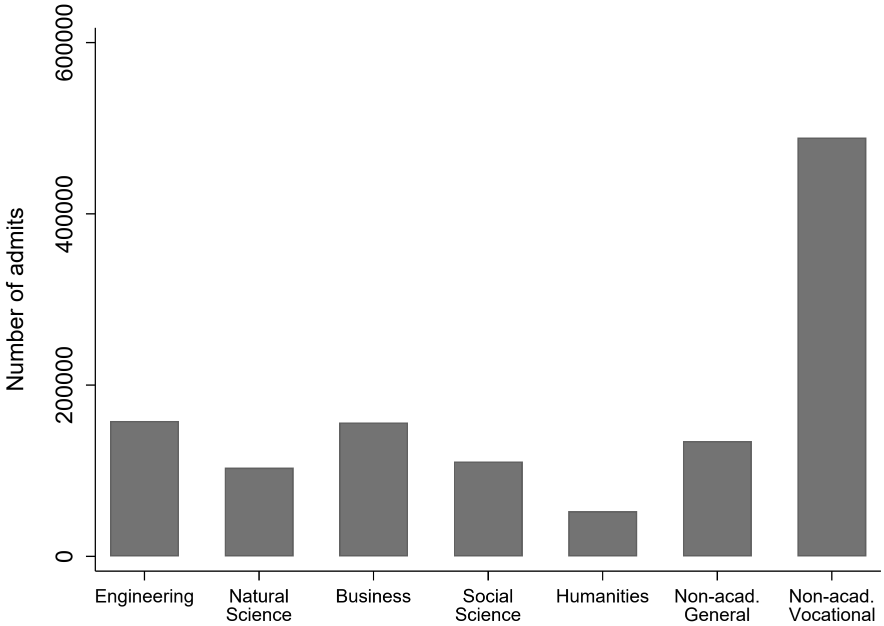
Notes: N = 233,034. See text for details on the tests. A positive sum is consistent with comparative advantage, a zero with random sorting, and a negative with comparative disadvantage. Standard errors in parentheses.
** p<.10, ** p<.05, *** p<.01*

APPENDIX FIGURES AND TABLES

"High School Majors, Comparative (Dis)Advantage, and Future Earnings"

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

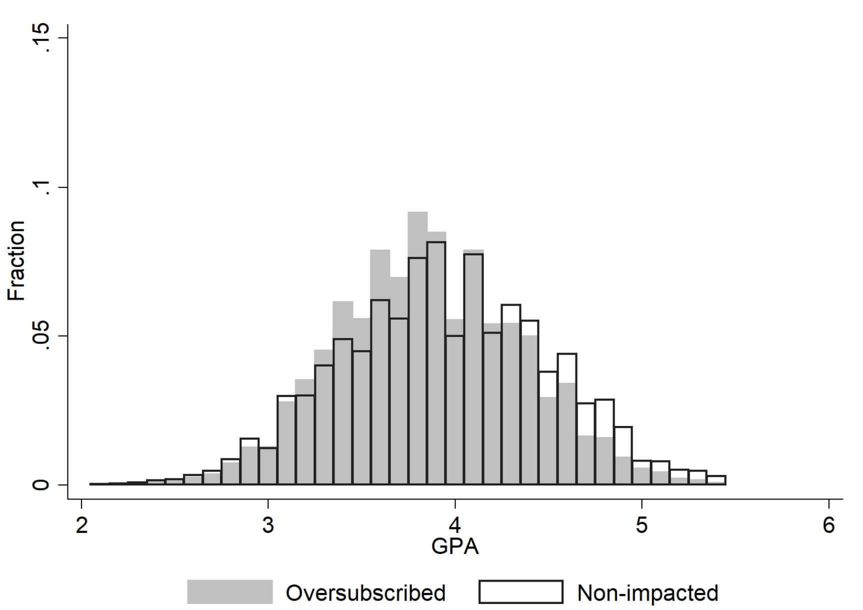
Figure A1. Number of students admitted to each major.



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

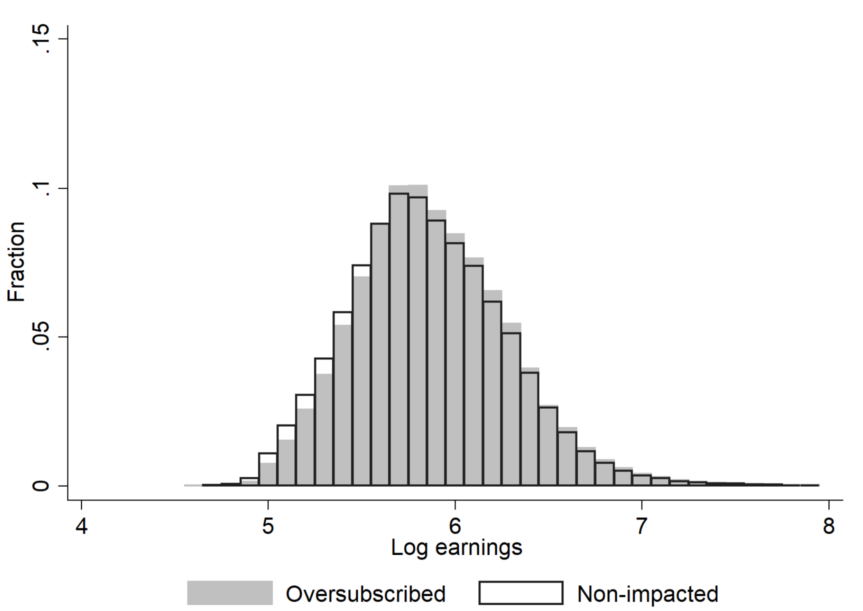
Figure A2: GPA and annual earnings in oversubscribed and non-impacted programs.

Panel A: Individual GPA



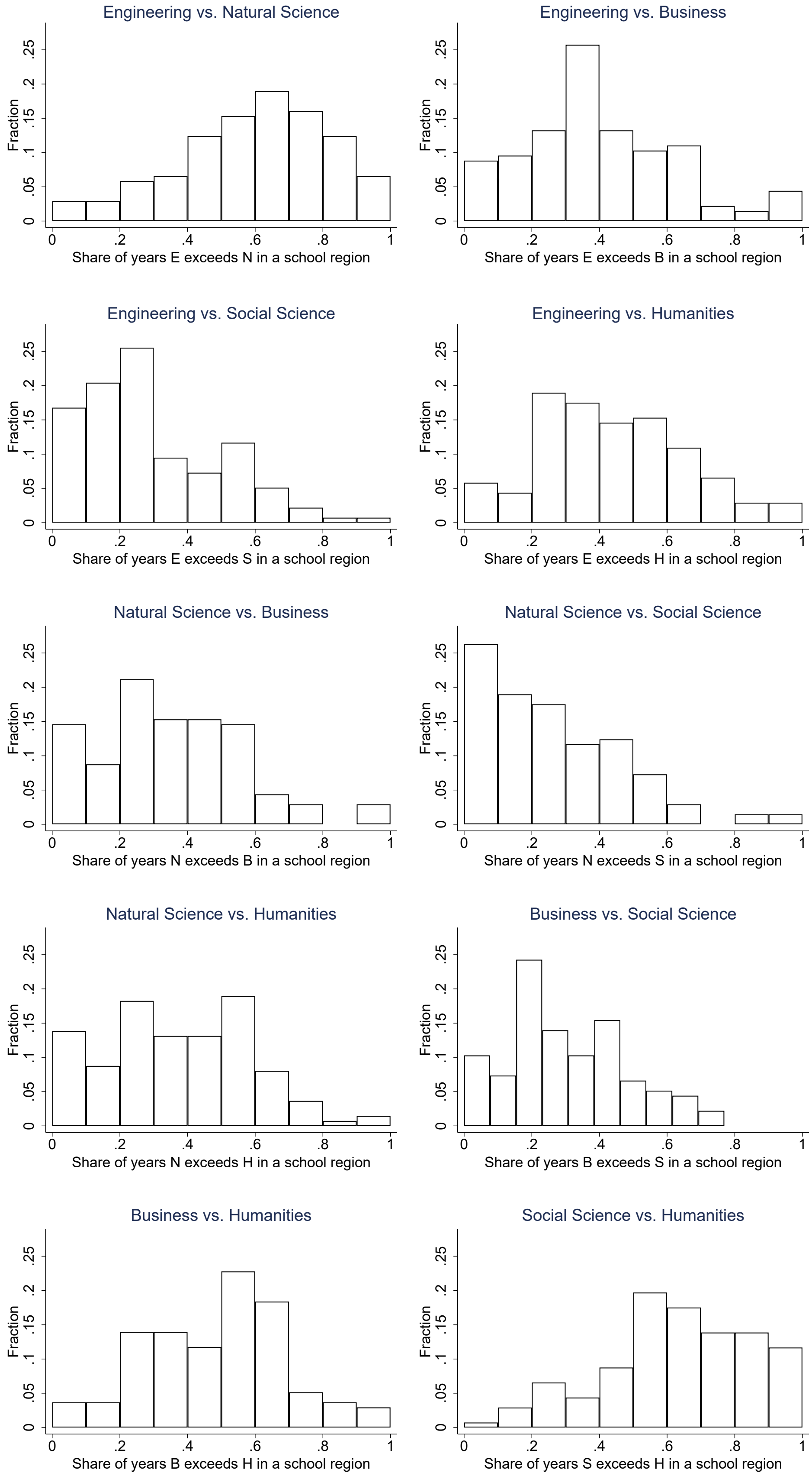
Notes: The grey bars plot the distribution of GPA for individuals in our baseline estimation sample of oversubscribed programs. The white bars plot the distribution of GPAs for individuals in non-impacted academic programs.

Panel B: Individual annual earnings at age 38



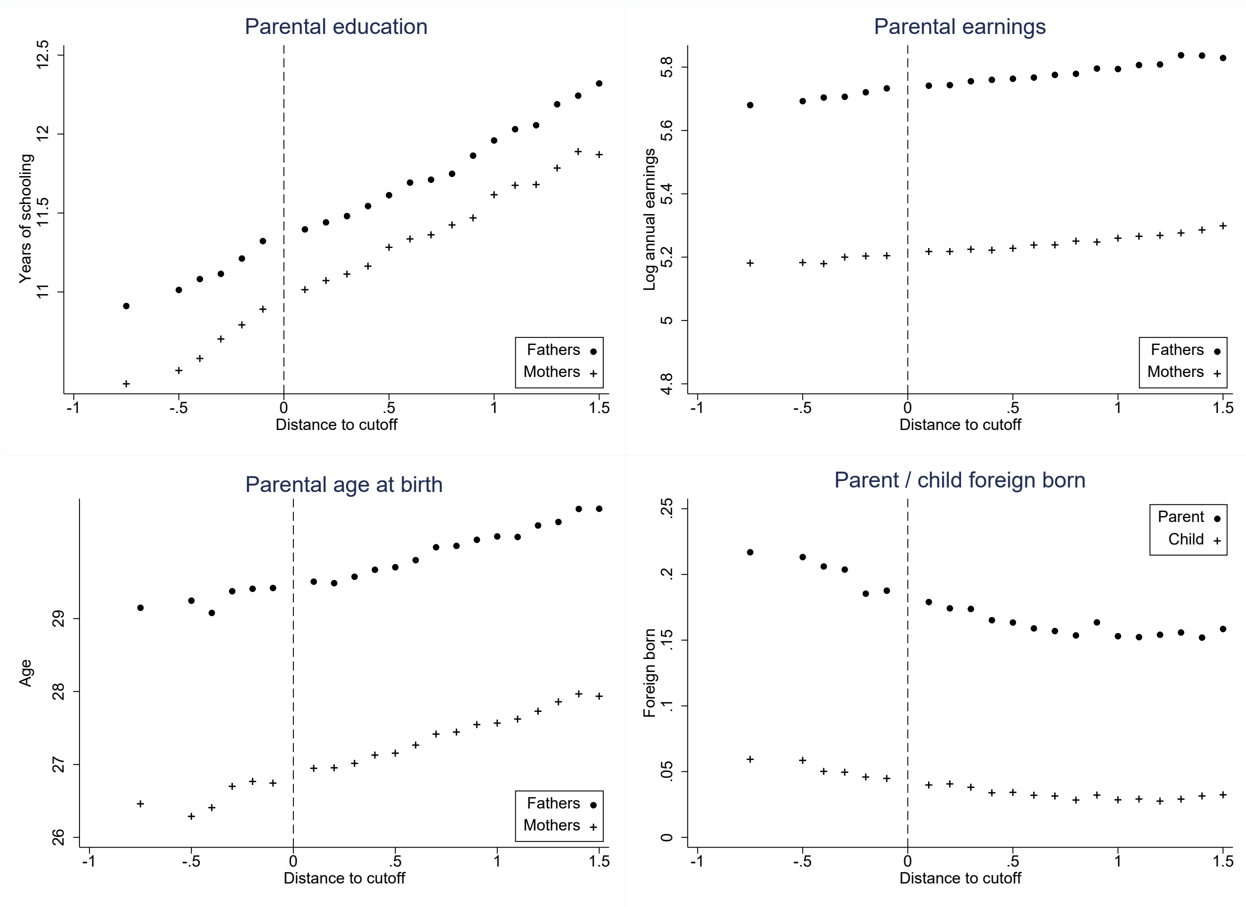
Notes: The grey bars plot the distribution of GPA for individuals in our baseline estimation sample of oversubscribed programs. The white bars plot the distribution of GPAs for individuals in non-impacted academic programs.

Figure A3. Within school region variation over time in relative major cutoffs.



For each combination of majors, the graphs plot the distribution across school regions of the share of years a cutoff for one major exceeds another (conditional on at least one of the two majors being oversubscribed).

Figure A4: Smoothness of predetermined variables at the cutoff.



Notes: Each dot is the average for the relevant outcome in a .1 GPA bin, except for the leftmost dot which is a .5 bin due to sparsity. 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 A4.

Table A1. 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	--	31,910	12,023	3,375	552	4,504	11,246
Natural Science	8,833	--	2,345	5,617	674	462	899
Business	7,656	6,687	--	29,850	8,135	18,254	13,559
Social Science	1,723	8,392	15,714	--	15,279	7,963	3,394
Humanities	413	566	2,305	7,202	--	2,233	1,269

Notes: Baseline sample of 233,034 individuals.

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

Variables	Oversubscribed programs	Share missing	Non-impacted programs	Share missing
Parent characteristics:				
Father age	29.74	.05	29.99	.07
Mother age	27.20	.02	27.33	.02
Father schooling	11.60	.05	11.29	.06
Mother schooling	11.23	.02	10.82	.02
Father earnings	5.76	.18	5.75	.20
Mother earnings	5.23	.25	5.20	.29
Foreign born parent	.16	0	.16	0
Child characteristics:				
Foreign born	.03	0	.03	0
Female	.51	0	.50	0
Age at application	15.99	0	15.99	0
GPA	3.86	0	3.94	0
Child outcomes:				
College degree	.45	0	.45	0
Log earnings	5.84	0	5.81	0
Observations	233,034		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.

Table A3. Comparison of major cutoffs across years within the same school region.

	Fraction of years with a higher cutoff		
Major combinations	1st major	2nd major	No difference
Engineering vs. Natural Science	.37	.25	.38
Engineering vs. Business	.28	.42	.30
Engineering vs. Social Science	.21	.53	.27
Engineering vs. Humanities	.31	.38	.31
Natural Science vs. Business	.24	.46	.30
Natural Science vs. Social Science	.18	.51	.31
Natural Science vs. Humanities	.24	.38	.39
Business vs. Social Science	.24	.48	.28
Business vs. Humanities	.37	.32	.31
Social Science vs. Humanities	.47	.21	.32

Notes: The table reports the average fraction of years with a higher cutoff for one major compared to another within the same school region. If both majors have a cutoff in a given year in the same school region, we compare the two to determine which is higher. If one major has a cutoff, but the other does not, we record the major with the cutoff as having a higher cutoff. "No difference" can either reflect that both majors have cutoffs which are equal or that neither major was oversubscribed.

Table A4. 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
-.084	-.039	-.008	-.005	-.169	.013	-.004	-.003
(.059)	(.054)	(.010)	(.009)	(.138)	(.118)	(.009)	(.005)
249,424	259,434	214,308	196,991	249,174	258,879	263,856	263,856

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-second best choice combinations. Standard errors in parentheses.

* $p<.10$, ** $p<.05$, *** $p<.01$

Table A5. Heterogeneity by age, gender, and parental education.

	Baseline (age 37-39)	Age 27-29	Males	Females	Low parental education	High parental education
E vs. N	.064***	.029**	.052***	.010	.061***	.066***
	(.017)	(.014)	(.019)	(.023)	(.018)	(.017)
E vs. B	.007	.001	.002	-.036	-.002	.013
	(.018)	(.014)	(.020)	(.023)	(.019)	(.019)
E vs. S	.059**	-.039*	.052*	.023	.068**	.053**
	(.025)	(.021)	(.027)	(.033)	(.028)	(.026)
E vs. H	.070*	.003	.064	.023	.065	.074*
	(.039)	(.031)	(.045)	(.055)	(.055)	(.042)
E vs. G	.010	-.027**	.001	-.020	.007	.015
	(.017)	(.013)	(.020)	(.039)	(.019)	(.020)
E vs. V	.020	.016	.007	-.015	.022	.019
	(.015)	(.012)	(.017)	(.030)	(.016)	(.016)
N vs. E	.039	-.011	.014	.041	.013	.050**
	(.025)	(.020)	(.027)	(.029)	(.028)	(.025)
N vs. B	.056**	.055**	.060*	.035	.027	.068**
	(.028)	(.023)	(.033)	(.031)	(.036)	(.029)
N vs. S	.075***	.023	.048	.075**	.055*	.084***
	(.028)	(.023)	(.032)	(.030)	(.031)	(.028)
N vs. H	.060	.010	-.012	.071*	.108**	.036
	(.037)	(.030)	(.063)	(.038)	(.045)	(.041)
N vs. G	.031	-.050	.106	-.065	.096	-.063
	(.052)	(.042)	(.074)	(.061)	(.061)	(.072)
N vs. V	-.032	-.051	-.073	-.024	-.010	-.040
	(.040)	(.033)	(.055)	(.044)	(.050)	(.044)
B vs. E	.046**	.014	.045*	.020	.020	.065***
	(.021)	(.017)	(.024)	(.024)	(.022)	(.022)
B vs. N	.091***	.071***	.102***	.066***	.068***	.105***
	(.017)	(.014)	(.021)	(.019)	(.019)	(.018)
B vs. S	.053***	.025*	.076***	.035*	.040**	.061***
	(.016)	(.014)	(.019)	(.018)	(.017)	(.017)
B vs. H	-.008	.016	.003	-.015	-.007	-.007
	(.018)	(.014)	(.026)	(.019)	(.018)	(.019)
B vs. G	-.011	-.010	.021	-.033***	-.017	-.003
	(.010)	(.008)	(.015)	(.012)	(.011)	(.011)
B vs. V	-.016	-.022**	-.008	-.027**	-.021*	-.007
	(.011)	(.009)	(.016)	(.014)	(.012)	(.012)
S vs. E	-.072***	-.069***	-.097***	-.069**	-.054*	-.078***
	(.026)	(.022)	(.033)	(.029)	(.031)	(.028)
S vs. N	.016	.022	-.001	.012	.016	.017
	(.018)	(.015)	(.024)	(.019)	(.020)	(.019)
S vs. B	-.066***	-.013	-.085***	-.060***	-.071***	-.062***
	(.014)	(.011)	(.019)	(.015)	(.015)	(.014)
S vs. H	-.030*	-.000	-.087***	-.026	-.020	-.035**
	(.017)	(.014)	(.024)	(.018)	(.018)	(.018)
S vs. G	-.073***	-.058***	-.099***	-.067***	-.067***	-.078***
	(.013)	(.011)	(.021)	(.015)	(.015)	(.015)
S vs. V	-.094***	-.063***	-.207***	-.075***	-.070***	-.111***
	(.016)	(.014)	(.030)	(.018)	(.019)	(.018)
H vs. E	.032	-.076	-.054	.019	.011	.057
	(.141)	(.118)	(.558)	(.115)	(.164)	(.179)
H vs. N	-.025	-.019	-.260**	.022	-.020	-.029
	(.039)	(.032)	(.104)	(.038)	(.055)	(.044)
H vs. B	-.124***	-.054***	-.196***	-.113***	-.125***	-.125***
	(.021)	(.017)	(.043)	(.022)	(.023)	(.023)
H vs. S	-.046**	-.047***	-.163***	-.032	-.035	-.056***
	(.021)	(.018)	(.031)	(.022)	(.022)	(.021)
H vs. G	-.100***	-.073***	-.149*	-.089***	-.052	-.147***
	(.028)	(.023)	(.077)	(.029)	(.036)	(.030)
H vs. V	-.111***	-.091***	-.403***	-.091***	-.137***	-.085**
	(.031)	(.026)	(.149)	(.031)	(.038)	(.036)
Corr.	.83		.77		.91	
N	233,034	220,360	233,034		232,882	

*Notes: See notes to Table 4 and text for details. Standard errors in parentheses. Notes: N males = 113,893, females = 119,195, low-skilled parents = 95,825 and high-skilled parents = 136,654. * p<.10, ** p<.05, *** p<.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	--	.024*	.024*	.031	-.014	.029**	.016
		(.013)	(.013)	(.020)	(.030)	(.012)	(.011)
Natural Science	.013	--	.006	-.010	-.020	.009	.030
	(.019)		(.022)	(.022)	(.031)	(.040)	(.033)
Business	.003	-.003	--	-.012	-.036**	.009	-.005
	(.015)	(.013)		(.013)	(.016)	(.008)	(.009)
Social Science	-.008	.015	.003	--	-.031**	.024**	.007
	(.018)	(.014)	(.011)		(.016)	(.011)	(.013)
Humanities	-.054	-.014	.008	.006	--	.036	.024
	(.100)	(.032)	(.019)	(.020)		(.027)	(.029)

Notes: N = 250,522. 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<.10, ** p<.05, *** p<.01*

Table A7. Multiple inference correction.

	Second choice						
First choice	Engineering	Natural Science	Business	Social Science	Humanities	Non-acad. General	Non-acad. Vocational
				Panel A: q-values after FDR control for Table 4			
Engineering	--	<.001	.699	.073	.184	.625	.107
Natural Science	.177	--	.020	.011	.134	.625	.386
Business	.018	<.001	--	<.001	.482	.261	.177
Social Science	.001	.482	<.001	--	.172	<.001	<.001
Humanities	.747	.482	<.001	.017	--	.007	.007
				Panel B: q-values after FDR control for Table 5			
Engineering	--	<.001	.708	.046	.134	.616	.246
Natural Science	.195	--	.096	.019	.169	.616	.528
Business	.068	<.001	--	.004	.708	.355	.244
Social Science	.019	.514	<.001	--	.146	<.001	<.001
Humanities	.818	.612	<.001	.068	--	.001	.001

Notes: The table reports multiple inference corrected q-values (False Discovery Rate control) using the qqvalue package in Stata (method: simes).

Table A8. Estimates using alternative specifications.

	Baseline	OLS (w/o GPA)	OLS (w/ GPA)	KLM IV
E vs. N	.065***	.015***	.041***	.027*
	(.017)	(.004)	(.004)	(.016)
E vs. B	.007	.076***	.047***	-.001
	(.018)	(.002)	(.002)	(.017)
E vs. S	.059**	.135***	.130***	.052**
	(.025)	(.003)	(.003)	(.024)
E vs. H	.070*	.195***	.172***	.068*
	(.039)	(.004)	(.004)	(.039)
E vs. G	.010	.222***	.125***	.015
	(.017)	(.004)	(.004)	(.016)
E vs. V	.020	.230***	.137***	.030**
	(.015)	(.003)	(.004)	(.014)
N vs. E	.039	-.015***	-.041***	-.042
	(.025)	(.004)	(.004)	(.027)
N vs. B	.056**	.061***	.006	.054**
	(.028)	(.003)	(.004)	(.027)
N vs. S	.075***	.120***	.090***	.078***
	(.028)	(.004)	(.004)	(.027)
N vs. H	.060	.180***	.131***	.054
	(.037)	(.004)	(.004)	(.034)
N vs. G	.031	.207***	.084***	.066
	(.052)	(.004)	(.005)	(.046)
N vs. V	-.032	.215***	.096***	.004
	(.040)	(.004)	(.005)	(.035)
B vs. E	.046**	-.076***	-.047***	-.009
	(.021)	(.002)	(.002)	(.022)
B vs. N	.091***	-.061***	-.006	.062***
	(.017)	(.003)	(.004)	(.016)
B vs. S	.053***	.059***	.084***	.056***
	(.016)	(.002)	(.002)	(.016)
B vs. H	-.008	.119***	.125***	.001
	(.018)	(.003)	(.003)	(.018)
B vs. G	-.011	.146***	.079***	-.008
	(.010)	(.003)	(.004)	(.009)
B vs. V	-.016	.154***	.091***	-.002
	(.011)	(.003)	(.003)	(.010)
S vs. E	-.072***	-.135***	-.130***	-.143***
	(.026)	(.003)	(.003)	(.028)
S vs. N	.016	-.120***	-.090***	-.033*
	(.018)	(.004)	(.004)	(.017)
S vs. B	-.066***	-.059***	-.084***	-.073***
	(.014)	(.002)	(.002)	(.014)
S vs. H	-.030*	.060***	.041***	-.030*
	(.017)	(.003)	(.003)	(.017)
S vs. G	-.073***	.087***	-.005	-.074***
	(.013)	(.004)	(.004)	(.012)
S vs. V	-.094***	.095***	.007*	-.073***
	(.016)	(.003)	(.004)	(.015)
H vs. E	.033	-.195***	-.172***	-.343***
	(.140)	(.004)	(.004)	(.131)
H vs. N	-.025	-.180***	-.131***	-.092***
	(.039)	(.004)	(.004)	(.035)
H vs. B	-.124***	-.119***	-.125***	-.128***
	(.021)	(.003)	(.003)	(.020)
H vs. S	-.046**	-.060***	-.041***	-.055***
	(.021)	(.003)	(.003)	(.020)
H vs. G	-.100***	.027***	-.046***	-.090***
	(.028)	(.004)	(.004)	(.026)
H vs. V	-.111***	.035***	-.034***	-.077***
	(.031)	(.004)	(.004)	(.029)
Corr. w/ baseline	1.00	.13	.44	.89
N	233,034	233,034	233,034	233,034

Notes: See notes to Table 4 and text in Section 4.4 for details. Standard errors in parentheses.
** p<.10, ** p<.05, *** p<.01*

Table A9. Mediation analysis.

	Baseline	Controls for years of schooling	Share explained	Controls for college major	Share explained	Controls for occupation	Share explained	All controls	Share explained
Panel A: Significant baseline estimates									
E vs. N	.064***	.048***	0.25	.052***	0.19	.023	0.64	.013	0.80
	(.017)	(.017)		(.017)		(.014)		(.015)	
E vs. S	.059**	.040	0.32	.065***	-0.10	.034	0.42	.030	0.49
	(.025)	(.025)		(.024)		(.021)		(.021)	
E vs. H	.070*	.055	0.21	.078**	-0.11	.016	0.77	.019	0.73
	(.039)	(.038)		(.037)		(.032)		(.031)	
N vs. B	.056**	.033	0.41	.047*	0.16	.039*	0.30	.031	0.45
	(.028)	(.028)		(.027)		(.023)		(.023)	
N vs. S	.075***	.047*	0.37	.072***	0.04	.057**	0.24	.054**	0.28
	(.028)	(.028)		(.027)		(.023)		(.023)	
B vs. E	.046**	.044**	0.04	.014	0.70	.019	0.59	.007	0.85
	(.021)	(.021)		(.021)		(.018)		(.017)	
B vs. N	.091***	.090***	0.01	.054***	0.41	.034**	0.63	.022	0.76
	(.017)	(.017)		(.017)		(.014)		(.014)	
B vs. S	.053***	.052***	0.02	.038**	0.28	.020	0.62	.015	0.72
	(.016)	(.016)		(.016)		(.014)		(.013)	
S vs. E	-.072***	-.081***	-0.13	-.065**	0.10	-.027	0.63	-.031	0.57
	(.026)	(.026)		(.025)		(.022)		(.022)	
S vs. B	-.066***	-.070***	-0.06	-.040***	0.39	-.026**	0.61	-.024**	0.64
	(.014)	(.014)		(.014)		(.012)		(.012)	
S vs. H	-.030*	-.035**	-0.17	-.002	0.93	-.026*	0.13	-.017	0.43
	(.017)	(.017)		(.017)		(.014)		(.014)	
S vs. G	-.073***	-.088***	-0.21	-.043***	0.41	-.022**	0.70	-.022**	0.70
	(.013)	(.013)		(.013)		(.011)		(.011)	
S vs. V	-.094***	-.112***	-0.19	-.038**	0.60	-.036***	0.62	-.031**	0.67
	(.016)	(.016)		(.016)		(.013)		(.013)	
H vs. B	-.124***	-.122***	0.02	-.072***	0.42	-.063***	0.49	-.055***	0.56
	(.021)	(.021)		(.020)		(.017)		(.017)	
H vs. S	-.046**	-.053**	-0.15	-.007	0.85	-.020	0.57	-.012	0.74
	(.021)	(.021)		(.020)		(.017)		(.017)	
H vs. G	-.100***	-.109***	-0.09	-.060**	0.40	-.052**	0.48	-.048**	0.52
	(.028)	(.028)		(.027)		(.023)		(.023)	
H vs. V	-.111***	-.124***	-0.12	-.051*	0.54	-.065**	0.41	-.057**	0.49
	(.031)	(.031)		(.030)		(.026)		(.026)	
Share explained >.50			0		5		10		12

Table A9. Mediation analysis, continued.

	Baseline	Controls for years of schooling	Share explained	Controls for college major	Share explained	Controls for occupation	Share explained	All controls	Share explained
Panel B: Insignificant baseline estimates									
E vs. B	.007	-.005	-	.013	-	-.002	-	-.010	-
	(.018)	(.018)		(.018)		(.015)		(.015)	
E vs. G	.010	-.005	-	.039**	-	-.004	-	-.003	-
	(.017)	(.017)		(.017)		(.015)		(.015)	
E vs. V	.020	-.003	-	.038***	-	-.003	-	-.006	-
	(.015)	(.015)		(.015)		(.013)		(.013)	
N vs. E	.039	.009	-	.014	-	.030	-	.019	-
	(.025)	(.025)		(.025)		(.021)		(.021)	
N vs. H	.060	.036	-	.063*	-	.024	-	.027	-
	(.037)	(.036)		(.034)		(.030)		(.029)	
N vs. G	.031	.012	-	.059	-	.026	-	.030	-
	(.052)	(.051)		(.050)		(.043)		(.043)	
N vs. V	-.032	-.068*	-	.005	-	-.011	-	-.010	-
	(.040)	(.039)		(.038)		(.033)		(.032)	
B vs. H	-.008	.003	-	-.001	-	-.026*	-	-.019	-
	(.018)	(.017)		(.017)		(.014)		(.014)	
B vs. G	-.011	-.009	-	-.005	-	-.020**	-	-.018**	-
	(.010)	(.010)		(.010)		(.008)		(.008)	
B vs. V	-.016	-.017	-	-.009	-	-.025***	-	-.024***	-
	(.011)	(.011)		(.011)		(.009)		(.009)	
S vs. N	.016	.005	-	.018	-	.009	-	.007	-
	(.018)	(.018)		(.018)		(.015)		(.015)	
H vs. E	.032	.020	-	-.064	-	-.026	-	-.043	-
	(.141)	(.139)		(.135)		(.113)		(.112)	
H vs. N	-.025	-.034	-	.003	-	-.011	-	-.006	-
	(.039)	(.038)		(.036)		(.032)		(.031)	
N	233,034	233,034		232,462		233,034		232,462	

Notes: "Share explained" is defined as 1 – [baseline estimate / (baseline estimate - mediation estimate)]. Panel A reports the estimates and the share explained for baseline estimates which are statistically different from zero. Share explained is not reported in panel B for the insignificant estimates, as it provides little insight for small and noisy estimates. Standard errors in parentheses.

** p<.10, ** p<.05, *** p<.01*

Table A10. Mechanisms: Years of schooling, college major, and occupation.

	Dependent variable: Baseline estimates			
Expected return due to:				
Years of schooling	.516*	-	-	.026
	(.282)			(.088)
College major	-	.954***	-	.360***
		(.132)		(.087)
Occupation	-	-	1.410***	1.099***
			(.094)	(.096)
R^2	.107	.652	.890	.945

Notes: We regress the thirty baseline estimates from Table 5 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<.10, ** p<.05, *** p<.01*