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High School Majors and Future Earnings†

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We study how high school majors affect adult earnings using a regression discontinuity design. In Sweden students are admitted to majors in tenth grade based on their preference rankings and ninth grade GPA. We find engineering, natural science, and business majors yield higher earnings than social science and humanities, with major-specific returns also varying based on next-best alternatives. There is either a zero or a negative return to completing an academic program for students with a second-best nonacademic major. Most of the differences in adult earnings can be attributed to differences in occupation, and to a lesser extent, college major. (*JEL* I21, I26, J24, J31)

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 that 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.

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 endogenously sort into majors. The problem is compounded by the fact that students have

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[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.

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 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 and 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 with very different curricula and which take at least three years to complete: engineering, natural science, business, social science, and humanities. In addition to these majors, which comprise roughly half of applicants, there are nonacademic two year programs. We focus on gaining admission to first-choice academic programs since admission into nonacademic programs was most often not limited. Hence, we use our RD research design to study individuals who have first-best academic choices and either a second-best academic or nonacademic choice.

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 completing different academic majors, (ii) the role of next-best choices, (iii) the benefits of academic versus vocational majors, and (iv) mechanisms related to future college major and occupation.

[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.

Our first empirical finding is that the earnings returns for some academic majors are generally positive, while they are generally negative for others. For example, the returns to engineering range from 0.7 percent to 7.0 percent, depending on an individual's next-best alternative field of study, while the returns to social science range from −9.4 percent to 1.6 percent. Earnings payoffs are 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 nonacademic programs where the earnings losses exceed 7 percent. The pattern of returns is consistent with individuals pursuing comparative advantage in earnings when first- and second-best choices include engineering, natural science, or business, while comparative disadvantage in earnings occurs with humanities.

Second, earnings payoffs vary substantially based on next-best alternatives. For example, there is a 9.1 percent return to completing business relative to a second-best choice of natural science, but essentially no return to completing business (−0.8 percent) 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. Our baseline earnings 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 nonacademic majors for marginal students. The estimated returns to completing a three-year academic program when the next best alternative is a two-year nonacademic program are either close to zero or negative. These results run opposite population-wide comparisons, which show substantially higher average earnings for academic versus nonacademic majors (except for humanities, where there is no difference). It is possible that marginal students have family backgrounds that make it harder to succeed in an academic major. It is also possible that these marginal students could struggle in an academic program that is not designed for their GPA level, but thrive in an environment where their relative ranking is higher and the academic requirements are lower. We find some empirical evidence for both of these explanations.

Fourth, most of the differences in adult earnings across high school majors can be explained by differences in occupation and, to a lesser extent, in college majors. 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 that 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 an RD design.[4] 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 15 or 16. Moreover, we find the returns to different high school majors is not primarily due to the pursuit of different college degrees but rather to 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 have a systematic ordering where some majors always require higher GPAs for admission, either within or across school regions. 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; Zilic 2018; 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 nonmonetary 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 percent 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

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

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, which ignores nonpecuniary factors, our results argue for (i) an expansion of business and STEM fields and a contraction of social science and humanities, and (ii) not pushing all students into academic studies over vocational programs.[5]

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 II discusses identification. Section III presents our results, and Section IV explores possible mechanisms. The final section concludes.

I. Setting and Data

A. *Academic and Nonacademic 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.[6] 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, has 25 percent 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

[5] 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.

[6] During the nine years of compulsory schooling, there is little specialization, except for the last three years where 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.

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 three 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.

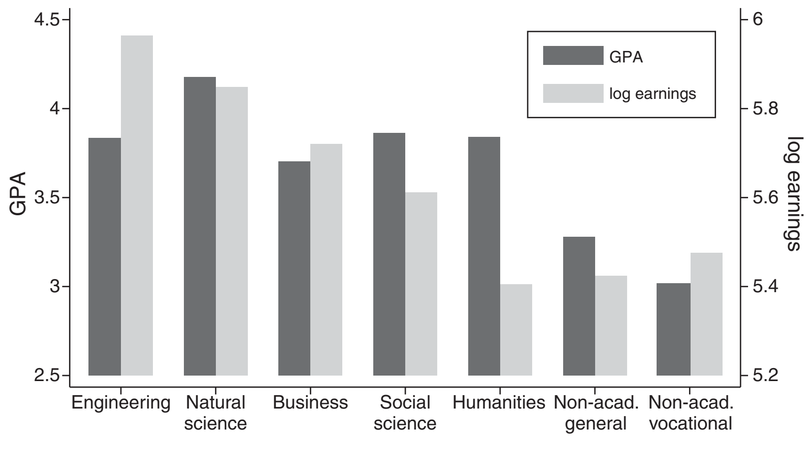

Figure 1. Ninth Grade Unadjusted GPA and Adult Earnings for Program Completers

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.

arts classes. Languages comprise 35 percent of the curriculum for social studies and 43 percent for humanities.[7]

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

[7] 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).

with higher average GPAs and higher average earnings. Students completing natural science have the highest GPAs, while those pursuing nonacademic vocational programs have the lowest. Earnings are highest for engineering and lowest for humanities.

In addition to these five academic programs, there were between 17–21 nonacademic programs offered. These nonacademic programs took two years to complete. There were 14–18 vocational programs aimed at preparing students for a career, and three general programs that 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, which is designed for students with lower GPAs, and are in a completely separate set of classes. Nonacademic graduates have the option to pursue community college-type programs or to attend adult education classes to become eligible for university studies (see Stenberg 2011).

Online Appendix Figure A1 displays the number of students admitted to each of the five academic majors plus the two aggregated nonacademic programs. Roughly half of the students are admitted to an academic major, with engineering and business being the most popular. The vast majority of individuals in nonacademic majors are in vocational as opposed to general programs.

We focus on the period 1977–1991 because the academic and nonacademic 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; nonacademic vocational programs were lengthened to three years; and nonacademic 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 to the same major offered by different schools, or in other municipalities, and substantially reduced the number of oversubscribed programs.

B. *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 nonacademic program). Applicants received a bonus of 0.2 to their GPA for being a minority gender applicant, defined as applying to a major that in the prior year had accepted less than 30 percent 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 six 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.[8]

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 ID.

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 social science, 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 an RD design to instrument for program completion. We can also use the sharp cutoff in admission decisions to estimate the effect of admission itself.

[8] In theory it is possible that only allowing six choices causes individuals to put a safe option down as their sixth choice, so as to make sure they get into at least one program. This seems unlikely in our setting, as only 0.2 percent of all applicants are admitted to their sixth choice (and only 1.0 percent even list a sixth choice). During the years 1982–1984, 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 online Appendix A).

C. *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 II, this information is vital for identifying an interpretable causal effect.[9]

During our sample period, the number of applications to high school increased. In 1977 only 60 percent of the ninth-grade cohort applied to high school, but by 1991 this had risen to 80 percent. Summed over all years, the population of first-time applicants between 1977 and 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.[10] 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 percent 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 percent of individuals who are admitted to a third- or lower-ranked choice, the preferred choice is defined as the choice with the lowest GPA cutoff above their accepted choice, and the next-best alternative as their accepted choice.[11] 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.

The number of individuals with each combination of first- and second-best choices in our baseline sample can be found in online 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 combi-

[9] This data was used in a government report from 1992 but had been reported as lost by the Swedish National Archives. We thank Hans-Eric Ohlson at Statistics Sweden for helping us locate the data.

[10] We further exclude individuals with GPAs at the cutoff where this is a mix of accepted and nonaccepted 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 1982–1984 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 8).

[11] 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 cutoff choice above their accepted choice. Using this alternative does not materially affect any of our results.

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	0.55	0.42
Natural science	18,830	395	50,583	1,457	0.27	0.21
Business	84,141	1,030	35,469	815	0.70	0.56
Social science	52,465	873	32,120	970	0.62	0.47
Humanities	13,988	396	23,681	1,467	0.37	0.21
Total	233,034	3,487	194,024	5,788	0.55	0.38

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. Nonimpacted programs do not have an excess supply of applicants, and so have unrestricted entry.

nations are those that 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 percent (engineering), 21 percent (natural science), 56 percent (business), 47 percent (social science), and 21 percent (humanities).

Using personal identification numbers, we link individual's field choice rankings and GPAs to population register data, which contains information on annual earnings for all individuals living in Sweden in a given year. Of the individuals we observe at age 15 or 16 when they make their schooling decisions, about 5 percent are no longer part of the Swedish population at ages 37–39 since they have either emigrated to another country or have died. Our main earnings measure takes the natural log of average earnings between the ages of 37–39, restricting the average to years in which individuals earn more than a minimal amount.[12] We take an average over three years to minimize measurement error and focus on years in which individuals have nontrivial labor force participation to get a better measure of earnings potential. This results in a sample that includes 93 percent of all individuals in the population, of which 87 percent are observed in all three years, consistent with individuals between ages 37–39 being in the prime of their working careers and, hence, having high attachment to the labor market.

The register data also includes information on socioeconomic background characteristics. Summary statistics for these predetermined parent and child

[12] Antelius and Björklund (2000) use a 100,000 SEK threshold, which translates into roughly US$12,000. We apply their threshold, accounting for wage growth and inflation, to other years in our sample. Antelius and Björklund find that using this threshold makes Mincer estimates using log annual earnings similar to those using log hourly wages in Sweden. We use the ages 37 to 39 primarily because this is the prime of an individual's working career but also because it 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.

characteristics are found in online 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. Online 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.

D. *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 is not.[13]

One wrinkle is that there can be a mix of accepted and nonaccepted 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 that indicated admission was random, but also documentation that said 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 nonaccepted 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

[13] 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 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 10 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; the observations that 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.

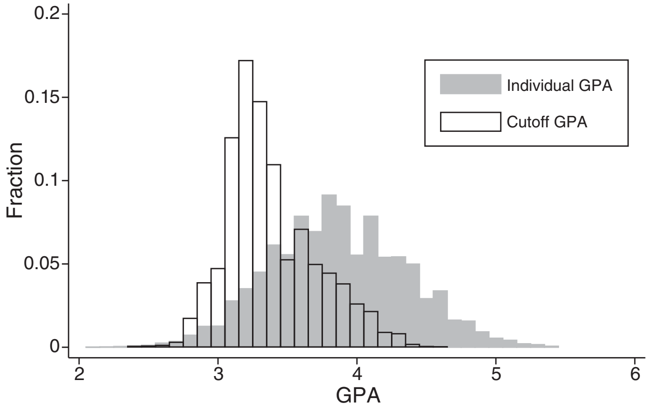

Figure 2. Cutoff GPA versus Individual GPA Distributions

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 the baseline sample. The gray bars plot the distribution of GPA for individuals in the baseline sample of 233,034 observations.

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 twenty-first 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 sixty-third 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. As online Appendix Figure A3 shows, the distribution of 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.[14]

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 percent of years within the same school region on average, while the reverse is true in 25 percent of years. In 38 percent of years both programs either have open

[14] We further note that most individuals have GPAs above the admissions threshold for their second-best choice. The fraction of students with GPAs above the cutoff for their second-best choice, by first-choice major, are 95 percent (engineering), 97 percent (natural science), 92 percent (business), 96 percent (social science), and 90 percent (humanities).

enrollment, or less commonly, identical cutoffs. Similar patterns are found for the other major combinations as reported in online Appendix Table A3.[15]

These facts regarding the major cutoffs are useful 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.

II. Identification

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. (2016) go on to discuss what IV (and by extension fuzzy RD) can and cannot identify when next-best alternatives are not observed. A randomly assigned cutoff for each major will eliminate selection bias, but without restrictive assumptions, RD will not estimate the return to any individual or group who choose one major over another. When next-best alternatives are available, however, RD can estimate the local average treatment effect (LATE) for each preferred versus next-best field (for further details applied to our setting, see Dahl et al. 2020).

A. *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 $j = 1, \ldots, J$ and $k = 1, \ldots, K$, 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 as follows:

$$y_{jk} = \sum_{jk} a_{jk} \mathbf{1}\left[x < c_j\right] g_{jk}^{l}(c_j - x) + \sum_{jk} a_{jk} \mathbf{1}\left[x > c_j\right] g_{jk}^{r}(x - c_j) + \sum_{jk} a_{jk} \mathbf{1}\left[x > c_j\right] \theta_{jk} + \alpha_{jk} + w'\gamma + e_{jk}, \tag{1}$$

where we have omitted the individual subscript for convenience. The running variable x is an individual's GPA, c_j is the cutoff GPA for admission to major j, g_{jk}^{l} are

[15] Online Appendix Figure A4 graphs the entire distribution of the within school region variation over time in relative major cutoffs.

unknown functions to the left of the cutoffs, g_{jk}^r 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 predetermined 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. We note that we estimate all of the margins in a single regression to increase precision.

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 either 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_{jk}^l and g_{jk}^r 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 two 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 five preferred choices (regardless of the next-best choice), and common slopes to the left of the cutoff for each of the seven 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. Removing this parametric assumption yields similar results, but with somewhat 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_{jk}^l and g_{jk}^r 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. The relevant first stage for this fuzzy RD captures the jumps induced by the admission cutoffs in completion probabilities for each combination of preferred and next best fields. When estimating the first stage, we use the same parametric functional form imposed in the reduced form.

B. *Threats to Validity*

Manipulation.—In our setting, the assumption of no perfect manipulation is that students cannot adjust their GPA to be just to the right of the cutoff for their

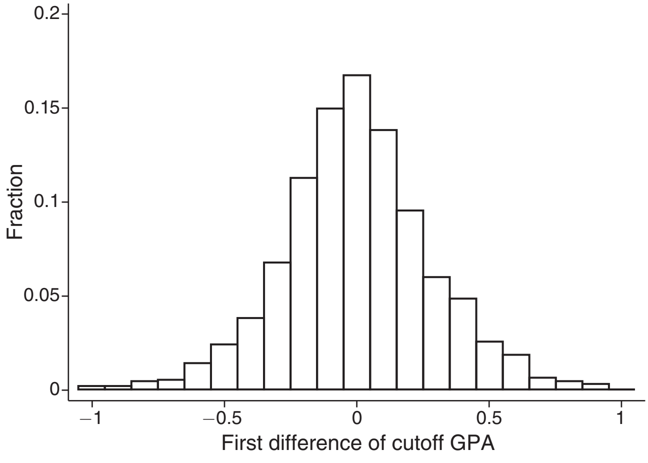

Figure 3. First-Differenced Cutoff GPA Distribution

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

preferred major. While it is possible to study harder and get higher grades, the required GPA to get accepted into a program is not known in advance, and varies from year to year. Figure 3 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 percent of the time.

As a test for manipulation, online Appendix Figure A5 checks whether predetermined characteristics are balanced around the admission cutoff. There are no discernible jumps at any of the cutoffs, and none of the corresponding estimates appearing in online Appendix Table A4 are 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.[16]

Monotonicity, *Exclusion*, *and Irrelevance*.—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.

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

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 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 there is only a small fraction switching, and based on anecdotal evidence, it is likely most of this switching takes place in the early fall of the first year due to the specialized nature of different curricula, which makes it difficult to switch programs later on (see Table 1).[17] 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 percent of students). We do find small effects of getting into a first-best choice on dropping out or switching to the nonacademic track, but they are not large enough to have a sizable impact on our estimates.[18] When we rerun our analysis excluding those who drop out or switch to the nonacademic track, none of the resulting estimates are statistically different from the baseline.

Finally, we require the irrelevance condition discussed in Kirkeboen et al. (2016), which 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. While this condition seems plausible in our setting, it is possible that it does not hold perfectly 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 is not.

III. 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.

A. *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.

[17] About 9 percent of individuals in our baseline sample switch from the major they are initially admitted to and complete another major (4.7 percent complete a nonacademic and 4.6 percent a different academic major). Switching rates vary somewhat by major: 11 percent (engineering), 13 percent (natural science), 6 percent (business), 9 percent (social science), and 16 percent (humanities).

[18] Using our baseline specification, we find a 0.9 percentage point increase (SE $=$ 0.3) in dropping out of high school and a 1.7 percentage point decrease (SE $=$ 0.6) in the probability of switching to the nonacademic track.

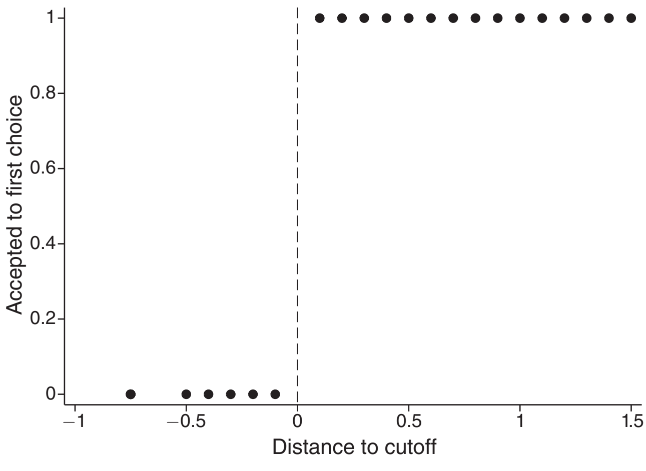

FIGURE 4. DISCONTINUITY IN ADMISSIONS AS A FUNCTION OF GPA

Notes: Each dot is the average acceptance rate in a 0.1 GPA bin, except for the leftmost dot, which is a 0.5 bin due to sparsity. GPA is measured relative to a normalized cutoff of 0. Baseline sample of 233,034 individuals.

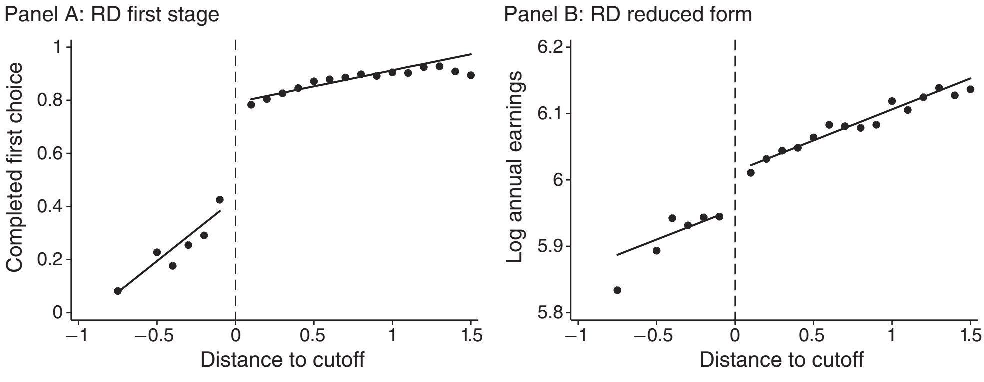

FIGURE 5. EXAMPLE OF ENGINEERING FIRST CHOICE VERSUS NATURAL SCIENCE SECOND CHOICE

Notes: Each dot is the average acceptance rate in a 0.1 GPA bin, except for the leftmost dot, which is a 0.5 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. $N = 31{,}910$.

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. Panel A 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 percent 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 that 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 percent. 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).

To begin, we use the baseline parameterization for our first stage, 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 IIA. 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 percent, it is only 25 percent 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.

B. *High School Majors 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.

Panel B 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 IC. 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.

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	—	0.345	0.401	0.248	0.255	0.386	0.395
		(0.010)	(0.011)	(0.015)	(0.029)	(0.011)	(0.009)
Natural science	0.397	—	0.424	0.338	0.317	0.292	0.309
	(0.016)		(0.018)	(0.017)	(0.026)	(0.032)	(0.025)
Business	0.469	0.458	—	0.468	0.431	0.530	0.512
	(0.014)	(0.012)		(0.012)	(0.012)	(0.007)	(0.008)
Social science	0.376	0.399	0.503	—	0.377	0.448	0.426
	(0.017)	(0.012)	(0.010)		(0.011)	(0.009)	(0.012)
Humanities	−0.098	0.212	0.434	0.369	—	0.287	0.270
	(0.027)	(0.025)	(0.015)	(0.014)		(0.016)	(0.019)

Notes: Baseline sample of 233,034 individuals. 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 online Appendix Table A2 (except for GPA, which, when normalized is the running variable). Standard errors in parentheses.

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	—	0.033	0.004	0.026	0.031	0.005	0.014
		(0.008)	(0.010)	(0.012)	(0.021)	(0.009)	(0.007)
Natural science	0.022	—	0.043	0.043	0.039	0.014	−0.020
	(0.014)		(0.016)	(0.015)	(0.022)	(0.025)	(0.019)
Business	0.034	0.066	—	0.035	−0.008	−0.008	−0.010
	(0.013)	(0.011)		(0.009)	(0.010)	(0.006)	(0.007)
Social science	−0.056	0.008	−0.043	—	−0.014	−0.043	−0.057
	(0.016)	(0.010)	(0.009)		(0.009)	(0.007)	(0.009)
Humanities	0.008	−0.018	−0.079	−0.030	—	−0.037	−0.043
	(0.024)	(0.022)	(0.012)	(0.011)		(0.012)	(0.014)

Notes: Baseline sample of 233,034 individuals. 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 online 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 IIC for details). Standard errors in parentheses.

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 IIA. 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.

Table 5—Returns to Different High School Majors: Fuzzy RD Estimates of Program Completion on log Earnings

	Second choice						
First choice	Engineering	Natural science	Business	Social science	Humanities	Non-acad. general	Non-acad. vocational
Panel A. Estimates							
Engineering	—	0.064	0.007	0.059	0.070	0.010	0.020
		(0.017)	(0.018)	(0.025)	(0.039)	(0.017)	(0.015)
Natural science	0.039	—	0.056	0.075	0.060	0.031	−0.032
	(0.025)		(0.028)	(0.028)	(0.037)	(0.052)	(0.040)
Business	0.046	0.091	—	0.053	−0.008	−0.011	−0.016
	(0.021)	(0.017)		(0.016)	(0.018)	(0.010)	(0.011)
Social science	−0.072	0.016	−0.066	—	−0.030	−0.073	−0.094
	(0.026)	(0.018)	(0.014)		(0.017)	(0.013)	(0.016)
Humanities	0.032	−0.025	−0.124	−0.046	—	−0.100	−0.111
	(0.141)	(0.039)	(0.021)	(0.021)		(0.028)	(0.031)

First choice	All	Acad.	Acad. versus non-ac.
Panel B. F-tests for equality across second choices			
Engineering	16.22	10.49	6.81
	[0.006]	[0.015]	[0.009]
Natural science	11.48	1.75	4.10
	[0.043]	[0.625]	[0.043]
Business	65.17	21.98	38.19
	[0.000]	[0.000]	[0.000]
Social science	44.66	20.86	16.06
	[0.000]	[0.000]	[0.000]
Humanities	17.82	15.22	2.92
	[0.003]	[0.002]	[0.087]

Notes: Baseline sample of 233,034 individuals. See notes to Table 4. Standard errors in parentheses. The *F*-tests in panel B test whether the estimates in each row in panel A are equal to each other. Standard errors in parentheses, *p*-values in brackets.

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. In panel A 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 percent 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 percent per year in Sweden (Meghir and Palme 2005; Black et al. 2018).[19]

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

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

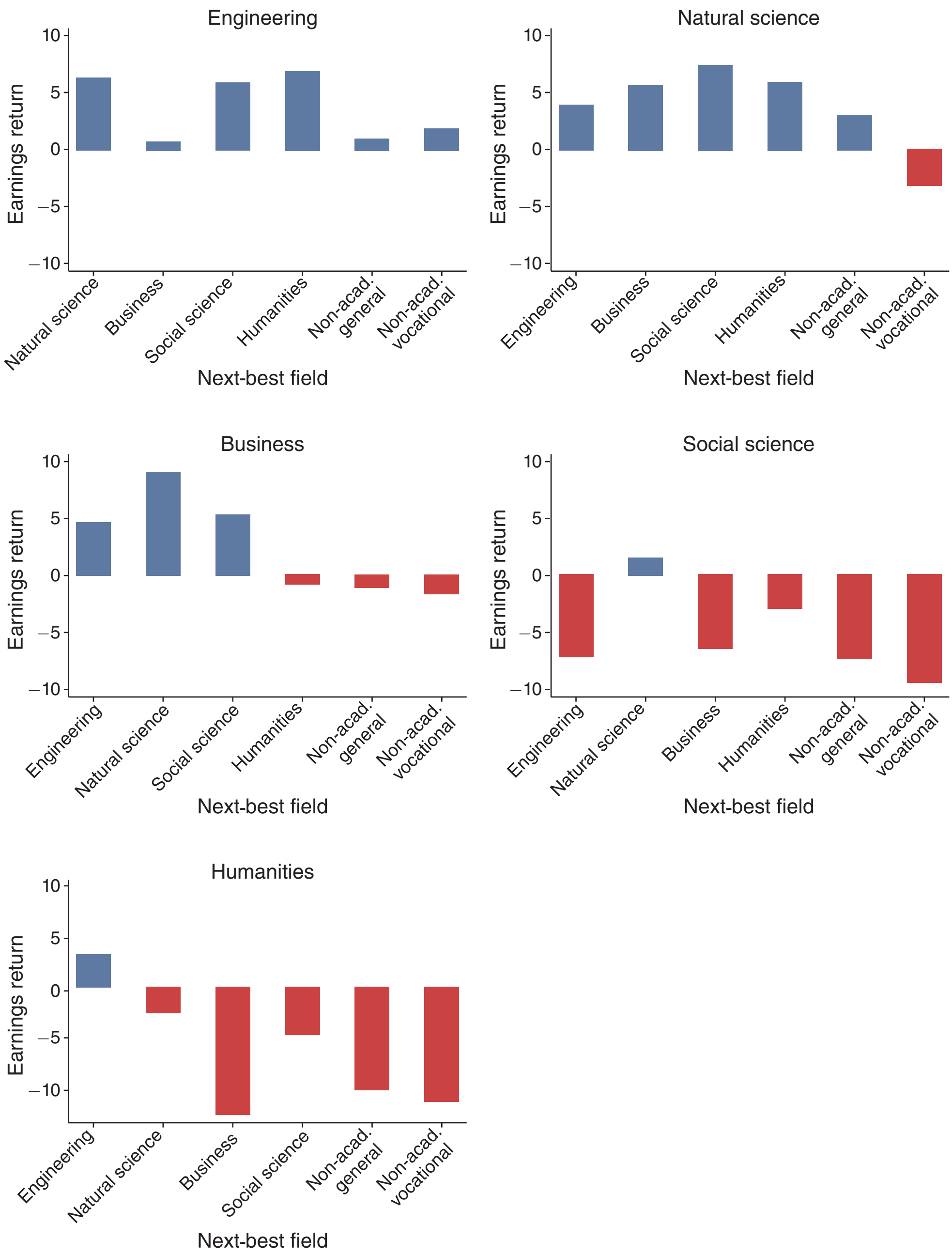

FIGURE 6. EARNINGS RETURN BY FIRST-SECOND CHOICE COMBINATION

Note: Baseline sample of 233,034 individuals.

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 percent to 7.0 percent. 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 nonacademic: the return to completing social science or humanities when the next-best alternative is a nonacademic program exceeds -7 percent.

Second, returns to different fields depend on next-best choices. For example, there is a 9.1 percent 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 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 three-year academic program when the next best alternative is a two-year nonacademic program are either close to zero or negative. This stands in sharp contrast to the population averages appearing in Figure 1, where earnings are higher for academic majors compared to nonacademic majors (except for humanities). We explore various explanations for this finding in Section IIID.

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 first column of panel B in Table 5. For each of the majors, we reject that next-best alternatives do not matter at standard levels of significance.

In the second column of panel B, we test whether there is significant variation in returns across second-best academic choices (ignoring the nonacademic 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 panel B, we test for whether there are average differences in returns for academically inclined students versus nonacademically inclined students, where the two groups are defined by having an academic versus nonacademic second-best choice. For example, for engineering the test is $\left(\hat{\pi}_{EN} + \hat{\pi}_{EB} + \hat{\pi}_{ES} + \hat{\pi}_{EH}\right)/4 = \left(\hat{\pi}_{EG} + \hat{\pi}_{EV}\right)/2$. For each of the five academic first-choice majors, we reject that the average difference is the same for academic and nonacademic second-choices.

Online Appendix Table A5 reports earnings returns 10 years earlier, when individuals are age 27–29. 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. In contrast, the returns to social science and humanities, which generally are negative at age 37–39, are less negative. 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.

Online Appendix Table A5 also reports results by gender and parental education. We apply our baseline specification, but which now also allows for separate cutoff jumps and separate slopes as a function of the running variable for each gender. The returns 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.

Online Appendix A explores robustness for our main estimates using alternative measures of earnings for the outcome variable, different specifications for the RD regression, and multiple inference adjustments. As discussed in detail in online Appendix A and its accompanying tables and figures, the pattern of estimates and their statistical significance remain essentially unchanged.

C. *Tests for Comparative Advantage*

A natural question is whether the findings in Table 5 are consistent with comparative advantage in major choice. 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* (see Sattinger 1993 and Kirkeboen et al. 2016).

Let π_{jk} denote the percent return for an individual who completes first choice *j* with second choice *k*, and similarly let π_{kj} denote the percent return for an individual who completes first choice *k* with second choice *j*. Comparative advantage implies $\pi_{jk} + \pi_{kj} > 0$, or in words, that individuals choose the major within a pair of choices that results in higher earnings for them. Likewise, comparative disadvantage implies $\pi_{jk} + \pi_{kj} < 0$.[20] Random sorting occurs when $\pi_{jk} + \pi_{kj} = 0$, i.e., when the return for individuals completing major *j* with second choice *k* is equal but opposite in sign to the return for individuals completing major *k* with second choice *j*. For further details, see Dahl et al. (2020).

In Table 6 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 percent premium (see Table 5). Looking at the reverse ordering of preferences, the return is 5.6 percent 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. Yet as the first row of Table 6 shows, the sum of the two estimates is 14.7, consistent with the pursuit of comparative advantage.

[20] Comparative disadvantage could happen if individuals value nonpecuniary factors, where the nonpecuniary factors are negatively correlated with a major's potential earnings. This can occur with full information, but it can also be the result of imperfect knowledge about relative payoffs across majors.

Table 6—Tests for Comparative Advantage and Disadvantage

Choice combinations	Sum of returns
Natural Science First – Business Second and Business First – Natural Science Second	0.147 (0.036)
Engineering First – Natural Science Second and Natural Science First – Engineering Second	0.103 (0.034)
Engineering First– Humanities Second and Humanities First – Engineering Second	0.102 (0.148)
Natural Science First – Social Science Second and Social Science First – Natural Science Second	0.091 (0.037)
Engineering First – Business Second and Business First – Engineering Second	0.053 (0.030)
Natural Science First – Humanities Second and Humanities First – Natural Science Second	0.035 (0.056)
Business First – Social Science Second and Social Science First – Business Second	−0.013 (0.024)
Engineering First – Social Science Second and Social Science First – Engineering Second	−0.013 (0.040)
Social Science First – Humanities Second and Humanities First – Social Science Second	−0.076 (0.030)
Business First – Humanities Second and Humanities First – Business Second	−0.131 (0.030)

Notes: Baseline sample of 233,034 individuals. 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.

The other rows in the table report tests for the other major pairs. The major choice combinations that 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. Comparative disadvantage could be explained either due to a lack of information or because students want to "follow their passions," despite this leading to lower wages. One field combination, engineering/humanities, occurs so rarely that although the estimated sum is large, it is not statistically different from zero.

These findings provide 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 nonmonetary 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.

Note that we cannot perform the same tests for comparative advantage for nonacademic majors, as we can only estimate returns for getting into a first-best academic major versus a nonacademic program, and not the other way around. This is because nonacademic majors are rarely oversubscribed, and so we cannot use an RD design to estimate returns to barely getting into a first-choice nonacademic program.

D. *Academic versus Nonacademic Returns*

For students with a second-best nonacademic choice, we find returns near zero for either engineering, natural science, or business, and returns between −7 and −11 percent for social science or humanities. This is not what population average earnings differences would have predicted. As Figure 1 shows, the gap in earnings between academic majors and nonacademic majors is large and positive, except for humanities, where it is close to zero. In this subsection we explore three possible reasons for the lack of a positive earnings return for first-best academic majors relative to nonacademic second-best choices.

A first possible explanation for why our estimates diverge from population average earnings differences is that our RD estimates capture the effect for individuals with lower GPAs. As a reminder, the average cutoff for entry into one of the academic majors is 3.44. In online Appendix Figure A6, we show the average earnings for each of the majors as we did in Figure 1, but restricting the sample to individuals with a 3.4 or 3.5 GPA. The graph continues to reveal a large earnings difference for engineering, natural science, and business relative to nonacademic programs. There is also some evidence the earnings gap for social science narrows and for humanities it becomes more negative. We conclude this first possible explanation does not drive the lack of a return for engineering, natural science, or business, but that it could be a contributing factor for the negative returns found for social science and humanities.[21]

A second possible explanation is that individuals considering nonacademic versus academic second-best choices have parents with nonacademic backgrounds, who therefore may be less able to help their children succeed if they are accepted into an academic major. We find some evidence for this for students with a GPA of 3.4 or 3.5: those with second-best nonacademic majors have parents with one fewer year of schooling compared to those with second-best academic majors.[22] Other unobservable background characteristics could vary as well, and these could also contribute to the patterns we observe.

A third possibility is that students who barely get into an academic program will be below average compared to their classmates, whereas they would have been above average in a nonacademic program (e.g., Denning et al. forthcoming). These marginal students could struggle in an academic program that is not designed for their GPA level but thrive in an environment where their relative ranking is higher and the academic requirements are lower. There is some evidence for this explanation. Students with a GPA near the average cutoff of 3.44 would be at the twentieth

[21] One might have predicted that graduates from academic versus nonacademic programs start with lower wages but have a steeper trajectory. However, we find little evidence for this. As shown in online Appendix Table A5, there is not much of a difference when looking at age 27–29 versus 37–39 for engineering, natural science, and business, and if anything, an even wider gap in earnings at later ages for social science and humanities.

[22] Students with second-best nonacademic majors have fathers and mothers with 10.9 and 10.6 years of schooling on average compared to 11.9 and 11.6, respectively, for those with second-best academic choices.

percentile of the GPA distribution for academic majors but at the seventy-second percentile for nonacademic majors.

Regardless of the explanation, the results for nonacademic second-best choices are interesting. They suggest that the type of individuals who have second-best nonacademic choices are not making an earnings mistake by pursuing engineering, natural science, or business as their preferred choice. But the type of individuals who have second-best nonacademic choices who prefer Social science or humanities suffer a large earnings loss. This pattern matches up with the negative returns we find for social science or humanities relative to academic second-best choices. These negative returns could be due to a lack of information or students valuing nonpecuniary factors.

E. *Comparison to OLS*

To highlight the twin 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. Online Appendix Table A11 reports OLS estimates that do not take into account an individual's next best choice.[23] We first estimate a model that 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 percent level.

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 online Appendix Table A11. 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 percent, and the return to natural science relative to engineering is −4.1 percent. In contrast, our RD estimates are positive for both of these margins. In summary, OLS yields misleading and incorrect conclusions.

IV. Mechanisms

Section III 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.[24]

[23] 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, we can calculate the returns for each of the 30 pairs of majors.

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

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 business requires three years of study while a vocational program only requires two, this could result in more years of education for individuals who complete the business major. It is also possible that different majors impact the probability of college attendance.

Second, the pattern of earnings we observe in Table 5 could be explained by 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, which could in turn affect future earnings. This channel could impact the 45 percent of individuals in our baseline sample who complete college, but cannot explain differential returns for the remaining 55 percent.

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 (10 categories), college degree type (205 categories), and occupation (319 categories) to see how the estimates are affected. We do this in online Appendix Table A12, adding each set of variables one at a time and then all three jointly. We split the table into two panels: the top for baseline estimates that 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 percent. Adding in college major dummies as mediating variables explains some of the variation, with 5 out of 17 estimates falling by more than 50 percent. The addition of occupation dummies shrinks many of the coefficients, with 10 out of 17 estimates falling by over 50 percent. In the final specification we add all three sets of mediating variables at once. The estimates shrink by between 28 percent and 85 percent, with 12 out of 17 estimates falling by more than 50 percent. 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 that 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.

To understand how we do this, consider the mechanism of occupation. To get an estimate of the predicted mean earnings for each completed high school major 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 that 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).[25] We similarly use these measures as the outcome variables in a RD model which parallels our baseline specification.

To assess the importance of the different mechanisms, we compare each set of estimates against our baseline estimates. In Figure 7 we plot the 30 different baseline estimates against the 30 different years of schooling estimates, the 30 different college major estimates, and the 30 different occupation estimates. To help with interpretation, suppose that each of the dots in the third graph 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 7, 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 percent, the return to earnings we estimated in Table 5 rises by 0.5 percent. Likewise, when the expected returns due to college major or occupation rises by 1 percent, the returns rise by 1.0 percent and 1.4 percent, respectively. Table 7 reports estimates of the corresponding regression lines.[26]

The three mechanisms are not necessarily independent or mutually exclusive. In the final column of Table 7, 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 percent but also remains significant. The R^2 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.

[25] 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.

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

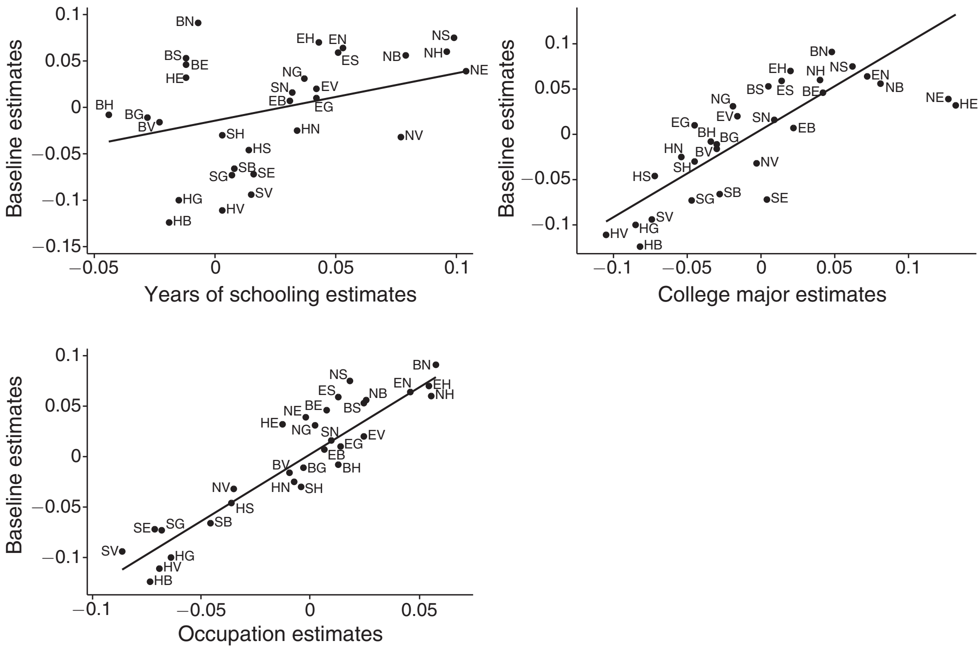

FIGURE 7. MECHANISMS: YEARS OF SCHOOLING, COLLEGE MAJOR, AND OCCUPATION

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 nonacademic, and vocational nonacademic, respectively. The solid line is the regression slope, using the inverse of the squared standard errors of the baseline estimates as weights. See Table 7 and the text for details.

TABLE 7—MECHANISMS: YEARS OF SCHOOLING, COLLEGE MAJOR, AND OCCUPATION

Expected return due to:	Dependent variable: Baseline estimates			
Years of schooling	0.516 (0.282)	—	—	0.026 (0.088)
College major	—	0.954 (0.132)	—	0.360 (0.087)
Occupation	—	—	1.410 (0.094)	1.099 (0.096)
R^2	0.107	0.652	0.890	0.945

Notes: We regress the 30 baseline estimates from Table 5 on 30 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.

V. 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 evidence on how high school majors affect future earnings. Using unique data from Sweden, our analysis yields four 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 nonacademic track for marginal students. Fourth, most of the differences in adult earnings can be attributed to differences in adult occupations and, to a lesser extent, college majors.

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 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. These findings are also 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 on comparative advantage and disadvantage argue against models relying on efficiency units (e.g., the Ben Porath model) or 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.[27] 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 nonpecuniary 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 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 two years of additional schooling. It would be interesting to know if similar patterns hold for other individuals.

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

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