High School Majors and Cognitive Skill Development*

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Abstract

This paper examines the causal effect of high school majors on cognitive skills. We compare students just above and below GPA admission cutoffs using a regression discontinuity design and find that fields of study at age 16 impact cognitive skills approximately two years later. Admission to STEM relative to non-academic programs raises both crystallized and fluid cognitive skills by about 0.05 standard deviations, while admission to business, social sciences, and humanities (BSH) relative to STEM lowers both by 0.10 standard deviations. Analyses of test components indicate that STEM programs particularly foster spatial reasoning and technical problem-solving skills.

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1 Introduction

Cognitive skills have been shown to be important for many outcomes later in life, such as labor market, health and well-being (Heckman et al., 2006; Hanushek and Woessmann, 2008; Lindqvist and Vestman, 2011; Hermo et al., 2022). These skills span from the ability to complete simple and complex tasks to the ability to learn, pay attention and solve problems (Cattell, 1971, 1987). While years of completed schooling have been shown to foster cognitive skills (Cahan and Cohen, 1989; Carlsson et al., 2015; Lundborg et al., 2014), there is limited evidence on whether the content of education – such as STEM, humanities/social sciences, or occupation-specific skills – also affects them. This question speaks directly to whether fields of education differ in how they build productive skills such as language or logic (Mincer, 1958; Becker, 1962, 1964), or instead serve as signals of ability (Spence, 1973). Understanding this can also inform education policy, as in many countries students choose a field of study in high school (Hanushek et al., 2017; OECD, 2023), and high school majors have been shown to affect earnings later in life (Altonji, 1995; Levine and Zimmerman, 1995; Rose and Betts, 2004; Altonji et al., 2012; Dahl et al., 2023; Stenberg and Tudor, 2024).[1]

This paper investigates the causal effect of high school majors on cognitive skill development, using a regression discontinuity design (RD) to compare individuals just above versus just below GPA admission cutoffs for different majors. We can account for individuals' stated first and second best alternatives using register data from all high school applications between the years 1977 and 1991 in Sweden. Using personal identification numbers, we link these data to administrative records for 18-year-old men who are required to complete a series of cognitive tests during military enlistment (Carlsted and Mårdberg, 1993; Lindqvist and Vestman, 2011). These data enable us to consider two dimensions of cognitive skills: fluid and crystallized skills that respectively capture the ability to reason through problems, and the ability to learn and acquire knowledge (Carlsson et al., 2015; Hermo et al., 2022; Cattell, 1971, 1987).

During our study period, students in Sweden enrolled high school at age 16 (10th grade), and then chose between five academic programs – engineering, natural science, business, social science, and humanities. These comprised roughly half of all applicants, with the remainder applying to non-academic programs. Students applying to secondary school programs ranked their preferred programs, and admission to oversubscribed programs was determined solely by cumulative ninth-grade GPA through a centralized strategy-proof allocation mechanism (Svensson, 1999). This system creates quasi-random variation around GPA cutoffs, enabling us to use the RD design to estimate effects for marginal students who are similar on observable characteristics but differ in admission outcomes.

For our main analyses, we merge these options into three groups: (i) Engineering or Natural Science (STEM), (ii) Business, Social Sciences, and Humanities (BSH), and (iii) non-academic

[1] Existing research has also explored how university field of study choices generate substantial effects on labor market outcomes (Altonji et al., 2016; Kirkeboen et al., 2016; Bleemer and Mehta, 2022; Hvidberg, 2023).

programs. Our regression discontinuity (RD) design examines students whose first preference was an academic program (STEM, or BSH) and whose second preference was from one of the other groups. We employ two approaches: a sharp RD design to estimate the causal impact of program admission at age 16 on cognitive skills at age 18, and a fuzzy RD design that uses sharp admission jumps as instruments for program enrollment.

We find that admission to a high school major at age 16 causally affects cognitive skill development by age 18. Students on the margin of admission to their preferred STEM program, rather than a non-academic second choice, experience increases in both crystallized and fluid skills. The effect is approximately 0.05 standard deviations (SD) for admission and about 1.5 times larger for enrollment. To put the magnitude of our estimates into perspective, they are comparable to those reported by (Carlsson et al., 2015) who find that 100 additional school days in academic programs increase crystallized skills by 0.10 SD. However, students on the margin of admission to their preferred BSH program, rather than a STEM second choice, experience a decrease of approximately 0.10 SD in both crystallized and fluid skills. These estimates underscore the role of educational specialization in fostering cognitive skills during late adolescence, and are consistent with earlier findings in the literature showing later-in-life labor market returns to STEM education (Joensen and Nielsen, 2009; Goodman, 2019; De Philippis, 2023; Dahl et al., 2023).

To explore some plausible mechanisms, we examine the role of curricula, individual characteristics and peers. First, we document that cognitive gains across high school majors at the RD margin align closely with curricular emphasis. STEM students show the largest improvements in spatial reasoning and quantitative problem-solving – skills that are cultivated through extensive coursework in mathematics, physics, and technology, which emphasize structured problem-solving and abstract reasoning (Wai et al., 2009; Uttal et al., 2013). Second, we document no heterogeneities by socio-economic status and GPA cutoff. Third, we show that STEM students at the RD margin are exposed to slightly more advantaged peers than BSH students, or non-academic students, which is aligned with our results indicating stronger cognitive skills for STEM students. In contrast, BSH students are surrounded by more advantaged peers than non-academic students, but this advantage does not translate into comparable cognitive gains for BSH students. Also, insofar as our exploratory analyses suggest that peer composition contributes to cognitive development, the influence of peers at the RD margin appears to be relatively stronger for crystallized than for fluid skills.

This paper contributes to four strands of literature. First, previous studies have investigated how additional schooling affects cognitive skills by exploiting the effect of school reforms or quasi-random variation in the timing of assessments (Cahan and Cohen, 1989; Carlsson et al., 2015; Agüero and Beleche, 2013; Aucejo and Romano, 2016; Lundborg et al., 2014; Ritchie and Tucker-Drob, 2018). Instead, provide causal evidence that, for an equal amount of schooling, the field of study (i.e., curricular content) also shapes cognitive skill development. This applies to the development of both fluid and crystallized cognitive skills for marginally accepted

students.

Second, estimating the impact of high school majors on cognitive development offers a direct test of whether educational content can foster human capital accumulation (Mincer, 1958; Becker, 1962, 1964), rather than operating solely as a signaling device (Spence, 1973). This complements earlier literature which has inferred support for human capital accumulation indirectly, by observing positive labor market returns to schooling components that are not directly observed by employers (Clark and Martorell, 2014; Aryal et al., 2022; Dahl et al., 2023).[2] Thus, our contribution is twofold: we shift the focus from years of schooling to field of study, and we test human capital formation using direct measures of cognitive development rather than labor market proxies.

Third, our paper contributes to the literature on ability tracking and general versus vocational education by identifying a mechanism through which tracking systems generate long-run effects. While existing work primarily emphasizes sorting and labor market returns (Hanushek and Woessmann, 2006; Pekkala Kerr et al., 2013; Malamud and Pop-Eleches, 2010; Dustmann et al., 2017; Brunello and Rocco, 2017; Golsteyn and Stenberg, 2017; Hanushek et al., 2017; Ollikainen et al., 2024), we show that tracking shapes cognitive skill development by channeling students into fields of study with systematically different curricula.

In doing so, we make a fourth contribution to the literature on field-of-study choice, which documents substantial impacts on earnings and mental health (Altonji et al., 2012; Kirkeboen et al., 2016; Dahl et al., 2023; Stenberg and Tudor, 2024). We show that cognitive skill development aligns with the estimated returns to high school in later-life outcomes, suggesting that skill formation during adolescence may be an important mechanism underlying these returns. This interpretation is consistent with extensive evidence on the long-run importance of cognitive skills (Heckman et al., 2006; Lindqvist and Vestman, 2011; Dal Bó et al., 2017; Hermo et al., 2022), as well as findings from psychology and neuroscience documenting continued brain development during adolescence (Roberts et al., 2006; Burnett et al., 2011; Blakemore and Mills, 2014; Bleidorn et al., 2022). These findings complement earlier work on early childhood education by highlighting the importance of understanding adolescence as a distinct and consequential developmental stage (e.g., Currie and Almond, 2011; Almond et al., 2018).

The paper is organized as follows. Section 2 describes the institutional setting, data and empirical strategy. Section 3 presents the main result and discusses plausible mechanisms. Section 4 summarizes the results and concludes.

[2]This interpretation is also consistent with evidence from the GED literature showing that individuals with similar certification but different skills experience different labor market trajectories (Cameron and Heckman, 1993; Tyler et al., 2000; Heckman and LaFontaine, 2006).

2 Institutional setting and empirical strategy

2.1 Institutional setting and admission process

The Swedish educational system mandates nine years of compulsory schooling, after which students are eligible to apply to upper secondary education. During the study period (1977-1991), students could choose among five academic and between 14 and 17 non-academic programs. Academic programs were intended to prepare students either for tertiary education or direct entry into the labor market. Approximately half of the academic graduates proceeded to higher education. Non-academic programs, on the other hand, were primarily vocational, aiming to equip students with practical competences for immediate employment after graduation.[3] Our measure of cognitive skills is assessed at age 18, when students are still enrolled in high school. As further detailed in Section 2.2, this standardized measure provides a consistent evaluation of cognitive performance across cohorts (Carlsson et al., 2015; Dal Bó et al., 2017; Hermo et al., 2022).

To enable meaningful statistically precise estimates across program types, we classify the academic majors into two groups: (i) Engineering and Natural science, which we refer to as STEM (Science, Technology, Engineering, and Mathematics) and (ii) Business, Social Sciences, and Humanities which we refer to as BSH. We treat non-academic programs – including vocational and general tracks – as a third, separate group. To illustrate their distinctions in coursework, Appendix Table A1 documents the curricular structure for academic programs. For example, STEM programs (engineering and natural sciences) share a strong emphasis on advanced mathematics and scientific coursework, with engineering further requiring technology-specific subjects in place of humanities and social sciences. In contrast, BSH programs allocate substantially more coursework to language and social sciences, with limited exposure to science. Unlike academic programs, non-academic programs devote the majority of their schedule either to broad non-vocational subjects (in general programs) or to highly specialized professional training (in vocational programs), with limited time allocated to academic coursework (Golsteyn and Stenberg, 2017).

Students applied for admission to a high school major within their region of residence, unless a desired major was unavailable locally between 1977-1991.[4] There were between 115 and 137 school regions, with a median of 927 applicants per region annually. Admission to oversubscribed programs was determined by a merit-based allocation mechanism using students' ninth-grade GPA – calculated as the mean grade across 10-12 subjects, on a scale from 1 to

[3] Graduates of non-academic programs could pursue community-college-type education or adult education as a pathway to university eligibility.

[4] The focus on the 1977-1991 period reflects the institutional stability of the educational system during these years. From 1992, a field reform merged the business, social science, and humanities programs; extended vocational programs to three years; and eliminated the general nonacademic program. The reform also introduced public funding for private schools, increasing school choice and reducing the incidence of oversubscribed programs.

5, rounded to one decimal point. These grades follow a normal distribution, with a population mean of approximately 3.2.[5]

Applicants ranked up to six preferred majors on a standardized form submitted at the end of 9th grade. Students could typically only rank their preferred major, not different combinations of institutions and a specific major. This enables us to identify the effect of the field of study rather than the combination of field and institution as we discuss later in Section 2.3. A centralized admissions office then processed these forms and assigned students to majors based on a serial dictatorship algorithm. The student with the highest GPA was placed in their top-ranked available field, followed by the next-highest GPA student, and so on. This mechanism eliminates incentives to misrepresent preferences and is shown to be Pareto efficient and strategy-proof (Svensson, 1999).

Oversubscription was determined by discrete class size constraints, capped at 30 students per class. A field was considered oversubscribed if the number of students listing a program as their top choice exceeded the number of available slots. For example, if 28 students selected a field as their top choice, all would be admitted. If 35 students did so, some would be excluded at the GPA cutoff. Schools could offer multiple classes for the same field depending on expected demand, leading to geographic and temporal variation in oversubscription. Focusing on programs that experienced excess demand, Appendix Table A2 presents descriptive statistics of students applying to oversubscribed and non-impacted programs. These look remarkably similar in their socioeconomic and demographic characteristics, suggesting that excess demand is not driven by differences in family background.

Importantly, oversubscription status is not indicative of a program's competitiveness in this context, as it can change from year to year with capacity – for example, adding a single class can shift a program from oversubscribed (impacted) to non-oversubscribed. Consistent with this, GPA cutoffs do not induce a stable vertical ranking of academic fields, as the relative ordering of cutoffs across programs varies across years and regions (Appendix Table A3).[6] Appendix Figure A4 presents the distribution of cutoff GPA values, with cutoff GPAs in white and the baseline sample distribution in gray, documenting where students on the margin of acceptance fall within the overall GPA distribution. For example, a GPA of 3.44 corresponds roughly to the 63rd percentile in the full cohort of ninth graders, including students who did not submit high school applications. Within the subset of students applying to competitive academic fields, however, the same GPA falls at the 24th percentile, illustrating that marginal admission occurs relatively low in the applicant achievement distribution.

[5]From 1982, a gender-based affirmative action policy granted a 0.2 GPA bonus to applicants from the underrepresented gender in a given field, defined as having had less than 30% representation nationally in the previous year (e.g., females applying to engineering or males to humanities). As a result, adjusted GPAs could exceed 5. Unless otherwise stated, all references to GPA henceforth refer to this adjusted GPA. Admission decisions were made based on GPA rounded to one decimal place.

[6]We distinguish between cutoffs where all applicants are accepted and those where there is a mix of accepted and non-accepted applicants. If there is a mix, the decimal number will be the cutoff, while if everyone is accepted at a given GPA and none at the level below, we define the cutoff as the average between the two.

2.2 Data on cognitive skills

We link administrative records on high school applications and admissions with data on demographics, and background characteristics from Statistics Sweden. We also merge data on cognitive skill tests from the Swedish National Service Administration. The combined dataset covers all males who applied to high school between 1977 and 1991 and who subsequently enlisted for mandatory military service.

These cognitive skill assessments were administrated when students were still enrolled in high school. Specifically, all males were required to report to an enlistment center around their 18th birthday, where they underwent a full-day process involving administrative paperwork, basic health screenings, and cognitive and physical assessments. There were few exceptions but our main analysis sample only includes individuals who were called while still in high school.[7]

During the enlistment process, cognitive skills are assessed using the Enlistment Battery 80, a set of standardized tests comparable in format to the U.S. Armed Services Vocational Aptitude Battery (ASVAB). These tests provide consistent assessments across cohorts and are administered independently of students' educational track or subsequent labor market outcomes. The military also maintained strict secrecy regarding the specific items and structure of these tests, ensuring that they were not publicly available or used in educational settings. As a result, students could not train or practice for the exam beforehand, minimizing the influence of test preparation and providing a more genuine measure of cognitive skills. Importantly, these cognitive assessments have been validated and shown to predict long-term outcomes (Lindqvist and Vestman, 2011; Carlsson et al., 2015; Dal Bó et al., 2017; Hermo et al., 2022).

The assessment includes four separate paper-and-pencil parts that cover: (i) technical comprehension, (ii) verbal, (iii) spatial reasoning, and (iv) logical thinking. Each test contains 40 questions arranged in ascending order of difficulty and is administered under time pressure (Carlsted and Mårdberg, 1993).

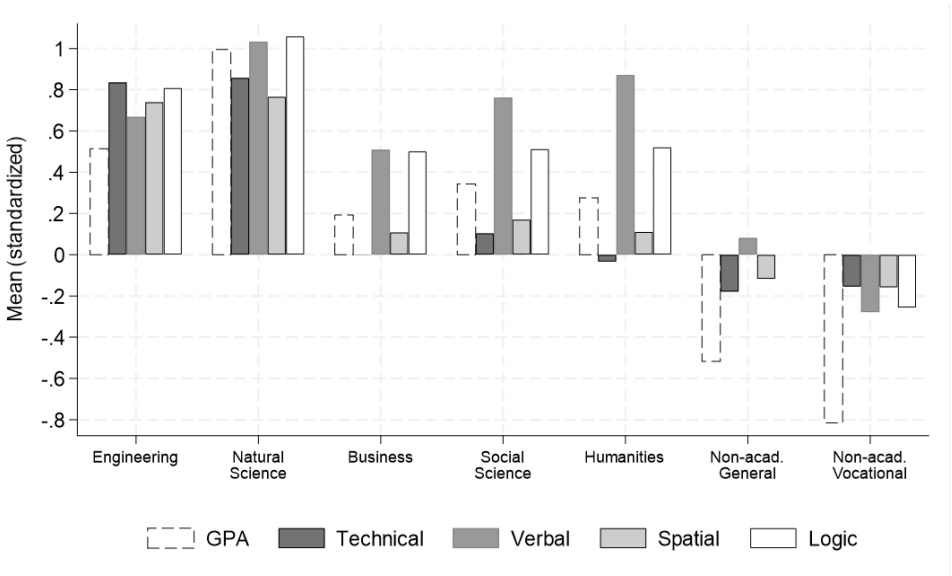
- The *technical* comprehension test consists of illustrated and text-based problems with three possible answers.
- The *verbal* test presents a word and requires the examinee to select the correct synonym from four options.
- The *spatial* reasoning employs a metal folding task, in which participants must determine the correct three-dimensional figure corresponding to a two-dimensional unfolded shape.
- The *logic* test provides sets of statements, conditions, and instructions, asking test-takers to deduce the correct answer through logical reasoning.

[7]The exceptions included individuals who have severe handicaps, are currently in prison, are institutionalized due to mental disorders, are non-citizens, or who live abroad (and can therefore postpone their enlistment date until they return to Sweden). This last group is primarily comprised of individuals who study abroad during high school, and therefore enlist the year they return to Sweden at age 19.

These four assessments are intended to capture two distinct dimensions of cognitive skills. The synonym and technical comprehension tasks reflect crystallized skill, while the spatial and logic tests reflect fluid skill (Cattell, 1971, 1987; Carlsson et al., 2015; Hermo et al., 2022). Crystallized skill refers to the capacity to apply acquired knowledge and learned skills, making it closely aligned with educational achievement. Fluid skill, in contrast, represents the ability to solve novel problems, detect patterns, and reason in unfamiliar contexts.[8]

This multidimensionality of cognitive skills is particularly relevant when comparing students across fields of study, as different curricula may place varying emphasis on accumulated knowledge versus problem-solving and abstract reasoning. If high school majors systematically select and train different cognitive skill profiles, we should then observe corresponding differences in both academic performance and performance on crystallized and fluid skill assessments. Figure 1 examines this prediction by presenting descriptive statistics on GPA and cognitive test outcomes by high school majors. Students in STEM major consistently achieve higher GPAs and cognitive scores. In contrast, students in BSH fields exhibit lower GPAs and moderate cognitive test performance, while those in non-academic programs show substantially lower results on both dimensions. These averages also indicate that the grouping effectively captures systematic academic and cognitive differences across fields, reinforcing the validity of this classification into STEM, BSH, and non-academic programs.

Figure 1: GPA and test scores by first-choice program applicants



Note. Male first-choice program applicants between 1977–1991. Average test scores and GPA from ninth grade by program type, from separate tests of four types of cognitive skills. Test scores and GPA are normalized by year to have mean zero and standard deviation one. $N = 494{,}072$.

[8]Psychologists have developed a range of instruments to capture these dimensions, such as the Wechsler Adult Intelligence Scale (WAIS). In these assessments, crystallized skill measures focus more heavily on verbal skills and accumulated knowledge. While fluid skill is commonly assessed through tasks such as abstract reasoning, puzzle solving, and pattern recognition.

2.3 Empirical strategy

This section describes how we estimate the causal effect of high school majors on cognitive skills. As emphasized by Kirkeboen et al. (2016), when individuals face multiple unordered alternatives, identification of causal returns requires more than quasi-random variation in admission to majors. A key challenge is that applicants differ not only in their preferred major but also in their second-best alternative. Ordinary Least Squares (OLS), which lacks information on these preferences, produces biased estimates for two reasons: first, because of self-selection into majors, and second, because individuals with the same preferred choice may diverge significantly in their next-best option. Even absent selection bias, OLS estimates are difficult to interpret since they represent weighted averages of returns across heterogeneous second-best alternatives, with weights that are unobserved.

Importantly, Kirkeboen et al. (2016) show that instrumental variables (IV) and regression discontinuity (RD) methods are similarly limited when next-best choices are not observed. While a randomly assigned admission cutoff removes selection bias, RD will not, without restrictive assumptions, identify the return to any group of students with a specific preferred field of study. When data on next-best alternatives are available, however, RD can recover how admission to a given preferred field – relative to the specific second-best option – affects cognitive skill development.

2.3.1 Regression discontinuity

To estimate the causal effect of admission to different majors, we exploit discontinuities in admission decisions based on cumulative GPA at the end of ninth grade (Kirkeboen et al., 2016; Dahl et al., 2023; Stenberg and Tudor, 2024). Let a_{jk} denote a dummy variable equal to 1 if an individual's preferred major is j and their next-best choice is k, where $j = 1, \ldots, J$ and $k = 1, \ldots, K$. The reduced-form effect of admission on cognitive skills for an individual with preference set (j, k), denoted y_{jk}, can be written as:

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

where x is the GPA running variable, c_j is the cutoff GPA for major j, $g^l_{jk}(\cdot)$ and $g^r_{jk}(\cdot)$ are unknown functions of distance from the cutoff on the left and right, respectively, α_{jk} are intercepts for each (j, k) combination, w is a vector of predetermined controls consisting of fixed effects for year, school region, and the number of weeks elapsed between the program start and the test date. These fixed effects absorb common time trends, persistent regional differences, and mechanical variation in test outcomes arising from differences in exposure to schooling. The error term e_{jk} captures idiosyncratic unobserved factors.

Because admission thresholds vary across years and regions, we normalize each cutoff to zero and recenter GPA accordingly. In principle, equation (1) allows for separate functional forms on both sides of the cutoff for every (j, k) pair. With 5 preferred fields and 7 next-best alternatives in our data, this requires 30 left-hand functions and 30 right-hand functions – an estimation problem that is highly data-intensive. To address this, we impose parametric restrictions on functional forms. Our baseline specification assumes that g^l_{jk} and g^r_{jk} are linear, and that slopes are common across fields: one slope for observations to the left of the cutoff and one slope for those to the right.[9] For the intercepts, we have a dummy for 1st and 2nd choice categories. In robustness checks, we remove this restriction, yielding similar estimates but with somewhat higher standard errors. Importantly, the discontinuities at admission cutoffs, represented by θ_{jk}, are always allowed to vary across both j and k, irrespective of the imposed functional form restrictions.

The coefficients θ_{jk} capture the causal effect of being admitted to field j relative to the next-best alternative k, with all margins estimated jointly in a single regression to maximize efficiency. This reduced-form parameter should be interpreted as the total effect of assignment to a field, encompassing differences in curriculum, peers, and field-specific characteristics, but not generic cutoff-related rank or threshold effects (see also Section 2.1).

As such, the reduced-form coefficients are informative about the causal impact of admission on cognitive skill development. However, admission does not mechanically translate into enrollment. We may therefore also be interested in the returns to field *enrollment*. To identify these effects, we implement a fuzzy RD design, where the first stage estimates the discontinuous jump in enrollment probabilities due to the admission thresholds for each (j, k) combination, using the same parametric structure as in the reduced form. Although post-admission reallocations may occur after initial offers are issued in July – reflecting both individual choices and local administrative discretion – and become less frequent once the academic year begins due to divergent curricula, our data capture the initial admission decision, which is a deterministic function of the GPA cutoff.

2.3.2 Regression discontinuity assumptions

Manipulation. A key assumption of the RD design is the absence of manipulation around the admission cutoff. In our setting, students cannot target a GPA just above the cutoff of their preferred field, as admission cutoffs are not known in advance and vary across years and regions. Appendix Table A3 documents this variability. In over 80 percent of cases, cutoffs for the same program differ across successive years.

To test for manipulation, we also examine predetermined characteristics around the cutoff.

[9] As robustness checks we also present estimates using a more flexible 12-slope specification, allowing slopes to vary for each of the preferred 5 fields (regardless of next-best choice), and for each next-best alternative (regardless of preferred choice). Results from the 2-slope baseline model are nearly identical to those from this 12-slope model (5 + 7 slopes), and broadly consistent with the fully flexible 60-slope specification, though the latter suffers from larger standard errors.

Appendix Figure A5 shows that these are balanced, with no statistically significant discontinuities. This is confirmed by formal tests shown in Appendix Table A6.[10]

Monotonicity, exclusion, and irrelevance. Identification of the causal effect of major enrollment additionally requires monotonicity, exclusion, and irrelevance assumptions. Monotonicity implies that crossing the admission cutoff cannot reduce the likelihood of completing the major. This "no defiers" condition is plausible in our setting.

Exclusion requires that crossing a cutoff affects outcomes only through major enrollment. In our context, switching between programs after enrollment is rare, and typically occurs early in the first year as specialized curricula make later switching difficult.

Finally, irrelevance requires that if an applicant does not complete their preferred major upon crossing the cutoff, they are not induced to complete an unrelated major instead (Kirkeboen et al., 2016). While this condition may be imperfect for enrollment, it holds by construction for admission, since thresholds generate sharp discontinuities: all applicants above the cutoff are admitted, and all below are not.

3 High school majors and cognitive skills

This section presents our main empirical findings. We begin with first-stage estimates that examine how admission is reflected in program enrollment. We then report results on the effect of high school majors on cognitive skills, before concluding with a series of robustness checks and suggestive evidence from an exploration of plausible mechanisms.

3.1 Main result

We begin by presenting estimates of how admission translates into program enrollment in Appendix Table A7, also illustrated in Appendix Figure A8. Individuals to the right of the vertical cutoff line are (initially) admitted to the field, whereas those to the left are not. Unlike admission, enrollment is not 100 percent discontinuous at the cutoff, as some admitted students enroll in other majors. This opens up slots for individuals who were initially not admitted, and enrollment shares therefore increase gradually closer to the cutoff. We use program enrollment to scale our reduced-form estimates in a fuzzy regression discontinuity (RD) design. We do not analyze the completion outcomes, as cognitive skills are measured before high school completion.

Table 1 presents the main result from the RD analysis on the effects of high school majors on cognitive skills, distinguishing between crystallized (Panel A) and fluid skills (Panel B) as

[10] As pointed out by Dahl et al. (2023), the commonly used tests for detecting density smoothness at the running-variable cutoff, such as those in McCrary (2008) and Cattaneo et al. (2018), are not suitable in this context. When the cutoff is determined by a rank-based measure and then normalized to zero, an artificial jump in the density is created, which invalidates these tests.

well as their respective subcomponents. Each table reports both reduced form and IV-enrolled estimates applying the regression discontinuity design. The reduced form estimates capture the intent-to-treat effect of being admitted to a given program, while the IV-enrolled estimates instrument actual enrollment with admission, accounting for the fact that not all admitted students enroll in their first-best program.

Panel A of Table 1 shows that admission to a STEM program, relative to both BSH and non-academic programs, significantly increases the crystallized skill, with the effect driven by an improvement in the technical skill. The reduced form estimated effects are roughly 0.05 SD, and somewhat larger (0.07 SD) when instrumenting for actual enrollment. In addition, admission to a BSH program, relative to STEM, significantly decreases crystallized skill by 0.10 SD, and by 0.15 SD with IV enrolled estimates. This effect is also driven by a decrease in the technical skill. RD graph illustrations for technical skills are presented in Appendix Figure A9. Regarding verbal skills, we find no meaningful differences across programs at the RD margin, suggesting that BSH relative to other fields does not provide an advantage in developing this skill.

Table 1: High school majors and cognitive skill development

Panel A	**Crystallized skill**		**Technical skill**		**Verbal skill**	
	Reduced form	*IV-enrolled*	*Reduced form*	*IV-enrolled*	*Reduced form*	*IV-enrolled*
STEM vs BSH	0.054*	0.076	0.104***	0.163***	-0.010	-0.029
	(0.029)	(0.050)	(0.034)	(0.058)	(0.029)	(0.050)
STEM vs non-academic	0.052**	0.074*	0.101***	0.149***	-0.012	-0.023
	(0.026)	(0.040)	(0.031)	(0.047)	(0.026)	(0.040)
BSH vs STEM	-0.103***	-0.155***	-0.180***	-0.266***	0.001	-0.002
	(0.028)	(0.044)	(0.033)	(0.052)	(0.030)	(0.047)
BSH vs non-academic	-0.015	-0.016	0.000	0.010	-0.027	-0.040
	(0.023)	(0.033)	(0.026)	(0.039)	(0.024)	(0.035)
Observations	87,116	87,116	87,116	87,116	87,116	87,116
Panel B:						
	Fluid skill		**Spatial skill**		**Logic skill**	
	Reduced form	*IV-enrolled*	*Reduced form*	*IV-enrolled*	*Reduced form*	*IV-enrolled*
STEM vs BSH	-0.019	-0.043	-0.045	-0.084	0.012	0.008
	(0.030)	(0.052)	(0.035)	(0.061)	(0.031)	(0.053)
STEM vs non-academic	0.062**	0.094**	0.097***	0.152***	0.014	0.018
	(0.026)	(0.040)	(0.031)	(0.047)	(0.027)	(0.041)
BSH vs STEM	-0.073**	-0.105**	-0.094***	-0.137**	-0.036	-0.050
	(0.029)	(0.046)	(0.034)	(0.053)	(0.030)	(0.047)
BSH vs non-academic	0.019	0.033	0.050*	0.081**	-0.015	-0.021
	(0.023)	(0.034)	(0.028)	(0.041)	(0.023)	(0.034)
Observations	87,116	87,116	87,116	87,116	87,116	87,116

Note. Panels A and B present respectively the RD estimates for crystallized and fluid skills. IV-enrolled models use as a first stage whether the individual enrolled in their first-best field, as a function of whether their GPA exceeded the admissions cutoff. The regressions are estimated within a window of -0.5 to 1.5, applying triangular weights and using linear functions of GPA which are allowed to vary to the left and to the right of the cutoff for each field of study category. Covariates included are fixed effects for year, school region and the number of weeks between program start and the date of the test, dummies for preferred program category as well as for next-best alternative. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Panel B of Table 1 reports corresponding estimates for fluid skills, combining the informa-

tion from the spatial and logic skill tests. STEM programs relative to non-academic programs increases fluid skills by approximately 0.06 SD in the reduced form, and by 0.09 SD with IV-enrolled estimates. This increase is mostly driven by an improvement in spatial skill. However, admission to BSH programs relative to STEM decreases fluid skill by 0.07 SD, with the effect again driven by spatial skill. In contrast, there are no significant differences on the margin between admission to STEM relative to BSH in fluid skills, with small and imprecisely estimated coefficients for logic and spatial skills across all comparisons. Finally, when comparing BSH and non-academic students on the margin, we find modest and marginally significant advantages for BSH in spatial skills but not in other domains. Appendix Figure A9 presents RD graphs pertaining to spatial skills.

The magnitude of the estimates can be compared with the findings of Carlsson et al. (2015) who show that an additional 100 days of schooling in an academic program increases crystallized cognitive skills by approximately 0.10 SD in Sweden. A back-of-the-envelope calculation therefore suggests that an increase in cognitive skills of 0.05-0.10 SD for students admitted to STEM rather than non-academic programs corresponds to roughly one quarter to one half of a year of schooling in terms of cognitive development.

As robustness checks, we first examine if the estimated coefficients are stable to the choice of bandwidth. Appendix Figure A10 show the results which indicate that they are stable across a wide range of bandwidths, and not driven by a particular specification of the local window around the GPA cutoff. Second, Appendix Figure A11 presents coefficient estimates from a battery of falsification tests, in which the same estimation strategy is applied to placebo cutoffs, showing no discontinuities in cognitive outcomes at artificial thresholds. This supports the validity of the identification strategy and suggests that the observed effects are driven by admission to the true cutoffs.[11] Third, Appendix Table A12 summarizes another set of specification checks: including a symmetric bandwidth (Column 2), additional demographic covariates (Column 3), excluding the years 1982-1984, when bonus GPA points were added for the first and second choices on an individual's ranking list (Column 4), allowing for 12 or 60 slopes in the specification of the running variable (Columns 5-6), using first-second choice specific intercepts (Column 7), excluding individuals with very low cognitive scores who may have intentionally failed the test (Column 8), and restricting the sample to those that eventually completed either their first or second choice program (Column 9). The estimated effects are highly consistent in sign, magnitude, and statistical significance across alternative specifications, providing additional confidence in the robustness of the results presented in Table 1.

Overall, STEM programs appear to enhance crystallized and fluid skill development, as expressed by our measures of technical and spatial skills. The features of the STEM curricula align closely with these findings (Wai et al., 2009; Uttal et al., 2013), as they devote substantially

[11] The placebo estimates yield three coefficients out of 114 that are outside the estimated causal effects. These three estimates all relate to the margin STEM vs. non-academic for spatial skills that display an coincidental increase in spatial skills for values of the running variable between 1.05 and 1.25. These observations do not drive the benchmark estimates in Table 2, as illustrated in the previous Figure A10.

more instructional time to quantitative and technical problem-solving (see also Section 2 and Appendix Table A1). We turn to an exploration of the plausible mechanisms in the next section.

3.2 Mechanism

We now turn to explore to what extent our findings reflect heterogeneous effects or can be explained by factors other than curriculum content.

We first consider heterogeneity by presenting the RD estimates by parental socio-economic background and student academic preparation (median GPA cutoff) in Table 2. For both parental education and parental income, the estimated effects for crystal and fluid outcomes are mostly similar in magnitude and sign across low and high SES groups (see Appendix Table A13 for subcomponent estimates). Although only marginally significant at the 10% level, the estimates suggest that high-SES students benefit more from admission to STEM (rather than to a non-academic second-choice program), and experience larger losses from missing STEM admission when instead admitted to BSH. This pattern is more pronounced when splitting by parental education, whereas no meaningful differences emerge when splitting by parental income. These findings suggest that the advantages associated with STEM admission rather than non-academic may be reinforced by parental educational background rather than financial resources alone.

Table 2: Heterogeneity: high school majors and cognitive development

	Parents' education		Parents' income		GPA	
	Crystallized	Fluid	Crystallized	Fluid	Crystallized	Fluid
STEM vs BSH × Low	0.038	−0.030	0.098***	−0.040	0.040	−0.025
	(0.043)	(0.045)	(0.036)	(0.037)	(0.041)	(0.043)
STEM vs BSH × High	0.054*	−0.008	0.000	0.006	0.077**	0.006
	(0.032)	(0.033)	(0.036)	(0.039)	(0.033)	(0.034)
STEM vs non-academic × Low	0.054	0.018	0.086***	0.062**	0.036	0.022
	(0.034)	(0.034)	(0.030)	(0.030)	(0.034)	(0.034)
STEM vs non-academic × High	0.102***	0.109***	0.061	0.059	0.103***	0.092***
	(0.034)	(0.034)	(0.040)	(0.040)	(0.035)	(0.034)
BSH vs STEM × Low	−0.066*	−0.011	−0.086***	−0.060*	−0.140***	−0.064*
	(0.037)	(0.038)	(0.032)	(0.033)	(0.036)	(0.037)
BSH vs STEM × High	−0.116***	−0.089***	−0.125***	−0.074**	−0.070**	−0.068**
	(0.029)	(0.032)	(0.033)	(0.036)	(0.031)	(0.033)
BSH vs non-academic × Low	0.010	0.042	−0.036	0.006	−0.057**	−0.022
	(0.028)	(0.029)	(0.025)	(0.026)	(0.027)	(0.028)
BSH vs non-academic × High	−0.039	−0.002	0.009	0.027	0.014	0.047
	(0.028)	(0.029)	(0.031)	(0.033)	(0.030)	(0.030)
Observations	86,862	86,862	86,497	86,497	87,116	87,116

Note. The table presents the heterogeneity RD estimates for crystallized and fluid skills. High parental education is defined as at least 12 years of schooling; high parental income is defined as income above the median; and high GPA is defined as greater than 3.3 (median GPA). The regressions are estimated within a window of -0.5 to 1.5, applying triangular weights and using linear functions of GPA which are allowed to vary to the left and to the right of the cutoff for each field of study category. Covariates included are fixed effects for year, school region and the number of weeks between program start and the date of the test, dummies for preferred program category as well as for next-best alternative. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

The results by academic preparation are also broadly similar in magnitude and sign across groups (see Appendix Table A14 for subcomponent estimates). Estimates are stable across low- and high-GPA students, with no systematic evidence of heterogeneous effects. Although the differences are not statistically significant, admission to STEM (rather than to a non-academic alternative) appears to benefit high-GPA students somewhat more. This pattern seems to be driven by low-GPA students who list a non-academic program as their second choice: these students experience a decrease in verbal and logical skills when they are admitted to STEM first-choice program. One possible interpretation is that this group benefits from curricula that are better aligned with their initial level of academic preparation.

We also explore whether changes beyond curriculum accompany admission at the cutoff. Table 3 documents changes in leave-out-means at the RD margin in school-major-year peer composition, regarding peer parental socioeconomic background (income and education), the share of peers that are foreign born or that are female, and the change in the individual's GPA relative rank at the RD margin. The results show that STEM students are surrounded by more advantaged peers than BSH students, whose classmates have higher parental education and income. This relative disadvantage could partly explain the weaker performance of BSH students compared to STEM. However, both STEM and BSH students share a substantially more advantaged peer environment compared with non-academic students, yet this advantage translates into much smaller cognitive gains for BSH. A possible interpretation is that exposure to high-performing peers may to some extent foster cognitive skills, but they are insufficient to explain our findings in the absence of other reinforcing mechanisms, most notably the curriculum.

Table 3: Mechanisms – High school major and characteristics of peers

	GPA rank	% Parent high education	% Parent high income	% Peers foreign born	% Female
STEM vs BSH	-0.031^{***}	0.138^{***}	-4.310	0.002	-0.232^{***}
	(0.006)	(0.028)	(6.268)	(0.001)	(0.009)
STEM vs non-academic	-0.357^{***}	0.382^{***}	4.235	-0.007^{**}	0.009
	(0.012)	(0.041)	(7.596)	(0.003)	(0.007)
BSH vs STEM	-0.018^{***}	-0.429^{***}	-24.074^{***}	-0.004^{***}	0.192^{***}
	(0.005)	(0.027)	(6.173)	(0.001)	(0.008)
BSH vs non-academic	-0.248^{***}	0.234^{***}	23.723^{***}	-0.014^{***}	0.155^{***}
	(0.008)	(0.030)	(8.395)	(0.002)	(0.006)
Observations	87,116	86,531	86,527	87,116	87,116

Note. The table reports differences in school-major-year peer characteristics in terms of relative GPA rank, share of parental education, share of parental income, share of peers foreign born and the female share across high school majors at the RD margin. High parental education is defined if parents have more than 12 years of schooling. High parental income is defined if parents' income is above the median. The regressions are estimated within a window of -0.5 to 1.5, applying triangular weights and using linear functions of GPA which are allowed to vary to the left and to the right of the cutoff for each field of study category. Covariates included are fixed effects for year, school region and the number of weeks between program start and the date of the test, dummies for preferred program category as well as for next-best alternative. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

We further explore these pathways in Appendix Table A15 by conducting a mediation anal-

ysis, in which school-major-year characteristics were introduced into the benchmark model. Overall, it is difficult to claim that any of these factors would systematically drive our results. The most consistent pattern is that verbal skills seem to improve with the gender share of the program (panel C). A caveat with this analysis is that we cannot assess the causal impact of the suggested mediating factors, and the results must consequently be interpreted with caution. Also, it could also be that part of our estimated effects operate through teachers and instructional practices specific to each program – such as pedagogical approaches, expectations, and emphasis on problem-solving – but we cannot directly assess this channel as there are no available data on teacher-level characteristics or teaching methods.

4 Conclusion

This paper shows that the choice of high school major at age 16 fosters both crystallized and fluid cognitive skills at age 18, using a regression discontinuity design and rich administrative data from Sweden. Admission into STEM programs at the RD margin enhances cognitive performance by 0.05 SD, particularly in spatial and technical skills.

These results have important implications for education policy. Policymakers may consider the potential cognitive returns of different educational programs when designing admission rules, capacity constraints, or incentives for specific high school majors. Likewise, students, parents, and school counselors can use these insights to make more informed decisions about major selection.

From a theoretical point of view, our findings support frameworks that emphasize the accumulation of field-specific human capital, suggesting that the content of educational programs plays an important role in the development of long-term cognitive skills (Mincer, 1958; Becker, 1962, 1964). An important avenue for future research is to examine how these cognitive gains compound over time and interact with subsequent choices of college major and occupation, potentially reinforcing field-specific skill accumulation through later educational and labor market pathways.

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Appendix to "High School Majors and Cognitive Skill Development"

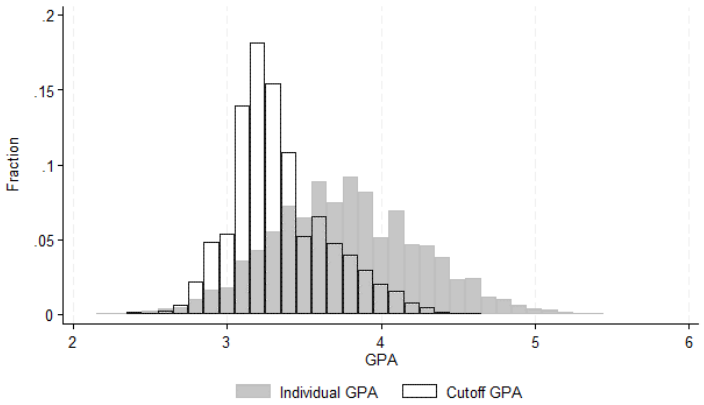
A Appendix Figures and Tables

Table A1: Course requirements: 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**

Note. 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 (Laroplan for gymnasieskolan)*; they remained unchanged during our sample period (1977–1991) but were modified in 1994.

Figure A4: Cutoff distribution versus individual GPA distribution



Note. The white bars plot the distribution of cutoff GPAs for oversubscribed programs, which vary by field, year, and school region. There are 3,487 oversubscribed programs in our estimation sample. The grey bars plot the distribution of GPA for individuals in our main analysis sample (N=106,618).

Table A2: Oversubscribed and non-impacted program sample sizes and summary statistics for all applicants with a first-choice academic program, 1977–1991

Baseline Sample:	**Oversubscribed programs**		**Non-impacted programs**	
	Individuals	**Programs**	**Individuals**	**Programs**
Engineering	42,346	785	32,216	1,080
Natural Science	9,428	392	26,675	1,437
Business	26,959	1,015	10,507	799
Social Science	12,863	855	7,651	920
Humanities	1,812	344	2,535	918
Total	**93,408**	**3,391**	**76,584**	**5,154**
GPA	3.59		3.76	
GPA adjusted	3.81		3.89	
Foreign born	0.02		0.02	
Foreign born parent	0.14		0.14	
Father earnings (SEK)	180,354		180,735	
Mother earnings (SEK)	106,348		113,330	
Father schooling	11.87		11.82	
Mother schooling	11.48		11.36	
Father age	45.88		46.43	
Mother age	43.20		43.51	
Observations	**87,116**		**76,584**	

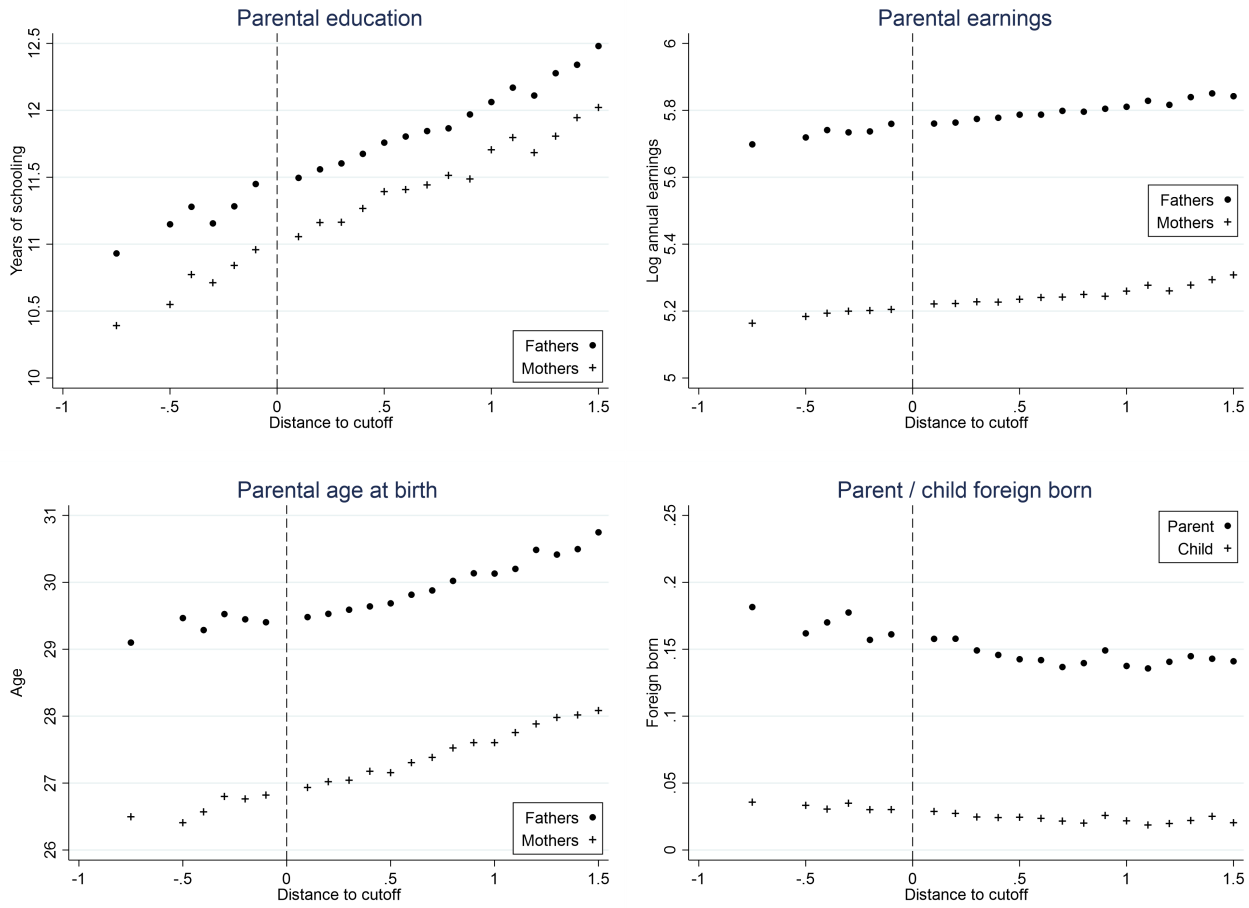
Note. Programs are defined by field, year, and school region. "Individuals" refers to the number/share of students, using the restrictions that form the baseline sample, applied to either an oversubscribed or non-impacted program. Non-impacted programs do not have an excess supply of applicants and therefore have unrestricted entry. Parent characteristics are measured in the year of application (the child's 16th year since birth).

Table A3: Comparison of field cutoffs across years within the same school region

Field combinations	**1st field**	**2nd field**	**No difference**
Engineering vs. Natural Science	0.37	0.25	0.38
Engineering vs. Business	0.28	0.42	0.30
Engineering vs. Social Science	0.21	0.53	0.27
Engineering vs. Humanities	0.31	0.38	0.31
Natural Science vs. Business	0.24	0.46	0.30
Natural Science vs. Social Science	0.18	0.51	0.31
Natural Science vs. Humanities	0.24	0.38	0.39
Business vs. Social Science	0.24	0.48	0.28
Business vs. Humanities	0.37	0.32	0.31
Social Science vs. Humanities	0.47	0.21	0.32

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

Figure A5: Smoothness of predetermined variables at the cutoff



Note. Each dot is the average for the relevant outcome in a .1 GPA bin. 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.

Table A6: Balancing tests for pre-determined characteristics

	Years of schooling father	**Years of schooling mother**	**Annual earnings father**	**Annual earnings mother**	**Age at birth father**	**Age at birth mother**	**Foreign born parent**	**Child foreign born**
Estimate	0.020	-0.022	9.252	1.402	-0.148	-0.097	0.002	0.003
	(0.047)	(0.045)	(10.487)	(7.476)	(0.109)	(0.091)	(0.006)	(0.003)
Observations	84,212	83,325	83,690	85,774	85,815	86,520	87,116	87,116

Note. Each column is an estimate from a separate RD regression, where the outcome is a linear function of the running variable (normalized GPA) within a window of -0.5 to 1.5, using triangular weights. All regressions include fixed effects for year, school region, and program, and a common slope on each side of the cutoff. Robust standard errors in parentheses. * p<.10, ** p<.05, *** p<.01

Table A7: First stage estimates for the probability of enrolling in a first-choice field

	Enrolled
STEM vs BSH	0.535***
	(0.019)
STEM vs non-academic	0.605***
	(0.015)
BSH vs STEM	0.592***
	(0.018)
BSH vs non-academic	0.654***
	(0.012)
Observations	87116

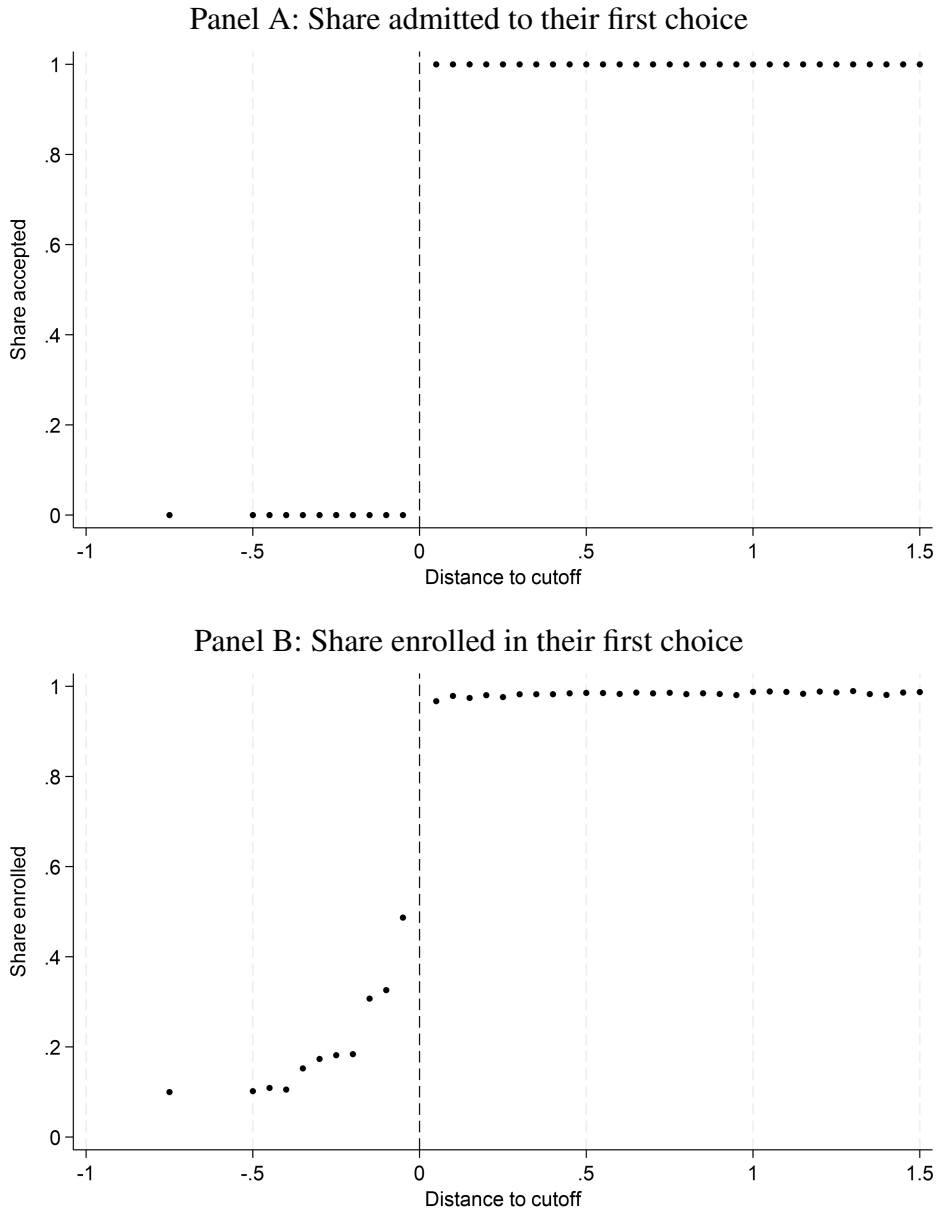
Note. The outcome variable is a dummy for whether the individuals enrolled in their first-best field, as a function of whether their GPA exceeded the admissions cutoff. The regressions are estimated within a window of -0.5 to 1.5, applying triangular weights and using linear functions of GPA which are allowed to vary to the left and to the right of the cutoff for each field of study category. Covariates included are fixed effects for year, school region, and the number of weeks between program start and the date of the test, as well as dummies for preferred program category and next-best alternative. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A12: Specification checks – reduced-form estimates using alternative specifications

	Baseline (1)	**Symmetric BW 0.75** (2)	**Add demo controls** (3)	**Excl. 1982–1984** (4)	**12 slope model** (5)	**60 slope model** (6)	**1st–2nd intercepts** (7)	**Exclude 1s** (8)	**Only completers** (9)
Panel A: Technical									
STEM vs BSH	0.104***	0.097**	0.101***	0.087**	0.102***	0.033	0.105***	0.114***	0.156***
	(0.034)	(0.038)	(0.034)	(0.036)	(0.034)	(0.046)	(0.033)	(0.032)	(0.040)
STEM vs non-academic	0.101***	0.099***	0.115***	0.086**	0.099***	0.069	0.098***	0.117***	0.176***
	(0.031)	(0.035)	(0.031)	(0.033)	(0.031)	(0.043)	(0.031)	(0.028)	(0.033)
BSH vs STEM	−0.180***	−0.189***	−0.172***	−0.185***	−0.176***	−0.151***	−0.181***	−0.163***	−0.195***
	(0.033)	(0.037)	(0.033)	(0.037)	(0.033)	(0.043)	(0.033)	(0.031)	(0.041)
BSH vs non-academic	0.000	0.013	0.006	0.011	0.007	0.015	−0.002	0.000	0.019
	(0.026)	(0.030)	(0.026)	(0.029)	(0.026)	(0.034)	(0.026)	(0.025)	(0.028)
Observations	87,116	64,443	87,116	73,367	87,116	87,116	87,116	86,155	75,942
Panel B: Verbal									
STEM vs BSH	−0.010	0.008	−0.013	−0.009	−0.021	−0.031	0.087***	−0.018	0.024
	(0.029)	(0.033)	(0.029)	(0.032)	(0.030)	(0.039)	(0.029)	(0.029)	(0.037)
STEM vs non-academic	−0.012	−0.006	0.003	−0.021	0.015	−0.014	0.001	−0.012	0.029
	(0.026)	(0.030)	(0.026)	(0.028)	(0.026)	(0.037)	(0.026)	(0.026)	(0.030)
BSH vs STEM	0.001	−0.018	0.009	−0.014	0.022	0.005	0.029	0.019	0.019
	(0.030)	(0.033)	(0.030)	(0.033)	(0.030)	(0.038)	(0.029)	(0.029)	(0.038)
BSH vs non-academic	−0.027	−0.015	−0.017	−0.023	−0.006	−0.002	−0.012	−0.037	−0.003
	(0.024)	(0.027)	(0.023)	(0.026)	(0.024)	(0.030)	(0.024)	(0.023)	(0.026)
Observations	87,116	64,443	87,116	73,367	87,116	87,116	87,116	86,155	75,942
Panel C: Spatial									
STEM vs BSH	−0.045	−0.042	−0.045	−0.045	−0.049	−0.088*	−0.026	−0.046	−0.012
	(0.035)	(0.039)	(0.035)	(0.038)	(0.036)	(0.048)	(0.035)	(0.033)	(0.042)
STEM vs non-academic	0.097***	0.105***	0.106***	0.103***	0.096***	0.072*	0.096***	0.101***	0.142***
	(0.031)	(0.036)	(0.031)	(0.034)	(0.031)	(0.043)	(0.031)	(0.029)	(0.034)
BSH vs STEM	−0.094***	−0.082**	−0.088***	−0.111***	−0.098***	−0.058	−0.088**	−0.097***	−0.082*
	(0.034)	(0.038)	(0.034)	(0.038)	(0.034)	(0.044)	(0.034)	(0.033)	(0.043)
BSH vs non-academic	0.050*	0.067**	0.053*	0.067**	0.054**	0.058	0.049*	0.034	0.064**
	(0.028)	(0.031)	(0.028)	(0.030)	(0.028)	(0.035)	(0.028)	(0.026)	(0.029)
Observations	87,116	64,443	87,116	73,367	87,116	87,116	87,116	86,155	75,942
Panel D: Logic									
STEM vs BSH	0.012	0.019	0.007	0.013	0.015	−0.017	0.100***	0.018	0.032
	(0.031)	(0.035)	(0.031)	(0.034)	(0.031)	(0.042)	(0.030)	(0.029)	(0.036)
STEM vs non-academic	0.014	0.021	0.022	0.002	0.031	0.013	0.022	0.014	0.072**
	(0.027)	(0.031)	(0.027)	(0.029)	(0.027)	(0.037)	(0.027)	(0.025)	(0.029)
BSH vs STEM	−0.036	−0.036	−0.030	−0.027	−0.047	−0.016	−0.033	−0.036	−0.074**
	(0.030)	(0.033)	(0.030)	(0.033)	(0.030)	(0.038)	(0.030)	(0.028)	(0.037)
BSH vs non-academic	−0.015	−0.024	−0.013	−0.019	−0.021	−0.026	−0.019	−0.011	0.013
	(0.023)	(0.026)	(0.023)	(0.025)	(0.023)	(0.030)	(0.023)	(0.023)	(0.025)
Observations	87,116	64,443	87,116	73,367	87,116	87,116	87,116	86,155	75,942

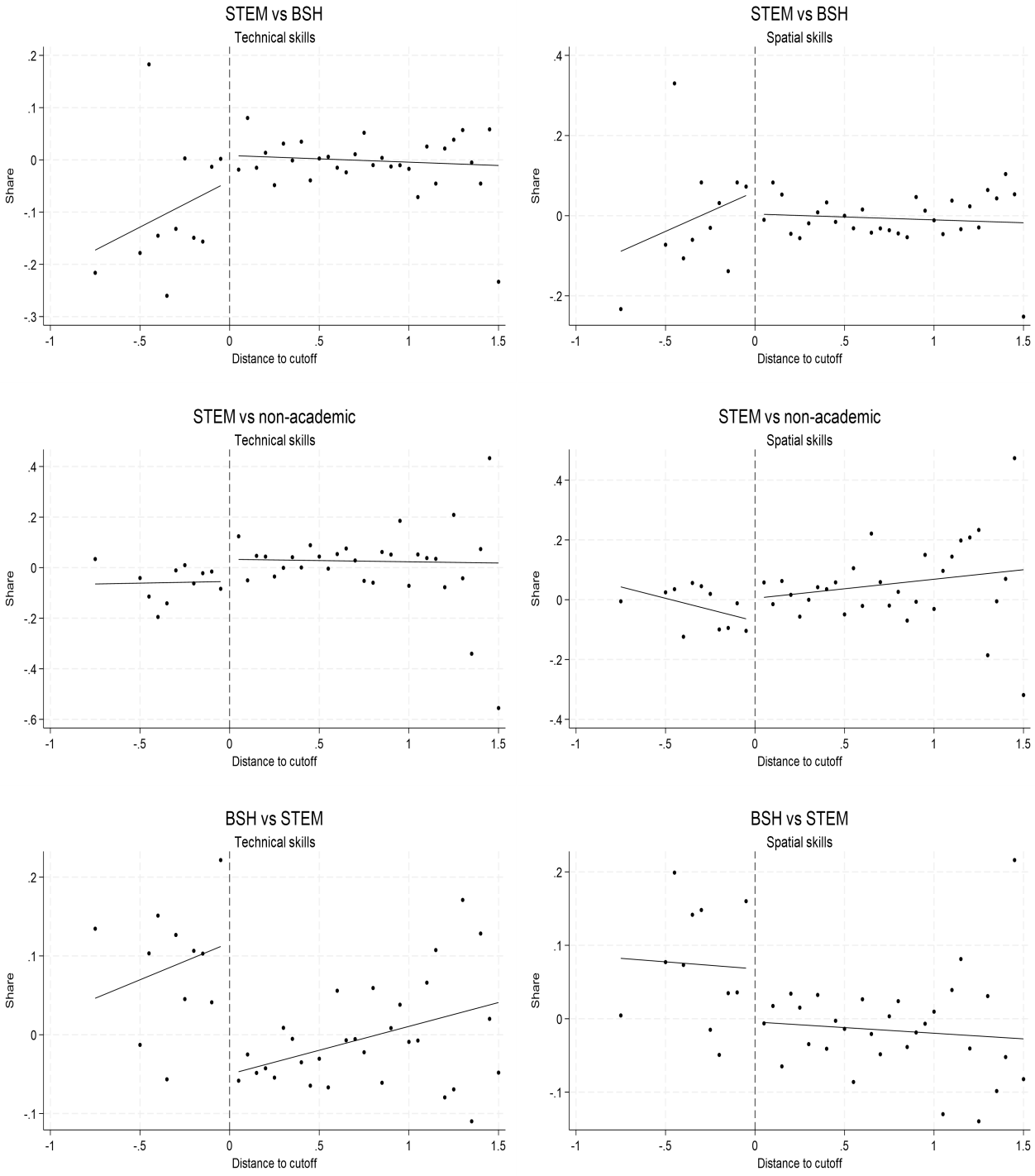
Note. Column 1 presents the main specification presented in Table 1. Column 2 reduces the right-hand side bandwidth to half. Column 3 includes the demographic controls listed in Table 2. Column 4 excludes the years 1982–1984, when bonus GPA points were added for the first and second choices on an individual's ranking list. Columns 5 and 6 apply increased number of slopes. Column 7 uses first-second choice specific intercept terms. Column 8 excludes individuals with a score of 1 on the cognitive test. Column 9 conditions on individuals that have completed either their first or second choice program. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Figure A8: Discontinuity in admission (panel A) and enrollment (B) as a function of GPA



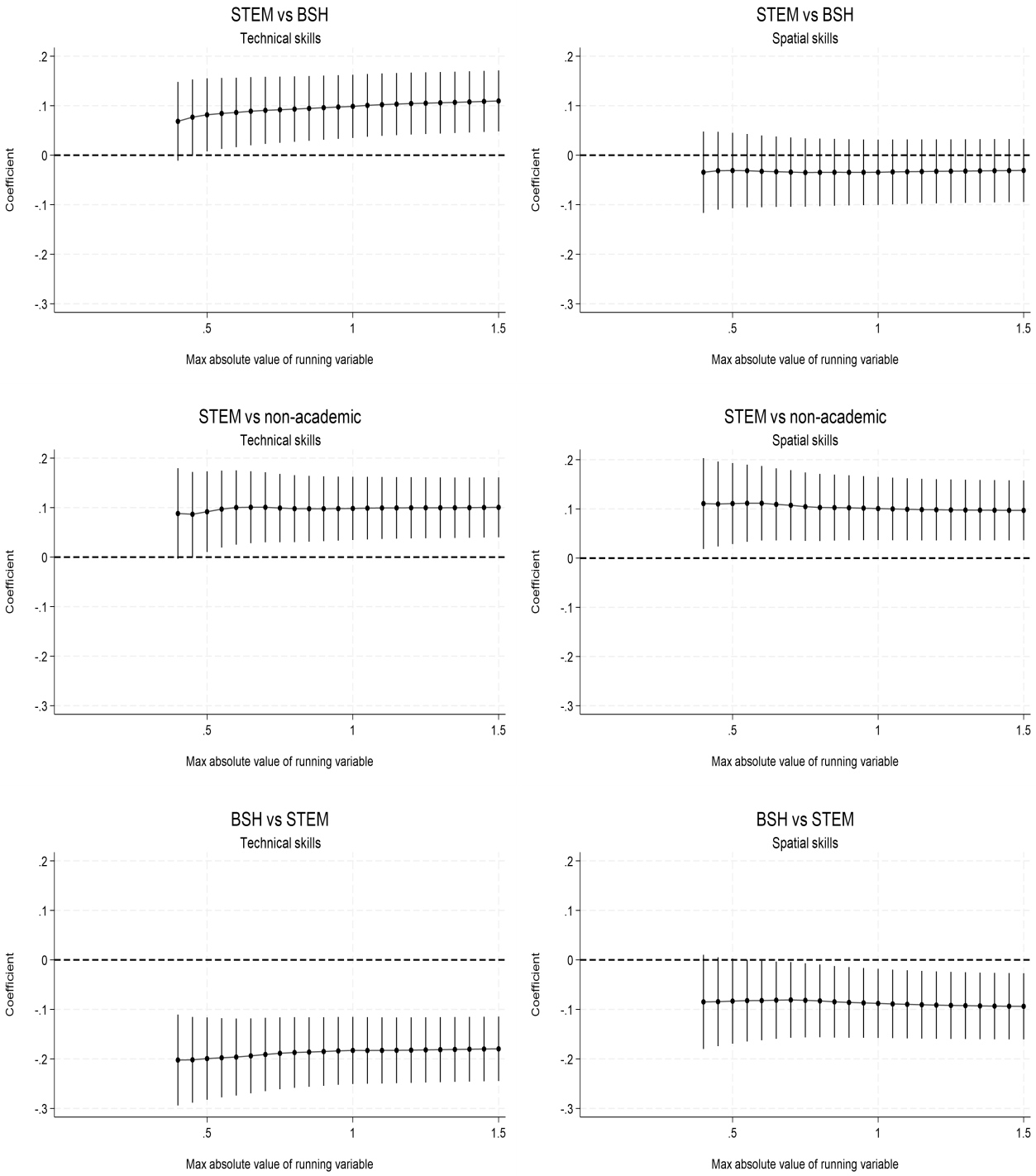
Note. Each dot is the average acceptance rate, or enrolment rate, in a 0.05 GPA bin, where GPA is measured relative to a cutoff normalized to zero. N = 87,116.

Figure A9: Sample with STEM as first choice. Cognitive skills by distance to cutoff



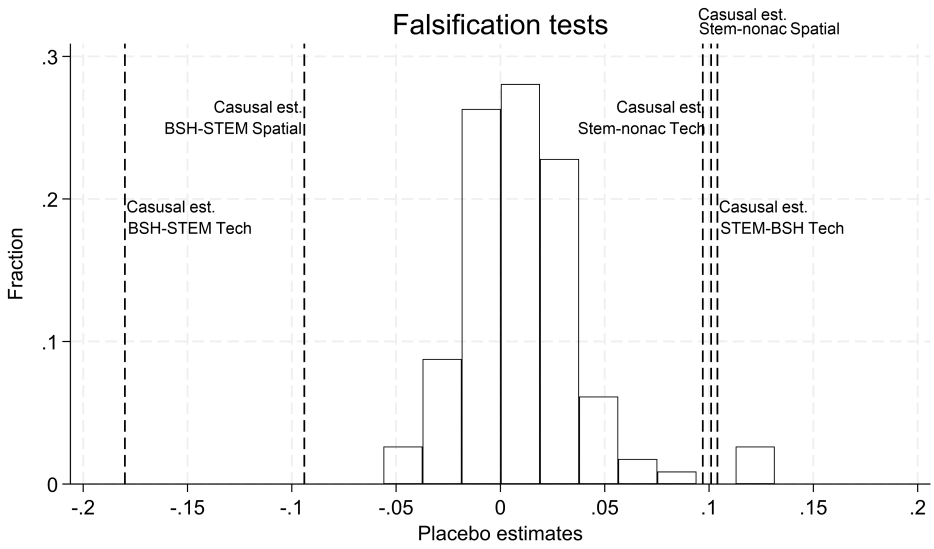
Note. Each observation is the average test score in 0.1 GPA bins, except for the leftmost dot, which is a 0.5 bin due to sparsity. The GPA is measured relative to a cutoff normalized to zero.

Figure A10: Sensitivity of results with respect to bandwidth choice



Note. Baseline RD estimates and associated 95% confidence intervals. The bandwidth 1.5 corresponds to the baseline regression specifications (see note to Table 1). The maximum bandwidth is gradually reduced in steps by .05. As long as the bandwidth-window is above 1.0, only the above-cutoff bandwidth is narrowed.

Figure A11: Randomization inference



Note. Distribution of placebo RD estimates using "fake" cutoffs. To avoid any jump at the true cutoff affecting these placebo estimates, the placebo windows start after the true GPA cutoff. We impose a minimum of plus or minus 0.3 GPA points on each side of the fake cutoff. Dashed vertical lines denote the statistically significant estimates reported in Table 1.

Table A13: Heterogeneity by parents' socio-economic status

Panel A: Education	**Technical**	**Verbal**	**Spatial**	**Logic**
STEM vs BSH * *Below 12 years of schooling*	0.130^{***}	-0.065	-0.060	0.007
	(0.049)	(0.044)	(0.052)	(0.046)
STEM vs BSH * *Above 12 years of schooling*	0.091^{**}	0.002	-0.016	0.002
	(0.038)	(0.031)	(0.038)	(0.035)
STEM vs non-academic * *Below 12 years of schooling*	0.091^{**}	0.000	0.064	-0.030
	(0.040)	(0.034)	(0.040)	(0.035)
STEM vs non-academic * *Above 12 years of schooling*	0.176^{***}	0.001	0.138^{***}	0.058^{*}
	(0.040)	(0.033)	(0.041)	(0.034)
BSH vs STEM * *Below 12 years of schooling*	-0.101^{**}	-0.014	-0.022	0.002
	(0.044)	(0.040)	(0.045)	(0.039)
BSH vs STEM * *Above 12 years of schooling*	-0.231^{***}	0.031	-0.125^{***}	-0.034
	(0.035)	(0.031)	(0.037)	(0.032)
BSH vs non-academic * *Below 12 years of schooling*	0.018	-0.002	0.057^{*}	0.017
	(0.033)	(0.029)	(0.034)	(0.028)
BSH vs non-academic * *Above 12 years of schooling*	-0.048	-0.019	0.014	-0.017
	(0.032)	(0.029)	(0.033)	(0.029)
Observations	86,862	86,862	86,862	86,862
Panel B: Income	**Technical**	**Verbal**	**Spatial**	**Logic**
STEM vs BSH * *Below median income*	0.165^{***}	0.004	-0.082^{*}	0.011
	(0.042)	(0.038)	(0.043)	(0.038)
STEM vs BSH * *Above median income*	0.053	-0.053	0.015	-0.003
	(0.043)	(0.035)	(0.045)	(0.040)
STEM vs non-academic * *Below median income*	0.134^{***}	0.014	0.088^{**}	0.025
	(0.035)	(0.031)	(0.036)	(0.031)
STEM vs non-academic * *Above median income*	0.131^{***}	-0.026	0.117^{**}	-0.011
	(0.047)	(0.039)	(0.047)	(0.040)
BSH vs STEM * *Below median income*	-0.157^{***}	0.009	-0.087^{**}	-0.021
	(0.037)	(0.034)	(0.038)	(0.034)
BSH vs STEM * *Above median income*	-0.224^{***}	0.010	-0.095^{**}	-0.037
	(0.041)	(0.035)	(0.043)	(0.036)
BSH vs non-academic * *Below median income*	-0.026	-0.038	0.006	0.005
	(0.030)	(0.027)	(0.031)	(0.025)
BSH vs non-academic * *Above median income*	-0.009	0.026	0.066^{*}	-0.020
	(0.036)	(0.032)	(0.038)	(0.033)
Observations	86,497	86,497	86,497	86,497

Note. The table presents the heterogeneity estimates by parent socio-economics status (education and income). The regressions are estimated within a window of -0.5 to 1.5, applying triangular weights and using linear functions of GPA which are allowed to vary to the left and to the right of the cutoff for each field of study category. Covariates included are fixed effects for year, school region and the number of weeks between program start and the date of the test, dummies for preferred program category as well as for next-best alternative. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A14: Heterogeneity by RD GPA cutoff

	Technical	**Verbal**	**Spatial**	**Logic**
STEM vs BSH * *Below median GPA cutoff*	0.108^{**}	-0.040	-0.014	-0.030
	(0.049)	(0.041)	(0.049)	(0.045)
STEM vs BSH * *Above median GPA cutoff*	0.117^{***}	0.018	-0.044	0.053
	(0.038)	(0.033)	(0.040)	(0.035)
STEM vs non-academic * *Below median GPA cutoff*	0.128^{***}	-0.071^{**}	0.090^{**}	-0.050
	(0.039)	(0.034)	(0.040)	(0.035)
STEM vs non-academic * *Above median GPA cutoff*	0.134^{***}	0.047	0.099^{**}	0.065^{*}
	(0.042)	(0.035)	(0.041)	(0.035)
BSH vs STEM * *Below median GPA cutoff*	-0.241^{***}	-0.001	-0.115^{***}	0.001
	(0.042)	(0.038)	(0.042)	(0.038)
BSH vs STEM * *Above median GPA cutoff*	-0.148^{***}	0.030	-0.074^{*}	-0.048
	(0.037)	(0.032)	(0.039)	(0.033)
BSH vs non-academic * *Below median GPA cutoff*	-0.036	-0.066^{**}	0.009	-0.049^{*}
	(0.031)	(0.028)	(0.033)	(0.028)
BSH vs non-academic * *Above median GPA cutoff*	-0.002	0.028	0.042	0.041
	(0.034)	(0.030)	(0.036)	(0.030)
Observations	87,116	87,116	87,116	87,116

Note. The table presents the heterogeneity estimates by the RD GPA cutoff, where below indicates values equal to or below the median and above indicates values above the median. The median GPA cutoff is 3.3. The regressions are estimated within a window of -0.5 to 1.5, applying triangular weights and using linear functions of GPA which are allowed to vary to the left and to the right of the cutoff for each field of study category. Covariates included are fixed effects for year, school region and the number of weeks between program start and the date of the test, dummies for preferred program category as well as for next-best alternative. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A15: High school majors and cognitive skills before and after conditioning on peers

	Benchmark	+ % SES	+ % Foreign	+ % Gender	+ GPA rank	+ All
Panel A: Crystallized						
STEM vs BSH	0.054*	0.036	0.055*	0.142***	0.054*	0.103***
	(0.029)	(0.030)	(0.029)	(0.030)	(0.029)	(0.031)
STEM vs non-academic	0.052**	0.007	0.043	0.048*	0.052**	0.019
	(0.026)	(0.029)	(0.028)	(0.026)	(0.026)	(0.029)
BSH vs STEM	-0.103***	-0.057*	-0.106***	-0.176***	-0.103***	-0.115***
	(0.028)	(0.029)	(0.028)	(0.028)	(0.028)	(0.030)
BSH vs non-academic	-0.015	-0.047**	-0.031	-0.073***	-0.014	-0.076***
	(0.023)	(0.024)	(0.023)	(0.023)	(0.023)	(0.024)
N	87,116	82,751	86,532	87,116	87,116	81,630
Panel B: Technical						
STEM vs BSH	0.104***	0.088**	0.103***	0.112***	0.106***	0.077**
	(0.034)	(0.035)	(0.034)	(0.035)	(0.034)	(0.036)
STEM vs non-academic	0.101***	0.066*	0.086***	0.100***	0.101***	0.065*
	(0.031)	(0.034)	(0.033)	(0.031)	(0.031)	(0.034)
BSH vs STEM	-0.180***	-0.154***	-0.181***	-0.186***	-0.180***	-0.135***
	(0.033)	(0.034)	(0.033)	(0.033)	(0.033)	(0.035)
BSH vs non-academic	0.000	-0.025	-0.024	-0.005	0.001	-0.023
	(0.026)	(0.028)	(0.027)	(0.027)	(0.026)	(0.028)
N	87,116	82,751	86,532	87,116	87,116	81,630
Panel C: Verbal						
STEM vs BSH	-0.010	-0.024	-0.006	0.141***	-0.011	0.108***
	(0.029)	(0.030)	(0.029)	(0.030)	(0.029)	(0.031)
STEM vs non-academic	-0.012	-0.056*	-0.014	-0.018	-0.013	-0.033
	(0.026)	(0.029)	(0.028)	(0.026)	(0.026)	(0.029)
BSH vs STEM	0.001	0.058*	-0.003	-0.124***	0.001	-0.067**
	(0.030)	(0.031)	(0.030)	(0.030)	(0.030)	(0.032)
BSH vs non-academic	-0.027	-0.060**	-0.030	-0.128***	-0.027	-0.113***
	(0.024)	(0.025)	(0.024)	(0.024)	(0.024)	(0.026)
N	87,116	82,751	86,532	87,116	87,116	81,630
Panel D: Fluid						
STEM vs BSH	-0.019	-0.036	-0.019	0.025	-0.016	-0.003
	(0.030)	(0.032)	(0.031)	(0.031)	(0.030)	(0.032)
STEM vs non-academic	0.062**	0.025	0.043	0.060**	0.062**	0.032
	(0.026)	(0.029)	(0.028)	(0.026)	(0.026)	(0.029)
BSH vs STEM	-0.073**	-0.061**	-0.079***	-0.109***	-0.073**	-0.097***
	(0.029)	(0.030)	(0.029)	(0.030)	(0.029)	(0.031)
BSH vs non-academic	0.019	-0.005	0.009	-0.010	0.020	-0.020
	(0.023)	(0.025)	(0.024)	(0.024)	(0.023)	(0.025)
N	87,116	82,751	86,532	87,116	87,116	81,630
Panel E: Spatial						
STEM vs BSH	-0.045	-0.063*	-0.045	-0.034	-0.043	-0.067*
	(0.035)	(0.037)	(0.036)	(0.036)	(0.035)	(0.038)
STEM vs non-academic	0.097***	0.070**	0.080**	0.097***	0.097***	0.072**
	(0.031)	(0.034)	(0.033)	(0.031)	(0.031)	(0.034)
BSH vs STEM	-0.094***	-0.081**	-0.099***	-0.103***	-0.094***	-0.083**
	(0.034)	(0.036)	(0.034)	(0.034)	(0.034)	(0.036)
BSH vs non-academic	0.050*	0.027	0.037	0.042	0.050*	0.025
	(0.028)	(0.030)	(0.029)	(0.028)	(0.028)	(0.030)
N	87,116	82,751	86,532	87,116	87,116	81,630
Panel F: Logic						
STEM vs BSH	0.012	-0.001	0.012	0.078**	0.015	0.062*
	(0.031)	(0.032)	(0.031)	(0.031)	(0.031)	(0.033)
STEM vs non-academic	0.014	-0.025	-0.001	0.011	0.014	-0.015
	(0.027)	(0.029)	(0.028)	(0.027)	(0.027)	(0.030)
BSH vs STEM	-0.036	-0.028	-0.044	-0.091***	-0.036	-0.091***
	(0.030)	(0.031)	(0.030)	(0.030)	(0.030)	(0.032)
BSH vs non-academic	-0.015	-0.036	-0.022	-0.060**	-0.015	-0.061**
	(0.023)	(0.024)	(0.024)	(0.023)	(0.023)	(0.025)
N	87,116	82,751	86,532	87,116	87,116	81,630

Note. The table reports an exploratory analysis of RD estimates of the effects of high school majors on skills before and after controlling for school-major-year peer composition in terms of peers' parental socio-economic status (SES), foreign background, gender and GPA relative rank. The regressions are estimated within a window of -0.5 to 1.5, applying triangular weights and using linear functions of GPA which are allowed to vary to the left and to the right of the cutoff for each field of study category. Covariates included are fixed effects for year, school region and the number of weeks between program start and the date of the test, dummies for preferred program category as well as for next-best alternative. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.