High School Majors and Cognitive Skill Development*

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Abstract

This paper studies whether high school field of study fosters cognitive development. We provide evidence on human-capital effects distinct from signaling, using cognitive tests administered during mandatory enlistment, whose content was kept secret and whose scores were not disclosed to employers. Exploiting GPA admission cutoffs and ranked program preferences in a regression discontinuity design in Sweden, we estimate the effects of admission to a preferred major relative to the next-best alternative. High school majors at age 16 causally affect cognitive skills by age 18: STEM admission relative to non-academic programs raises crystallized and fluid skills by 0.05 standard deviations, while business, social science, and humanities (BSH) versus STEM lowers them by 0.10. Analysis of test components suggests that curricular differences help explain these effects and that what students study can shape cognitive skill development during adolescence.

JEL codes: I21, J24, I26.
Keywords: Skills, Cognitive development, Schooling, Field of study.

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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 range 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; Hansen et al., 2004; 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 vocational training – also affects them. Understanding this can inform education policy, as in many countries students choose a field of study in high school (Hanushek et al., 2017; OECD, 2023), and these choices 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).[1] The question also speaks directly to whether fields of study differ in how they build productive skills such as language or logic (Mincer, 1958; Becker, 1962, 1964), beyond any role they may also play in signaling ability (Spence, 1973, 1974).

This paper studies whether the high school major a student pursues, rather than the amount of schooling received, causally affects cognitive skill development. In Sweden, students entered high school at age 16 and ranked their preferred programs. Admission to oversubscribed programs was determined solely by cumulative ninth-grade GPA through a centralized strategy-proof allocation mechanism (Svensson, 1999), generating admission cutoffs that varied across programs, regions, and years. Using a regression discontinuity design (RD) and ranked application data for more than 87,000 high school applicants between 1977 and 1991, we exploit these cutoffs to estimate the effect of admission to a preferred major relative to the specific next-best alternative the student would otherwise have pursued (Kirkeboen et al., 2016; Dahl et al., 2023).

Using personal identification numbers, we link the application records with administrative data covering all 18-year-old men required to complete a series of cognitive tests during mandatory military enlistment. The tests were administered independently of students' educational track, their specific content was kept secret, and scores were not disclosed to civilian employers (Carlstedt and Mårdberg, 1993), limiting both test preparation and the scope for the assessments to function as labor-market signals. Moreover, these assessments have been validated and shown to predict outcomes later in life (Lindqvist and Vestman, 2011; Hermo et al., 2022). They allow us to distinguish between two dimensions of cognitive skills: crystallized skills, which reflect acquired knowledge and learned skills and are measured by the verbal and technical tests, and fluid skills, which capture the ability to solve novel problems and reason in unfamiliar contexts and are measured by the spatial and logic tests (Carlsson et al., 2015; Cattell, 1971, 1987).

We find that admission to Engineering and Natural Science (STEM), rather than a non-

[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).

academic program, increases both crystallized and fluid skills by about 0.05 standard deviations. By contrast, admission to Business, Social Science, and Humanities (BSH), rather than STEM, reduces crystallized skills by about 0.10 standard deviations and fluid skills by about 0.07 standard deviations. These effects are concentrated in technical and spatial skills, while we find little evidence of effects on verbal or logic skills. Estimates that instrument actual enrollment with admission are roughly 1.5 times as large. To put these magnitudes into perspective, Carlsson et al. (2015) find that 100 additional school days in academic programs increase crystallized skills by 0.10 SD. Using this estimate as a benchmark, our STEM effects of 0.05–0.10 SD correspond roughly to one-quarter to one-half of a school year.

To explore plausible mechanisms, we examine whether the pattern of effects is consistent with curricular exposure, students' reported school experiences, changes in observable peer composition, and heterogeneity by student background and prior academic preparation. 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 and technical tests, consistent with the structured problem-solving and abstract reasoning required in STEM coursework (Wai et al., 2009; Cortes et al., 2015; Uttal et al., 2013). By contrast, we find no corresponding BSH advantage in verbal skills, despite these programs' greater emphasis on languages. Second, we link the administrative data to student responses from the nationally representative Evaluation Through Follow-up (UGU) study for the cohorts born in 1967 and 1972, surveyed during the first year of high school ($N < 2{,}000$). These data show that STEM students experience a more demanding and less supportive school environment. Third, we examine whether changes in peer composition align with the estimated effects on cognitive development. Most peer characteristics change little at the RD margin, and the observed changes do not align closely with the pattern of cognitive effects. Finally, we examine whether the estimated effects vary with students' socioeconomic background and prior academic preparation and find limited evidence of systematic heterogeneity. Taken together, the evidence suggests that demanding quantitative coursework may foster cognitive skills.

This paper contributes to three strands of literature. First, previous studies have investigated how additional schooling, whether measured in years or days, affects cognitive skills by exploiting school reforms or quasi-random variation in the timing of assessments (Cahan and Cohen, 1989; Hansen et al., 2004; Agüero and Beleche, 2013; Pekkala Kerr et al., 2013; Lundborg et al., 2014; Brinch and Galloway, 2012; Carlsson et al., 2015; Aucejo and Romano, 2016; Ritchie and Tucker-Drob, 2018). We extend this literature by providing causal evidence that, beyond the amount of schooling received, the field of study also shapes cognitive skill development. Our results show that high school majors affect both fluid and crystallized cognitive skills among marginally admitted students.

Second, our paper contributes to the literature on human capital accumulation and educational signaling (Mincer, 1958; Becker, 1962, 1964; Spence, 1973, 1974). Schooling can raise wages either by building productive skills or by signaling pre-existing ability, making it diffi-

cult to distinguish these mechanisms using labor-market outcomes alone. By directly measuring cognitive skills through assessments administered during mandatory enlistment, whose scores were not disclosed to employers, we provide evidence of human capital accumulation that is distinct from labor-market signaling. This complements previous studies that use labor-market outcomes to assess the relative roles of human capital accumulation and signaling (Clark and Martorell, 2014; Arteaga, 2018; Aryal et al., 2022; Dahl et al., 2023; Busso et al., 2025).[2]

Third, our paper contributes to the literature on educational tracking and general versus vocational education by showing that assignment to different high school majors affects cognitive development during adolescence, complementing previous research on its effects on educational attainment and labor-market outcomes (Hanushek and Woessmann, 2006; 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).[3]

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

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.[4]

To obtain statistically precise estimates across program types, we classify the academic majors into two groups: (i) Engineering or Natural science, which we refer to as STEM (Science, Technology, Engineering, and Mathematics) and (ii) Business, Social Science, and Humanities

[2]Related evidence on educational signaling comes from the sheepskin literature, which documents wage premiums associated with degree receipt conditional on years of schooling (e.g., Jaeger and Page, 1996). Evidence from the GED (General Educational Development) literature further shows 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; Jepsen et al., 2016).

[3]Our findings complement Ollikainen et al. (2024), who find no significant effects of assignment to academic versus vocational tracks on cognitive skills by using a a RD based on GPA admission cutoffs between academic and vocational tracks in Finland. We extend their analysis by distinguishing between different high school majors and their specific next-best alternatives, revealing heterogeneity in cognitive skill effects that may be masked by aggregate comparisons between academic and vocational tracks.

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

which we refer to as BSH. We treat non-academic programs – including vocational and general tracks – as a third, separate group. To illustrate differences in coursework, Appendix Table A1 documents the curricular structure for academic programs, which have the same total number of instructional hours. STEM programs devote nearly half of their instructional time to mathematics, natural science, and technology, whereas BSH programs place greater emphasis on social sciences and languages. Non-academic programs focus more on occupation-specific training, with less emphasis on academic coursework (Golsteyn and Stenberg, 2017).

Students applied for admission to high school majors within their region of residence, unless a desired major was unavailable locally between 1977 and 1991.[5] There were between 115 and 137 school regions, with a median of 927 applicants per region annually. Applicants ranked up to six preferred majors on a standardized form submitted at the end of ninth grade. Students could rank majors, but not different combinations of institutions and majors. This feature allows us to identify the effect of field of study rather than the combination of field and institution, as discussed in Section 2.3. A centralized admissions office then assigned students to majors using a serial dictatorship algorithm. The student with the highest ninth-grade GPA was placed in their highest-ranked available field, followed by the next-highest GPA student, and so on. This mechanism eliminates incentives to misrepresent preferences and is Pareto efficient and strategy-proof (Svensson, 1999).

Oversubscription arose from discrete class-size constraints, with classes capped at 30 students. A field was considered oversubscribed when the number of applicants exceeded the number of available slots, in which case admission was determined by ninth-grade GPA. GPA was calculated as the mean grade across 10–12 subjects on a scale from 1 to 5 and rounded to one decimal point. Grades were approximately normally distributed, with a population mean of about 3.2.[6] For example, if 28 students selected a field as their top choice, all would be admitted, whereas if 35 did so, some would be excluded at the GPA cutoff.[7] Schools could offer multiple classes for the same field depending on expected demand, generating geographic and temporal variation in oversubscription.

Importantly, oversubscription does not necessarily indicate that a program is more compet-

[5]From 1992, a field reform merged the business, social science, and humanities programs; extended vocational programs to three years; and eliminated the general non-academic program. The reform also introduced public funding for private schools, increasing school choice and reducing the incidence of oversubscribed programs.

[6]In 1982–1984, 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.

[7]When defining the cutoff GPA, we distinguish between GPA cutoffs where all applicants are accepted and those where there is a mix of accepted and non-accepted applicants. If everyone is accepted at a given GPA, say 3.4, and none at the level below, 3.3, we define the cutoff as the average between the two, 3.35. If there is a mix of admitted and rejected applicants at 3.4, the cutoff will be defined as 3.4. However, in these cases of mixed cutoffs, the individuals with the GPA exactly at the cutoff are dropped because the criteria for accepting or rejecting individuals were not always random. Our sharp discontinuity then comes from observations above and below the mixed cutoff GPA.

itive, as the number of slots can vary from year to year. For example, adding a single class can shift a program from being oversubscribed to accepting all applicants. Consistent with this, GPA cutoffs do not generate a stable vertical ranking of academic fields as their relative ordering varies across years and regions (Appendix Table A2). Appendix Table A3 also shows that students applying to oversubscribed and non-oversubscribed programs are remarkably similar in their socioeconomic and demographic characteristics, suggesting that excess demand is not systematically associated with differences in family background. To characterize where these admission margins fall in the broader achievement distribution, Appendix Figure A4 compares the cutoff GPA distribution with the baseline GPA distribution. For example, the average cutoff GPA across the academic programs of 3.44 corresponds to roughly the 63rd percentile in the full cohort of ninth graders, including students who did not submit high school applications, but only the 24th percentile among students applying to oversubscribed academic fields. Thus, marginal admission occurs relatively low in the achievement distribution of academic-program applicants.

2.2 Data on cognitive skills

We link administrative records on high school applications and admissions with demographic and background characteristics from Statistics Sweden and 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.

The cognitive skill assessments were administered around age 18, while students were still enrolled in high school. To ensure comparable schooling exposure, our main analysis focuses on 18-year-olds called for enlistment before the summer, when students in two-year non-academic programs were still enrolled. All males were required to report to an enlistment center, where they underwent a full-day process involving administrative paperwork, basic health screenings, and psychological assessments.[8]

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 assessments have three features that make them particularly useful for our analysis. First, they provide consistent measures of cognitive development across cohorts and were administered independently of students' educational track. Second, the military maintained strict secrecy regarding the specific items and structure of the tests, which were not publicly available or used in educational settings. This limited students' ability to prepare specifically for the tests, making the scores less likely to reflect test preparation. Third,

[8]Once assigned an enlistment date, individuals had strong incentives to comply, as failure to appear could result in fines or imprisonment (Sveriges riksdag, 1941). Exceptions were limited to individuals with severe disabilities, those who were incarcerated or institutionalized for mental disorders, non-citizens, and those residing abroad. The last group consists primarily of individuals studying abroad during high school, who typically enlisted upon returning to Sweden at age 19.

because the scores were not disclosed to civilian employers, they could not directly function as a labor-market signal. Together, these features make the assessments a direct measure of cognitive skills, distinct from both test preparation and employer signaling. Finally, 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 (Carlstedt and Mårdberg, 1993).

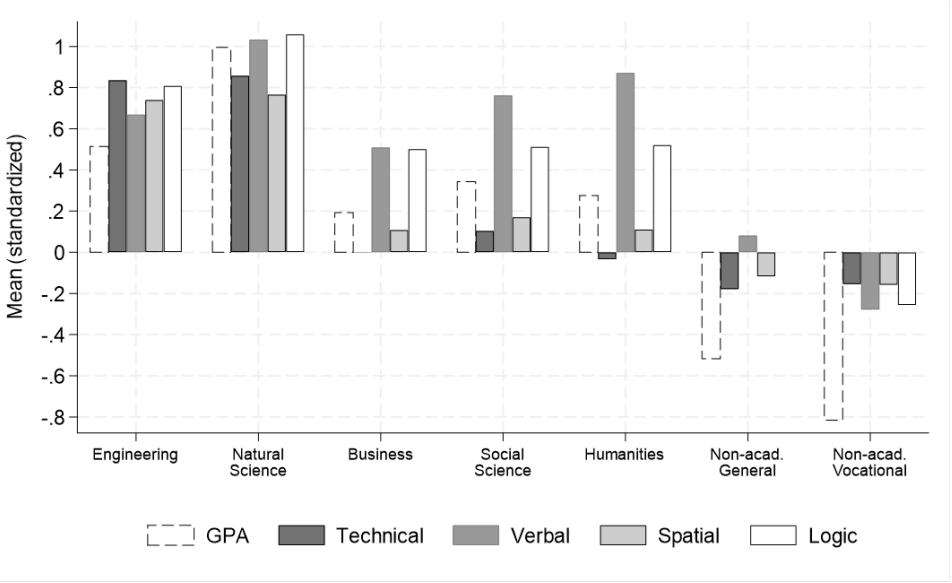
- The *technical* comprehension test consists of illustrated and text-based problems with three possible answers.
- The *verbal* test presents a word and requires the examinee to select the correct synonym from four options.
- The *spatial* reasoning test employs a mental folding task, in which participants 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.

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.[9] We treat the two aggregate indices – crystallized and fluid skills – as our main outcomes and use the four subtests – technical, verbal, spatial, and logic – to assess which components drive the aggregate effects. This focus also limits concerns about multiple comparisons.

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 select and train different cognitive skill profiles, we should then observe corresponding differences in both academic outcomes and performance on crystallized and fluid skill assessments. Figure 1 examines these patterns by presenting descriptive statistics on GPA and cognitive test scores by

[9]Psychologists have developed a range of instruments to capture these dimensions. Crystallized intelligence is typically assessed using measures that draw on acquired knowledge and verbal abilities, whereas fluid intelligence is commonly assessed using novel problem-solving and abstract reasoning tasks (Cattell, 1971; Nisbett et al., 2012).

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



Note. Male first-choice program applicants between 1977–1991. Average enlistment cognitive-test scores and ninth-grade GPA 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. $N = 494{,}072$.

high school majors. Students in STEM majors 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.

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, applicants differ not only in their preferred major but also in the alternative they would pursue if not admitted. OLS estimates are therefore difficult to interpret: in addition to self-selection into majors, they combine returns relative to heterogeneous and unobserved next-best alternatives. Importantly, Kirkeboen et al. (2016) show that quasi-experimental variation in admission does not by itself solve this problem. Even when an admission cutoff removes selection bias, IV or RD estimates cannot generally identify the return to a preferred field without information on the alternative students would otherwise pursue. When next-best alternatives are observed, however, RD can identify the effect of admission to a preferred field relative to the specific alternative the student would otherwise attend.

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, 2027). 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 $k = 1, \ldots, K$. For an individual whose preferred major is j and next-best alternative is k, let y_{jk} denote cognitive skill. The reduced-form specification 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 decisions are based on GPA rounded to one decimal place, the running variable is discrete. We therefore normalize each year-region-program-specific cutoff GPA to zero and recenter GPA accordingly. In the baseline specification, we estimate the model within an asymmetric local window of $[-0.5, +1.5]$ around the cutoff, using triangular weights. This choice reflects the fact that support is thinner below the cutoff on most admission margins: the narrower left-hand window avoids very sparse cells, while the wider right-hand window improves precision without materially changing the local comparison. In cases where admission switches between two adjacent rounded GPA values (so there is no mixed admit/reject group at a single GPA value), we define the cutoff as the midpoint between those two values. In the appendix, we show that the results are similar under alternative bandwidth choices, including a symmetric window.

Within this local window, 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, making the specification highly parameter-intensive. We therefore impose restrictions on these 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. For the intercepts, our baseline specification includes separate indicators for preferred-program and next-best-alternative categories. We relax these slope and intercept restrictions in robustness

checks, obtaining similar estimates.[10]

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 effects (see also Section 2.1).

Admission, however, does not mechanically translate into enrollment. Post-admission reallocations may occur after initial offers are issued in July, reflecting both individual choices and local administrative discretion, although such reallocations become less common once the academic year begins and curricula diverge. To estimate the effect of actual enrollment, we implement a fuzzy RD design, using the discontinuous jump in admission at the cutoff to instrument for enrollment for each (j, k) combination and maintaining the same parametric structure as in the reduced form. The reduced-form admission effects are our primary policy-relevant estimands, since admission is the margin directly affected by program capacity and allocation rules. The fuzzy RD estimates complement these results by translating the admission effects into effects of actual enrollment among compliers.

2.3.2 Regression discontinuity assumptions

Manipulation. The RD design assumes 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 (see Section 2.1). Appendix Table A2 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. Appendix Figure A5 shows that these are balanced, with no statistically significant discontinuities. This is confirmed by formal tests shown in Appendix Table A6.[11]

Monotonicity, exclusion, and irrelevance. Identification of the causal effect of enrollment in a major additionally requires monotonicity, exclusion, and irrelevance assumptions. First, monotonicity implies that crossing the admission cutoff cannot reduce the likelihood of enrolling in the preferred major, which is plausible in our setting. Second, exclusion requires that

[10] For the slopes, we consider a more flexible 12-slope specification, allowing slopes to vary for each of the 5 preferred fields (regardless of next-best choice) and for each next-best alternative (regardless of preferred choice), as well as a fully flexible 60-slope specification. Results from the 2-slope baseline model are nearly identical to those from the 12-slope model and broadly consistent with the 60-slope specification, though the latter yields larger standard errors. For the intercepts, we allow them to vary flexibly across first- and second-choice pairs, again obtaining similar estimates.

[11] As pointed out by Dahl et al. (2023), an artificial jump in the density is created when the cutoff is determined by a rank-based measure and then normalized to zero. For this reason, 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.

crossing a cutoff affects outcomes only through enrollment in the preferred major. 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 enroll in their preferred major when crossing the cutoff, they are not induced to enroll in an unrelated major instead (Kirkeboen et al., 2016). While this condition may be imperfect for enrollment, it holds by construction for admission, since cutoffs 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 running a series of robustness checks. This is followed by a discussion on possible mechanisms based on suggestive evidence from heterogeneity analyses and, to a lesser extent, survey data.

3.1 First stage results

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 gradually increase closer to the cutoff. We use program enrollment to scale our reduced-form estimates in a fuzzy regression discontinuity (RD) design. Because cognitive skills are measured before students complete high school, we focus on admission and enrollment rather than program completion.

3.2 Main results

Table 1 presents the main results from the RD analysis on the effects of high school majors on cognitive skills, distinguishing between crystallized (Panel A) and fluid skills (Panel B) and their respective subtests. Each panel reports both reduced-form and IV-enrolled estimates. As discussed in Section 2.2, the aggregate indices are our main outcomes, while the subtests are used to interpret the underlying pattern of effects. Each preferred-program/next-best-alternative combination represents a distinct group of individuals at the admission margin of the first choice. Each estimate captures the effect of admission to a preferred field relative to a specific next-best alternative for applicants at that cutoff.

Panel A of Table 1 shows that admission to a STEM program, relative to both BSH and non-academic programs, significantly increases crystallized skill, with the effect driven by technical skill. The reduced-form estimates are roughly 0.05 SD, and about 50 percent larger when instrumenting for actual enrollment.[12] Admission to a BSH program relative to STEM, by contrast, decreases crystallized skill by 0.10 SD, with the effect again driven by 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, despite BSH programs' greater emphasis on languages.

Table 1: High school majors and cognitive skill development

Panel A:	**Crystallized skill**		**Sub-dimensions**			
			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**		**Sub-dimensions**			
			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 of high school majors on crystallized and fluid skills. STEM denotes Engineering and Natural Science (Science, Technology, Engineering, and Mathematics); BSH denotes Business, Social Science, and Humanities; non-academic refers to vocational and general programs. IV-enrolled models use as a first stage whether the individual enrolled in their preferred field, as a function of whether their GPA exceeded the admission 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 infor-

[12] We cannot estimate the impact of program completion since the tests are conducted before students finish high school.

mation from the spatial and logic skill tests. Admission to STEM relative to non-academic programs increases fluid skills by 0.06 SD, and by 0.09 SD with IV-enrolled estimates. This increase is driven by an improvement in spatial skill. Admission to a BSH program relative to STEM, by contrast, decreases fluid skill by 0.07 SD, with the effect again driven by spatial skill. Appendix Figure A9 presents RD graphs for spatial skill. At the STEM vs. BSH margin, however, we find no significant effect on fluid skill, with small and imprecisely estimated effects on both spatial and logic skills. The difference between the STEM-BSH and the BSH-STEM margins reflects that the estimates apply to different groups of students, with different revealed preferences and potentially different comparative advantages, which may lead to different effects across the two local populations. Finally, admission to BSH relative to a non-academic program produces a modest positive effect on spatial skill, but no significant effect on aggregate fluid or logic skill. More generally, we find no significant effects on logic skill at any of the four admission margins.

The magnitude of the estimates can be compared with 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. Using this benchmark, assuming a scholar year is approximately 200 days, it suggests that the 0.05–0.10 SD increase in cognitive skills among students admitted to STEM corresponds to roughly one quarter to one half of a school year of cognitive development.

Estimates can also be compared with existing evidence on earnings. Lindqvist and Vestman (2011) estimate that a one-standard-deviation increase in cognitive skill is associated with roughly 9 percent higher wages, while Dahl et al. (2023) report reduced-form earnings effects of admission to STEM programs which with yield weighted average estimates of about 2–3 percent when BSH is second choice. [13] Interpreting these earnings effects entirely through the cognitive-skill channel would therefore require cognitive gains of approximately 0.25–0.30 SD. By comparison, this back-of-the-envelope calculation means our estimated 0.05 SD effect of STEM admission would account for 15–20 percent of the reduced-form earnings effect. The remaining effect would therefore have to operate through other channels, potentially including further education, occupational sorting, other skills, or credential effects.

Taken together, these results are consistent with high school majors contributing to cognitive skill development, particularly in technical and spatial domains. Our findings are also consistent with research in psychology and neuroscience suggesting that adolescence is a period of continued cognitive and brain development (Burnett et al., 2011; Blakemore and Mills, 2014). Because cognitive skills are measured while students are still in school, before labor-market outcomes are realized, the estimated effects reflect differences in measured skill rather than employer valuation of educational credentials. The results therefore provide direct evidence that field of study contributes to human-capital accumulation (Mincer, 1958; Becker, 1962, 1964),

[13] See Table 4 and Table A1 of Dahl et al. (2023), weighted by the number of applicants for each margin (2.1 percent) or the unweighted average (3.1 percent).

while leaving open the possibility that signaling or credential effects also contribute to subsequent labor-market returns (Spence, 1973, 1974).

3.3 Robustness checks

As robustness checks, we first examine whether the estimated RD coefficients are stable to the choice of bandwidth. Appendix Figure A10 shows that the RD estimates 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 our RD estimation strategy is applied to placebo cutoffs. We find little evidence of discontinuities in cognitive outcomes at these artificial cutoffs. This supports the validity of the identification strategy and suggests that the observed effects are driven by admission at the true cutoffs.[14] 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 a 0.2 GPA bonus was granted to first choice applicants from the underrepresented gender in a given field (Column 4), allowing for 12 or 60 slopes in the specification of the running variable (Columns 5-6), using preferred/next-best-pair-specific intercepts (Column 7), and excluding individuals with very low cognitive scores who may have intentionally failed the test (Column 8).The estimated effects are highly consistent in sign, magnitude, and statistical significance across alternative specifications, providing further confidence in the robustness of the results presented in Table 1.

3.4 Mechanisms

Our RD estimates identify the effect of admission to a field of study, which may include the impact of multiple factors – including curriculum, school experiences, peers, relative rank, teachers, and classroom practices. We seek to make a distinction between these mechanisms by exploring whether the observed effects are consistent with curricular exposure, peer composition or relative GPA rank. We then turn to examine heterogeneity across students background characteristics and finally report survey data responses about student-reported school experiences.

First, we relate the estimated effects to curricular exposure. Section 3.2 shows that the effects of STEM are concentrated in the technical and spatial tests, while we find no corresponding BSH advantage in the verbal test. These results mirror differences in curriculum. As documented in Section 2 and Appendix Table A1, mathematics, natural science, and technology account for roughly 40–60 percent of instructional time in STEM programs, compared with

[14]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 a 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 1, as illustrated in Appendix Figure A10.

about 15–20 percent in BSH programs. The concentration of estimated gains in technical and spatial skills is consistent with evidence that quantitative, scientific, and technical training can develop these forms of reasoning (Wai et al., 2009; Cortes et al., 2015; Uttal et al., 2013).

Second, we complement the administrative data with student survey data from the Evaluation Through Follow-up (UGU) study, which draws nationally representative samples of Swedish students. We use the cohorts born in 1967 and 1972, surveyed during the first year of high school on their perceived academic demands, support at school and at home, school experience, and educational intentions. Appendix Table A13 documents that STEM enrollees are subject to greater academic demands. STEM students report significant differences in terms of keeping up relative to expectations, keeping up with demands during lessons, and school-related stress is higher than among BSH enrollees.[15] They also report somewhat less support from teachers and from home, while enjoyment of high school and intentions to complete the current program are similar across fields. If these descriptive comparisons characterize the experiences of STEM and BSH students, the STEM cognitive gains are consistent with the reported more demanding school environment.

Third, we examine changes in peer composition and relative rank at the RD margin in Table 2. At the BSH vs. STEM margin, students admitted to a BSH program have peers whose average parental education is 0.17 years lower (about two months) and whose parental income is approximately SEK 14,900 lower, with the latter difference statistically significant only at the 10 percent level. The share of foreign-born peers falls by 0.4 percentage points, and the individual's within-school-field GPA rank falls by 0.018, or less than two percentile points. These socioeconomic and rank differences are relatively modest and provide limited support for peer composition or relative rank as the main channel behind the negative cognitive effects of BSH relative to STEM. In contrast, the female share of peers increases by 19 percentage points while our estimates indicate that cognitive skills decline, contrary to evidence that a higher female peer share tends to improve cognitive outcomes (Lavy and Schlosser, 2011; Gong et al., 2021).

Relative to non-academic programs, both STEM and BSH students are exposed to peers with more educated parents, with parental education increasing by about 0.34–0.39 years (roughly 4–5 months of schooling). Peers' parental income is also higher for BSH relative to non-academic programs, while the corresponding estimate for STEM is small and imprecise. These socioeconomic differences are modest and, importantly, do not mirror the pattern of cognitive effects, which are concentrated in STEM. At the STEM–non-academic margin, the female share of peers is essentially unchanged, making gender composition unlikely to explain the cognitive gains associated with STEM. Finally, changes in relative GPA rank are considerably larger when the second choice is non-academic: admission to first-choice STEM and BSH lowers students' relative rank by 0.357 and 0.248, respectively, compared with assignment to a non-

[15] Restricting the comparison to STEM and BSH enrollees in the RD analysis sample with UGU data yields a similar pattern of greater academic demands and less support in STEM (Appendix Table A14).

Table 2: High school majors and peer characteristics

	Peer characteristics				
	Parent income	Parent education	Foreign born	Female	GPA rank
STEM vs BSH	-3.128	0.080^{*}	0.002	-0.232^{***}	-0.031^{***}
	(8.421)	(0.044)	(0.001)	(0.009)	(0.006)
STEM vs non-academic	4.209	0.338^{***}	-0.007^{**}	0.009	-0.357^{***}
	(9.433)	(0.048)	(0.003)	(0.007)	(0.012)
BSH vs STEM	-14.890^{*}	-0.171^{***}	-0.004^{***}	0.192^{***}	-0.018^{***}
	(8.198)	(0.045)	(0.001)	(0.008)	(0.005)
BSH vs non-academic	18.920^{**}	0.385^{***}	-0.014^{***}	0.155^{***}	-0.248^{***}
	(8.641)	(0.040)	(0.002)	(0.006)	(0.008)
Observations	87,116	87,116	87,116	87,116	87,116

Note. The table presents the RD estimates of high school majors on school-major-year peer characteristics in terms of annual parental income (SEK thousands), parental education (years of schooling), foreign born, females, and GPA rank across high school majors at the RD margin. Peer composition refers to the leave-one-out mean of the characteristics of students enrolled in the same school-major-year cell. STEM denotes Engineering and Natural Science (Science, Technology, Engineering, and Mathematics); BSH denotes Business, Social Science, and Humanities; non-academic refers to vocational and general programs. 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.

academic second choice. Yet cognitive gains emerge only for STEM, providing little support for relative rank as the main explanation.

Finally, we examine heterogeneity in the RD estimates by parental socioeconomic background and by the level of the GPA cutoff. Appendix Table A15 shows that most estimated effects are similar in sign and magnitude across groups, although splitting the sample reduces precision (see Appendix Tables A16 and A17 for subtest estimates). The differences across groups are not statistically significant, but at the STEM vs. non-academic margin, the estimates suggest that the effects of STEM admission may be larger for students with more highly educated parents and at higher-cutoff admission margins. At lower-cutoff margins, admission to STEM rather than to a non-academic program reduces verbal skill, while the estimate for logic skill is also negative but imprecise (Appendix Table A17). These patterns suggest some heterogeneity across admission margins, although the differences should be interpreted cautiously given their limited precision.

To summarize, our analyses suggest that curricula is the mechanism best aligned with our estimates. However, we cannot fully distinguish mechanisms related to curricular content from closely related differences in pedagogy, teacher characteristics, or academic expectations. STEM programs may differ not only in the subjects they teach, but also in instructional practices and broader features of the learning environment. While the curriculum data document substantial differences in subject exposure, and the survey evidence describes differences in students' perceived academic demands across fields, we are unable to separately identify their

contribution from these other program-specific features. Disentangling these channels will be a challenge for future research.

4 Conclusion

This paper studies the causal effect of high school majors at age 16 on both crystallized and fluid cognitive development at age 18. Using GPA admission cutoffs and ranked program preferences in a regression discontinuity design, together with rich administrative data from Sweden, we show that admission to STEM rather than a non-academic program increases both crystallized and fluid skills by approximately 0.05 SD, with gains concentrated in technical and spatial skills. This pattern is consistent with differences in curricular exposure across fields.

These results have important policy implications. First, they suggest that cognitive development depends not only on the amount of schooling, but also on what students study. Admission to different fields of study can shape cognitive skills even within universal secondary education. Second, the effects of STEM expansion depend on which alternative program students would otherwise attend and on the particular dimension of cognitive skills considered.

More broadly, our findings are consistent with frameworks that emphasize field-specific human-capital accumulation (Mincer, 1958; Becker, 1962, 1964). An important avenue for future research is to examine how these cognitive gains evolve 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.

Table A2: 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

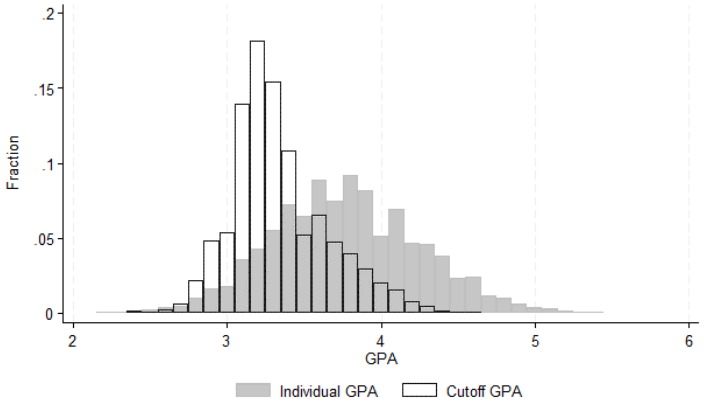
Note. The table presents the average fraction of years with a higher cutoff for one field compared to another within the same school region. If both fields have a cutoff in a given year in the same school region, we compare the two to determine which is higher. If one field has a cutoff, but the other 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.

Table A3: Oversubscribed and non-oversubscribed program sample sizes and summary statistics for all applicants with a first-choice academic program, 1977–1991

Baseline Sample:	**Oversubscribed programs**		**Non-oversubscribed 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	3.71		3.76	
GPA adjusted	3.81		3.89	
Foreign born	0.02		0.02	
Foreign born parent	0.14		0.14	
Father log earnings (SEK)	5.24		5.21	
Mother log earnings (SEK)	5.01		4.93	
Father schooling	11.87		11.82	
Mother schooling	11.48		11.36	
Father age	46		46	
Mother age	43		43	
Observations	**87,116**		**76,584**	

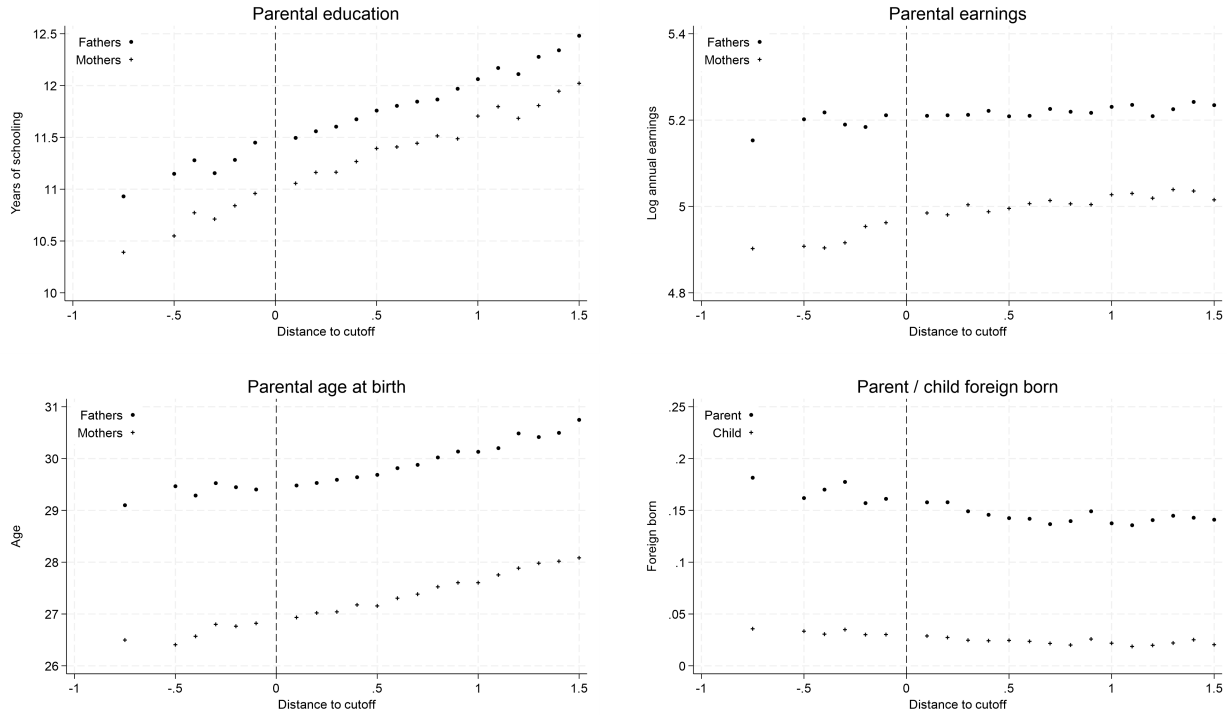
Note. Programs are defined by field, year, and school region. "Individuals" refers to the number/share of students, using the restrictions that form the baseline sample, applied to either an oversubscribed or non-oversubscribed program. Non-oversubscribed programs do not have an excess number of applicants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

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. The grey bars plot the distribution of GPA for individuals in our main analysis sample.

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 log father's earnings**	**Annual log mother's earnings**	**Age at birth father**	**Age at birth mother**	**Foreign born parent**	**Child foreign born**
RD estimate	0.020	-0.022	-0.004	-0.006	-0.148	-0.097	0.002	0.003
	(0.047)	(0.045)	(0.013)	(0.015)	(0.109)	(0.091)	(0.006)	(0.003)
Observations	84,212	83,325	77,797	78,951	85,815	86,520	87,116	87,116

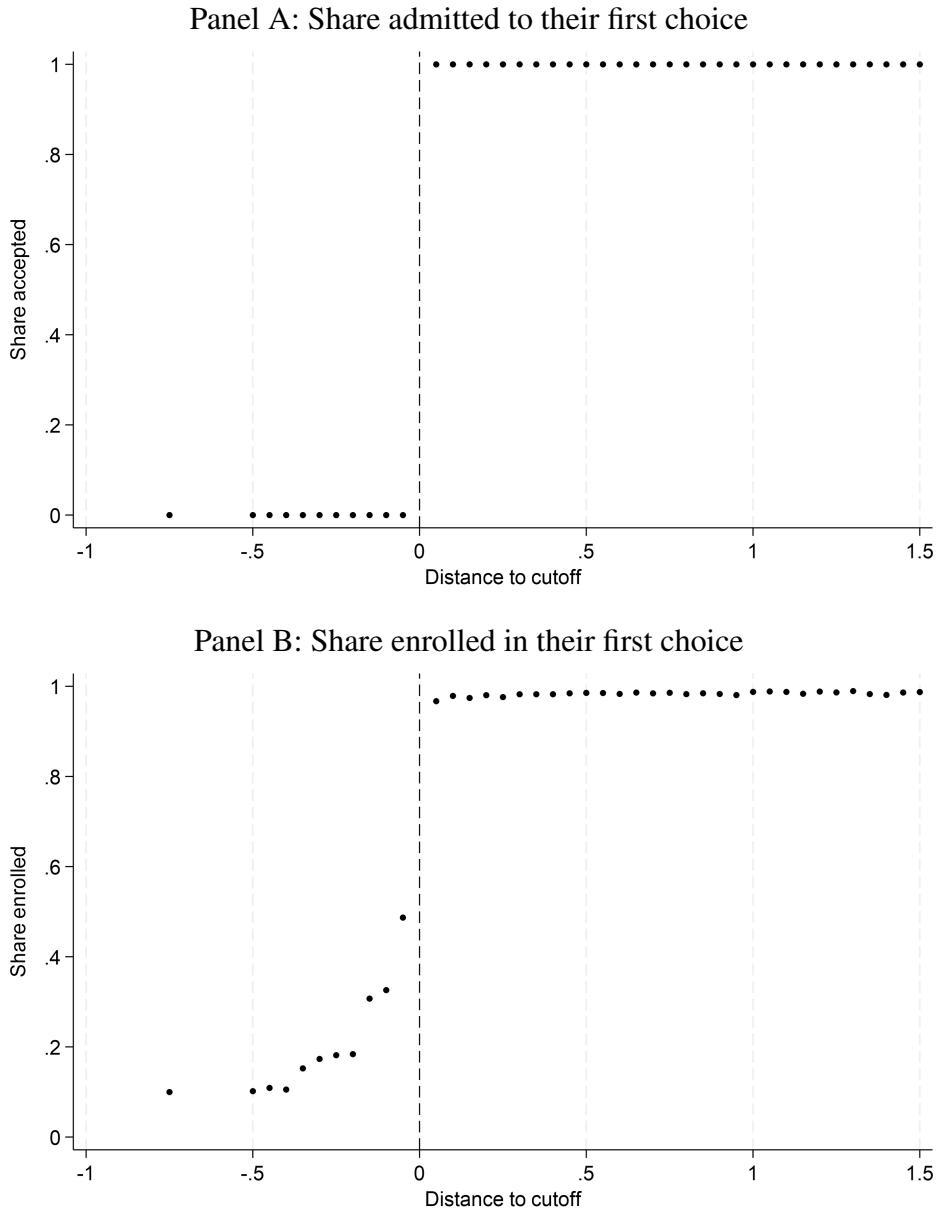
Note. Each column is an estimate from a separate RD regression, where the outcome is a linear function of the running variable (normalized GPA) within a window of -0.5 to 1.5, using triangular weights. All regressions include fixed effects for year, school region, and program, and a common slope on each 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	87,116

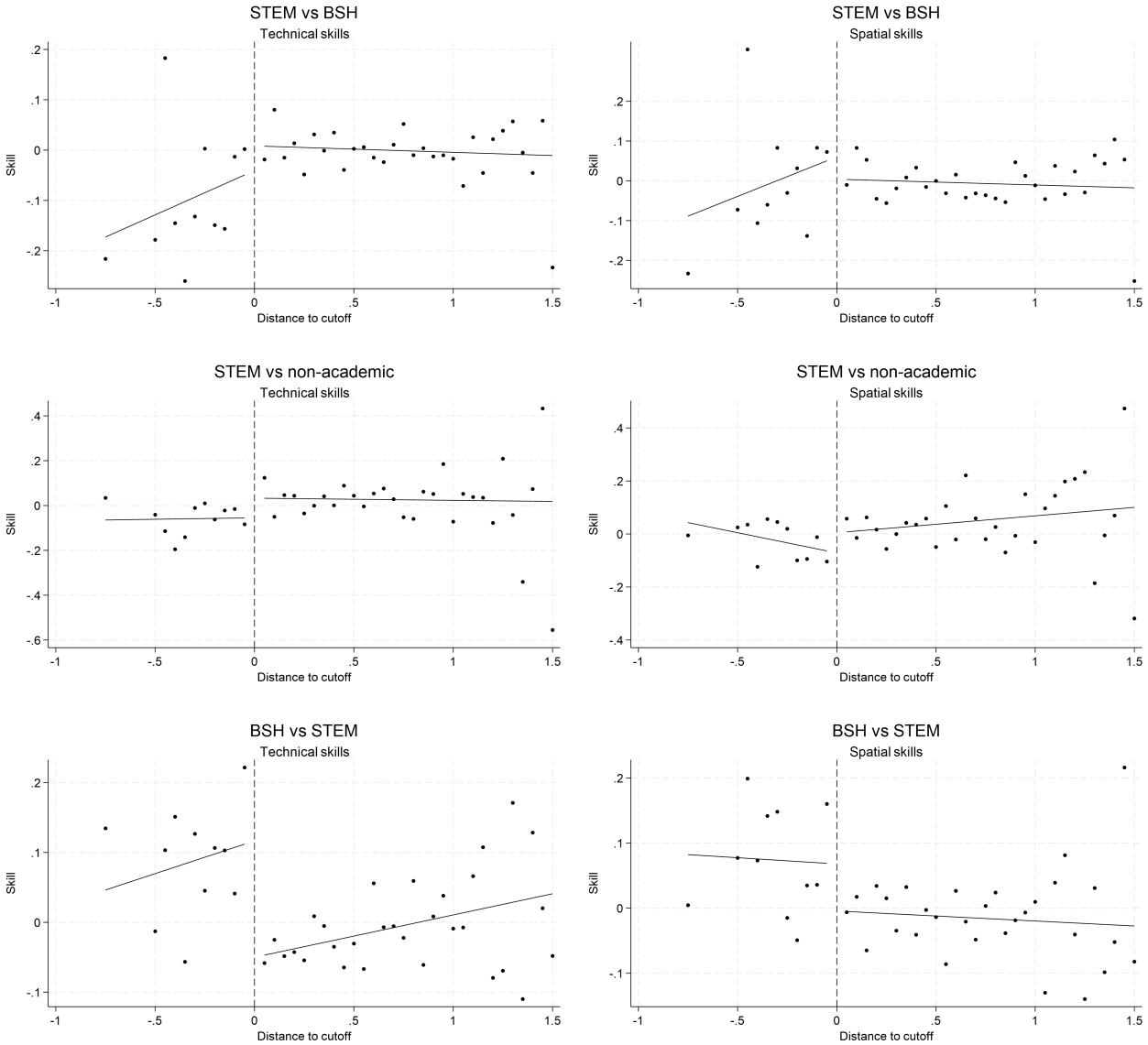
Note. The outcome variable is a dummy for whether the individuals enrolled in their preferred field, as a function of whether their GPA exceeded the admission cutoff. STEM denotes Engineering and Natural Science (Science, Technology, Engineering, and Mathematics); BSH denotes Business, Social Science, and Humanities; non-academic refers to vocational and general programs. 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.

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



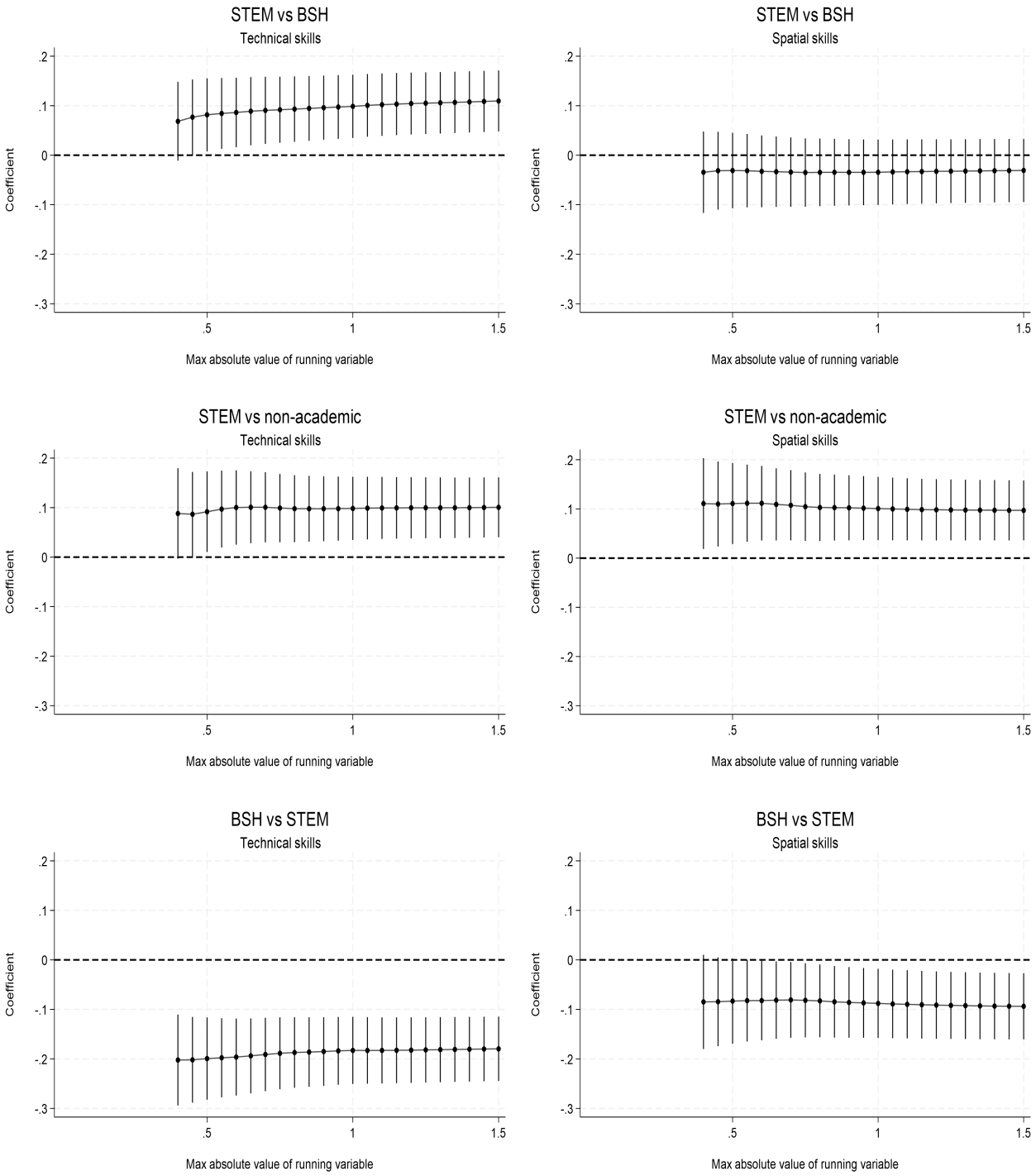
Note. Each dot is the average acceptance rate, or enrollment rate, in a 0.05 GPA bin, where GPA is measured relative to a cutoff normalized to zero. N = 87,116.

Figure A9: Cognitive skills by distance to the admission cutoff



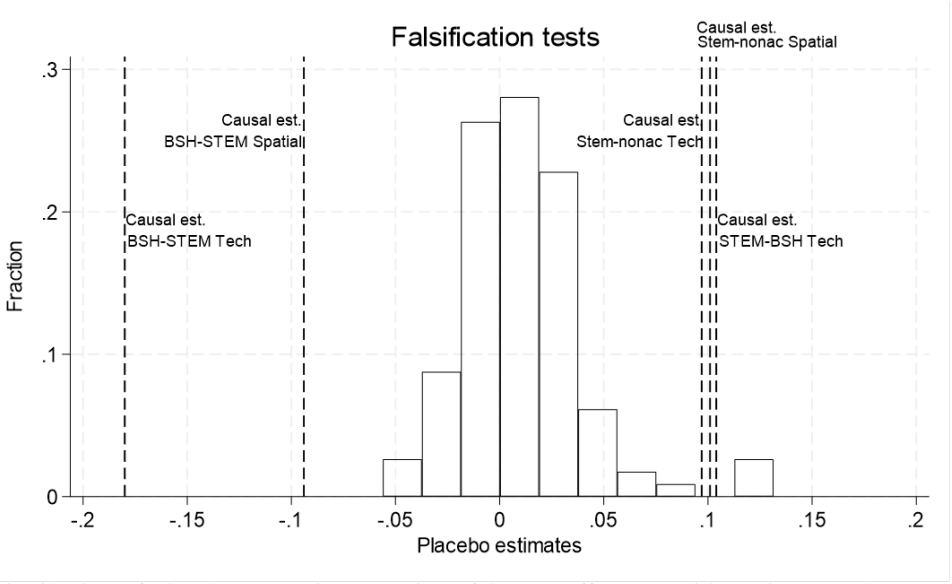
Note. Each observation is the average test score in 0.1 GPA bins, except for the leftmost dot, which is a 0.5 bin due to sparsity. The GPA is measured relative to a cutoff normalized to zero.

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: Falsification tests



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 A12: Robustness checks – reduced-form estimates using alternative specifications

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

Note. The table presents the robustness checks for reduced-form effects of the main specification in Table 1. STEM denotes Engineering and Natural Science (Science, Technology, Engineering, and Mathematics); BSH denotes Business, Social Science, and Humanities; non-academic refers to vocational and general programs. Column 1 presents the main specification presented in Table 1. Column 2 uses a symmetric bandwidth of 0.75. Column 3 includes the demographic controls listed in Appendix Table A3. Column 4 excludes the years 1982–1984, when a 0.2 GPA bonus was granted to applicants from the underrepresented gender in a given field (see Footnote 6). Columns 5 and 6 allow an increased number of slopes. Column 7 uses preferred/next-best-pair-specific intercepts. Column 8 excludes individuals with a score of 1 on the cognitive test. Robust standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A13: Academic demands and school environment among STEM and BSH enrollees

	Mean		p-value	
	STEM	BSH	Unadjusted	GPA-adjusted
A. Academic demands				
Ease of keeping up relative to expectations (1–5)	2.86	3.20	< 0.001	< 0.001
Can keep up during lessons (0–1)	0.80	0.89	< 0.001	< 0.001
Does not feel overly stressed at school (0–1)	0.64	0.72	< 0.001	< 0.001
Can concentrate during lessons (0–1)	0.83	0.78	0.022	0.293
B. Support and school experience				
Can ask teachers for help when needed (1–4)	3.79	3.83	0.084	0.025
Receives sufficient help from teachers (0–1)	0.74	0.77	0.100	0.009
Receives help with schoolwork at home (0–1)	0.75	0.83	< 0.001	< 0.001
Does not feel lonely at school (0–1)	0.97	0.97	0.617	0.901
Enjoyment of high school (1–5)	4.18	4.20	0.620	0.124
C. Persistence and educational intentions				
Persists when faced with a difficult task (0–1)	0.86	0.89	0.062	0.002
Intention to complete current program (1–3)	2.87	2.88	0.617	0.467
Observations	1,328	643		

Note. The sample includes all STEM and BSH enrollees with available Evaluation Through Follow-up (UGU) student-questionnaire data for cohorts born in 1967 and 1972. The first two columns report group means. The range of each measure is shown in parentheses. All measures are oriented so that higher values indicate a more favorable school experience, stronger intentions to continue the current program, or greater academic confidence. Negatively worded questionnaire items have been reverse-coded accordingly. The unadjusted p-values test differences in means between STEM and BSH enrollees. The GPA-adjusted p-values test the corresponding differences controlling for ninth-grade GPA. Values reported as 0.000 in the underlying output are shown as $p < 0.001$.

Table A14: Academic demands and school environment among STEM and BSH enrollees in the RD sample

	Mean		p-value	
	STEM	BSH	Unadjusted	GPA-adjusted
A. Academic demands				
Ease of keeping up relative to expectations (1–5)	2.89	3.25	< 0.001	< 0.001
Can keep up during lessons (0–1)	0.79	0.91	< 0.001	< 0.001
Does not feel overly stressed at school (0–1)	0.63	0.75	< 0.001	< 0.001
Can concentrate during lessons (0–1)	0.81	0.77	0.133	0.581
B. Support and school experience				
Can ask teachers for help when needed (1–4)	3.78	3.87	0.007	0.003
Receives sufficient help from teachers (0–1)	0.72	0.84	< 0.001	< 0.001
Receives help with schoolwork at home when needed (0–1)	0.77	0.86	0.002	0.002
Does not feel lonely at school (0–1)	0.98	0.98	0.476	0.276
Enjoyment of high school (1–5)	4.21	4.22	0.863	0.369
C. Persistence and educational intentions				
Persists when faced with a difficult task (0–1)	0.88	0.87	0.951	0.382
Intention to complete current program (1–3)	2.86	2.87	0.819	0.732
Observations	497	301		

Note. The sample is restricted to STEM and BSH enrollees in the RD analysis sample with available Evaluation Through Follow-up (UGU) student-questionnaire data from the cohorts born in 1967 and 1972. The first two columns report group means. The range of each measure is shown in parentheses. All measures are oriented so that higher values indicate a more favorable school experience, stronger intentions to continue the current program, or greater academic confidence. Negatively worded questionnaire items have been reverse-coded accordingly. The unadjusted p-values test differences in means between STEM and BSH enrollees. The GPA-adjusted p-values test the corresponding differences controlling for ninth-grade GPA. Values reported as 0.000 in the underlying output are shown as $p < 0.001$.

Table A15: Heterogeneity: high school majors and cognitive development

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

Note. The table presents the heterogeneity RD estimates of high school majors on crystallized and fluid skills. High parental income is defined as income above the median; high parental education is defined as at least 12 years of schooling and high GPA cutoff is defined as greater than 3.3 (median GPA cutoff). STEM denotes Engineering and Natural Science (Science, Technology, Engineering, and Mathematics); BSH denotes Business, Social Science, and Humanities; non-academic refers to vocational and general programs. 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 A16: Heterogeneity by parents' socioeconomic status

Panel A: Parent 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
Panel B: Parent 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 * *At least 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 * *At least 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 * *At least 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 * *At least 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

Note. The table presents the heterogeneity estimates by parents' socioeconomic status (income and education). STEM denotes Engineering and Natural Science (Science, Technology, Engineering, and Mathematics); BSH denotes Business, Social Science, and Humanities; non-academic refers to vocational and general programs. 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 A17: Heterogeneity by GPA cutoff (median split)

	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 by GPA cutoff (median split), where below indicates values equal to or below the median and above indicates values above the median. The median GPA cutoff is 3.3. STEM denotes Engineering and Natural Science (Science, Technology, Engineering, and Mathematics); BSH denotes Business, Social Science, and Humanities; non-academic refers to vocational and general programs. 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.