Yes, You Can! Effects of Transparent Admission Standards on High School Track Choice: A Randomized Field Experiment

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ABSTRACT

High school track choice determines college access in many countries. We hypothesize that some qualified students avoid the college-bound track simply because they overestimate admission requirements. To test this hypothesis, we designed a randomized field experiment that communicated the admission standards of local secondary schools on the academic track to students in Hungary before the application deadline. We targeted the subset of students ("seeds") who occupied the most central position in the classroom-social networks, aiming to detect both direct effects on the track choice of targeted seeds and spillover effects on their untreated peers. We found neither a direct effect nor a spillover effect on students' applications or admissions on average. Further analyses, however, revealed theoretically plausible heterogeneity in the direct causal effect of the intervention on the track choice of targeted seeds. Providing information about admission standards increased applications and admissions to secondary schools on the academic track among seeds who had a pre-existing interest in the academic track but were unsure of their chances of admission. This demonstrates that publicizing admissions standards can set students on a more ambitious educational trajectory. We discuss implications for theory and policy.

JEL codes: C93, I20, D91, J24

Keywords: High school track choice; randomized field experiment; educational aspirations; spillover effect; Hungary.

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Az átlátható felvételi követelmények hatása a középiskola képzéstípusának kiválasztására: Egy randomizált terepkísérlet

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ÖSSZEFOGLALÓ

A középiskola képzéstípusának kiválasztása meghatározza a későbbi továbbtanulási döntéseket, pl. azt, hogy valaki mennyire könnyen tud bekerülni a felsőoktatásba. Feltételezésünk szerint sok, tanulmányi eredményeit tekintve arra alkalmas diák azért nem jelentkezik a gimnáziumi képzésre, mert túlbecsüli a felvételi követelményeket. Ennek a hipotézisnek tesztelésére egy randomizált terepkísérletet végeztünk, amely egy információs kampányból állt. A középiskolába történő jelentkezési határidő előtt közöltük a nyolcadik osztályos diákokkal, hogy a környékükön lévő gimnáziumokba mik voltak a felvételi követelmények az előző évben. Ezt az információt a diákoknak azon csoportja ("központi diákok") kapta meg, akik az osztályon belüli kapcsolathálóban központi helyen voltak. A kutatási design így lehetővé tette az információs kampány direkt és indirekt hatásának szétválasztását. Az információs kampány nem befolyásolta azt, hogy az átlagos diák gimnáziumba jelentkezett-e vagy felvették-e őt oda. Ugyanakkor elméletileg alátámasztható heterogenitást tártunk fel az információs kampány direkt hatásában. Az átlátható felvételi követelményekről való informálás azon központi diákok körében növelte a gimnáziumi jelentkezést, akik korábban gimnáziumba akartak jelentkezni, de saját felvételi esélyüket alacsonyra értékelték. Mindez alátámasztja azt a feltételezésünket, hogy az átlátható felvételi követelmények ambiciózusabb továbbtanulási döntéseket eredményeznek. Tanulmányunkban megvitatjuk eredményeink elméleti és közpolitikai jelentőségét.

JEL: C93, I20, D91, J24

Kulcsszavak: középiskola választás, randomizált terepkísérlet, direct és indirect oksági hatások, továbbtanulási szándék

I. Introduction

Increasing college enrollment is a common objective of educational policies around the world. In many countries, however, college access is determined at relatively young ages by track choices in secondary (high) school. Choosing the wrong track can derail students' educational trajectories and ultimately diminish their socio-economic attainment. The challenge for any educational policy thus is to ensure that no talent is wasted as students choose educational tracks.

Educational track choices centrally depend on students' and their families' academic aspirations. High aspirations, however, do not automatically translate into corresponding choices (Weiss and Steininger 2013), for example, if decisions are made under uncertainty and students are not confident in their chances of success (Sjögren and Sällström 2004).

Past sociological research has argued that students may forgo advanced education if they do not expect to succeed in their chosen track, conditional on being admitted (Breen and Goldthorpe (1997). Even earlier in the process, however, we argue that students will not even apply to their preferred track if they do not expect to gain admission in the first place. If students systematically overestimate hurdles to admission, then correcting their misperceptions by communicating the actual admission standards prior to application may empower some qualified students to apply to, and ultimately attend, a more demanding track in high school.

To test the hypothesis that opaque admission standards may deter applications and prevent admissions of qualified students to high schools on the academic track, we conducted a randomized field in 26 Hungarian schools. A few months before rising 8th graders had to submit their applications for secondary school, we showed them the grades of students who had previously been admitted to local high schools on the academic track ("grammar school"). Unbeknownst to many students and parents, many students are admitted to grammar school despite having low grades. Students were then instructed to compare their own grades to the grades of students that had previously been admitted to each local grammar school, with the aim of empowering qualified students to apply to grammar schools themselves.

Since some educational decisions are subject to peer influence (Anelli and Peri 2017; Zölitz and Feld 2017, Fletcher 2012; Lyle 2007), we aimed to detect both direct and spillover effects of our information campaign. In order to maximize spillover, we therefore systematically selected the most central students in the classroom as seeds to receive information about admission standards.

We found neither a direct effect on the seeds who received the intervention nor a spillover effect on the seeds' peers with respect to applications or admissions to grammar school on average. Further analyses, however, revealed theoretically plausible heterogeneity in the direct treatment effect on seeds' track choice. Providing information about admission standards increased applications and admissions to the academic track among seeds that had a pre-existing interest in the academic track. The intervention did not influence seeds who did not previously intend to apply to the academic track. This is plausible, since the intervention was not designed to motivate interest in the academic track per se, only to clarify the admission standards to the academic track.

Our study contributes to the literature on educational choice in several ways. First, our findings suggest that students' perceptions of their chances of admission are biased, that these perceptions can be changed by clarifying admission standards, and that clarifying admission standards can causally affect track choice in high school, presumably by affecting students' perceptions of their chances of admission. Second, by intervening on students' rather than parents' information set, we show that adolescents possess agency in far-reaching educational decisions. Third, our results emphasize an arguably neglected cognitive dimension of sociological rational choice theories (Breen and Goldthorpe 1997) and the theory of planned behavior (Ajzen 1985; Ajzen and Fishbein 1980). Whereas prior information campaigns that sought to influence educational choice were designed to raise educational aspirations by expounding the economic returns to education, our study provides field-experimental evidence that increasing the perceived probability of admission helps translate abstract aspirations into manifest behavior.

The rest of this paper is organized as follows: Section 2 elaborates on our theoretical framework and reviews prior research; Section 3 introduces the Hungarian setting; Section 4 details the study design; Section 5 reports results. Section 6 offers concluding remarks.

2. Theoretical framework

Track choice and the self-perceived chances of success

Sociologists and psychologists have long argued the importance of aspirations for reasoned action (Ajzen 1985; Ajzen and Fishbein 1980; Fishbein and Ajzen 1975). In a rational choice framework, aspirations are informed by the expected payoff (costs and benefits) of the aspired state. Whether individuals act on their baseline aspirations, however, also depends on their self-perceived probability of success, i.e., their confidence in their own ability to succeed in the action (Bandura, Adams, and Beyer 1977). Theorists variously incorporate perceived chances of success in the concepts of perceived behavioral control (Ajzen 1991) and self-efficacy (Bandura 1982; Bandura 1986).

Applied to educational choice, this suggests that a student's ex-ante expectation of success may contribute to their educational track choice. Prior work has argued that educational choice responds to the probability that the student will succeed academically in their chosen track. Even earlier in the process, we argue that students may not even apply to their preferred track of they do not expect to gain admission.

Students' perceptions of their chances of admission likely depends on students' beliefs about admission standards. This provides an opportunity for intervention. Schools in many educational systems cannot publish exact admission cutoffs before receiving students' applications, because admission cutoffs depend on the applicant pool. Therefore, applicants have little means to gauge whether their academic record qualifies them for admission to any particular track or school. If students systematically overestimate admission standards, then even qualified students may be discouraged from applying to schools on the academically oriented track, a decision that limits future educational opportunities and socioeconomic achievement.

Since admissions to selective schools are often competitive (Blossfeld et al. 2016), students' chances of admission also depend on their rank in the competition (Tran and Zeckhauser 2012). Behavioral economists argue that relative performance feedback is especially motivating for students who rank highly but lack information (Bandiera, Larcinese, and Rasul 2015). Azmat and Iriberri (2010) showed that relative performance feedback helps students set their optimal level of effort, since their relative position informs whether their efforts will be rewarded.

If relative performance feedback increases the self-confidence of well-performing students, and self-confidence influences educational decisions, then providing students with information about admission thresholds may influence their track choice. Previous observational research suggests that students whose self-perceived academic performance is high have a higher chance of admission to grammar schools in Hungary (Keller 2018) and are more likely to apply to tertiary education even if their grades are lower than average (Keller 2016). Similarly, students' self-perceived success probability for different courses of study increased the likelihood of opting for college rather than trade schools among Dutch graduate students of academically oriented high schools (Tolsma, Need, and de Jong 2010).

The empowerment of students with a disadvantaged family background is especially important, since advantaged families are more likely to push their children to apply to more demanding educational tracks (Gambetta 1987). For example, Barone, Schizzerotto et al. (2017) argue that information biases result in social inequalities in track choice. Therefore, the empowerment of children from low-status families might reduce existing inequalities in educational choices.

Prior information campaigns to influence educational choice

Information campaigns are popular interventions in field experiments on educational choice. Most prior information campaigns studied college enrollment decisions (e.g., Bettinger et al. 2012; Hoxby and Turner 2013; Oreopoulos and Dunn 2013; Loyalka et al. 2013; Carrell and Sacerdote 2013; McGuigan, McNally, and Wyness 2014; Kerr et al. 2014; Castleman, Page, and Schooley 2014; Barone, Schizzerotto, et al. 2017; Ehlert et al. 2017; Peter and Zambre 2017; and Oreopoulos, Brown, and Lavecchia 2017; see Herbaut and Geven 2020 for a recent review).

By contrast, very few studied secondary track choice, which determines eligibility for college enrollment in many stratified educational systems in Europe and elsewhere. One notable exception is Barone, Assirelli et al.'s (2017) field experiment in Italy, which targeted low-educated mothers of high-performing students prior to their children's secondary-track choice. Mothers were read a short message over the phone, explaining that their children had the grades to succeed on the academic track and would not expect economic disadvantage from choosing the academic track. This intervention increased students' enrollment in the academic track by 10.1 percentage points ($p < 0.1$). Dinkelman and Martínez (2014) showed a 15-minute movie with testimonials on the value of hard work and the availability of financial aid in college to low-income middle-school students in Chile. This intervention increased enrollment in academically oriented high schools by 6.3 percentage points ($p < 0.1$).

Most prior campaigns aiming to stimulate educational choices provided parents or students with information about the cost of education (Hoxby and Turner 2013; Oreopoulos and Dunn 2013; Dinkelman and Martínez 2014) or about the economic value of education (Jensen 2010; Nguyen 2008; Peter and Zambre 2017). Fewer provided information about procedural aspects of the application process and deadlines (Hoxby and Turner 2013; Castleman, Page, and Schooley 2014). To our knowledge, no prior field experiment investigated how uncertainty about admission standards affects secondary school choice.

Prior information campaigns conveyed information in three ways. One group of studies provided information in writing via websites, (Oreopoulos and Dunn 2013; McGuigan, McNally, and Wyness 2014), surveys (Booij, Leuven, and Oosterbeek 2012), or brochures (Hoxby and Turner 2013). Other studies provided information in person or over the phone, through a teacher or a trained specialist (Jensen 2010; Loyalka et al. 2013; Kerr et al. 2014; Carrell and Sacerdote 2013; Castleman, Page, and Schooley 2014; Bettinger et al. 2012; Barone, Assirelli, et al. 2017; Barone, Schizzerotto, et al. 2017; Ehlert et al. 2017; Peter and Zambre 2017). A third group of studies provided information via role models with similar backrounds as the targeted students who offered personal testimonies about their own educational careers (Dinkelman and Martínez 2014; Nguyen 2008; Herber 2015).

Our study differs from previous studies in several ways. First, our study is the first to focus exclusively on reducing uncertainty about admission standards for academically selective secondary schools. Second, our study is the first randomized field experiment to evaluate spillover effects of an information campaign in educational choice. Third, our study is the first randomized information campaign on educational choice in Eastern Europe, where Hungary represents a test-case for other highly stratified educational systems with early tracking (Horn, Keller, and Róbert 2016).

The role of peer influence in educational decisions

Sociologists have long argued that adults, parents, and teachers exert persuasive power on school choices in adolescence (Buchmann and Dalton 2002; Haller and Butterworth 1960; Sewell and Shah 1968). Furthermore, peers become role models as well as sources of social influence over attitudes and behaviors (Cillessen 2007; Veenstra and Dijkstra 2011; Veenstra et al. 2013).

Randomized experiments on peer effects in educational decisions, however, are rare, mainly focusing on choices after compulsory education. Anelli and Peri (2017), analyzed the college-major choice of Italian high school students and found that male students with fewer female peers are more likely to choose male-dominated majors. Zölitz and Feld (2017) found that Dutch female college students of business and economics exposed to a higher proportion of female peers are less likely to choose math-intensive majors. Investigating cadets at the U.S. Military Academy West Point, Lyle (2007) found support for role model effects: an increase in the fraction of sophomores in the company intending to study engineering increased the probability that other freshmen choose engineering as a major. These findings indicate that peers can influence educational choices. To the best of our knowledge, no prior study has investigated peer effects on secondary track choice.

3. Setting: Track Choice in Hungarian Secondary Schools

We study track choice in Hungarian secondary education (Kóczy 2010). Similar to other European countries, secondary education in Hungary is stratified into three tracks. Grammar schools (*gimnázium*) form the most academically selective track and aim to prepare students for college. Vocational schools (*szakközépiskola*) form the least academically oriented track and prepare students for manual professions and trades. Mixed schools *(szakgimnázium*) contain components of both the academic and the vocational track.

We focus on applications to grammar school because of their disproportionate importance as a gateway to tertiary education and student's subsequent life chances. Although all students who pass the final high-school examination (*érettségi*) in grammar or mixed schools are eligible for enrollment in tertiary education, in practice, grammar-school graduates dominate college enrollment, and their advantage has been increasing over time. In 2016, grammar school graduates had a 16-percentage point advantage for entering tertiary education over mixed school graduates, up from a 9-percentage point advantage in 2007 (Varga, 2018: 244). In 2016, 72 percent of college freshmen were grammar school graduates.[1] The economic returns to college, in turn, are higher in Hungary than in any other OECD country: young adult college graduates earn more than twice as much as individuals who do not graduate from college (OECD 2008:173).

The application to secondary education is a multistage, nationally coordinated matching process. In the spring semester of 8th grade, the last year of general (un-tracked) primary education, primary schools submit students' ranked preferences for secondary schools to the national Admission Center, an office within Hungarian Educational Authority.[2] Students may rank any number of schools across all tracks, free of charge. Secondary schools know which students have applied to them, but they do not know how highly students have ranked each school. Secondary schools then rank applicants by considering between one and three criteria. These criteria are fixed within school but vary across schools. First, all

[1] Hungarian Educational Authority, email dated June 6, 2017.

[2] Students' preference rankings are signed by students and their parents. In a 2006 survey, 75 percent of ninth graders reported having made their application choice on their own (Keller 2018). Since schooling is compulsory until age 16, virtually all students must enroll in secondary education.

secondary schools consider prior grades (typically year-end grades from 7th grade and fall-semester grades from 8th grade) in a range of core subjects, including Hungarian grammar and literature, math, history, and a foreign language. Second, some secondary schools require scores from a centrally administered, national admissions exam in mathematics and reading comprehension.[3] Third, a minority of secondary schools requires a personal interview. In the final step, the national admission center matches each applicant to their most-preferred school among that schools that will admit them using a Gale-Shapley algorithm (Gale and Shapley 1962).[4] During the 2017-2018 school year, 81,883 8th grade students in Hungary participated in the application process, of which 36.4% were admitted to grammar schools (Hungarian Educational Authority, 2017).

Considerable uncertainty surrounds the applications and admissions process. Although all schools publish which criteria they will consider for admission on their websites (i.e., grades, exam, and personal interview), anecdotal evidence suggests that many students do not know the criteria considered by the schools in their vicinity. Most importantly, admission cutoffs (for grades and admission exam scores) are not known to teachers, parents, or students prior to application, because cutoffs depend on the current years' applicant pool. Furthermore, the grades of previously admitted students are also unknown. Therefore, although students know their own grades, they do not know whether their qualifications fall above or below the admission threshold for any particular grammar school in their local area.

Uncertainty about admission standards plausibly leads to some amount of mismatch between track choice and student ability if students rank their application preferences based on mistaken beliefs about their own performance relative to the admission threshold at the schools they wish to attend. Therefore, qualified students may refrain from ranking a grammar school as first choice despite being qualified for grammar school, an action all but guaranteeing that they will not attend grammar school.

Results from the National Assessment of Basic Competencies (NABC), a mandatory PISA-like standardized testing program in Hungary, provide some evidence for this mismatch. Figure 1 shows 2005 NABC-score distributions in mathematics and reading for a nationally representative sample of 8th graders (finishing elementary school) from the 2006

[3] Participation in the admission exam requires registration. Students usually complete the admission exam in mid-January and receive their results by early February, before they apply to secondary schools in mid-February.

[4] Students who do not qualify for any of their ranked schools in the general application process must participate in a special application process where they can apply for admission to any secondary school that still has seats available.

Hungarian Life Course Survey (HLCS), by the secondary track that the same students attended in 9th grade. Although the means of the test score distributions differ substantially between vocational, mixed, and grammar schools, there is great variance and consequently substantial overlap in students' measured competencies across tracks. Judging by NABC scores alone, about 34 percent of students in vocational schools and 30 percent of students in mixed schools have higher scores in mathematics and reading than the bottom quartile of grammar school students. This demonstrates that student sorting into secondary tracks is not perfect, and it suggests that a substantial number of students who do not attend grammar school could have attended grammar school. [5]

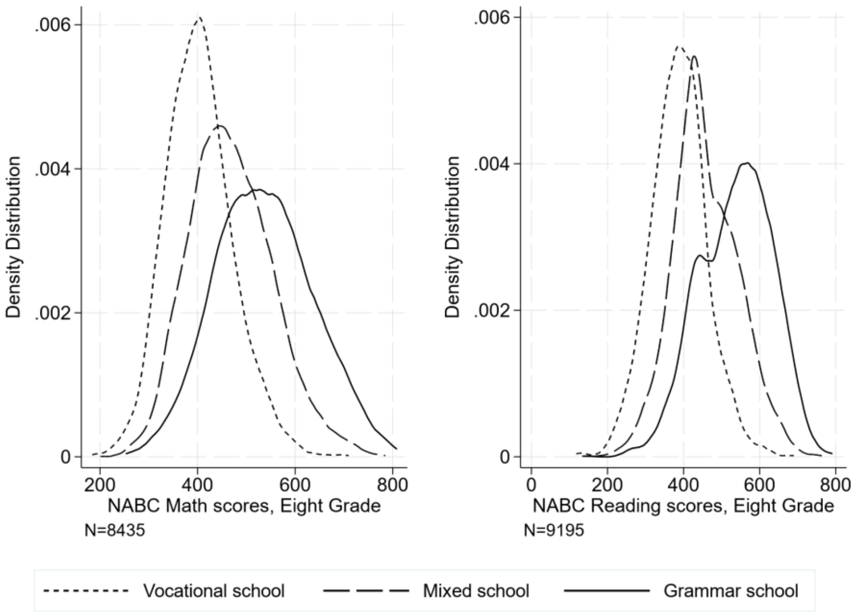

Figure 1. Overlap of Hungarian reading comprehension and mathematics test scores from the National Assessment of Basic Competencies (NABC) in 8th grade (2005) by upper-secondary track enrollment in 9th grade (2006). Hungarian Life Course Survey (HCLS) (2006). Authors' calculations.

4. Study Design, Sample, and Methods

We carried out a pair-matched cluster randomized field experiment in 26 Hungarian primary schools. Randomization occurred at the school level. Our design has two distinctive features.

[5] NABC scores are not considered in secondary-school admissions. Clearly, students in vocational, mixed, and grammar school may differ on other admission-relevant characteristics.

First, our study focused on empowering qualified students to apply to schools on the college-bound track by revealing which grammar schools were within their reach. To this end, our design provided not only information about absolute admission thresholds, but also individualized information about the student's own position relative to the admission threshold. Second, we prioritized the detection of spillover effects by providing the information only to the most central students in each classroom. Our study was powered to detect medium-sized direct effects and spillover effects of the intervention on targeted students and on untreated peers, amounting to approximately 20 percentage point and 10 percentage point increases in applications, respectively.[6]

Sample: Our sample included 26 Hungarian primary schools, drawn from a larger panel study conducted by the Research Center for Educational and Network Studies (RECENS) at the Centre for Social Sciences, Budapest. The RECENS panel is concentrated in the disadvantaged Northern and Central regions of Hungary and therefore over-represents students of low socio-economic status and of Roma ethnicity. Compared to the national average, students in the RECENS panel performed 0.33 standard deviations lower in mathematics and 0.37 standard deviations lower in reading comprehension on the 2015 nationally standardized NABC competency assessment of sixth graders (authors' calculations).

Our field experiment included all schools from the RECENS panel willing to participate in the study. Within these schools, we focused on the 671 students out of a total of 702 students in 39 participating 8th grade classrooms who had previously provided active written parental consent to participate in the RECENS panel study. Failure to provide parental consent was non-differential between treatment and control schools (3.7 percent vs. 5.1 percent, respectively, $p = 0.36$). After dropping 4 students with missing outcomes (3 in the control and 1 in the treatment group), our analytic sample includes 667 students. Schools were blind to their future treatment (or control) status at the time of enrollment.

Blocking and Randomization: Following best-practice recommendations for cluster randomized trials, we first paired the 26 schools on the first principal component of twelve school-level characteristics, derived from the RECENS panel survey (when students were in 7th grade) and from the May 2015 NABC (when students were in sixth grade). Pair matching reduces bias if the two schools in each matched pair are roughly the same size, and it increases efficiency if pair membership predicts the outcome (Imai, King, and Nall 2009).

Therefore, our blocking variables include all pertinent variables available to us at the time of randomization, including grade-point averages (GPA), average NABC test scores,[7] and the share of students in each school who had previously (in 7th grade) expressed intentions to apply to grammar school.

Using a random number generator, we allocated one school within each matched pair to the intervention (treatment) and one school to no intervention (control). School-level descriptive statistics for all blocking variables for each school are given in Appendix Table A1.

Targeted seeds and non-targeted peers: We divided the students in each classroom into seeds and peers. In schools randomized to the treatment condition, seeds received the intervention, while peers did not. In schools randomized to the control condition, we identified the students who would have served as seeds, even though nobody received treatment (Figure 2).

		Students	
		Seeds	**Peers**
Schools	**Treated**	Treated seeds N=76	Treated peers N=254
	Control	Control seeds N=79	Control peers N=262

Figure 2. Sample partition

Seed students are defined as the 20 percent most-central students in each classroom who consented to participate in the study. Seeds were selected based on social network information gathered in earlier waves of the RECENS panel study. Building on Banerjee et al.'s (2013)

[7] At the time, we could only access NABC scores at the school level, but not at the individual level.

measure, we operationalized centrality as having the highest reach to other students in the classroom via direct and indirect (lower-weighted) connections in the combined directed network of friendship, advice-giving, and admiration nominations. We selected the most central students as seeds in this manner to maximize the chance of detecting spillover effects in the classroom.

Consent: We obtained active written consent for the intervention from the parents of all seeds in both treated and control schools.

Covariate balance: Appendix Table A2 shows descriptive statistics and covariate balance for students in the analytic sample in treatment and control schools, for two sets of variables: the five variables that were included in the blocking score and additional variables that were not yet available to us at the time of randomization. Since the latter variables were not used for blocking, they provide a stronger randomization check in our analytic sample. Table A2 shows that the sample is well balanced. We found no statistically significant differences between students in the treated and control schools, between seeds in treated and control schools, or between peers in treated and control schools. Remaining imbalances are small and tend to balance out across covariates. For example, students in the treatment schools have somewhat better grades in Hungarian language and grammar, history and foreign languages, but somewhat worse grades in math.

Nonetheless, we observe that treated seeds (but not peers) are more likely than control seeds to report an early intention to apply to grammar school in the 7th grade survey (46 vs. 35 percent, respectively, $p = 0.19$), although the difference is not statistically significant. Since intention to apply may translate into actual applications, all analyses control for baseline characteristics, including and pre-intervention intentions to apply for grammar school.

Descriptive statistics for the analytic sample: Table 1 shows that half of the students in our study were girls, one third were of Roma ethnicity, and less than half of mothers and fathers had graduated from high school. Since seeds were specifically selected to be central within their classroom social network, seeds were more likely to be girls, less likely to be of Roma ethnicity, had parents with more education, and had higher baseline grades than peers.

Descriptive results corroborate our assumption that intention to apply correlates with the perceived likelihood of admission. After controlling for GPA, students who did plan to apply to grammar school in 7th grade (one year before the actual application) estimated their own admission chances to be nearly one unit higher on an 11-point scale (p<0.001) than students who did not plan to apply to grammar school.

Seeds were more likely than peers to report prior plans to apply to grammar school (41 vs. 24 percent, $p < 0.01$). Similarly, seeds reported a 1-point higher perceived likelihood of admission to grammar schools than peers on an 11-point scale ($p < 0.01$). Treated students' higher intentions to apply to grammar school are a mixed blessing. On one hand, their greater intentions and central position in the classroom network may be advantageous for generating spillover effects. On the other hand, their peers are probably less susceptible to influence since they show less baseline interest in grammar school. Similarly, seeds' greater confidence in their admission chances might also raise doubts about the relevance of the treatment for them.

Table 1. Descriptive statistics of the main variables in the analysis

	All students, N = 667			Seeds, N = 155			Peers, N = 512		
	Mean	SD	% missing	Mean	SD	% missing	Mean	SD	% missing
Baseline covariates									
Female %	0.50	0.50	8.10%	0.62	0.49	6.45%	0.47	0.50	8.59%
Roma ethnicity %	0.33	0.47	3.30%	0.29	0.45	1.29%	0.34	0.47	3.91%
Parents' education ≥ high school %	0.30	0.46	7.80%	0.34	0.48	5.81%	0.29	0.45	8.40%
Intention to apply to grammar school (=1 if yes) %	0.28	0.45	2.85%	0.41	0.49	0.00%	0.24	0.43	3.71%
Perceived likelihood of admission to grammar school; *range: 0-10*	6.13	2.70	9.30%	6.86	2.43	6.45%	5.90	2.74	10.16%
GPA, 7th grade; *range: 1-5*[a]	3.59	0.84	0.00%	3.99	0.79	0.00%	3.47	0.82	0.00%
Outcomes									
Applied grammar school as first choice %	0.27	0.45	0.00%	0.43	0.50	0.00%	0.22	0.42	0.00%
Admitted to a grammar school %	0.23	0.42	0.00%	0.37	0.49	0.00%	0.19	0.39	0.00%
Treatment and targeting									
Treated; *range: 0, 1*	0.51	0.50	0.00%	0.49	0.50	0.00%	0.51	0.50	0.00%
Seed; *range: 0, 1*	0.23	0.42	0.00%	1.00	0.00	0.00%	0.00	0.00	0.00%

[a] School subjects are graded from 1 to 5, where 5 is best

Intervention: The intervention took place in October 2016, four months before students had to submit their applications to secondary school and two months before students had to register for the national admissions exam (Appendix Figure A1). The intervention consisted of lectures, discussions, and exercises, spanning two consecutive standard lessons of 45 minutes, with one 15-minute break. To guarantee treatment homogeneity, we trained one female professional coach who had experience with the targeted age group to deliver the intervention. We pre-tested the intervention in one school outside of our sample in a different Hungarian county.

The intervention comprised three components. First, we informed the seeds of the likely GPA requirements for admission to all grammar schools in the local area. Specifically, for each grammar school within a 30-km radius of the seed's primary school, we showed the seed the minimum and median GPA in 7th grade core subjects among students who had been admitted to the grammar school in the previous year (Figure 3). The coach spent approximately 15 minutes presenting this information, using PowerPoint slides, paper handouts, and verbal explanations. The coach explained that although admission cutoffs can vary from year to year, they are quite stable within any given school. Therefore, our intervention provided students with pertinent (if incomplete) information about grammar school admission standards in their local area.

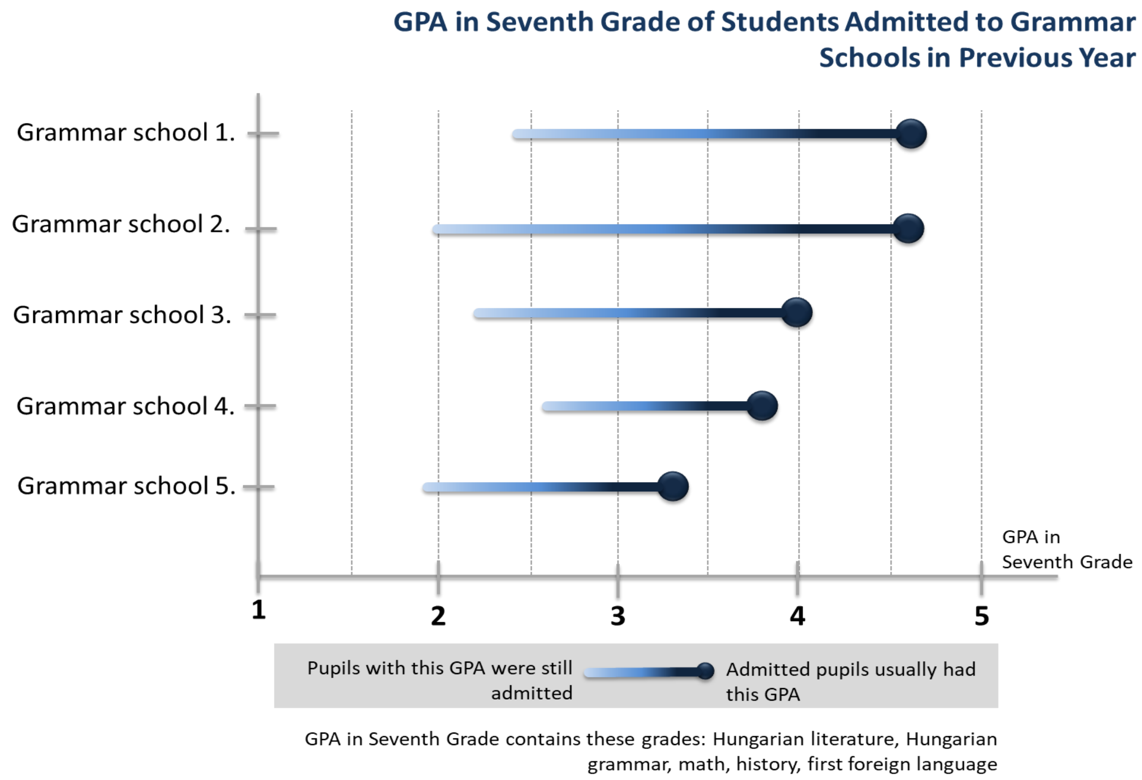

Figure 3. Sample graph shown to the treated seeds during the intervention (graphs shown to students contained school names).
Note: The graph shows the minimum and median GPA of the students who were admitted to each grammar school in the local area (30-km radius) in the previous school year. Grades range from 1 = worst to 5 = best.

Second, we asked the seeds to compute their own 7th grade GPA in the core subjects, and to relate their own GPA to the prior year's admission thresholds of the local grammar schools. The coach assisted in the computation where necessary. This exercise informed the seeds which grammar schools would likely admit them. Almost every student (95% of seeds and peers) exceeded the GPA that would have been sufficient to gain admission to at least one local grammar school in the previous year.

Third, we instructed the seeds to act as ambassadors to spread what they had learned to their peers. The coach led role-playing exercises to train seeds to talk to their peers about admission standards.[8] To motivate seeds to talk to their peers, each seed received one white plastic wristband with the slogan (in Hungarian: "Let's apply to grammar schools!".[9]

[8] In a typical scenario, a seed would meet a peer during the break after the intervention and tell him or her "I have learned that I have a good chance of getting admitted to [insert list of grammar schools]. I know that you are a stronger/weaker student than I am, and you should try to apply to [insert list of grammar schools]."
[9] Jelentkezz Te is Gimibe!

Wristbands are popular among teenagers and have been employed to provide encouragement in prior field experiments (Paluck and Shepherd 2012). Each seed additionally received five blue wristbands with the same slogan. The coach instructed the seeds to give a blue wristband and a one-page leaflet summarizing the GPA thresholds of local grammar schools from the intervention to peers with whom they had discussed the topic. Finally, students were asked to register their distributed wristbands online.

Implementation check: To check whether the intervention successfully conveyed the intended information, we administered a short survey to the treated seeds immediately before and after the intervention, asking basic questions about the application process to grammar school, seeds' plans to apply to grammar school, and seeds' subjective probability of admission if they were to apply.

Table 2 shows the instant impact of the treatment by comparing treated seeds' responses immediately before and after the intervention. Before the intervention, the seeds were already reasonably well informed about minimum criteria in the admissions process. After the intervention, nearly all seeds knew the correct answers. Specifically, after the intervention, 97 percent of the seeds correctly stated that "everybody can apply to grammar school," compared to 76 percent before the intervention. Importantly, the treated seeds' self-assessed chance of admission to grammar school (irrespective of the students' intentions to apply) increased by 1.2 points (0.5 standard deviations) from 6.5 to 7.7 on an 11-point scale ranging from "0: not at all likely" to "10: very likely." The intervention also increased the seeds' stated intention to apply to grammar school from 50 percent before to 71 percent after. All differences in Table 2 are statistically significant at the $\alpha = 0.01$ level and reflect a medium-sized effect (standardized absolute effect sizes range between 0.4 and 0.7).

Since providing personalized information about past GPA admission cutoffs at local grammar schools relative to seeds' own performance increased the seeds' intentions to apply to grammar schools, this validates our premise that students' prior beliefs about admission standards deter them from applying to grammar school, at last in the very short run. We evaluate the effect of the intervention on students' behavior (application and admission to grammar school) in the results section.

Table 2. Immediate efficacy of the treatment: survey responses of treated seeds immediately before and after the intervention

	N	Intends to apply to grammar school	Everybody can apply to grammar school	Admission is possible only with good grades	Perceived likelihood of admission (0-10)
Before	76	50.00%	76.32%	32.89%	6.46
After	76	71.05%	97.33%	5.26%	7.67
Difference	152	0.211**	0.210**	-0.276**	1.211**
Effect size	152	0.430	0.617	-0.700	0.627

Notes: Models include school-pair fixed effects to account for the pair-matched design. ** p<0.01, * p<0.05, + p<0.1. Effect size equals the pre-post difference divided by the pooled standard deviation.

Coding of Key Variables: The treatment variable is coded $T_{ip} = 1$ if student i in school k of school-pair p attended a treated school, and $= 0$ if the student attended a control school.

We analyzed two outcome variables, Y_{ip}, supplied from administrative records by the Hungarian Educational Authority. The first outcome is coded $= 1$ if the student ranked any grammar school as his or her first choice in the application, and $= 0$ otherwise. This captures the immediate goal of the trial to increase grammar school applications. We focus on the first-ranked school because students are admitted to their most highly ranked choice among schools to which they applied and qualified for admission. Hence, students who rank a less selective mixed school before a more selective grammar school will almost certainly be admitted to the mixed school, even if they also qualified for the more selective grammar school.[10] The second outcome is coded $= 1$ if the student was admitted to a grammar school, and $= 0$ otherwise. Clearly, affecting actual admission is the ultimate goal of the intervention.

We draw baseline covariates, $\boldsymbol{X}_{ip}$ for all students from two sources. The RECENS panel provides students' gender (male or female), ethnicity (non-Roma Hungarian and Roma Hungarian), parental education ($= 1$ if at least one parent had graduated from high school, $= 0$ otherwise), prior intentions to apply to grammar school ($= 1$ if yes and $= 0$ if no), and subjectively assessed chances of admission to grammar school if the student were to apply (ranging from $= 0$: "I would definitely not be admitted" to $= 10$ "I would definitely be admitted"), all measured prior to the intervention in 7th grade. We obtained students' baseline school grades for 7th grade from their application data, provided by the Hungarian Educational Authority.

[10] Throughout Hungary, 74.5% of students are admitted to their first choice (Hungarian Educational Authority 2017).

Estimation: We estimated the causal effects of the information campaign on grammar school application and admission using standard linear probability models. We executed each analysis three times: for the entire sample of students to estimate the overall causal effect of the intervention on all students; for the sample of the seeds to estimate the direct causal effect of the intervention on the seeds; and for the sample of the peers to estimate the causal spillover effect of the intervention on the peers (VanderWeele and An 2013).

We first estimated the average effect of the intervention on the outcome using the following equation:

$$Pr(Y_{ip} = 1) = \alpha + \beta_1 T_{ip} + \boldsymbol{\delta X}_{ip} \ + \ \boldsymbol{\theta}_p + \varepsilon_{ip} \qquad (1)$$

The coefficient on treatment, β_1, identifies the average causal effect of the intervention by virtue of randomization under the added assumption that there is no spillover across schools (Imai, King, and Nall 2009).[11] To reap the gains of our pair-matched cluster randomized design, we include a vector of fixed effects, $\boldsymbol{\theta}_p$, for the matched school pairs. We further aimed to increase efficiency by controlling for individual-level baseline covariates. (Since covariates were not randomized, their coefficients, $\boldsymbol{\delta}$, do not warrant a causal interpretation.) Missing covariates were not imputed. Since randomization occurred at the school level, we clustered standard errors at the school level (Abadie et al. 2017).

Next, we elaborated equation 1 to explore how the causal effect of the intervention varies by select baseline covariates (all measured in 7th grade). First, we investigated effect heterogeneity by whether or not students had stated the intention to apply to grammar school before the intervention by interacting prior application intentions with treatment. Second, we additionally evaluated how the causal effect of the intervention varies by students' perceived likelihood of admission to grammar school (measured regardless of whether they intended to apply to grammar school), by adding all two-way interactions and one three-way interaction between treatment, perceived likelihood of admission, and prior intentions to apply to grammar school.

In addition to presenting results of our linear probability models on the natural risk-difference scale, we also present standardized effect sizes, which divide the risk-difference by

[11] To test for cross-school contamination of the intervention, we asked students in control schools if they had seen the wristbands given to and distributed by seeds in treated schools. Out of 307 respondents, only 17 students reported having seen such a wristband. Of these, only 6 correctly reported having seen a wristband on a student from a treated school. This indicates that contamination, if present, was minimal.

the pooled standard deviation. As a robustness check, we re-estimated all models using logistic regression (shown in the Online Appendix).

5. Results

Table 3 shows results for the average effect of the information campaign on grammar school applications and grammar school admissions for all students, and separately for seeds and peers. We found no statistically significant results either for the overall average effect on all students (first rows), the average direct effect on the treated seeds (second rows), or the average spillover effect on untreated peers (third rows) without controlling for covariates (Panel A) or with controls for covariates (Panel B). The point estimates for the average direct causal effects on grammar school applications and admissions among the seeds, however, are all in the expected positive direction, reaching 9 percentage points for both the probability of application and the probability of admission. This effect is similar in size to the effects reported in related information campaigns on enrollment in academically oriented secondary schools (Barone, Assirelli, et al. 2017; Dinkelman and Martinez 2014). Our estimate corresponds to a standardized effect size of 0.2 on treated seeds, which our study was not powered to detect at the conventional 0.05 level of statistical significance.

Table 3: Estimated average causal effects of the information campaign on applications and admissions to grammar school for seeds, peers, and all students.

			Applied to grammar school in 1st place	Admission to grammar school	N
Panel A: No control	Overall effect on all students	Estimate	0.022	0.006	
		SE	(0.031)	(0.041)	667
		Effect size	0.049	0.015	
	Direct effect on the seeds	Estimate	0.092	0.099	
		SE	(0.065)	(0.084)	155
		Effect size	0.186	0.204	
	Spillover effect on the peers	Estimate	0.004	-0.019	
		SE	(0.026)	(0.033)	512
		Effect size	0.008	-0.049	
Panel B: With control	Overall effect on all students	Estimate	0.030	0.013	613
		SE	(0.026)	(0.033)	
		Effect size	0.066	0.030	
	Direct effect on the seeds	Estimate	0.034	0.032	145
		SE	(0.038)	(0.056)	
		Effect size	0.067	0.065	
	Spillover effect on the peers	Estimate	0.021	-0.002	468
		SE	(0.026)	(0.029)	
		Effect size	0.050	-0.006	

Notes: All models include school-pair fixed effects to account for the pair-matched design. Robust standard errors (clustered at school level) in parentheses, ** p<0.01, * p<0.05, + p<0.1
Controls: Intended to apply to grammar school (=1; 7th grade); GPA (7th grade); Roma ethnicity (=1); Girl (=1); Parent's education ≥ high school (=1).
Effect size is calculated by dividing the estimated parameter by the pooled standard deviation.

The results shown in Table 3, however, average across important effect heterogeneity. Table 4 presents a sub-group analysis that shows that providing information about admission standards had a sizeable and statistically significant effect on students who had pre-existing plans to apply to grammar school. Among seeds who intended to apply to grammar school, the effect of the intervention increased *applications* to grammar school by $(0.381 - 0.124) * 100 = 25.7$ percentage points ($p < 0.01$, Column 2). Treated seeds with prior plans to apply to grammar school also had a $(0.232 - 0.064) * 100 = 16.8$ percentage-point higher chance of *admission* to grammar school than seeds in untreated schools ($p = 0.09$, Column 5). By contrast, we found no statistically significant effect on the applications or admissions of seeds who did not have pre-existing plans to apply to grammar school, and we found no spillover effects on untreated peers regardless of their prior intentions to apply either. The difference between the effects of the information campaign on seeds with prior plans to apply to grammar school and those without such plans was statistically significant for both applications ($p < 0.01$) and admissions ($p < 0.05$). This suggests that providing information about admission thresholds empowered those students to apply who were already interested in grammar school, but it did not change the minds of those students who did not already intend to apply to grammar school.

Table 4. Interaction analysis for the effect of the information campaign on grammar school *applications* and *admissions* by the students' prior intention to apply to grammar school.

	Applied to grammar school in 1st place			Admission to grammar school		
	(1) All students	(2) Seeds	(3) Peers	(4) All students	(5) Seeds	(6) Peers
Treated (T)	-0.010	-0.124	0.014	-0.030	-0.064	-0.027
	(0.030)	(0.074)	(0.027)	(0.036)	(0.072)	(0.032)
Intended to apply to	0.183**	0.228*	0.171**	0.161**	0.186*	0.154**
grammar school (I)	(0.056)	(0.097)	(0.057)	(0.047)	(0.083)	(0.052)
T × I	0.135+	0.381**	0.027	0.145+	0.232*	0.096
	(0.069)	(0.122)	(0.079)	(0.074)	(0.112)	(0.086)
Constant	0.064	0.086	0.051	0.079+	0.171	0.069
	(0.051)	(0.134)	(0.066)	(0.040)	(0.119)	(0.047)
Mean dep. var in the control group	0.256	0.394	0.212	0.222	0.338	0.185
Observations	613	145	468	613	145	468

Note: All models include school-pair fixed effects to account for the pair-matched design. Robust standard errors (clustered at school level) in parentheses, ** p<0.01, * p<0.05, + p<0.1
Controls: Intended to apply to grammar school (=1; 7th grade); GPA (7th grade); Roma (=1); Girl (=1); Parent's education ≥ high school (=1).

To understand the mechanism by which the intervention increased applications among seeds who had prior plans to apply to grammar school, Figure 4 further explores effect heterogeneity jointly by seeds' prior intentions to apply and by their self-perceived likelihood of admission (if they were to apply). Results show that the positive effect of the intervention on grammar school applications is entirely concentrated among students who, prior to the intervention, (a) intended to apply to grammar school and (b) judged their likelihood of admission to be low.[12] For example, among seeds who were interested in applying to grammar school and perceived their likelihood of admission (if they were to apply) to be 5 on a scale of 0 to 10 (mean = 6.9 among seeds), we estimate that the intervention increased the probability of admission by 48 percentage points ($p = 0.03$). By contrast, we did not detect statistically significant evidence that the intervention affected applications among seeds who intended to apply to grammar school and were certain of their admission, ($p = 0.77$). The difference between the effects on seeds with prior intentions to apply who reported a low vs. high perceived likelihood of admission was statistically significant at the 0.1 level. We did not detect effects of the intervention on seeds without prior plans to apply to grammar school,

[12] Excluding the four seeds who reported a perceived likelihood of admission $= 0$. affected p-values but not the qualitative pattern of the results shown in Figure 4.

regardless of their perceived likelihood of admission. The difference between the effects on seeds with vs. without plans to apply was statistically significant at the 0.05 level for all but the highest perceived likelihoods of admission, as shown in Figure 4. (See Appendix Tables A3 and A4 for the corresponding regression tables on applications and admissions.)

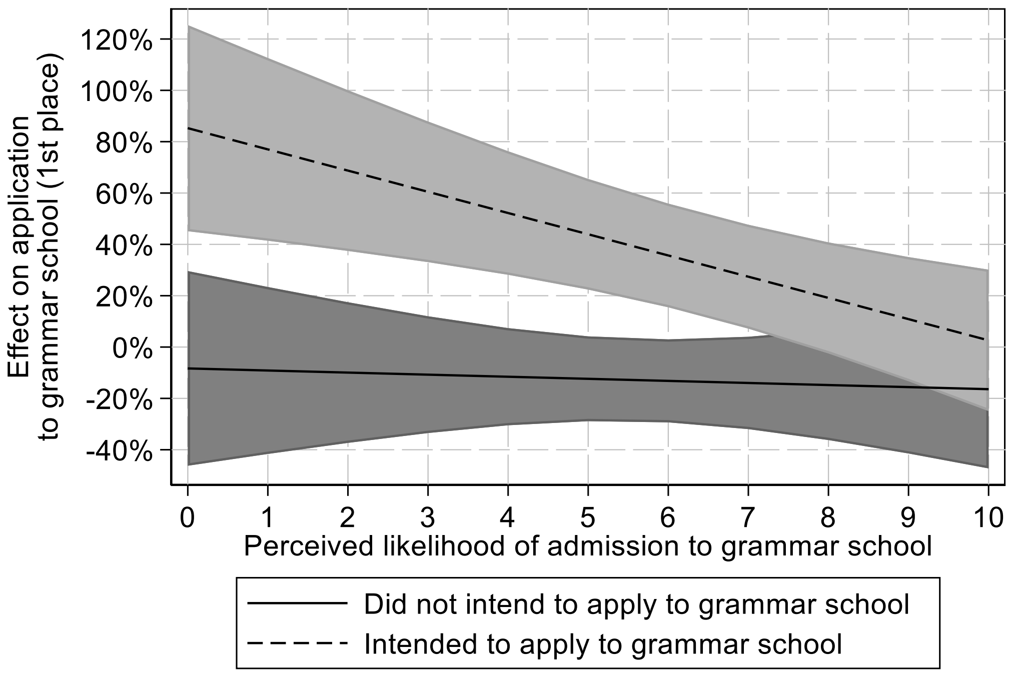

Figure 4. Effects of the intervention on the probability of application to grammar school (in 1st place) among seeds ($N = 144$) by seeds' baseline intention to apply to grammar school and seed's perceived likelihood of admission to grammar school. Point estimates and 95% confidence intervals.

Additional exploratory analyses did not detect differential effects on seeds' or peers' grammar school applications separately by parental education (whether a parent had graduated from high school) (Table A5) or students' baseline GPA (Table A6).

6. Discussion and Conclusion

In many tracked educational systems, students face educational choices with far-reaching consequences at a young age. Students and their parents, however, often make these choices on the basis of incomplete or even incorrect information. Poorly informed choices can lead to lost opportunities and adverse social outcomes. If misinformation is socially selective, the resulting educational choices may exacerbate existing social inequalities in educational attainment and economic outcomes.

Social scientists have mostly sought to remove information barriers in educational choice by providing information on the economic costs of, and economic returns to, education. Several interventions (e.g., Jensen 2010; Nguyen 2008; Hoxby and Turner 2013; Peter and Zambre 2017) aimed to motivate college enrollment by publicizing the earnings advantages of college graduates (thus increasing students' expected returns to education) or the availability of financial aid (thus lowering expected cost).

Our intervention, by contrast, focused on reducing students' uncertainty about admissions standards without engaging the cost of, or returns to, education. We hypothesized that primary school students (or their parents) in Hungary systematically overestimate the hurdles to admission to academically selective high schools, which are the main conduit to tertiary education. Correcting this misperception by showing students the (often quite low) minimum and median GPA of the students who had been admitted to local grammar schools in the previous year was expected to specifically motivate under-confident students to apply and, if qualified, to gain admission to grammar school.

Results were broadly consistent with expectations. Although we did not find statistically significant effects of the intervention on average, we did find that the intervention increased the probability of both application ($p < 0.01$) and admission ($p < 0.1$) to grammar school among treated seeds who had prior plans to apply to grammar school. Since our information campaign should be expected specifically to influence under-confident students who overestimated admissions requirements, and not to motivate previously uninterested students to apply (e.g., by extolling economic benefits of education), it makes sense that our subsequent exploratory analysis found especially large effects on grammar school applications among treated students who had pre-existing plans to apply but lacked confidence in their chances of admission. This effect heterogeneity parallels recent findings from another information campaign in Germany that sought to promote college enrollment by expounding the economic benefits of education, which similarly found effects only among students with pre-existing plans to enroll in college (Ehlert et al. 2017).

Our results highlight the role of uncertainty and cognitive hurdles in educational choice: biased beliefs can deter students from applying to the academically selective track in high school. This finding is encouraging for policy. Although family background, academic performance, and structural factors may dominate track choice, none of these factors are amenable to easy interventions. Our study demonstrates that a light-touch intervention that simply communicates admissions standards can affect track choices by empowering students to apply and gain admission to the academic track.

Beyond its policy implication, our paper makes three theoretical contributions to the literature on educational choice. First, our results indicate that adolescent students appear to have considerable agency in secondary track choice. Without denying the importance of parents in steering educational decisions (Barone, Assirelli, et al. 2017), our field experiment generates effects by intervening on students', but not on parents', information set. Second, students' perceptions of their probability of admission appear biased. Third, light-touch interventions designed to influence students' perceived probability of admission by clarifying admission standards can exert a causal effect on students' track choices.

It is interesting to speculate about the implications of scaling our intervention nationally to raise all students' awareness of admission standards. Clearly, if the number of seats in Hungarian grammar schools were fixed, then scaling the intervention would not increase students' overall probability of admission to grammar school. Instead, the intervention would change the applicant pool and affect the composition of the students who are admitted to grammar school. Specifically, it would increase admissions among highly qualified but under-confident students who do not apply under the current regime, and it would diminish the chances of confident but currently only marginally qualified students who would lose out to their newly emboldened, better qualified, peers.

Hence, our intervention is not premised on the (controversial) assumption that all students should enter the academic track (Cullen, Jacob, and Levitt 2006). While publicizing (low) admission standards from previous years might also motivate some unqualified students to apply, the intervention does not actually lower admission standards (which are set by schools). On the contrary, if the number of seats in grammar schools remains fixed, then the intervention would indirectly increase admission standards by encouraging more qualified students to apply.

These arguments raise important questions about the distributional consequences of clarifying admission standards for social inequality. *A priori*, these implications are ambiguous. On one hand, since more students from disadvantaged than from advantaged families lack confidence in their chances of admission to grammar school ($p < 0.01$), publicizing the de-facto quite low admission standards of grammar schools in Hungary might especially empower underprivileged students. On the other hand, since students from more privileged backgrounds have higher grades and higher educational aspirations on average, raising admissions standards via the resulting increased competition might decrease the chances of socio-economically disadvantaged students. Assessing the trade-off between these opposing forces for inequality in access to secondary, and ultimately tertiary, education requires future empirical work.

We note several limitations. Most obviously, we failed to detect spillover effects on the track choices of untreated peers. Following mounting field-experimental evidence that some educational choices are subject to social influence (Anelli and Peri 2017; Lyle 2007; Zölitz and Feld 2017), we designed the experiment to study the effect of information sharing among primary school students. However, we did not find any evidence on track choice of untreated peers on average or among any subgroup of peers.

The failure to find spillover effects to peers could be due to multiple factors. First, it might be that the intervention was effective only among students who had prior plans to apply to grammar school, but far fewer peers than seeds turned out to have such plans (24 vs. 41 percent). Second, seeds might not have sufficiently tried to rally their peers, perhaps in order to limit competition in the admissions process.[13] Third, it is possible that students do not meaningfully influence each other's secondary track choice, at least when the influence operates through the transmission of factual information about admission standards rather than, for example, the promise of economic gain, or normative pressure.

As a second limitation, we only studied the short-term behavioral consequences of the intervention on grammar school applications and admissions. It would be desirable for follow-up studies further to track long-term outcomes and consequences for unequal access to educational opportunities.

Third, as with all field experiments, generalizability is an open question. While we believe that the effects of uncertainty about admission standards are relevant for many tracked school systems with competitive admissions, our specific field site was located in disadvantaged counties of rural Hungary, and treated seeds were systematically selected for network centrality. Since the study was more effective among students with plans to apply for grammar school and less effective among students who were ex-ante confident in their

[13] Follow-up inquiries four months after the intervention indicate that the seeds put middling effort into persuading their peers. Out of 76 treated seeds, 55 percent reported having distributed the leaflets with admission information for local grammar schools, and 74 percent reported having distributed wristbands, but only 32.5 percent of peers reported that the seeds had explained the workshop to them, 28 remembered receiving an information sheet, and 51 percent remembered receiving a wristband from the seeds. Supplementary analyses (not shown) found no spillover effect among peers who reported receiving information material or wristbands from treated seeds.

chances of admission, the effect of scaling the intervention to the general student population in these counties could be larger or smaller, because seeds were both more likely to have plans and to possess greater confidence in their chances of admission than their untreated peers.

Fourth, our study is premised on the assumption that more qualified students overestimate than underestimate the difficulty of admission. Empirically, this appears to have been the case in our sample. If, by contrast, students systematically underestimated admission hurdles, reducing uncertainty about admission thresholds may discourage, rather than encourage applications.

Future implementations of our intervention could be strengthened in several respects. For example, they might provide information to all students rather than only selected seeds; incorporate timely reminders or reinforcements closer to the date of the application deadline; and provide information on threshold values for all admissions criteria, not just grades. If students are additionally informed about the returns to education (Barone, Assirelli, et al. 2017), future studies should evaluate interactions and possible trade-offs between these elements.

In sum, our field experiment indicates that increasing students' knowledge about admissions standards can increase applications and admissions to academically selective secondary schools. Future research should follow up on our suggestive evidence that greater transparency might improve the match between students' qualifications and schools' admission requirements.

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Appendix Tables

Table A1. School-level scores for the 12 blocking variables by school pair and treatment status

	School Pair	Blocking score (Principal Component)	Variables in the Blocking score											
			(1) N of parallel classes	(2) N of grammar schools in 30-km radius	(3) Girl (%)*	(4) Intended to apply to grammar school (%)*	(5) Mother's education ≥ high school (%)*	(6) Father's education ≥ high school (%)*	(7) Roma (%)**	(8) 5th grade GPA**	(9) 6th grade GPA**	(10) Math test-score (6th grade)**	(11) Reading test-score (6th grade)**	(12) N of missing subjects with missing grades+ (7th grade)*
Control Schools	1	−1.67	1	4	0.31	0.06	0.07	0.13	0.93	2.75	2.80	−0.63	−0.81	9
	2	−1.50	1	7	0.65	0.12	0.00	0.00	0.83	3.37	2.85	−0.97	−0.83	0
	3	−0.84	2	4	0.53	0.13	0.17	0.27	0.38	3.84	3.04	−1.46	−1.15	1
	4	−0.67	2	1	0.41	0.22	0.21	0.21	0.69	3.69	3.65	−0.94	−1.19	1
	5	−0.62	1	4	0.61	0.04	0.46	0.08	0.23	3.34	3.52	−0.94	−0.44	0
	6	−0.11	2	6	0.50	0.21	0.36	0.39	0.47	3.61	3.46	−0.47	−0.73	0
	7	0.00	1	10	0.43	0.14	0.42	0.42	0.33	3.50	3.78	−0.44	−0.63	1
	8	0.01	2	4	0.45	0.31	0.28	0.21	0.30	3.79	3.57	−0.61	−0.45	0
	9	0.36	1	5	0.72	0.33	0.39	0.22	0.28	4.18	4.23	−0.56	−0.22	0
	10	0.45	2	9	0.57	0.18	0.46	0.42	0.62	4.06	3.73	0.05	−0.19	0
	11	1.01	2	8	0.52	0.45	0.48	0.39	0.22	4.23	4.09	−0.29	−0.13	0
	12	1.34	1	9	0.39	0.26	0.62	0.52	0.05	4.51	4.42	−0.02	0.01	0
	13	1.80	2	6	0.44	0.48	0.69	0.57	0.00	4.50	4.37	0.21	0.17	0
Treated schools	1	−1.57	1	3	0.67	0.07	0.00	0.00	0.92	3.44	3.01	−0.29	−1.23	0
	2	−1.06	1	5	0.50	0.11	0.19	0.06	1.00	3.69	3.43	−0.94	−0.80	0
	3	−0.87	1	4	0.46	0.15	0.09	0.09	0.45	3.43	3.42	−0.75	−0.78	0
	4	−0.71	1	6	0.53	0.05	0.06	0.18	0.81	3.53	3.50	0.15	−0.43	0
	5	−0.26	1	5	0.48	0.19	0.50	0.25	0.50	3.68	3.37	−0.39	−0.25	9
	6	−0.23	2	9	0.47	0.24	0.26	0.21	0.58	3.72	3.82	−0.83	−0.85	0
	7	−0.02	2	1	0.43	0.17	0.35	0.16	0.39	4.05	3.91	−0.22	−0.40	9
	8	0.12	2	4	0.49	0.16	0.42	0.21	0.13	3.73	3.39	−0.46	0.03	0
	9	0.24	1	6	0.40	0.12	0.28	0.33	0.67	3.84	3.87	0.65	0.11	1
	10	0.76	2	6	0.43	0.24	0.49	0.35	0.03	3.93	3.94	−0.04	−0.10	0
	11	0.88	1	7	0.56	0.33	0.65	0.59	0.24	4.19	4.06	−0.46	−0.27	0
	12	1.03	1	1	0.36	0.55	0.50	0.50	0.10	4.10	4.04	−0.01	−0.20	0
	13	2.12	3	5	0.48	0.49	0.84	0.76	0.01	4.31	4.16	0.44	0.39	0

*Source: RECENS Survey, 5th wave, Spring 2016.

** Source: National Assessment of Basic Competences (NABC), 2015, 6th graders

[+] The RECENS survey asked schools to provide students' 7th-grade mid-term school grades for nine subjects: Hungarian grammar, Hungarian literature, Mathematics, History, Foreign language, Geography, Biology, Chemistry, Physics. Column 12 shows the number of subjects for which schools did not send students' grades. At the time of randomization, this was the only information available about students' 7th-grade grades.

Table A2. Covariate balance in the analysis sample

	Panel A All students				Panel B Seeds				Panel C Peers			
	Treatment schools (Mean)	Control schools Difference[e]	N (T+C)	Regression t-statistic[c]	Treatment schools (Mean)	Control schools Difference[e]	N (T+C)	Regression t-statistic[c]	Treatment schools (Mean)	Control schools Difference[e]	N (T+C)	Regression t-statistic[c]
Blocked variables[a]												
Intended to apply to grammar school (7th grade)	0.29	−0.02	651	0.26	0.46	−0.11	155	−1.36	0.24	0.00	496	1.06
Roma	0.29	0.07	648	0.55	0.22	0.13	153	1.36	0.31	0.06	495	0.27
Girl	0.49	0.03	614	0.99	0.65	−0.06	145	−0.32	0.44	0.05	469	1.29
Mother's education ≥ high school	0.47	−0.07	615	−0.70	0.58	−0.09	146	−0.66	0.43	−0.07	469	−0.46
Father's education ≥ high school	0.37	−0.02	616	−0.04	0.42	−0.04	146	−0.48	0.36	−0.02	470	0.20
Non-blocked variables[b]												
Hungarian literature, 7th grade	3.37	−0.02	670	0.36	3.88	−0.05	155	−0.02	3.22	−0.02	515	0.36
Hungarian grammar, 7th grade	3.31	−0.07	670	−0.24	3.87	−0.16	155	−0.81	3.15	−0.06	515	0.08
Math, 7th grade	3.15	0.07	662	1.01	3.67	0.00	154	0.06	3.00	0.08	508	1.28
History, 7th grade	3.33	−0.02	670	0.87	3.92	−0.21	155	0.87	3.33	−0.02	670	0.87
Foreign language, 7th grade	3.49	−0.12	654	−0.55	4.09	−0.30	153	−1.45	3.31	−0.07	501	−0.09
GPA, 7th grade[d]	3.61	−0.04	670	−0.04	4.07	−0.15	155	−0.98	3.47	−0.02	515	0.52
Perceived likelihood of admission to grammar school (7th grade)	6.20	−0.16	606	−1.59	7.00	−0.28	145	−1.44	5.59	−0.14	461	−0.78

Notes. This table shows covariate balance for student-level covariates in the analysis sample of 671 students. Blocking was conducted at the school level; for balance on school level variables, see Table A1. Individual-level values of variables in the lower part of the table became available after randomization. Missingness in blocked variables is due to survey non-response. Missingness in non-blocked variables occurs because not all students received grades in all subjects.

** $P<0.01$, * $P<0.05$, + $P<0.1$

[a] RECENS Survey, fifth wave, Spring 2016

[b] Data from students' secondary school application records, February 2017, provided by the Hungarian Educational Authority.

[c] The *t*-statistics shown are from two-sided *t*-tests from regressions of each variable on the treatment indicator and school-pair indicators, with standard errors clustered at the school level to account for the study design.

[d] School subjects are graded from 1 to 5, where 5 is the best.
[e] Difference = Control - Treatment

Table A3. Three-way interaction effect of the treatment with baseline intention to apply to grammar school and perceived likelihood of admission. Dependent variable: **Applied to grammar school in first place.**

	All students	Seeds	Peers
Treated (T)	−0.009	−0.133	0.028
	(0.031)	(0.079)	(0.029)
Intended to apply to grammar school (I)	0.109+	0.161	0.121+
	(0.060)	(0.108)	(0.060)
Perceived likelihood of admission (P)	0.001	0.009	−0.003
	(0.007)	(0.024)	(0.005)
T × I	0.210*	0.480**	0.044
	(0.077)	(0.142)	(0.090)
T × P	0.008	−0.008	0.020
	(0.012)	(0.029)	(0.014)
I × P	0.061**	0.062	0.047+
	(0.019)	(0.036)	(0.027)
T × I × P	−0.071**	−0.075+	−0.045
	(0.025)	(0.042)	(0.041)
Constant	0.058	0.090	0.037
	(0.050)	(0.120)	(0.072)
Mean dep. var. in the control group	0.262	0.4	0.218
Observations	602	144	458

Note: The models include school-pair fixed effects to account for the pair-matched design. The perceived likelihood of admission is mean-centered. Robust standard errors (clustered at school level) in parentheses, ** p<0.01, * p<0.05, + p<0.1

Controls: Intended to apply to grammar school (=1; 7th grade); GPA (7th grade); Roma (=1); Girl (=1); Parent's education ≥ high school (=1).

Table A4. Three-way interaction effect of the treatment with baseline intention to apply to grammar school and perceived likelihood of admission. Dependent variable: **Admission to grammar school.**

	All students	Seeds	Peers
Treated (T)	−0.027	−0.075	−0.018
	(0.037)	(0.072)	(0.035)
Intended to apply to grammar school (I)	0.121*	0.137	0.137*
	(0.046)	(0.081)	(0.053)
Perceived likelihood of admission (P)	−0.005	−0.014	−0.003
	(0.006)	(0.021)	(0.005)
T × I	0.156+	0.227+	0.116
	(0.086)	(0.115)	(0.107)
T × P	0.012	0.021	0.015
	(0.012)	(0.029)	(0.012)
I × P	0.041+	0.065*	0.019
	(0.020)	(0.031)	(0.029)
T × I × P	−0.027	−0.009	−0.040
	(0.032)	(0.044)	(0.048)
Constant	0.075+	0.152	0.062
	(0.040)	(0.127)	(0.049)
Mean dep. var. in the control group	0.227	0.343	0.19
Observations	602	144	458

Note: The models include school-pair fixed effects to account for the pair-matched design. The perceived likelihood of admission is mean-centered. Robust standard errors (clustered at school level) in parentheses, ** p<0.01, * p<0.05, + p<0.1

Controls: Intended to apply to grammar school (=1; 7th grade); GPA (7th grade); Roma (=1); Girl (=1); Parent's education ≥ high school (=1).

Table A5. Interaction analysis for the effect of treatment by parental education.
Dependent variable: **Applied to grammar school in first place.**

	(1) All students	(2) Seeds	(3) Peers
Treated (T)	0.051	0.092	0.038
	(0.031)	(0.066)	(0.032)
Parental education ≥ high school (PEdu)	0.084*	0.204	0.056
	(0.037)	(0.144)	(0.051)
T × PEdu	−0.071	−0.170	−0.059
	(0.045)	(0.143)	(0.066)
Constant	0.064	0.086	0.051
	(0.051)	(0.134)	(0.066)
Mean dep. var. in the control group	0.256	0.394	0.212
Observations	613	145	468

Note: The models include school-pair fixed effects to account for the pair-matched design.
Robust standard errors (clustered at school level) in parentheses, ** p<0.01, * p<0.05, + p<0.1
Controls: Intended to apply to grammar school (=1; 7th grade); GPA (7th grade); Roma (=1); Girl (=1); Parent's education ≥ high school (=1).

Table A6. Interaction analysis for the effect of treatment by baseline GPA. Dependent variable: **Applied to grammar school in first place.**

	(1) All students	(2) Seeds	(3) Peers
Treated (T)	0.029	−0.025	0.024
	(0.027)	(0.050)	(0.029)
GPA	0.171**	0.174**	0.156**
	(0.029)	(0.061)	(0.027)
T × GPA	0.049	0.139	0.032
	(0.039)	(0.086)	(0.050)
Constant	0.046	0.086	0.045
	(0.053)	(0.140)	(0.063)
Mean dep. var. in the control group	0.256	0.394	0.212
Observations	613	145	468

Note: The models include school-pair fixed effects to account for the pair-matched design.
Robust standard errors (clustered at school level) in parentheses, ** p<0.01, * p<0.05, + p<0.1
Controls: Intended to apply to grammar school (=1; 7^{th} grade); GPA (7^{th} grade, mean-centered); Roma (=1); Girl (=1); Parent's education ≥ high school (=1).

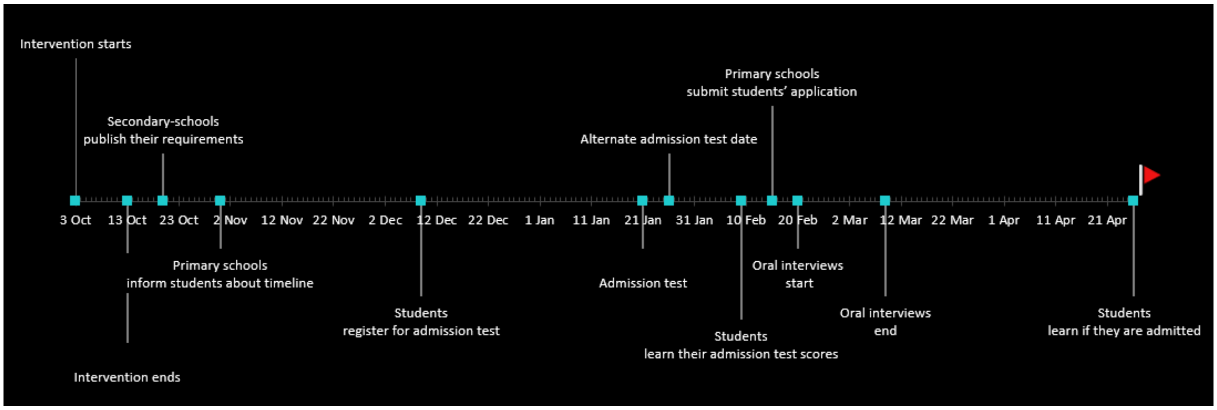

Figure A1. Timeline of the application process 2016/17 and the intervention

Online Appendix Tables

Table OA1. Estimated average total causal effects of the treatment on application and admission to grammar school–logit regression and logit coefficients.

			Applied to grammar school in 1st place	Admission to grammar school
Panel A: No controls	Overall effect on all students	Estimate	0.144	0.046
		SE	(0.209)	(0.292)
		N	645	612
	Direct effect on the seeds	Estimate	0.500	0.566
		SE	(0.339)	(0.464)
		N	139	131
	Spillover effect on the peers	Estimate	0.029	−0.170
		SE	(0.213)	(0.286)
		N	430	409
Panel B: With controls	Overall effect on all students	Estimate	0.335	0.133
		SE	(0.263)	(0.327)
		N	593	567
	Direct effect on the seeds	Estimate	0.491	0.726
		SE	(0.430)	(0.649)
		N	130	125
	Spillover effect on the peers	Estimate	0.290	-0.043
		SE	(0.295)	(0.310)
		N	398	378

Notes: All models include school-pair fixed effects to account for the pair-matched design. Robust standard errors (clustered at school level) in parentheses, ** p<0.01, * p<0.05, + p<0.1. Sample size differs from the linear probability models reported in the body of the text because of perfect prediction.

Controls: Intended to apply to grammar school (=1; 7th grade); GPA (7th grade); Roma ethnicity (=1); Girl (=1); Parent's education ≥ high school (=1).

Table OA2. Interaction analysis for the effect of the information campaign on grammar school *applications* and *admissions* by the students' prior intention to apply to grammar school. Logit regression, logit coefficients.

	Applied to grammar school in 1st place			Admission to grammar school		
	(1) All students	(2) Seeds	(3) Peers	(4) All students	(5) Seeds	(6) Peers
Treated (T)	0.001	-1.178	0.351	-0.252	-0.336	-0.245
	(0.343)	(0.879)	(0.331)	(0.403)	(0.903)	(0.400)
Intended to apply to grammar school (I)	1.121**	0.968	1.254**	1.095**	0.998	1.106**
	(0.392)	(0.853)	(0.301)	(0.279)	(0.969)	(0.324)
T × I	0.754	4.095**	-0.143	0.769*	2.024+	0.428
	(0.527)	(1.517)	(0.402)	(0.346)	(1.109)	(0.397)
Constant	-1.366**	-3.376**	-1.117**	-1.768**	-4.090**	-1.220*
	(0.385)	(1.020)	(0.384)	(0.594)	(1.240)	(0.549)
Observations	593	130	398	567	125	378

Note: All models include school-pair fixed effects to account for the pair-matched design. Robust standard errors (clustered at school level) are in parentheses, ** p<0.01, * p<0.05, + p<0.1. Sample size differs from the linear probability models reported in the body of the text because of perfect prediction.

Controls: Intended to apply to grammar school (=1; 7th grade); GPA (7th grade); Roma (=1); Girl (=1); Parent's education ≥ high school (=1).

Table OA3. Three-way interaction effect of the treatment with baseline intention to apply to grammar school and perceived likelihood of admission. Dependent variable: **Applied to grammar school in first place.** Logit regression, logit coefficients

	All students	Seeds	Peers
Treated (T)	-0.027	-1.265	0.389
	(0.333)	(0.955)	(0.326)
Intended to apply to grammar school (I)	0.647	0.543	0.912**
	(0.428)	(1.093)	(0.314)
Perceived likelihood of admission (P)	0.043	-0.147	0.057
	(0.113)	(0.388)	(0.084)
T × I	1.342*	4.589**	0.044
	(0.585)	(1.624)	(0.475)
T × P	0.075	0.054	0.183
	(0.145)	(0.463)	(0.155)
I × P	0.342*	0.603	0.235
	(0.142)	(0.480)	(0.181)
T × I × P	-0.546**	-0.591	-0.381
	(0.211)	(0.611)	(0.306)
Constant	-1.235**	-2.851*	-1.049**
	(0.376)	(1.433)	(0.370)
Observations	584	129	390

Note: The models include school-pair fixed effects to account for the pair-matched design. The perceived likelihood of admission is mean-centered. Robust standard errors (clustered at school level) in parentheses, ** p<0.01, * p<0.05, + p<0.1 Sample size differs from the linear probability models reported in the body of the text because of perfect prediction.

Controls: Intended to apply to grammar school (=1; 7th grade); GPA (7th grade); Roma (=1); Girl (=1); Parent's education ≥ high school (=1).

Table OA4. Three-way interaction effect of the treatment with baseline intention to apply to grammar school and perceived likelihood of admission. Dependent variable: **Admission to grammar school.** Logit regression, logit coefficients

	All students	Seeds	Peers
Treated (T)	-0.319	-0.502	-0.220
	(0.393)	(0.811)	(0.410)
Intended to apply to grammar school (I)	0.793**	0.501	1.022**
	(0.276)	(0.746)	(0.230)
Perceived likelihood of admission (P)	-0.075	-0.559+	0.002
	(0.103)	(0.285)	(0.086)
T × I	1.082**	2.269*	0.747+
	(0.405)	(0.927)	(0.442)
T × P	0.203	0.504	0.211
	(0.150)	(0.417)	(0.160)
I × P	0.277+	0.782+	0.090
	(0.154)	(0.409)	(0.179)
T × I × P	-0.402+	-0.204	-0.470
	(0.228)	(0.616)	(0.319)
Constant	-1.666**	-4.090**	-1.191*
	(0.563)	(1.455)	(0.582)
Observations	558	124	372

Note: The models include school-pair fixed effects to account for the pair-matched design. The perceived likelihood of admission is mean-centered. Robust standard errors (clustered at school level) in parentheses, ** p<0.01, * p<0.05, + p<0.1. Sample size differs from the linear probability models reported in the body of the text because of perfect prediction.
Controls: Intended to apply to grammar school (=1; 7^{th} grade); GPA (7^{th} grade); Roma (=1); Girl (=1); Parent's education ≥ high school (=1).

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Table OA5. Interaction analysis for the effect of treatment by parental education. Dependent variable: **Applied to grammar school in first place.** Logit regression, logit coefficients

	(1) All students	(2) Seeds	(3) Peers
Treated (T)	0.736+	1.408	0.665+
	(0.382)	(0.891)	(0.365)
Parental education ≥ high school (PEdu)	0.921*	3.102+	0.711+
	(0.427)	(1.780)	(0.397)
T × PEdu	-1.028*	-2.335	-0.923+
	(0.502)	(1.789)	(0.549)
Constant	-1.707**	-4.862**	-1.217**
	(0.416)	(1.295)	(0.354)
Observations	593	130	398

Note: The models include school-pair fixed effects to account for the pair-matched design. Robust standard errors (clustered at school level) in parentheses, ** p<0.01, * p<0.05, + p<0.1. Sample size differs from the linear probability models reported in the body of the text because of perfect prediction.
Controls: intended to apply to grammar school (=1; 7th grade); GPA (7th grade); Roma (=1); Girl (=1); Parent’s education ≥ high school (=1).

Table OA6. Interaction analysis for the effect of treatment by baseline GPA. Dependent variable: **Applied to grammar school in first place.** Logit regression, logit coefficients

	(1) All students	(2) Seeds	(3) Peers
Treated (T)	0.112	-1.975*	0.225
	(0.344)	(0.828)	(0.407)
GPA	1.565**	0.755	1.580**
	(0.335)	(0.799)	(0.432)
T × GPA	0.463	3.811**	0.161
	(0.456)	(1.215)	(0.615)
Constant	-1.413**	-3.194**	-1.047*
	(0.430)	(1.003)	(0.463)
Observations	593	130	398

Note: The models include school-pair fixed effects to accout for the pair-matched design. Robust standard errors (clustered at school level) in parentheses, ** p<0.01, * p<0.05, + p<0.1. Sample size differs from the linear probability models reported in the body of the text because of perfect prediction.
Controls: Intended to apply to grammar school (=1; 7th grade); GPA (7th grade, mean-centered); Roma (=1); Girl (=1); Parent's education ≥ high school (=1).