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THESIS BOOKLET

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The Effect of Elite Secondary School Programs on University Outcomes in Hungary

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Budapest, 2023

Institute of Economics

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1. Introduction

Most of the education systems in developed countries start streaming their students to different educational pathways (tracks) at the secondary school level, which usually takes place after grade 8 when students are 14-16 years old, by choosing between academically oriented and vocational programs. Within the academic track, there are special schools and programs that aim to educate the best and brightest in the cohort: elite academic high schools have competitive learning environments and highly selective admissions such as grammar schools in the UK and exam schools in the USA, which is of great interest to educational researchers and policymakers. Parents bring their children to elite programs hoping to provide them with higher chances for further university education, possibly at prestigious universities. Also, to guarantee their progress with better teachers, among high-achieving peers with high social status, which could all contribute to their success not only in post-secondary education but also in their lives and labor market position later on. Although elite schools are a widely researched area in economic literature, there are still gaps that need further scientific scrutiny. For example, it remains unclear whether these “elite tracks” deliver on their promises and provide better education for their students contributing to the efficiency of the school system. It is also unclear whether these institutions only help conserve the status of already privileged students through selectivity, harming the equalizing role of the education system, or whether they provide actual social mobility to students coming from less affluent backgrounds. Hungary has a unique type of elite track, where selection happens in grade 4 or 6, much earlier than in most of the comparable elite programs. Accordingly, the institutional framework of these schools is related to early tracking in education, such as literature from elite schools.

The effect of elite schools and tracking are widely researched areas in academia. The primary issue for students and parents is whether applying to elite programs is worthwhile and what kind of gains (if any) can be made if admitted. For policy makers on the other hand, not only elite program's effectiveness can be interesting, but its possible impact on inequality as well. My research focuses on identifying the effects of elite programs. I compare students who entered the elite program in Grade 5 or 7 (treated group) to students who entered the general program in Grade 9 (control group) in the same academic high schools. I address the following questions:

1. What is the role of test scores and family background in admission to elite programs? Which is the stronger factor? How does it relate to inequality?
2. Do elite programs help increase students' test scores more than the alternative track (general programs)? Is this impact (if any) substantial or negligible?

3. To what extent do elite programs impact post-secondary education outcomes, such as enrollment to BA and MA level, obtaining a degree, type of major, and quality of university? Where is the greatest impact?
4. What are the most important channels of the mechanism? Is it through improved test scores, grade point average (GPA), aspiration, or something else?
5. Are the effects heterogeneous? Do students of different test scores or backgrounds experience different effects?

Based on the literature, it is still unclear whether attending elite programs results in higher test scores, and the findings regarding longer-term outcomes such as post-secondary education, earnings, and well-being (e.g. occupational rank and health) are also mixed. The reason for the lack of consensus in the literature is twofold: it stems from differences in education systems that result in context dependency, as well as methodological issues.

First, it stems from methodological issues. Selection to elite schools is usually not random which makes identifying their causal effects challenging. Second, differences in education systems result in context dependency. It is difficult to compare studies conducted in different countries because tracking practices vary greatly based on type, proportion, implementation, and starting age of tracks, for example. Furthermore, societies with different historical backgrounds and institutions respond to policies differently.

This study exploits rich administrative individual panel data in Hungary to study how elite program enrollment affects students' university outcomes. First, I analyze the selection mechanism, and show that not only test scores, but also family background influences admission to elite programs. Then I measure the effect of elite programs on test scores and post-secondary education, where I find positive effects. Afterwards, I look at the causal effects of elite program enrollment on higher education outcomes using two identification strategies, relying on the unconfoundedness assumption: ordinary least-squares (OLS) and propensity score matching (PSM). Both estimates show significant 3-4 percentage points effects of elite secondary school programs on university enrollment and completion rates. These differences are much lower than raw differences but are non-negligible, and the relative impact of elite secondary school programs on university completion and MA enrollment is much greater in magnitude compared to that on BA enrollment. Suggestive evidence shows that a substantial part of the enrollment effect can be attributed to improvement in school performance – test scores and GPA. I also offer further suggestive evidence on the potential mechanism through teacher/school quality. I also look at heterogeneity in the dependent variable (university enrollment) by distinguishing majors (STEM[1], arts, medical, law and governance), internationally recognized universities and publication performance of universities (university quality), and MA enrollment. Findings are inconclusive about STEM track choice, but I find significant and non-negligible positive effects on the quality of the university where students enroll and on the probability of whether students continue MA-level education as well. A detailed heterogeneity analysis is

[1] Abbreviation for science, technology, engineering, and mathematics.

conducted on groups based on explanatory variables (e.g. gender, SES, test scores, primary school quality) shows that students of more disadvantaged groups benefit more from elite programs. That being said, the effects on more privileged students are primarily significant, but smaller in magnitude.

My third identification strategy relaxes the unconfoundedness assumption. There are likely several unobserved sources of selection to elite programs that also affect higher education outcomes (such as motivation, parental support, non-cognitive skills, etc.). Thus, I apply instrumental variables (IV) estimation to test for omitted variable bias in the current estimates, and the results do not indicate the presence of severe bias. Moreover, I conducted several robustness tests to support the findings, and the main coefficients remain stable throughout different specifications.

The study aims to extend the knowledge relating to the effects of elite secondary school programs in international and Hungarian literature as well. The contribution of this research is threefold. First, this study examines and compares short-run (test scores) and long-run post-secondary outcomes together in a similar setting. Second, to the best of my knowledge, this research is the first to look at university outcomes in the context of elite programs in Hungary. Third, considering university quality as an outcome in the international literature regarding the effect of elite schools is rare, and the application of international rankings as a quality measure is unique.

2. Literature Review

2.1. Elite programs in Hungarian secondary schools

Differences and lack of agreement related to the effect of tracking in the international literature stem not only from methodological issues but also from the differences in countries' education systems and tracking types. Two distinct forms of tracking exist on the secondary level: between-school tracking is more present in Europe, in contrast, within-school tracking is more prevalent in the US. Elite programs in Hungary are comparable to exam schools in the US and grammar schools in the UK, in the sense that they both have selective admission, they are more prestigious than their comprehensive counterparts, higher-ability peers are present, these schools have academic focus and they aim to prepare their students better for university. The comparison with the international findings is challenging because the elite programs in Hungary are a special type of institute which is the mixture of early tracking and elite schools. Tracking (grouping based on ability) in Hungary begins at the end of 4^{th} and 6^{th} grade only for elite programs, whereas the academic versus vocational tracking happens later at the end of 8^{th} grade. Similarly to other countries, most of the students in Hungary spend only their last 4 years of education in tracked schools, but elite selection happens earlier and those students spend 8 or 6 years in elite programs, whereas the internationally it lasts mostly 4 years. In this sense, the topic relates to the literature of early tracking, which typically examines the selection and the inequality effects (fairness) of this form of education. On the other hand, elite programs are typically separate classes within academically oriented secondary schools, which is a form of within-school tracking, so only partially comparable to between-school tracking prevalent in other countries. As a consequence, “elite programs” in Hungary are not directly comparable to the “elite schools” literature but relates to it due to the fact that high-ability students are selected there as well.

Various forms of tracking exist in Europe, but the Hungarian system is unique. Whilst the median student will study 8 years in general (primary and lower secondary) schools (*általános iskola*) from ages 6 to 14 and continue their studies in 4-year long tracked upper secondary school in either of the academic (*gimnázium),* mixed *(technikum)* or vocational *(szakközépiskola*) programs, there are two ‘elite’ programs that stand out. Some of the secondary schools (typically with 4-year long academic programs) also offer 8- or 6-year long academic programs that cream-skim the best (highest status) students in grades 4 or 6 (ages 10 or 12) from the primary schools (see Schiltz et al, 2019), offering an early to academic secondary schools. Students are admitted to elite programs after 4th and 6th grade based on their GPA, individual admission interviews, and centralized mathematics and Hungarian language admission exam scores, where each school can individually decide the weights of these criteria, but the test scores should account for at least 50%. Figure 2 provides an overview of the education system in Hungary, where the analysis sample is highlighted with red. I refer to the 6-year long and 8-year long academic tracks in a secondary school together as “elite programs” (treatment) and call the regular 4-year long academic program “general”. In this study, the analysis sample consists of two types of students: the treated group attends elite program (which is a subgroup of “Secondary general school programs” on Figure 2), and the control group attends general program.

2.2. Impact of early tracking and elite schools

Elite programs exist as the alternative to the last 2 or 4 years of education in primary schools when students are in grades 5-8, so elite students can attend secondary school earlier and stay there longer which is a case of earlier tracking. On the other hand, they are also considered as a form of ability grouping where better teachers, more advanced curriculum, and peer effects are present due to merit-based admission. The question is whether these 'elite' programs are living up to their promise and whether they are really helping to increase the proportion of students going on to university.

2.2.1. Early tracking and inequality

The empirical literature on the impact of early tracking has produced controversial results. In general, early tracking age harms low-ability, low socioeconomic-status students, but estimates for high-ability students are often positive, therefore tracking might contribute to the persistence of inequality (Borghans et al, 2020; Van Elk, Van der Steeg, and Webbink, 2011). On the other hand, in their experimental study in Kenya, Duflo, Dupas, and Kremer (2011) found positive effects for both high- and low-achieving groups: the direct effect of high-achieving peers is positive, whereas tracking also benefited lower-achieving pupils indirectly by allowing teachers to match instruction to students’ needs.

2.2.2. Elite school's impact on short- and long-term outcomes

Elite school literature is typically focused on short-term educational outcomes, and the findings are inconclusive. The identified effect sizes vary between studies, which is not surprising in the presence strong context dependency under different education systems. Most of the scientific evidence suggests that the impact of these selective schools on academic performance measured by student test scores is negligible: see for example Clarks' (2010) study of a UK district, and research conducted on US data by Dobbie and Fryer (2011) and Abdulkadiroğlu et al. (2014). Meanwhile, some studies also find positive effects: Horn (2013) in Hungary, Pop-Eleches and Urquiola (2013) in Romania, and a bit different context but rigorous experimental evidence from Duflo, Dupas, and Kremer (2011) in primary schools of Kenya.

On the other hand, there is a gap in the elite school literature about their effects on long-term outcomes. What is more, the studies in this field are generally limited to measuring only university enrollment rates. Abdulkadiroglu, Angrist, and Pathak (2014) is one of the few studies that looks at college quality as well: they measure it with selectivity as defined by Barron's, based on the competitiveness of admission. Lu (2021) distinguishes prestigious Russell Group universities, and Shi (2020) investigates STEM completion. These indicators paint a more nuanced picture and tell us more than simple enrollment rates. My study contributes to this line of research by distinguishing between field of studies, levels of studies (BA and MA enrollment) and university quality by creating a unique measure based on international rankings. The comparison of short-and long-term effects of elite programs in a similar setting is also rare in the international literature, so my study also aims to extend the knowledge in this area.

Elite schools have various definitions and different settings, which makes the comparison between studies challenging. Common features of these institutions are that they have a selective admission based on ability, they are more popular than the other schools, they have higher-achieving students, academic focus, and they are public high schools[2]. I conclude the relating literature based on the most relevant characteristics in Table 8, and I also collected some important additional information about the context of the studies. This summary highlights the main findings and provides a platform for comparison of short-and long-term effects. These papers are discussed more detailed in the thesis, where I describe the most common econometric methods in elite school literature and present the studies by research design. To sum up, effect sizes and significance for academic outcomes are mixed. RDD is a commonly used quasi-experimental setting, that focuses on the marginal student around the admission score, but its findings cannot be generalized due to heterogeneous effects throughout the ability distribution. OLS and logit models might suffer from omitted variables bias, hence they can be complemented with IV estimation to draw causal implications. Furthermore, the absence of short-term test score effects might reverse to other

[2] There are two exceptions: Duflo, Dupas, and Kremer (2011) study primary schools, and Horn (2013) study not exclusively high schools, but elite programs start earlier at middle school level (grade 5 or 7).

beneficial outcomes in the long run (such as higher university enrollment and quality) and parents take into account both while shaping their school preferences.

2.3. Measuring universities

The conventional outcomes to measure tertiary education are enrollment and completion. I also find it important to differentiate between institutes based on quality and types of majors to get more detailed insight into post-secondary learning outcomes and success. I will examine the following outcomes:

- Enrollment: start attending a university.
- Completion: obtaining a degree.
- Field of studies (STEM, art, medical, law and governance).
- Quality of university: international rankings and bibliometric indicators.
- Enrollment in MA studies.

Enrollment at the BA level gives us a first insight into post-secondary learning outcomes, whereas completion of university studies (obtaining a degree) measures a somewhat different aspect since drop-out is also a relevant factor and university enrollment rates can differ substantially from completion rates. To get a more detailed understanding, I will distinguish STEM majors since they could be of high policy relevance and look at differences in those fields, which are ranked high in students' preference rankings (art, medical studies, see university ranking in Csató and Tóth (2020) based on revealed preferences of applicants) and also those which lead to a traditionally prestigious degree and position of power (medical, law and governance). I also identify internationally recognized universities by synthesizing the performance of Hungarian universities in international rankings and looking at their publication performance based on Field Weighted Citation Impact (FWCI). These variables can show heterogeneity of the main outcome: enrollment. We can also observe a further layer of higher education, which is enrollment at the MA level, but the database is not long enough to observe any subsequent stages (for example PhD studies, or labor market transition).

I create a summarized ranking variable that is 1 if the given institute is listed in at least two of the following rankings: THE, QS, USNews, ARWU, in the period 2010-2020 (or closest data available). I chose this time interval because students in my sample could start higher education earliest in 2010, and most probably finish by 2020. Rankings often use 5-year averages, therefore the chosen period also seems reasonable. Based on these conditions, 8 universities were considered to be elite, and roughly 50% of students in the sample attended these universities.

3. Data and Methodology[3]

3.1. Dataset

The dataset consists of the linked administrative panel sample of the Hungarian population between 2008 and 2017 (Admin3). The sample selection is random 50% of the individuals who have valid social security numbers (TAJ) in Hungary, and datasets can be linked with an anonym identification code that is hashed social security numbers of the individuals. The education panel of the Admin3 is constructed by integrating student, class, and school-level information from the following datasets: National Assessment of Basic Competencies (NABC), registers for higher and for public education institutions of the Educational Authority, the higher education statistics (FIR) and the Hungarian Academy of Sciences Geography (GEO) database[4].

Furthermore, two special indicators were constructed for specific reasons: a family background index to incorporate SES into one variable, and one single primary school quality[5] measure that captures numerous elements of quality.

In addition, I have obtained several higher education aggregate indicators that can be used to measure quality and often serve as a basis for international rankings. First, bibliographic measures from SciVal (2022a) such as field weighted citation index (FWCI), citation per publication, output, citations, awards, and number of authors available for 17 universities in Hungary complemented with quartile range from Scimago (2022). Second, I created a ranking dummy variable, which distinguishes internationally recognized universities based on several international ranking (see in 2.3. Measuring universities).

In Table 9 I list the variables, their meaning, and their type. I define the treatment variable (elite program participation) with program type in grade 10.

I have used two main samples for these results. The first is the sample of 6th grade students in 2008 (Sample 6, baseline sample), the cohort that we can follow the longest from their enrollment into elite programs in 2008 until 2017, way into their higher education career. Another advantage of this sample is that pre-treatment control variables are available: test scores and other controls are measured at the end of grade 6th, so right before they start the elite program[6]. However, very few of them finish higher education by 2017 so we cannot observe their

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[3] The database construction and model specification outlined in this such as the baseline regression part builds on the previously published preliminary findings of this research (Csóka and Horn, 2022). However, revised, updated and more detailed results are presented in this study. The co-author consented to the use of the research results in the dissertation. My overall contribution to the publication was 80%.
[4] See further documentation and information about the Admin3 database at the adatbank.krtk.mta.hu website and in Sebők (2019).
[5] I am grateful to János Köllő for providing his codes and data for the primary school quality indicator. The methodology and construction of the indicator is entirely based on his ongoing recearch with Luca Nagy.
[6] It is true only for the 6-year long elite programs, as the students of 8-year long programs have already been enrolled in elite program since 2 years at the time when their measurement happens at the end of 6th grade.

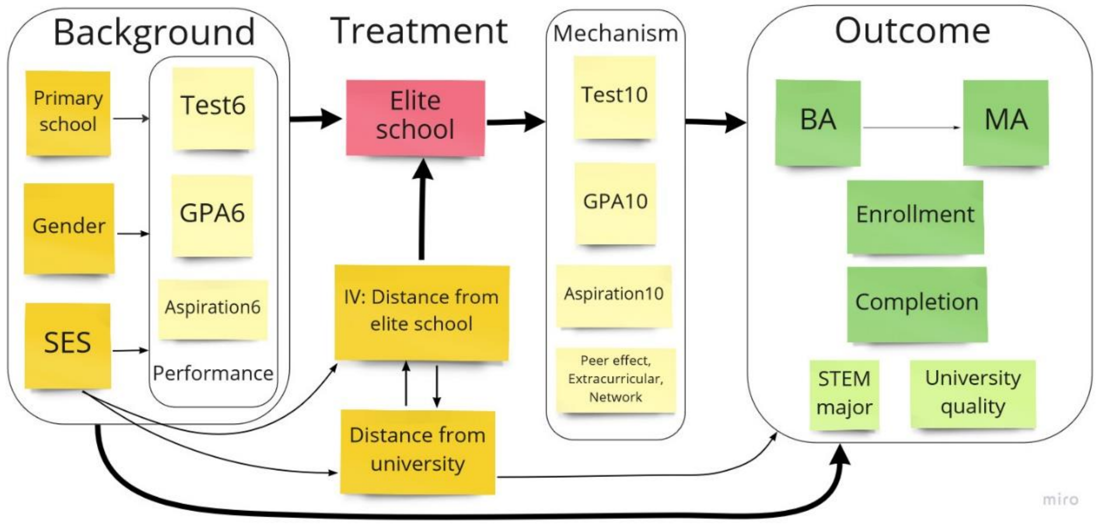
completion (degree) and whether they continued their studies on MA level or not. I constructed another sample to be able to investigate these two important outcomes as well. That is the cohort in 10th grade in 2008 (Sample 10), where we can observe their university completion rates (degree) and MA enrollment with a much higher probability in 2017. On the other hand, the drawback of this sample is that their pre-treatment control variables in 6th grade are not available, since the dataset starts only in 2008 when they finish 10th grade. I selected students who attend academic tracks: either 6- or 8-year long elite programs (whose secondary education begins earlier at grade 5 or 7) or 4-year long general programs (whose secondary education begins at grade 9). It is important to bear in mind that the elite students are a selected group within the already selected students who attend academic programs. Students in vocational tracks are not considered in this analysis. I applied data transformations to increase the size of the samples used for analysis.

3.2. The model

Prior research suggests that socioeconomic status (SES) and gender influence a person's educational ability, such as grades (GPA), non-cognitive skills, and values, hence educational outcomes (Brunello and Checchi, 2007, Scheeren, 2022a, b). I proxy ability by the NABC test scores, in addition, we can also observe (in the case of 6-year long elite programs) pre-treatment GPA and aspiration variables as well as socioeconomic status, gender, and primary school characteristics where students are coming from[7]. I argue that conditional on these individual variables selection into treatment is close to independent of the potential outcomes, therefore using the OLS or PSM will identify average treatment effects reasonably well. The identifying assumptions are unconfoundedness and overlap. I assume that we can observe all variables that have an impact on both enrollment to elite secondary school programs (T) and university outcomes (Y), meaning no omitted variables bias, therefore unconfoundedness holds. In addition, these observed variables predict well - but not perfectly - the enrollment in elite programs (overlap), so that average treatment effects can be identified using simple OLS and PSM methods. Unconfoundedness in social sciences is in most cases a strong assumption, and one cannot prove the lack of omitted variable bias. To overcome this obstacle, I also apply an instrumental variable. See Figure 1 below for the causal map that clarifies the assumed mechanisms.

[7] Due to the limited availability of primary school indicators – since these variables are unavailable for the students in 8-year long elite programs in baseline 6th grade cohort – I do not use it in the main regressions, but I show in the thesis chapter **Error! Reference source not found.**, that inclusion of these variables does not alter the results.

Figure 1: Causal map



I use OLS linear probability model with school fixed effects and PSM to estimate the effect of elite programs on higher education outcomes. I include various controls as outlined in Figure 1 above and estimate the following model:

$$Y_{ips} = \alpha + \beta T_{ps} + \delta X_{ips} + \gamma_s + \varepsilon_{is}$$

Where Y represents the outcome dummy variable (e.g. 10th grade test score, university enrollment and completion, STEM major) of student i in (elite) program p in school s, X includes controls on the individual level (eg. gender, socioeconomic status, and 6th grade school performance measured with NABC test scores, GPA, and educational aspiration), γ_s is school fixed effects measured in 10th grade and ε_i is the individual error term clustered at the school level. T stands for the elite program treatment: 1 if someone attended an elite program (either 8-year or 6-year long track), 0 if not.

In addition to these methods based on unconfoundedness, I address the assumption of no omitted variables by applying an IV estimation strategy (see section 5.1. Instrumental variables approach) as a robustness check. I use the distance from the closest school with an elite program as an instrument. This variable does not meet the exclusion assumption, since it can be correlated with the distance from the closest university and other socioeconomic factors, which can also influence the probability of university enrollment. Thus, I also condition on socioeconomic background and university distance in the IV models.

4. Results

4.1. Descriptive statistics

Table 10 summarizes the descriptive statistical results for the most relevant variables. The first two columns show sub-sample averages, and in the third column, there are differences and their statistical significance.

University outcomes (enrollment, STEM major, and ranking) are higher for elite students, meaning that on average they enroll at a university with a 17,6 percentage point greater probability, and choose a STEM major with a 4,8 percentage point greater probability, in addition, they attend elite universities more often. The rate of female students and the proportion of those who get free meals is lower in elite programs. On average, those attending elite secondary school programs have higher test scores and academic aspirations, furthermore, also have a better family background. All differences between the two groups listed in the table are statistically significant at a 1% level (except for free meals, here the 10% level applies). The greatest differences can be observed in the parents' education and SES index, which means that elite students have much socioeconomic status, and their parents have higher education.

4.2. Impact on university outcomes

I applied OLS and PSM models to investigate the effect of elite high school programs on several university outcomes. In the summary tables (Table 1, Table 2) I show how the effect size changes in the different model specifications – namely raw difference, fixed effects model, control variables model and the most preferred model including both control variables and fixed effects. I consider the specification using both control variables and fixed effects the most preferred one: on one hand, we need to condition on individual characteristics (control variables such as gender, SES, and before-treatment school performance) since these influence both the selection into treatment and the outcome variables, so omitting them would result in biased estimates due to endogeneity. On the other hand, school fixed effects control for various unobserved characteristics at the school level.

4.2.1. Conventional measures: enrollment and degree

Table 1 presents the summary of the main regressions. The results in different samples (6^{th} grade in 2008 or 10^{th} grade in 2008) with different outcome variables (enrollment and completion) are shown in separate columns. The completion counts within 9 years after 10th grade (because the database is until 2017), so supposedly within 7 years after enrollment in the university. The initial database covered 50% of the Hungarian population in the given cohorts, but since there are fundamental differences between school types in Hungary, in order to get a more precise estimate I have filtered out vocational schools from the sample, and analyses are run exclusively on academic schools: either usual academic 4-year programs or elite academic 6–8-year programs. The subsample of academically oriented students is where the identification assumption is the least troublesome, since these students are similar in the sense of academic orientation. That is, we are actually comparing students in general 4-year programs with students in 6-8-year elite programs. Results of 4 types of OLS model specifications are shown: either with or without control variables and school fixed effects. I used the control variables listed under Table 1. Fixed effects models control for all unobserved school-level characteristics, such as the composition of peers and teacher quality.

Table 1: Empirical results: enrollment and degree

	Y: Enrollment, Sample 6	Y: Enrollment, Sample 10	Y: Degree, Sample 10
Ymean [N]	63,8 [18358]	70,5 [18626]	46,5 [18626]
Ymean in general [N]	60,2 [14591]	67,2 [14873]	43,5 [14873]
Ymean in elite [N]	77,8 [3767]	83,4 [3753]	58,2 [3753]
β, OLS no controls, no f.e.	17,6*** (1,6)	16,2*** (1,4)	14,7*** (1,5)
β, OLS no controls, with f.e.	10,6*** (1,4)	9,1*** (1,2)	9,1*** (1,5)
β, OLS with controls, no f.e.	4,5*** (1)	3,6*** (0,9)	3,5*** (1)
β, OLS with controls, with f.e. [N]	3,5*** (1,2) [17722]	3,4*** (0,9) [17514]	3,4*** (1,2) [17514]
β, PSM with controls (ATT)	4*** (1,2)	3,4*** (1)	4,2** (1,6)
β, PSM with controls (ATU)[8]	3,4	4,6	1,7
β, PSM with controls (ATE)	3,5	4,4	2,2

Significant at ∗∗∗ 1%, ∗∗ 5%, and ∗ 10% levels. Baseline specification: f.e. is school fixed effects in 10th grade, and standard errors in parentheses clustered at the school level (521 schools in sample 6, and 530 schools in sample 10). Controls in the OLS models: proxies for socioeconomic status such as cheap and free meals, free books, number of computers, cars, bathrooms, books at home and variables of family background: number of siblings living together, how much help they get in preparation for school (learning, homework) at home, parental age, education, and employment. Also, individual controls: gender, year of birth, language preparatory class dummy, school performance: education aspirations, GPA, math and reading test scores at the end of the given grade. Imputation applied for all control variables except gender and test scores. Matching is on education aspirations, math and reading test scores at the end of the given grade, family background index (parental education, books at home, and computers at home), and gender. The number of treated/control groups in the 6th grade sample is (2868/2239), and in the 10th grade sample is (2471/1961)[9]. ATT is calculated using the nearest neighbor method with common support, where standard errors are bootstrapped (repetition=50).

Results are shown in percentage points.[10] Out of the 18358 students in the 6th grade sample, 14591 attend general and 3767 elite programs. Higher education enrollment is 60,2% in general and 77,8% in elite programs, hence raw enrollment difference is 17,6 percentage points. In other words, students who attend elite programs are on average 17,6 percentage points more likely to continue their education at universities than students in general programs (if those factors are excluded that influence enrollment in these elite programs, namely the control variables). When we look at differences within a given school (OLS fixed effects model), this difference decreases to 10,6 percentage points. This implies that there are significant within-school differences in the success of the

[8] Command „psmatch2" used in Stata16 does not show significance for ATE and ATU estimates.
[9] In this subsample all matching variables (gender, math and reading test score, aspiration and SES index) should be nonmissing to be able to run the algorithm in a reasonable way, and each treated observation has one nearest neighbor in the control group, on the other hand, one observation in the control group can be the nearest neighbor of more than one treated observations, hence the smaller size of the control group.
[10] Coefficients are rounded to one decimal place for better readability, so they might not add up exactly due to rounding.

general 4-year and elite programs. The most preferred OLS model uses controls and fixed effects as well: if someone went to an elite program instead of a 4-year long academic program within the same school, taking into account the observable selection by characteristics between programs within schools (holding other variables constant), than s/he has a 3,5 percentage point higher probability to enroll to university. In other words, an elite student with the same individual characteristics and attending the same school is 3,5 percentage points more likely to enroll in university than his or her general secondary school counterpart. The PSM algorithm yields similar results as the OLS controls model: average treatment effect on treated (ATT) estimate is 4 percentage points, furthermore average treatment effect on untreated (ATU) is 3,4 points and the average treatment effect (ATE) is 3,5 points. This implies, that the regression used has good properties: the assumption of the OLS linear function form does not have a large impact on the results, furthermore, the lack of common support in OLS estimation does not bias the results.

To be able to examine completion rates, I have run the regressions on the 10^{th} grade cohort as well. In this cohort, we can compare the effect of elite programs on enrollment and completion. Elite programs increase the probability of enrolling in university by about 3,4 (both in OLS and PSM) percentage points in the 10th grade sample. The enrollment regressions in Sample 10 show only slightly lower effect sizes than the results in Sample 6 and even compared to the baseline sample mean (63,8% to 3,5% and 70,5% to 3,4%). As here I controlled for 10th-grade observable characteristics (test scores, GPA, aspiration, SES, and gender) I expected effect sizes to be much smaller, as elite programs increase test scores more than the alternative track, and also have a positive effect on GPA. In other words, in sample 10 we are actually "over-controlling" for the effect of elite programs, so the resulting estimates are a lower bound of the real effect.

The effect of elite programs on university completion is similar in percentage points (around 3,4-4,2) to that on the enrollment probability (3,4%) but considering that the baseline probability is much lower for completing the university (46,5%) than to enroll in one (70,5%) the relative effect size of elite programs on completion is much larger than on enrollment. This can be due to the higher value-added of elite programs that can be better utilized at the university education (e.g. better teaching, curriculum, higher-achieving peers, more performance-oriented environment) or some unobserved heterogeneity in the data (e.g. if students who apply for elite programs are more diligent and determined that biases the results upward).

4.2.2. Further indicators: field of studies, quality of a university, MA enrollment

Raw differences in Table 2 show, that significantly more elite students go to STEM, arts, and medical majors (although the latter is only significant in sample 6), and they go to internationally recognized universities with 5 percentage points greater probability, and also more likely to start MA level studies. On the other hand, there is no difference in the probability of law and governance majors and FWCI. Once we control for observable characteristics and school fixed effects, differences between the probability of attendance of the examined majors disappear. The lack of impact is likely because elite programs provide a comprehensive education with academic focus, and in most of the cases different specializations (for example advanced training in mathematics, natural sciences or literature) only appear at the last years. No effect is identified on attending higher publication performance research universities (measured by the citation performance of the universities, FWCI) but elite students enroll in internationally recognized universities and MA level studies with a greater probability.

Table 2: Empirical results: Field of studies, university quality, MA enrollment (sample 10)

		Coefficient of Elite: β (s.e.)	
Outcome	Mean [N]	Raw difference	OLS with controls, with f. e.
STEM	31,5 [13102]	4,5*** (1,1)	-0,1 (1,4) [12366]
Arts	1,1 [13102]	0,6** (0,3)	0,2 (0,4) [12366]
Medical	8,4 [13102]	1,1 (0,7)	0,3 (0,8) [12366]
Law and governance	7,5 [13102]	-0,6 (0,6)	1,1 (0,8) [12366]
Ranking	61,0 [13126]	4,9*** (1,6)	2,6* (1,5) [12389]
FWCI	1,13 [11582]	-0,01 (0,01)	0,01 (0,01) [10952]
MA enrollment	22,1 [18626]	9,8*** (1,1)	2,7** (1,3) [17514]

Significant at ∗∗∗ 1%, ∗∗ 5%, and ∗ 10% levels. Baseline specification, see notes under Table 1.

4.3. Compare short- and long-term educational effects

To sum up, the results, I have created Table 3 which includes the summary statistics of main outcome indicators: the number of observations, mean by program type (elite and general), the overall sample mean, standard deviation, the raw difference between elite and general academic secondary school students, finally the effect size in the most preferred OLS model that includes control variables and fixed effects as well. The first four rows (above the line) include results of short-term educational outcomes of elite programs, and the rest are longer-term post-secondary educational effects.

Table 3: Summary of results

	Sample	N	Elite	General	Mean	Sd	Difference	N	Effect
Math test	6	17291	1828	1734	1753	176	95***	16726	21***
Read test	6	17296	1783	1714	1728	153	70***	16731	14***
GPA	6	13667	4,13	3,92	3,96	0,65	0,21***	13223	0,05***
Aspiration	6	14702	0,92	0,82	0,84	0,37	0,10***	14229	0,001
Enrollment	6	18358	0,78	0,60	0,64	0,48	0,18***	17722	0,035***
STEM	6	11716	0,33	0,28	0,29	0,45	0,05***	11345	-0,014
Ranking	6	11719	0,66	0,61	0,62	0,49	0,06***	11348	0,029**
Degree	10	18626	0,58	0,44	0,47	0,50	0,147***	17514	0,034***
MA enrollment	10	18626	0,30	0,20	0,22	0,42	0,098***	17514	0,027**

Significant at $***$ 1%, $**$ 5%, and $*$ 10% levels. Models include f.e. and controls, see baseline specification under Table 1. The first N shows the number of students with nonmissing outcome variable, and the second N shows the number of observations in the models where the effect is calculated.

This way we can compare the significance and effect sizes of the different measures. If we look at the short-term outcomes, elite programs impact test scores and GPA positively, on the contrary, we rule out any effect on aspiration. The effect on math test scores is more than on reading test scores. Findings are inconclusive about STEM track choice (and also about the probability of choosing other fields such as arts, medical, law and governance see Table 2), but elite programs have substantial effects on the quality of universities where students enroll, as well as on acquiring a degree and progressing to MA-level education. Compared to the respective baseline probabilities, the coefficient of elite programs is the highest in the case of MA enrollment, followed by degree, enrollment, and ranking, which means that the effect on BA completion (degree) and MA enrollment is greater in magnitude than on the simple BA enrollment rate. Short-and long-term educational effects are both positive, and effect sizes are comparable to 7-10% of standard deviation in the sample and always smaller than raw differences, which means that a substantial part of the variation is explained by observable pre-treatment characteristics (students with higher test scores and higher SES attend elite programs), but elite programs are associated with better short- and long-term educational outcomes even after controlling for these various individual and school-level characteristics.

The main takeaway of this section is, that the effects of elite programs on both short-and long-term educational outcomes are significantly positive (except university aspirations and choice of STEM major), and their size is comparable to roughly 7-10% of the standard deviation. It was also worth investigating not only BA enrollment and acquiring a degree, but also looking deeper and discovering the impact on the quality of the university to where students get admitted, and the probability of enrollment in MA level studies, to get a more detailed understanding of the effect of elite programs beyond the conventional higher educational outcomes.

4.4. Heterogeneity

I analyze heterogeneity to see whether there are differences in the magnitude of effect between groups based on gender, socioeconomic status, education aspirations, test scores as well as school-level test-score value added (school quality) to get a more detailed description of what drives the results. I estimated the same model but separated the sample into “advantageous” and “disadvantageous” groups based on the heterogeneity variables one by one, for example first I estimated for the subsample of male than female students, second, I took the subsample of students who initially (in grade 6) planned to go to university than the ones who didn’t. Table 4 shows the coefficient of the elite program in the different subsamples. Students who have at least one parent with a university degree belong to the group “parent with degree”, students who meet the following criteria: upper quartile in math and reading test scores and above median GPA belong to “good student”, whereas “good math” is defined as the upper quartile of ability distribution based on mathematics test scores. The two subsamples are always complementary sets of the whole sample, although sometimes they do not add up to 17722 sample size, since heterogeneity analysis is not conducted on observations with missing values in the given variable (for example, there are 7218 high-SES and 7252 low-SES students in the sample, and for 3252 students SES index is not available).

Table 4: Heterogeneous effects in subsamples (sample 6, enrollment)

	Coefficient of Elite: β (s.e.)[N]			
Subsample	Advantaged	Disadvantaged	Subsample	Diff
Male	2,4 (1,8) [7402]	4,9*** (1,5) [10320]	Female	-
Plan university	2,2* (1,3) [12843]	10,5** (5) [2408]	Not plan university	**
Parent with degree	3,1** (1,3) [7264]	4,9*** (1,7) [10458]	Parent without degree	-
Good student	2 (2,2) [1582]	4,0*** (1,5) [12489]	Not good student	-
Good math	3,1* (1,6) [4432]	4,2*** (1,5) [13290]	Not good math	-
SES > median	2,2* (1,3) [7218]	6,6*** (2,4) [7252]	SES <= median	**

Significant at ∗∗∗ 1%, ∗∗ 5%, and ∗ 10% levels. Models include f.e. and controls, see baseline specification under Table 1. Additional information: each of the heterogeneity variables examined in separate models should be nonmissing in the respective model. The significance of the difference between the coefficients for advantaged and disadvantaged subgroups is reported in the last column.

There are positive effects in all cases, although with a higher p-value (lower significance) for advantaged groups. The effect is even insignificant for males and good students, meaning that they would enroll in university with a similar probability even if they would not attend elite programs. Although it does not necessarily mean that elite programs are not beneficial for them, but insignificant estimate might be also due to decreased size of the given subsample. The analysis conducted on separate subsamples shows that students with low initial university aspirations benefit from elite programs the most. Low-SES students also experience a high increase in university enrollment probability (6,6 percentage points), furthermore, attendance of elite programs for pupils whose neither parent has a tertiary education and for female students result in a 4,9 percentage points higher probability of university enrollment, ceteris paribus. To test the equality of regression coefficients, I used the formula proposed by Paternoster et al. (1998). The difference between the coefficients of the subgroups is significant in cases of university aspirations and SES. For other heterogeneity variables I cannot reject the null hypotheses of equal coefficients of advantaged and disadvantaged subgroups. It means, that those students who initially (at the end of 6th grade) did not plan to attend university benefit significantly more from elite programs than those who did. Similarly, students with SES under the median experience higher gains than those, who have above-median SES. For gender, parental background, and initial test scores the evidence about the differences between the subgroups is inconclusive.

There seems to be an equalizing effect between students attending elite programs since findings imply a greater positive effect for more disadvantaged students: low-SES, lower school performance, lower aspirations students, and girls. Although sorting into elite programs harms equality in the society since it is not merely based on ability but on SES as well (which is linked to test scores if sorting happens at earlier ages), equality of outcomes is increased among students admitted to elite programs.

5. IV Estimation, Robustness

5.1. Instrumental variables approach

To address causality and test the robustness of the OLS model I apply IV approach. We need a variable that explains the selection into elite programs well (strong first stage) but does not explain our outcome (entering into higher education) through any other channel than elite program participation (exclusion restriction). There is one candidate for such an exogenous variance: the distance of home from the nearest elite program in grade 6. I will be using the regional variation (distance) from home to the closest elite program as an instrument, similar to the one proposed by Card (1999) generally and used by Schiltz et al (2019) for Hungary. In this setting, the exclusion assumption of IV is not met, since the geographical dispersion of schools and universities might not be independent of each other, and socioeconomic factors influence housing options thereby both distance from the nearest elite program and from the university.

5.1.1. First stage: effect of distance on elite program participation

We can show that living farther away from an elite program (ceteris paribus test scores, family background, and distance from university) has a negative effect on entering elite programs. The results of the first stage regression are presented in Table 5, separately for the 6th and 10th grade cohorts, regressions include the entire set of control variables, the same as in the previous models.

Table 5: First stage results

Y: Elite	Sample 6	Sample 10
Distance from elite program	-0,005***	-0,005***
	(0,0006)	(0,0006)
Distance from university	0,001**	0,0006
	(0,0005)	(0,0005)
Observations	15538	13802
R-Square	0,1168	0,1169
F-statistic	56,7	65,2

Significant at ∗∗∗ 1%, ∗∗ 5%, and ∗ 10% levels. Standard errors are in parentheses. Models include f.e. and controls, see baseline specification under Table 1. Additional information: controls also include distance from the student's home to the closest school with elite program, and to the closest university. Outcome variable is the treatment (1 if the student attends elite program and 0 otherwise). Effective F-statistic of Montiel Olea and Pflueger (2013) is reported.

The F-statistic is greater than 10, which shows that the IV is not weak. The interpretation of the coefficient -0,005 is the following: students that live one additional km away from the nearest elite program (6- or 8-year long, whichever is closer), have a 0,5 percentage points less chance of entering an elite program, holding other variables constant. I emphasize again, that this effect is conditional on all variables included in the regression. The coefficient of distance from the closest elite program is the same in both cohorts, which implies the reliability of the results. As in our data, 20% go to these elite programs (only students in academic high schools are in the data), this is an important, albeit not very strong effect. Distance from the closest university, however, is positively associated with elite program participation - which could be due to the fact that both elite programs and universities are more concentrated in cities -, in addition, the effect size is smaller than for distance from school, and imprecisely estimated.

5.1.2. Two-stage least squares IV estimator

In this paragraph, I will present the IV estimator for the effect of elite program participation on university enrollment in the 6th and 10th grade samples. Table 6 below shows the results of the estimations: the effect of elite programs on university enrollment in 6th and 10th grade cohorts. OLS shows that students in elite programs have about 16-18 percentage points higher chance of enrolling in a university (see Table 1: Empirical results: enrollment and). This drops to roughly 4 percentage points if we control for many observed characteristics. The story does not change if we include school fixed effects (between school selection is much larger than within school selection), and the most preferred OLS coefficient is around 3,5 percentage points. If we instrument the treatment with distance, the effect size increases substantially (significant on the 5% level). That is, students that are on the margin (i.e. they are affected by the distance from the nearest elite program, so either chose this track because they are close to it, or not chose it because they are far, while otherwise they would do the opposite) are 10,1-7,8 percentage points more likely to enroll to university if they go to an elite program.

Table 6: 2SLS results (enrollment)

University enrollment	Sample 6		Sample 10	
	OLS	IV	OLS	IV
Elite	0,035***	0,101	0,034***	0,078
	(0,012)	(0,095)	(0,009)	(0,085)
Observations	17722	15538	17514	13802
Adjusted R-Square	0,307	0,274	0,336	0,327

Significant at ∗∗∗ 1%, ∗∗ 5%, and ∗ 10% levels. Models include f.e. and controls, see baseline specification under Table 1. Additionally, IV models control for the distance from home to the closest university.

A possible reason why the conditional IV coefficient turned out to be insignificant is the substantial decrease in sample size due to the use of distance data: ZIP codes of students were missing in 2679 cases, those students immediately dropped out of the sample when merging the two datasets. If we consider the effect size, the roughly 8 percentage points effect – even if it is imprecisely estimated – is slightly higher than the corresponding OLS coefficient, so it supports the positive effect. However, if the exclusion restriction does not hold, the IV estimate can also be biased. A shortcoming of the IV estimator is that it gives a good estimate of the effect of the endogenous causal variable (T) only among observations, whose elite program participation (T) is affected by the exogenous instrument (Békés and Kézdi, 2021 p.619), these students are called compliers. In case of imperfect compliance, the effect on always-takers (supposedly high-ability high-SES students, and middle-ability high-SES students whose parents are committed to getting their child into an elite program) and never-takers (it is hard to speculate who could that be) cannot be measured by IV.

5.2. Robustness

During the analysis, I applied imputation and used different samples (sample 6 and 10) and subsamples of them (for example, without 8-year long track, or nonmissing GPA and aspiration) so I find it important to check thoroughly whether these steps could have altered the results or not. I have conducted several robustness checks to see whether the main coefficient (3,5%: OLS estimate of enrollment effect in the most preferred model) is sable throughout different specifications. Essentially, my assumption is that these modifications did not change the model and its findings substantially, so results are not driven or influenced by these steps and the coefficients in the different samples are comparable. What is more, if the coefficient is stable in different samples and different periods, it implies the generalizability of the findings.

First, I run conditional logit models to test whether the choice of functional form matters in the regression. Although coefficients are not directly comparable, conditional logit gave similar results as OLS (linear probability model) in terms of direction and significance of the effect, which implies that the linearity assumption in OLS and the possibility of predicted values outside of the 0-1 interval is not a major issue. Second, I tested whether certain subgroups of the sample could drive the results. From the results we can conclude that the effect is similar in 6- and 8-year long programs. I also reduced sample by cutting the upper part of the ability-distribution. I defined a group of so-called "always-takers" in Sample 6 and Sample 10 as well: those students who are in the best decile in their sample based on either math or reading scores will probably go to university anyway, so I dropped them from the samples. This resulted in slightly greater coefficients, which is in line with the fact that IV estimates (where only compliers are considered and no always-takers) are greater than OLS estimates. The variable transformations to deal with missing values also did not change the main coefficient considerably.I also conducted a panel analysis using not only those students who finished 6th grade in 2008 but I also included those two additional cohorts who finished 6th grade in 2009 and 2010 as well. In the pooled OLS model with controls and school fixed effects, I also included year fixed effects, because average enrollment rates varied yearly. In this panel sample, students of elite programs are associated with 5,1 percentage points greater probability of enrolling in a university, ceteris paribus. The coefficient is significant and higher than in sample 6, so it is also conceivable that the effect is typically somewhat greater than what I identified.

Table 7: Robustness of elite coefficient under different specifications (sample 6, enrollment)

Model	Coefficient of Elite: β (s.e.) [N]
Baseline (with f.e. and controls)	3,5*** (1,2) [17722]
Conditional logit	0,26*** (0,07) [17480]
Without 8-year elite program	3,1** (1,5) [16027]
Without 6-year elite program	3,7** (1,7) [15459]
Without "always-takers"	4,5*** (1,4) [14824]
Before imputation	3,4** (1,4) [13742]
Panel	5,1*** (0,8) [51505]

Significant at *** 1%, ** 5%, and * 10% levels. Models include f.e. and controls, see baseline specification under Table 1. Additional information: models from second row are named based on their one modification compared to the baseline. Conditional logit: log of odds ratio is reported[11].

Robustness checks show a relatively stable coefficient throughout different specifications and indicate no sign of error due to the choice of linear form and imputation, and it seems reasonable to handle 8-and 6-year long elite programs together since there is no significant difference in effect sizes. What is more, if the coefficient is stable in different samples and different periods, it implies the generalizability of the findings. The results of the panel model in 2008-2010 suggest that the estimated elite coefficient might be even higher than what was measured in the 6th grade cohort.

A shortcoming of the database is that post-secondary enrollment is only observed at Hungarian universities, so we cannot observe if and whether a student is enrolled in a foreign university[12]. However, elite students emigrate

[11] Odds ratio is calculated from the reported coefficients in the following way: $e^{0,26}=1,30$.

[12] Although there is an ongoing research which tries to identify Hungarian students in the Admin3 database who enrolled at a foreing university, and with the help of these results I might correct the enrollment variable later.

with a higher probability (see Cook, 2003) so supposedly, the “underestimation[13]” of university outcomes is greater among elite students. As a consequence, my estimates are rather a lower bound of the actual effect size, due to the mismeasurement of tertiary enrollment abroad.

6. Discussion

There is a consensus in the literature that tracking harms equality, but the results about its effectiveness are inconclusive, which leads to the questioning of their existence. Context dependence is essential, as countries have different education systems and cultures, and studies (even those using standard RDD methods) find mixed results under different circumstances. It is also necessary to distinguish between short- and long-term impacts: while most studies find no short-term effects on test scores (although there are exceptions, see Pop-Eleches and Urquiola [2013] and Horn [2013]), long-term effects on university enrollment are generally positive.

This study exploits rich administrative matched data for students and institutions in Hungary to explore how students are being selected into elite secondary school programs and how attending them affects their test scores and university outcomes. I also developed a university ranking indicator that captures international recognition and quality to obtain an outcome variable with greater variability than simple enrollment rates, and measure also the institutions’ quality which is also essential for human capital accumulation. The probability of attending elite programs is influenced by both family background (SES) and test scores at a similar magnitude, whereas within test scores, differences in mathematics seem to be a stronger driver of elite program admission than reading. The selectivity of 8- and 6-year long elite programs do not differ substantially, so it is reasonable to consider them as one treated group. Short- and long-run educational effects of elite programs were also compared, and the effects were positive in both cases. Given the same pre-treatment characteristics, test scores and GPA of students in elite programs change more favorably than their counterparts in general programs. However, changes in university aspirations do not differ significantly. The main focus of this research was post-secondary education outcomes: OLS- and PSM-estimates show significant, 3-4 percentage points effects of elite secondary school programs on university enrollment and completion rates, although the differences are much lower than raw enrollment and completion differences, and the impact on completion is nearly twice the magnitude. Similarly, I have found greater effects on MA enrollment than BA enrollment, and also a significant positive impact on the probability of attending internationally recognized universities, which implies that it is worth looking deeper than simple enrollment rate since it shows heterogeneity in university quality and difference in effect sizes for different outcomes. In the case of choosing certain fields of majors (STEM, medical, arts, law and governance), I have found insignificant effects.

[13] Falsely assigning 0 value for university enrollment, because they attend a university abroad which we cannot observe.

Roughly half of the impact of elite programs is due to more favorable changes in the school performance of elite students than comparable general students: improvements in test scores and lower decreases in GPA. The results are heterogeneous between groups created based on gender, school performance, SES, and primary school quality, where students from more disadvantaged backgrounds generally benefit more from participating in elite programs. I used the distance from home to the closest school with an elite program as IV to uncover causality, and the two-stage least squares coefficient shows an 8-10 percentage points effect, which is close to the OLS estimate but slightly greater than that, although insignificant. Robustness checks show that the elite coefficient is stable throughout different specifications, and the panel model implies that it might be even higher in other periods than in sample 6.

Regarding the size of the impact on enrollment, my finding is in line with evidence of a similar study in the UK: Clark (2010) shows between 2-5 p.p., whereas the odds ratio of 1,2 at Lu (2021) translates to roughly 12% percentage point difference in our case[14] and comparable to the most preferred model (with controls and school fixed effects) that shows 3 p.p. but most probably this coefficient is a lower bound of the one in Lu's (2021) study because school fixed effect is a stronger over-control for school-level bias than the mean KS2 attainment in the other paper so in our setting we underestimate the effect compared to hers. Students who clear the threshold in a US boarding school have a 5-6 percentage points higher likelihood to enroll in college (Shi, 2020) compared to the (very high) baseline probability of 92% within non-admits close to the cutoff. Beuermann and Jackson (2022) found that attending preferred selective schools in Barbados increases the probability of entering and completing post-secondary university preparation programs[15] by 2 percentage points, which translates to higher university attainment for women later but not for men. The direction and significance of the effect are similar, the magnitude is comparable – even though grammar schools in the UK, boarding schools in the US, and education systems in Barbados and Hungary differ in several ways - which suggests that these results may generalize to other countries and other settings as well. On the other hand, it is also important to emphasize context dependency and that studies often find null effects as well.

Effectiveness and equality are two important factors in the characterization of education systems. If we look at the effect size of the OLS estimates, elite programs have a positive, significant association with not only university enrollment and completion, but with the quality of a university and further studies at MA level as well, even after controlling for observable student and school characteristics. These findings imply that elite programs are an effective way of educating their students, as they have a higher value-added in terms of university outcomes on

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[14] If the odds ratio would be 1,2 in my setting, then it would mean that elite students have 1,2 times greater probability to attend university, conditional on the control variables. Taken that the enrollment rate of general students is around 60% in Sample 6, and 1,2*60=72 wich is the comparable elite students enrollment, this odds ratio would results in 72-60=12 p.p. difference.

[15] The Caribbean Advanced Proficiency Examination (CAPE) is two-year tertiary-level program, equivalent to the British Advanced levels examinations. Student who aim to attend university will take the CAPE, since passing certain units is a common admission requirement for universities (Beuermann and Jackson, 2022 p. 730).

average than their counterparts: general academic secondary school programs. In this sense, they deliver on the explicitly stated promises to increase the higher education participation of their students. On the other hand, the findings are controversial if we look at the equality effects. As students with higher SES sort into elite programs and these tracks increase the probability of positive long-term education outcomes (enrollment, completion, and ranking of university) it results in growing inequality of opportunity. However, we can observe an equalizing effect among students admitted to elite programs, as students with disadvantaged backgrounds (lower SES, lower school performance) benefit more from this program than their privileged peers. It would be essential to increase access to and participation in these elite programs for students with more disadvantaged backgrounds to counterweight the inequality effect.

My findings are subject to several caveats. As previously stated, elite programs in Hungary are unique because they are selected earlier in time (after grades 4, and 6), and selection occurs within schools where the alternative is the general academic route (after grade 8). As a result of context dependence, my findings are not directly comparable to the standard elite school literature, in which selection occurs later, at a single point in time and between schools. Second, the unconfoundedness assumption might not hold because unobserved characteristics (such as parental motivation and non-cognitive skills) probably correlate positively with both the treatment and outcome variables. As a consequence, some of my results based on this assumption may be upward biased. Third, while IV estimation may resolve part of the unobserved selection, the exclusion restriction may not be met, and the estimation could also be problematic due to weak IV. In addition, it is not trivial to determine who are the compliers, the subsample of students where we can identify the effect.

Last but not least, I would like to mention some unanswered questions that could be the topic of further research. First, it would be interesting to look into what policy measures could help increase the participation rates of more disadvantaged students in elite programs and make the selection less background-independent and more ability-based. Increasing information among students with less privileged status (with lower parental education, for instance) and in rural areas, or a form of teachers' recommendation system could be worth considering. Arany János Talent Program tries to bridge the gap and help students from more disadvantaged backgrounds attend academic secondary schools that prepare them better for higher education. However, segregation within schools between classes still exists as a challenge. Second, it would be interesting to discover further channels of the mechanism and find what causes more favorable changes in GPA, mathematics, and reading test scores in elite programs compared to general programs. Is it due to better teachers sorting to higher-achieving elite classes, differences in curriculum, or peer effects? Third, in this study, I analyzed the post-secondary education outcomes but did not include other important potential long-term effects on labor market participation, wages, and well-being due to limited time coverage of the database. The new wave of the Admin3 database will include data not only until 2017 but 2021, which will make it possible to observe early labor market outcomes as well. On the other hand, this database will also make it possible to analyze the university completion rates for the 6th grade sample, where before-treatment school performance is available, so we can identify the effect more precisely.

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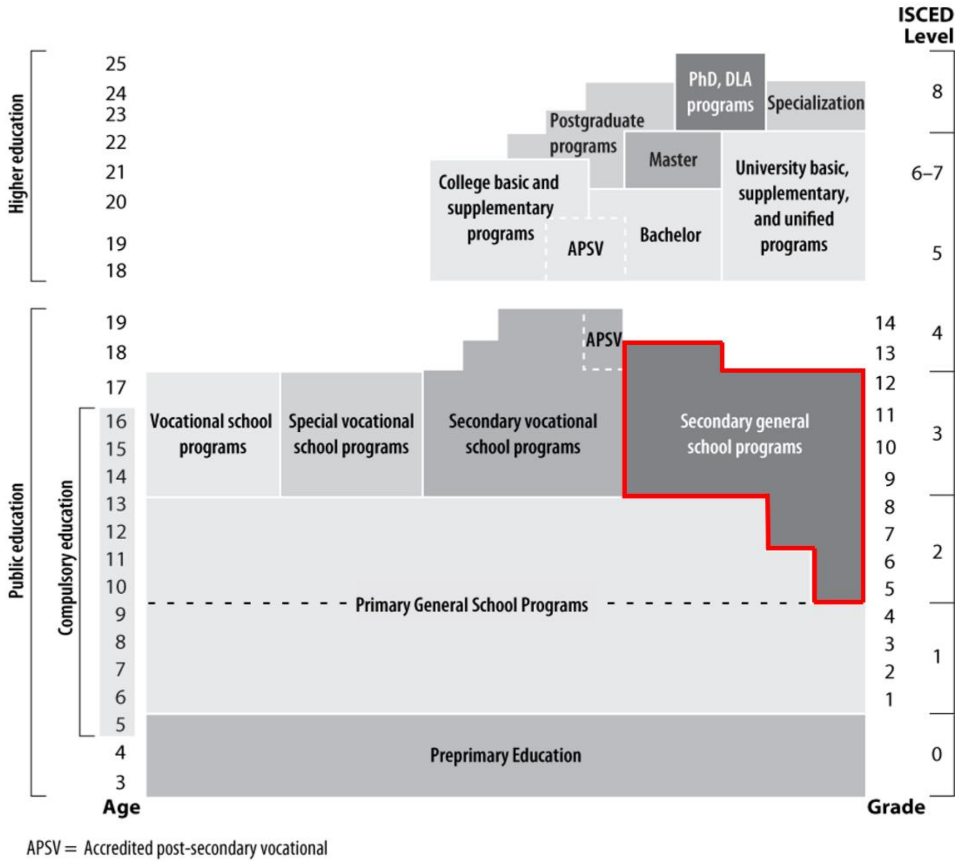
Appendix

Figure 2: The overview of the education system in Hungary



Source: Mullis et al (2016). Analysis sample highlighted in red (own edit).

Table 8: Literature summary on elite schools

Paper	Estimated effect	Result	Design	Context	Term
Duflo, Dupas, and Kremer (2011)	ATE on test scores (18 months later)	Positive	RCT	Kenya, primary school, ability grouping	Short
Deming and his co-authors (2014)	ATE on college enrollment, quality and degree completion	Positive	RCT	USA, lottery, integration	Long
Abdulkadiroglu, Angrist, and Pathak (2014)	LATE on test scores	Zero	fuzzy RDD	USA: Boston, New York	Short
	LATE on college enrollment and quality (Barron's selectivity)	Zero	fuzzy RDD	USA: Boston, New York	Long
Pop-Eleches and Urquiola (2013)	LATE on high-stakes graduation test (12th grade)	Positive	RDD	Romania	Short
Beuermann and Jackson (2022)	LATE on test scores (10th grade)	Zero	RDD	Barbados	Short
	LATE on postsecondary school completion	Positive	RDD	Barbados	Long
Clark (2010)	ATE, LATE on test scores	Positive, inconclusive	OLS, fuzzy RDD	UK	Short
	LATE on university enrollment	Positive	Probit	UK	Long
Lu (2021)	ATE on university enrollment and quality (Russell Group)	Inconclusive	Logistic regression	UK	Long
Horn (2013)	ATE on test scores (10th grade NABC)	Positive, inconclusive	OLS, IV	Hungary (8 or 6 years long elite programs)	Short

Table 9: Variables description

Dependent variables			
Variable	Meaning	Type	
Enrollment	Enrolled in university	Dummy (1: yes)	
Field of studies (e.g. STEM)	Enrolled in a major in certain fields of study (e.g. STEM)	Dummy (1: yes)	Quality
Ranking	Enrolled in an internationally recognized university[16]	Dummy (1: yes)	
FWCI	Field Weighted Citation Index of the university	Numerical	
Degree	Obtains a university degree	Dummy (1: yes)	
MA enrollment	Enrolled in university at MA level	Dummy (1: yes)	

[16] See definition at the end of **Error! Reference source not found.**.

Independent variables			
Language preparatory	Participated in language preparatory program	Dummy (1: yes)	Program type
Elite	Participated in an elite program	Dummy (1: yes)	Program type
Mathematics test score	NABC score	Numerical	School performance
Reading test score	NABC score	Numerical	School performance
GPA	Grade point average of last semester: 5 is best and 1 is worst	Numerical	School performance
Aspiration	Plan to enter university	Dummy (1: yes)	School performance
Female	Gender of the student	Dummy (1: yes)	Individual
Birthyear	Year of birth of the student	Numerical	Individual
Primary school score	The underlying latent factor of quality.	Numerical	Individual
Primary school quality	3 bins based on score: lower quartile, upper quartile, or middle.	Categorical	Individual
SES index	Family background composite index	Numerical	Socioeconomic status
Cheap meal	Entitled to a cheap meal in school (state subsidy)	Dummy (1: yes)	Socioeconomic status
Free meal	Entitled to a free meal in school (state subsidy)	Dummy (1: yes)	Socioeconomic status
Free book	Entitled to free books in school (state subsidy)	Dummy (1: yes)	Socioeconomic status
Computer	Number of computers in the household	Numerical	Socioeconomic status
Car	Number of cars in the household	Numerical	Socioeconomic status
Bath	Number of bathrooms in the household	Numerical	Socioeconomic status
Book	Number of books in the household (binned values)	Categorical	Socioeconomic status
Siblings	Number of siblings living together	Numerical	Socioeconomic status
Parental education	Educational attainment level, separately for mother and father	Categorical	Socioeconomic status
Parental employment	Has a regular job, separately for mother and father	Dummy (1: yes)	Socioeconomic status
Parental age	Age, separately for mother and father	Numerical	Socioeconomic status
Help	Whether family helps with homework at least once a week	Categorical	Socioeconomic status
Distance elite	Distance from home to the closest school with an elite program by car (in kilometers between ZIP codes)	Numerical	Instrument
Distance university	Distance from home to the closest university by car (in kilometers between ZIP codes)	Numerical	Instrument

Table 10: Difference between elite and general secondary school programs (sample 6)

	Elite	General	Mean diff.
Math test (10th grade)	1828.332	1733.586	94.746***
Read test (10th grade)	1783.296	1713.542	69.754***
Enrollment	0.778	0.602	0.176***
STEM	0.326	0.278	0.048***
Ranking	0.664	0.608	0.056***
Female	0.541	0.591	-0.050***
Math test (6th grade)	1683.447	1594.523	88.923***
Read test (6th grade)	1670.596	1604.707	65.889***
Aspiration	0.934	0.819	0.115***
SES index	0.885	0.477	0.408***
Mother university	0.441	0.294	0.147***
Father university	0.386	0.240	0.145***
Free meal	0.017	0.022	-0.005*
Car in household	0.890	0.844	0.046***
Books in household	0.660	0.561	0.099***
Siblings	1.240	1.160	0.079***

Significant at ∗∗∗ 1%, ∗∗ 5%, and ∗ 10% levels. N=18358. Outcome variables are above the line: test scores as outcome are measured at the end of 10^{th} grade. Control variables are under the line and measured at the end of 6^{th} grade.