The Implication of School Admission Rules for Segregation and Educational Inequality

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Research Report

Abstract: In this research project we investigate how the design of school assignment rules affects social segregation and its implications for social inequality. The context of our study is the Hungarian school system, which is characterized by early ability tracking combined with a centralized assignment mechanism based on the student-proposing deferred-acceptance algorithm. The project is split into three interrelated parts. First, we investigate the different factors that affect social segregation through the school assignment process. Second, we analyze how segregation resulting through school assignments affects inequality in student achievement. In a final step, we plan to combine insights from the first two parts in a counterfactual analysis to evaluate different assignment rules.

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1 Introduction

School systems differ in how they allocate students to schools in several ways. Some require students to attend the closest school in their neighborhood, whilst others sort students into different schools based on their ability. Most school allocation systems are not coordinated and students must individually apply to each school. In these decentralized systems, some applicants may receive offers from multiple schools and others end up with no offer. However, in recent years an increasing number of countries introduced centralized school assignment mechanisms to increase the efficiency and fairness of this process. France and Finland, for instance, assign students to secondary schools based on a variant of the student-proposing deferred-acceptance algorithm introduced in 2007/08 (Hiller & Tercieux 2013, Salonen 2014). Spain already uses a centralized assignment process and has done so since the 1980s, applying the Boston mechanism as matching procedure (Calsamiglia 2014). These are only some examples of assignment mechanisms applied by European countries.[1] There are now a wide range of matching algorithms applied in many education systems around the world.

An important question is if the design of assignment rules affects social segregation, and whether this increases social inequality. In this paper, we study these questions in the context of the Hungarian school system. The Hungarian school system is an interesting case because it assigns students to one of three tracks after 8th grade based on student abilities, and this form of tracking is typically assumed to exacerbate social segregation (see, e.g., Brunello & Checchi 2007, Schütz et al. 2008, Ammermueller 2013). Furthermore, since 2000 the assignment process is completely centralized at the national level and follows the student-proposing deferred-acceptance algorithm. This is important, because the information embedded in the centralized assignment process allows us to investigate different sources of social segregation and to credibly estimate their potential effects on student achievement.

Our analysis proceeds in three steps. First, we investigate the different factors that might affect social segregation through the school assignment process. Ability tracking is likely to be the most important factor because of the large skill gaps between students from low and high socio-economic backgrounds. However, social segregation might also result from differences in student and parental preferences. Prior research, for example, points to a strong socio-economic (SES) gradient in parental preferences for better schools (see, e.g., Burgess et al. 2015). On the supply side, students' priorities at schools – aside from ability – might help to explain why students from different SES backgrounds end up in different schools. To understand these sources of segregation, we model both student preferences and their priorities at different schools. This will be followed by an estimation of the underlying parameters based on the observable assignments, and student and school rankings from the centralized school assignment mechanism.

[1] See the country profiles compiled by members of the Matching-in-Practice Network at http://www.matching-in-practice.eu/ for an overview.

Box 1. ***Students' preferences*** *over schools are assumed to consist of two components: a vertical preference component V_s that depends on, for instance, the student composition of school s and a horizontal component H_{is}, which measures the preference for nearby schools – either in terms of geographical distance or socio-economic distance. Formally, the student utility function can be written as:*

$$U_{is} = g(V_s, H_{is}) \tag{1}$$

where $g(.)$ is a function of the vertical preference component V_s and of H_{is}, the distance of student i to school s. V_s itself might be a function depending on variables from which parents try to infer the school quality and on variables measuring a school's peer environment, e.g. the average test score of students at school s or the ethnic composition at school s.

Priority rankings *of schools over applicants are based on a weighted index V_{si}. The index for a specific student i at school s depends on the student's grades at primary school G_i, a school-specific entrance exam score E_{si}, and on an interview component T_{si}. The index V_{si} can be seen as a school-specific function $w_s(.)$ of the previously mentioned arguments.*

$$V_{si} = w_s(G_i, E_{si}, T_{si}) \tag{2}$$

Students with better grades, better test scores, and a better performance in the interviews are granted ceteris paribus higher priority at a school.

The empirical analysis needs to address two challenges. First, to *estimate students' preferences*, we need to consider that even under the deferred acceptance algorithm used in Hungary, it is only a weakly dominant strategy for students to report truthful rank-order lists (ROLs) over schools. That is, it remains rational for students to submit strategic ROLs by either (i) omitting schools they deem unattainable or (ii) truncating their ROLs if they are confident to be assigned to more preferred schools.[2] To recover the true preferences, our empirical strategy builds on two plausible identifying assumptions, namely, that (i) the matching is stable[3] and (ii) students play only undominated strategies.[4] Under these assumptions, Fack et al. (2019) show how preferences can be estimated with discrete choice models and personalized (ex-post feasible) choice sets, when each student's priority at each school is observed. However, in Hungary, and most places worldwide, schools only report priorities over the set of students who actually apply to them. A second challenge thus is the *estimation of students' priorities* when students' choice sets are

[2] Both types of omissions have been consistently observed in the field (Chen & Kesten 2019) and in the lab (Castillo & Dianat 2016).

[3] Stability implies that a student's assigned school must be her top choice among her ex-post feasible schools.

[4] I.e., a school is ranked above another one if the former is preferred to the latter.

latent. In our empirical strategy, we therefore endogenize the choice sets and impose stability and strategy bounds on the latent match valuations. This method is implemented in a Bayesian estimator with a Gibbs sampler that allows us to obtain point-identified parameter estimates. The approach generalizes estimators previously proposed for the marriage market (Logan et al. 2008, Menzel & Salz 2013), from a one-to-one matching to a many-to-one matching setting, which is suitable for the school admissions problem studied in this project.

In a second step, we investigate whether segregation (through school assignment) affects student achievement and exacerbates inequality. This requires the estimation of the valued-added of attending a certain school type.

Box 2. *The* ***valued-added*** *of attending a certain school type can be seen as a combination of different effects. Formally, this can be modelled as:*

$$H_{it} = f(H_{i,t-1}, PQ_t, TQ_t, SQ_t) \tag{3}$$

where H_{it} is the human capital accumulated at time t and $f(.)$ is an increasing function in each of its argument. PQ_t, TQ_t, and SQ_t are the quality of peers, teachers and school, respectively.

We are interested in estimating the contribution of the combination of the inputs on student achievement in Box 2. The major challenge associated when estimating their contributions (or valued-added) is to properly account for the non-random sorting of students to certain types of schools according to their ability. As preferences and rankings are non-randomly formed, segregation might occur. Ignoring student sorting might thus lead to an overestimation of the value-added of a certain school type if primarily highly able students are selected into this school type.

As a starting point, we investigate the causal effect of attending different school tracks. We do this using recent methodological advances by Abdulkadiroğlu et al. (2019) to explicitly exploit the centralized school assignment mechanism currently used in Hungary. Abdulkadiroğlu et al. (2019) show how to combine an earlier concept they refer to as DA propensity score (Abdulkadiroğlu et al. 2017) with a regression discontinuity type approach to convincingly estimate the effects schools have on their students. The basic idea is that in a centralized school assignment system conditioning on a coarse function of student preferences and priorities completely eliminates any omitted variable bias resulting from the correlation of student preferences and school assignment. In addition, restricting the analysis to applicants near admission cutoffs corrects for selection bias that is a result of better students being more likely to be admitted to more selective schools.

In a third step, we bring together the analysis in the previous two steps. The analysis in step 1 allows us to construct counterfactual allocations of students to schools under different timing regimes of ability tracking, i.e. the assignment of children to different school types after 4th, 6th or 8th grade. These counterfactual allocations are obtained by feeding the estimated preferences and priorities into the deferred acceptance assignment mechanism.

Box 3. ***Assignment of students to schools*** *is based on three inputs: student preferences over schools, the rankings of schools over the student applicants, and the school's capacity constraints. The centralized assignment mechanism* μ *is conducted based on student preferences and school rankings over students, taking capacity constraints into account. The mechanism assigns each student* $i \in I$ *to a school* $s \in S$*, formally* $\mu : I \longmapsto S$.

This counterfactual analysis allows us to draw conclusions on whether different tracking regimes aggravate segregation and social inequality in Hungary. Together with the causal models on learning outcomes from step 2, we can then predict counterfactual educational outcomes and evaluate different school assignment rules.

Our preliminary results indicate that applying the approaches in steps 1 and 2 in the Hungarian setting is a promising avenue for future research. On step 1, Monte-Carlo evidence presented in this paper confirms that our estimator allows us to obtain point-identified preference paramters. On step 2, we demonstrate that the empirical approach in Abdulkadiroğlu et al. (2019) can be used to investigate the causal effects of attending a grammar school, the highest school track in Hungary. Even with just a single wave of students, we are able to obtain reasonably precise estimates which suggest that grammar schools in Hungary increase student achievement as measured by standardized test scores at the end of grade 10 by between 9 to 11 percent of a standard deviation. Importantly, estimates from commonly applied valued-added models are twice as large as these effects, thus demonstrating the importance of correctly accounting for selection.

This report is structured as follows. Section 2 reviews the related literature. Section 3 describes the Hungarian school system and the current secondary school assignment mechanism. Section 4 presents the Hungarian data sources available for our analysis and the outcome measures that can be used to estimate the value-added of a certain school type. Section 5 discusses different econometric strategies to use the information from rank order lists to estimate students preferences and presents Monte-Carlo evidence. Section 6 introduces some theory that is necessary to understand how centralized school assignment can be used to estimate causal effects of school valued-added; discusses the estimation strategy, and tests the feasibility of this estimation strategy for the Hungarian case with some preliminary results. Section 8 concludes with a research outlook.

2 Literature

2.1 Preference Estimation

The following two sections review the related literature first on preference estimation in school markets and second the estimation of school value added effects. The first strand of the literature to which we contribute to is the estimation of students' preferences and schools' priorities from observed data. The approach is developed and implemented in a companion paper (Aue et al. 2020). There are several methods for preference estimation with more or less restrictive underlying assumptions. The most common identifying assumption is truth-telling, where under the SOSM, a student is truth-telling if she submits her k most preferred schools. Abdulkadiroğlu et al. (2017) and Che & Tercieux (2019), for example, follow this assumption in their analysis of the New York City high school match. However, truth-telling is only a weakly dominant strategy, even when schools can be listed at no cost. Commonly observed and rationalizable strategies that are inconsistent with truth-telling include skipping "infeasible" schools and truncating ROLs after "safe" schools. A less restrictive identifying assumption that we follow in Aue et al. (2020) is stability, which implies that a student's assigned school must be her top choice among her ex-post feasible schools. Asymptotic stability is guaranteed ex-post under fairly general conditions (Fack et al. 2019).

The most pervasive problem for inference in two-sided matching models is that stable matching games may possess multiple equilibria for a given set of preferences. This constitutes a problem because standard empirical models do not specify the equilibrium selection rule. A model specifying stability is therefore incomplete (Tamer 2003). For the most part, the literature has therefore focussed on complete models (see Fox 2009, Chiappori & Salanié 2016, for surveys of the literature). One means to ensure uniqueness of the stable matching (and therefore completeness of the model) is to impose assumptions restricting the form of utility functions. This approach is followed by Sørensen (2007), who assumes preferences on both sides of the market to be pairwise aligned in the two-sided matching of firms and venture capitalists, Chen (2013), who assumes a global ROL on one side of the market for corporate loans (which is a special case of pairwise alignment), and Agarwal (2015), who restricts programmes' preferences for medical residents to be homogeneous in the US medical match. Another means to ensure uniqueness is to focus on contexts where it is given by the design of the admissions process. In the context of school choice and college admissions, several papers exploit the score-based admissions priorities used by educational authorities, which guarantee a global ROL and a unique stable matching. For school choice, this has been applied for Paris (Fack et al. 2019), for college admissions in Mexico (Bucarey 2018), Turkey (Akyol & Krishna 2017) and Norway (Kirkebøen 2012).

In Aue et al. (2020), we deviate from the literature in that we estimate incomplete models. We do this exploiting properties regarding the uniqueness of a stable matching in either large or imbalanced markets (Azevedo & Leshno 2016, Ashlagi et al. 2017). We implement this, using different identification assumptions, ranging from weak truth-telling on one side to stability on the other. We also consider the argument in Artemov et al. (2017) that stability is a more innocuous assumption than undominated strategies in that it permits inconsequential ‘mistakes’ (in the sense of playing dominated strategies). In the analysis, stability is therefore used both with and without the additional assumption of undominated strategies. We write the likelihood for students’ preferences and schools’ priorities and estimate them jointly in a Bayesian estimator using a Gibbs sampler.[5] We find that incomplete models perform significantly better than reduced form estimates and operate on less restrictive assumptions and data requirements than complete models. They perform best for large matching markets and can be parallelized to speed up parameter convergence.

2.2 School Value-Added

One focus in the value-added literature is, whether attending highly-selective schools or high-quality schools has a positive impact on students’ learning outcomes. So far, however, the literature is rather inconclusive about the effect. Clark (2010) estimates the effect of attending a selective high-school (grammar school) in the UK on academic achievement exploiting variation in the probability of attending a selective high-school (fuzzy RDD) induced by the assignment mechanism that is primarily based on an assignment test score. The study finds only small test-score effects, but large impacts on course-taking and university enrollment. Dobbie & Fryer (2011) estimate the reduced form effect of getting an offer from one of the three most prominent New York public exam high schools on academic attainment and achievement exploiting admission test score cutoffs (sharp RDD). They find that getting an offer has little impact on test-scores, college enrollment and college graduation, but increases the rigor of high school courses taken and the probability for a graduation with an advanced high school degree. Akyol & Krishna (2017) find that highly-selective Turkish high schools do not have a higher average value-added (as measured by the expected difference in students’ college and high school entrance exam test scores corrected for mean-reversion bias) using simulation based methods. In the analysis, they exploit detailed information on the assignment mechanism of students to high schools which is based on the student-proposing deferred acceptance algorithm. The selectivity of a school is measured in terms of the admission cutoff test score. Abdulkadiroğlu et al. (2014) make an important methodological contribution. In their paper, they show how to craft a sharp regression discontinuity design from a sophisticated assignment mechanism, the deferred acceptance assignment mechanism, in order to estimate

[5] In the case of a one-sided market, where schools are non-strategic, a two-step approach to this problem could be derived from He & Magnac (n.d.). First, estimate school priorities for all students using schools’ observed ranking over applicants. Second, use the estimated priorities to construct personalized choice (or consideration) sets and apply the estimation strategy in Fack et al. (2019). This approach is less feasible in the two-sided market analysed in this paper.

the reduced form impact of receiving an admission offer from a certain school.

Another branch of the literature introduces some school quality measures in order to estimate the effect of attending a high-quality type school on outcomes of interest. Jackson (2010) introduces the average peer test scores at a school as quality measure for that school and investigates the impact of attending a school with higher-achieving peers in Trinidad and Tobago on academic outcomes by an IV approach that exploits discontinuities created by the assignment mechanism for secondary schools. The main finding is that attending a high-quality school seems to improve student's academic test score outcomes. Pop-Eleches & Urquiola (2013) estimate the impact of having access to secondary schools of higher quality (here: higher admission cutoffs) in Romania on academic achievement and behavioral responses exploiting a sharp RDD induced by the assignment mechanism. They find positive effects of having access to high-quality schools on students' graduation tests, negative effects on students' self-esteem and negative effects on the effort of parents to educate their children.

Several studies also exploit assignment mechanisms which include randomized lotteries to estimate the effect of winning a lottery on student outcomes. Cullen et al. (2006) exploit randomized lotteries, which determine high school admission in Chicago Public Schools for oversubscribed schools, to compare students who win lotteries, and are thus assumed to attend higher-quality schools, to students who loose lotteries. They find no evidence that lottery winners benefit across a wide range of traditional academic measures, however, there is evidence for benefits on some nontraditional measures such as self-reported disciplinary incidents and arrest rates. Abdulkadiroğlu et al. (2017) propose an econometric strategy to efficiently exploit lottery tie-breakings in centralized assignment mechanisms. The proposed method rests upon propensity-score-based stratification, where the propensity score is computed using information on preferences, priorities, and school capacities.

3 Assignment to Secondary Education in Hungary

Students apply for secondary education during their last year of primary school. Generally, secondary education in Hungary starts in grade 9. Students can decide between three different secondary school tracks: vocational training schools, vocational grammar schools, and grammar schools. Grammar schools are academically-oriented. After having passed a maturity exam (secondary school diploma), graduates from grammar schools can enter higher education. At the other end of the spectrum are vocational training schools. students in these schools acquire a vocational qualification, which does not enable them to enter higher education. The vocational grammar schools is a mixture of the grammar and vocational training school. Individuals passing the maturity exam at a vocational grammar school are also allowed to enter higher education.

Students applying for secondary schools apply for school-course combinations. Since

2000 each student is allowed to submit a strict preference ranking of arbitrary length and apply for any school-course combination nationwide. There are centralised entrance exams and popular schools often also organise special entrance exams or interviews for each course program they offer. Schools rank their applicants according to a weighted average of primary school grades, entrance exam scores, and an interview score. The weights are determined by the schools, but have to satisfy certain conditions. Given student and school rankings the final assignment to a secondary school-course is organised at the national level via a centralised matching scheme. The final matching is performed by a software using the student-proposing deferred acceptance algorithm, which works as (Abdulkadiroğlu & Sönmez 2003):

> *Step 1:* Each student proposes to her first choice. Each school tentatively assigns its seats to its proposers one at a time following their priority order. Any remaining proposers are rejected.
>
> In general, at
>
> *Step k:* Each student who was rejected in the previous step proposes to her next choice. Each school considers the students it has been holding together with its new proposers and tentatively assigns its seats to these students one at a time following their priority order. Any remaining proposers are rejected. The algorithm terminates when no student proposal is rejected and each student is assigned her final tentative assignment.

The Hungarian mechanism does not use a tie-breaker. Schools are required to generate strict rankings of students. After obtaining the results of the centralized assignment mechanism, schools decide how to form classes. An extra matching round is conducted for unmatched students and unfilled courses, which is organized at the school level.

4 Data

We will use a combination of administrative and survey data for this study. First, the so-called *KIFIR*[6] data contain outcomes of the national centralized matching scheme in Hungary. We currently have access to the outcomes for the year 2015 which covers the universe of 8th grade students in Hungary applying for secondary education in 2015. The data contain information on the students' rankings over school-course combinations, the school's ranking over the students, and final matches. Overall, there are 88,401 students who apply to 6,181 different school-course combinations offered by 1,035 schools. The

[6]The abbreviation KIFIR can be translated into English as "secondary enrollment information system".

average number of schools listed by each students is 4.47 and 94.4% of students are matched to a secondary school in the first matching round.

The second data source is the *NABC*[7] data which contain results of standardized tests taken at the end of grades 6, 8, and 10 by all students in Hungary. Currently, we have access to the grades 8-10 version for the years 2015 and 2017, respectively. The data include raw scores for math and reading test scores. In addition, they contain information from student surveys, which were answered on a voluntary basis with relatively high response rates (approximately 80%). The survey component yields information regarding the students' previous school career (e.g. past GPAs (overall and by subject), number of classes repeated), the students' demographics and family background (e.g. parental education, parental occupation and employment status, family composition), the students' career aspirations and subject preferences, and the students' out-of-school activities. Furthermore, the data include information from school questionnaires that provide, amongst others, information on the school maintainer, school site, and the school size.

The KIFIR and NABC data can be merged via unique student identifiers. Test scores from the NABC 2015 data set are measured at grade 8 in primary school and thus provide us with *pre-assignment information* on student academic achievement Test scores from the NABC 2017 data set are measured at grade 10 in secondary school and thus provide us with *post-assignment information* on student academic achievement However, not all students contained in the KIFIR 2015 data set can be linked to the NABC data sets. This is mainly due to two reasons. First, schools provided on a voluntary basis a student identifier which can link the two data sources. Second, 5.33% of the students in the KIFIR 2015 data set were matched to so-called early-selective grammar schools which start at grade 5 or 7, for which we do not have the adequate NABC data. In total, we have information on pre-assignment and post-assignment academic achievement for 54,013 students who applied for secondary education in 2015.

The third data source which is available for our analysis is the KIRSTAT data set[8] which contains administrative information on school/school-site characteristics. For instance, it contains information on the number of students and the student distribution by gender and programme, the number of teachers and the teacher qualification, as well as extensive information on the within-school infrastructure (e.g. number of classrooms, number of computers, number of gyms).

5 Modelling Student Preferences and School Priorities

The main goal of our analysis is to estimate the value-added of grammar schools in comparison to vocational grammar schools in Hungary. School tracking represents a highly controversial policy which is often assumed to aggravate social inequality and increase

[7]NABC stands for National Assessment of Basic Competencies.

[8]The abbreviation KIRSTAT can be translated into English as "public education information system".

inequality of opportunity (Brunello & Checchi 2007). Therefore, before turning to our main analysis, it is of importance to investigate and understand the major determinants of how students form preferences over the schools and how schools form priority rankings over the students. This analysis is not necessary for the value-added estimation, but sheds some light on whether students with comparably low opportunities are more likely to indicate preferences for less prestigious school tracks and whether schools from more prestigious tracks are more likely to prefer students from socially more advantaged backgrounds. In addition, the modelling of preferences and priorities might allow us to evaluate changes in the assignment mechanism. Finally, the value-added estimation results will allow researchers to draw some initial conclusions on whether school tracking in Hungary increases social inequality or not.

In the school choice setting students can form preferences and choose over a finite set of distinct alternatives. A common approach to estimate preference parameters for discrete choices is to fit a structural model using a multinomial logit estimation. The resulting estimated coefficients on certain product features (here school features) can be used in a counterfactual simulation to evaluate hypothetical policy or supply changes. In the school choice setting these simulations form the basis to evaluate a change of school rules or the assignment mechanism (see for example Kessel & Olme (2018)).

Despite our data being remarkably detailed, we need to overcome two technical problems. The first one is about estimating students' preferences: although in the deferred-acceptance algorithm, it is a weakly dominant strategy for students to report their complete rank-order lists (ROLs) of schools truthfully, stated ROLs may differ from the real ones because students submit strategic ROLs by either omitting schools which they deem unattainable or by truncating their ROLs if they are confident to be assigned to more preferred schools. Both types of omissions have been consistently observed in the field (Chen & Kesten 2019) and in the lab (Castillo & Dianat 2016); and are particularly important for us because the average student in Budapest ranks only 4 schools, even when they are allowed and encouraged to rank all schools.

A second technical complication is about estimating schools' priorities: Hungarian schools only report priorities over the set of students who actually apply to them and not over the entire set of students. Thus, even though Fack et al. (2019) have recently shown how to estimate students' preferences without assuming truth-telling behavior, we cannot directly apply their discrete choice methods which rely on observing complete schools' priorities over students (for example, when schools' priorities are based on a centralized exam). In Hungary, and many other countries, schools' priorities are based on tests, interviews, and previous grades with weights decided by each school.

5.1 Identification Assumptions

To overcome the technical challenges in preference estimation, our empirical strategy builds on two identifying assumptions. The approach is developed and implemented in a companion paper (Aue et al. 2020). Our first assumption is that the observed assignment is stable, which implies that a student's assigned school must be her top choice among her ex-post feasible schools (and vice versa for schools). We apply a stability-based estimator, similar to Fack et al. (2019) and Akyol & Krishna (2017). In their settings, ex-post feasible choice sets can be constructed because each student's priority at each school is observed. In our setting, students' and schools' choice sets are latent and therefore need to be endogenized to point-identify parameters. The method is implemented in a Bayesian estimator using a Gibbs sampler that imposes stability bounds on the latent match valuations. This approach generalizes the matching estimator, proposed in Logan et al. (2008) and Menzel & Salz (2013) for the marriage market, from a one-to-one matching to a many-to-one matching setting, which is suitable for the school admissions problem studied in this paper.

Our second identification assumption is that students use undominated strategies, i.e. a school is ranked above another one if the former is preferred to the latter. The submitted ROLs then reveal the true partial preference order of students over schools (Haeringer & Klijn 2009). A methodological contribution of this paper is to refine the stability-based estimator with by including this partially observed preference ordering. Our method is tested in Monte-Carlo simulations, which show that both of our identifying assumptions yield unbiased estimators for students' preferences. In summary, we develop an estimator that builds on existing methods but allows for point-identification in more general school choice contexts and is readily implemented in the open source statistical package R *matchingMarkets* (Klein 2019).

5.2 Monte-Carlo Evidence

In this section, we compare estimation approaches that are based on different identifying assumptions as laid out above, and we show that a combination of stability and undominated strategies allows us to point identify students' preference parameters and schools' priority parameters. The data generating process of our Monte Carlo study is borrowed from Fack et al. (2019), but with slight adjustments. We consider markets with $T \in \{100, 200, 500\}$ students and six schools with a total capacity of $0.95 \cdot I$ seats. Students' utility over schools is given by

$$U_t(s) = \delta_s - d_{ts} + \gamma(a_t \cdot \bar{a}_s) + \epsilon_{ts}$$

where δ_s is a school fixed effect, d_{ts} is the distance from student t to school s, a_t is the students' grade and $\bar{a}_s$ is the average grade of all students at school s (or put differently, the schools' academic quality). ϵ_{ts} follows a standard normal distribution. For exposition purposes, we assume that δ_s is known to the econometrician and therefore enters the estimation as an additional co-variate. The school's valuation over students (which translates into the student's priority at that school) is given by

$$V_s(t) = a_t + \eta_{st}$$

where η_{is} is also standard normally distributed. In the market, students choose their optimal application portfolio, given their (equilibrium) beliefs about admission probabilities, and a small application cost. For details, see the online appendix to Fack et al. (2019). Our major departure from their approach is the assumption that a student's relative ranking at a school is unknown to the econometrician. Instead, the econometrician observes the relative rankings of students who applied at school s. Further, normally distributed errors are used on both sides of the market.
We simulated $M = 100$ independent realizations of these markets, and for every sample m, we estimated students' preferences over schools, and schools' priorities over students using the following different sets of identifying assumptions:

1. benchmark: rank ordered probit for student and college preferences, based on true ROLs
2. weak truth-telling
3. stability
4. undominated strategies
5. stability + undominated strategies

Figure 1 shows the distribution of our estimation errors for the above methods and across one hundred different data sets, and Tables 1 and 2 show the corresponding mean squared error and bias statistics. As expected, the benchmark case where the complete ROLs are known on both sides allows us to identify the parameters very precisely. Further, the estimates for student preferences that are derived under the assumption of weak truth-telling are biased. This too is to be expected because the assumption of weak truth-telling does not hold in the data generating process, as students may skip schools where their chances of admission are small. Interestingly though, estimates for the schools' priority function are very good. When the estimation is conducted using only the stability assumption, the results are very noisy, and also biased. The previous literature on the estimation of preferences in two-sided matching markets has reached a consensus that the preference parameters are only identified under certain assumptions on the observable characteristics (Weldon 2016, pp.158-168) or certain preference structures such as perfectly aligned preferences (Agarwal & Diamond 2014), and may not be identified at all in other circumstances. The estimates that are derived under undominated strategies are much more precise, but also appear to suffer from a bias. Finally, when we combine stability and undominated strategies, our estimates are virtually indistinguishable from the estimates that are derived using the true and complete ROLs.

Figure 1: Distributions of estimation errors in 100 Monte Carlo simulations

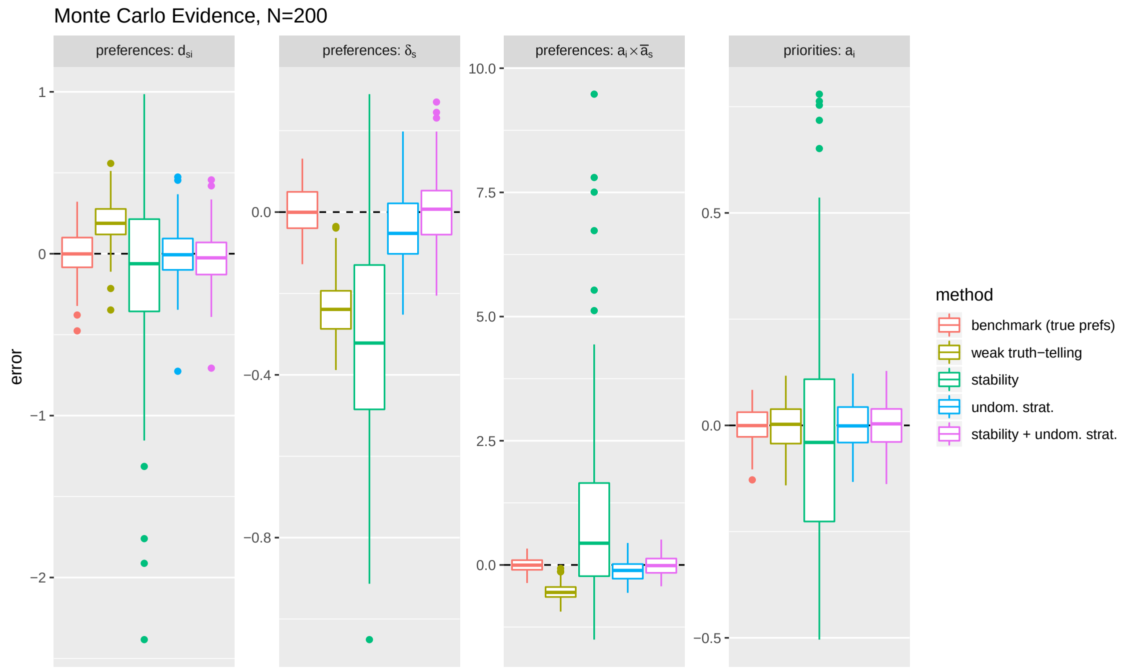

Table 1: Mean squared error (MSE) for various estimation methods

method	preferences d_{is}	δ_s	$a_i \cdot \bar{a}_s$	priorities a_i
benchmark (true prefs)	0.0186	0.0039	0.0225	0.0018
weak truth–telling	0.0598	0.0581	0.3243	0.0032
stability	0.2903	0.1597	4.8612	0.0788
undominated strategies	0.0338	0.0103	0.0539	0.0030
stability + undom. strat.	0.0323	0.0088	0.0448	0.0030

Table 2: Bias statistics for various estimation methods

method	preferences d_{is}	δ_s	$a_i \cdot \bar{a}_s$	priorities a_i
benchmark (true prefs)	-0.0071	0.0025	0.0023	-0.0013
weak truth–telling	0.1937	-0.2302	-0.5425	0.0004
stability	-0.1273	-0.3132	0.9949	-0.0204
undominated strategies	0.0055	-0.0421	-0.1179	0.0001
stability + undom. strat.	-0.0219	0.0134	-0.0183	0.0026

6 Exploiting Centralized Assignment to Estimate School Track Effects

6.1 Estimating Causal Effects despite Non-Random Sorting

The fundamental problem in estimating the causal effects of attending a certain school, school-program, or school-track is the non-random sorting of students to schools. If some schools attracts better students, higher tests scores at these schools cannot simply be attributed to a school's quality in a value-added sense. To overcome this issue of student sorting, we plan to apply recent methodological advances by Abdulkadiroğlu et al. (2019) that exploit the structure of centralized assignment mechanisms to obtain causal estimates of attending a certain school. The main idea is that under centralized assignment, offers at specific schools are determined solely by student preferences and the ranking of students at schools. These variables are therefore the only two confounding factors that need to be taken into account in causal analysis. The method by Abdulkadiroğlu et al. (2019) eliminates these two sources of bias in two steps: First, to control for bias induced by students' preferences, they propose to control for a scalar function of student preferences, the *theoretical local propensity score*. Compared to full conditioning on student preferences, which is often impractical because the number of student types in terms of preferences is large, this propensity is much coarser and hence yields substantial efficiency gains. Second, eliminating bias from the correlation between potential outcomes of a student and his or her rank position at schools, should be achieved by only comparing applicants in a narrow window around school specific cutoffs, similar to a regression-discontinuity design. A description of the underlying theory and the resulting estimation strategy are discussed next.

6.2 The Theoretical Local Propensity Score

To explain the method in more detail, we first need to introduce some notation and adapt the fairly general results from Abdulkadiroğlu et al. (2019) to the school-assignment mechanism in Hungary. Let $s = 1, ..., S$ index schools to which applicants can apply, where $s = 0$ denotes an outside option. I denotes the set of applicants with i indexing individual applicants. The theoretical results of Abdulkadiroğlu et al. (2019) are based on the assumption of a large-market model with a unit continuum of applicants.[9]

The *student-proposing DA* assignment mechanism employed in Hungary takes into account three components: student preferences, school rankings of applicants, and capacity constraints. Capacity constraints for schools are collected in the vector $q = (q_0, q_1, ..., q_S)$, where q_s denotes the proportion of students in I that can be seated at school s. We assume

[9]Abdulkadiroğlu et al. (2019) verbally formalizes a unit continuum as follows: "The continuum can be interpreted as the limit of a sequence that repeatedly doubles the number of applicants of each type while doubling each school's capacity." (p.8).

that all students can be seated at the outside option school 0, i.e. $q_0 = 1$.

Applicant preferences are denoted by $\succ_i$, where $s \succ_i s'$ implies that applicant i prefers school s to school s'. By θ we denote *applicant type*, defined as the combination of an applicant's preferences for all schools ($\succ_i$). Θ denotes the set of possible types.

Schools rank applicants based on school-specific *tie-breakers*. These are typically derived from earlier grades, test scores, as well as interviews. Let the random variable R_{is} denote applicant i's tie-breaker at schools s, where $R_{is} < R_{js}$ means that schools s prefers applicant i to j. We assume that R_{is} is scaled to be distributed over $[0,1]$ with continuously differentiable cumulative distribution function F_s^i. Importantly, tie-breakers may be correlated with type.

Next, define

$$F_s(r|\theta) = E[F_s^i(r)|\theta_i = \theta] \tag{4}$$

to be the fraction of type θ applicants with a school-s tie-breaker below r.

The DA mechanism results in a set of school-specific *randomization cutoffs*, denoted by τ_s, which is the tie-breaker of the last student offered a seat at school s if it ends up full. Otherwise, $\tau_s = 1$. Finite-market cutoffs are typically random since they depend on the realization of tie-breakers. However, the large-market ("continuum") assumption implies constant cutoffs.

Let Θ_s denote the set of applicant types who list s and let

$$B_{\theta s} = \{s' \in S | s' \succ_\theta s\} \text{ for } \theta \in \Theta_s \tag{5}$$

denote the set of schools type θ prefers to s.

Abdulkadiroğlu et al. (2019) introduce the important concept of *local qualification risk*, which refers to the probability that applicants clear τ_s at a specific school s without regard to eventual school assignment. We divide applicants to a particular school into three distinct qualification risk groups by partitioning the support of tie-breaker R_{is} into intervals around τ_s. Given bandwidth δ, these intervals are defined by

$$t_{is}(\delta) = \begin{cases} n & \text{if } R_{is} > \tau_s + \delta \\ a & \text{if } R_{is} \leq \tau_s - \delta \\ c & \text{if } R_{is} \in (\tau_s - \delta, \tau_s + \delta] \end{cases} \tag{6}$$

where n, a, and c stand for never seated, *always seated*, and *conditionally seated*, respectively. We say that never seated applicants face no qualification risk at school s. That is, they would never receive an offer from school s because their tie-breaker is well above school-s's cutoff. Always seated students are applicants with tie-breakers well-below τ_s who have qualification risk of one. They always receive an offer unless they are matched to schools they prefer to s. We will refer to the last group of conditionally seated applicants, who face non-degenerate qualification risk at s, as *marginal* at s. Importantly, in the limit, students marginal at a specific school face qualification risk of 0.5 as δ approaches zero.

These classifiers are collected in the vector

$$T_i(\delta) = [t_{i1}(\delta), \ldots, t_{is}(\delta), \ldots, t_{iS}(\delta)]'.$$

Let $D_i(s)$ indicate an offer from school s. We can now define the central concept of the local DA propensity score as a function of type and cutoff proximity:

$$\psi_s(\theta, T) = \lim_{\delta \to 0} E[D_i(s) | \theta_i = \theta, T_i(\delta) = T],$$

for $T = [t_1, \ldots, t_s, \ldots, t_S]' \in \{n, a, c\}^S$. This describes assignment risk for applicants with tie-breaker values above, below, and near cutoffs for any and all schools in the match. We require that all tie-breaker distributions be continuously differentiable at randomization cutoffs and that these cutoffs be distinct:

assumptionassumption1 (a) For every s and for $r = \tau_1, \ldots, \tau_S$, $F_s^i(r|e)$ is continuously differentiable with $F_s^{i'}(r|e) > 0$ given any event e of the form that $\theta_i = \theta$, $R_{is} > r_u$ for $u = 1, \ldots, s-1$, and $T(\delta) = T$. (b) $\tau_s \neq \tau'_s$, for any schools $s \neq s'$ and $\tau' \neq 0$.

Let W_i be any applicant characteristic that is unaffected by school assignment. This includes potential outcomes as well as demographic characteristics. Using this set-up, Abdulkadiroğlu et al. (2019) derive the following compact and useful characterization of local assignment risk in continuum DA with general tie-breaking:

theoremtheorem1 (Local Score with General Tie-Breaking) Consider continuum DA with school-specific tie-breakers, distributed according to $F_s(r|\theta)$, and suppose that Assumption **??** holds. Let W_i be any predetermined applicant characteristic. For all $s \in S$, $\theta \in \Theta_s$, $T = [t_1, \ldots, t_s, \ldots, t_S]' \in \{n, a, c\}^S$, and w, we have

$$\lim_{\delta \to 0} E[D_i(s) | \theta_i = \theta, T_i(\delta) = T, W_i = w] = \psi_s(\theta, T),$$

where $\psi_s(\theta, T) = 0$ if (a) $t_s = n$; or (b) $t_b = a$ for some $b \in B_{\theta s}$. Otherwise,

$$\psi_s(\theta, T) = \begin{cases} 0.5^{m_s(\theta,T)} & \text{if } t_s = a \\ 0.5^{1+m_s(\theta,T)} & \text{if } t_s = c. \end{cases} \tag{7}$$

where $m_s(\theta, T) = |\{s' : t_{s'} = c \text{ for some } s' \in B_{\theta s}\}|$.

As can be seen, the local DA score for type θ applicants is determined by the schools θ prefers to s. Relevant schools are those at which applicants to s are marginal. That is, with tie-breakers in a δ-neighborhood of the school-specific cutoffs. The variable $m_s(\theta, T)$ counts the number of preferred schools where this is the case.

Theorem 1 distinguishes three cases. It starts with the simplest case where qualification risk at school s is zero. Zero qualification risk applies to all applicants to s who either never qualify ($t_s = n$) or are sure to qualify for a more preferred school ($t_b = a$ for some $b \in B_{\theta s}$).

In the other two cases, applicants are not to sure to improve on s. In this case, qualification risk at any more preferred school is 0.5 for marginal students and zero otherwise. Hence, for always seated applicants at s, the probability to be seated at s is given by the conditional probability of failing to qualify for more preferred schools, which is $0.5^{m_s(\theta,T)}$. Those marginal at s need to also qualify at s, which explains the addition of 1 to the exponent in the second line of equation (7).

Theorem 1 yields the crucial conditional independence relation:

$$\lim_{\delta \to 0} P[D_i(A) = 1 | \theta_i = \theta, T_i(\delta) = T, W_i = w, \psi_s(\theta, T) = p] = p \tag{8}$$

for $p \in [0,1]$. In other words, fixing on $\psi_s(\theta, T)$, DA-generated offers are independent of type and any W_i that is unaffected by treatment. Local conditional independence allows to eliminate any omitted variable bias by conditioning on $\psi_s(\theta, T)$. Importantly, $\psi_s(\theta, T)$ is typically far coarser than the underlying type distribution.

6.3 Estimating the Local Score

In the following section we describe how the theoretical results above can be used to obtain consistent estimates of the causal effect of attending a grammar school in Hungary. The basic idea is to estimate the theoretical local DA score described in Theorem 1 by its empirical counterpart and use local linear regression to minimize potential bias from insufficiently narrow bandwidths around randomization cutoffs.

First, we re-scale tie-breakers R_{is} to lie in $(0,1]$ using the following transformation

$$\frac{R_{is} - min_j R_{js} + 1}{max_j R_{js} - min_j R_{js} + 1}$$

Next, an empirical bandwidth δ_N has to be chosen. Given δ_N, $t_{is}(\delta_N)$ can be determined for each i and s. This is used to compute

$$\hat{m}_s(\theta, T) = |\{s' : t_{s'} = c \text{ for some } s' \in B_{\theta s}\}|. \tag{9}$$

In applications empirical bandwidths should be determined separately for each cutoff.

Next, the propensity score estimator is constructed by plugging these ingredients into the formula in Theorem 1. If $t_{is}(\delta_N) = n$ or $t_{is}(\delta_N) = a$ for some $s' \in B_{\theta s}$, then

$$\hat{\psi}_s(\theta, T; \delta_N) = 0 \tag{10}$$

Otherwise,

$$\hat{\psi}_s(\theta, T; \delta_N) = \begin{cases} 0.5^{\hat{m}_s(\theta,T)} & \text{if } t_{is}(\delta_N) = a \\ 0.5^{1+\hat{m}_s(\theta,T)} & \text{if } t_{is}(\delta_N) = c. \end{cases} \tag{11}$$

Note that τ_s and $\hat{m}_s(\theta, T)$ in this expression are sample quantities. Abdulkadiroğlu et al. (2019) show that the resulting estimator is consistent under certain regularity conditions.

So far, we have only discussed how to compute local propensity scores for individual schools. However, we are interested in estimating causal effects of attending a certain school type, namely grammar school. Since any match yields a single offer, this can be done by simply summing up the local propensity scores for all grammar schools (Abdulkadiroğlu et al. 2019).

Not all students end up attending the school they were matched with. Therefore, we will instrument grammar school attendance by an offer dummy D_i that indicates any grammar school offer. Let grammar school attendance be represented by the dummy C_i, then, we can write the 2SLS system as follows:

$$
\begin{aligned}
Y_i &= \beta C_i + \sum_x \alpha_1(x) d_i(x) + g(\mathscr{R}_i) + \eta_i \\
C_i &= \gamma D_i + \sum_x \alpha_2(x) d_i(x) + h(\mathscr{R}_i) + \nu_i,
\end{aligned}
\tag{12}
$$

where β is the causal effect of interest, $d_i(x)$ is a dummy indicating whether the local propensity score estimate is equal to x, and

$$
h(\mathscr{R}_i) = \sum_{s \in S} w_{1s} a_{is} + \kappa_{is}[w_{2s} + w_{3s}(R_{is} - \tau_s) + w_{4s}(R_{is} - \tau_s)\mathbb{1}(R_{is} > \tau_s)],
$$

where $R_i \equiv [R_{i1}, ..., R_{iS}]'$ is the vector of tie-breakers, a_{is} indicates whether applicant i applied to program s, and $\kappa_{is} = a_{is} \times \mathbb{1}(\tau_s - \delta_s < R_{is} < \tau_s + \delta_s)$ indicates applicants in a bandwidth of size δ_N around randomization cutoff τ_s. The local linear control function $h(\mathscr{R}_i)$ serves to minimize the approximation error resulting from the fact that typical bandwidths will be insufficiently narrow to drive the propensity score for qualification risk to the theoretical limit of one-half and avoid omitted variable bias from tie-breakers.

7 Preliminary Results

In this section, we present some preliminary results based on the wave of 8th graders who participated in the centralized matching scheme in 2015. We will soon get access to more waves of data which should greatly enhance the sample size.

As a first step, we check whether the theoretical results and estimation techniques presented above can be used to study the effects of different school tracks in the Hungarian data. Most importantly, the limiting nature of Theorem 1 raise the question of whether the conditional independence property (8) also holds in practice. That is, whether the data allow for empirical bandwidths (δ_N) that retain enough observations, while being sufficiently narrow to balance characteristics of students with the same local propensity score but different school track offers.

To check this, we test whether receiving a grammar school offer predicts pre-determined student characteristics conditional on saturated propensity score controls. That is, we estimate (12) using various student characteristics as outcome variable. We restrict the sample to observations with non-degenerate risk of grammar school assignment (i.e. $\sum_s \hat{\psi}(\theta; T) \in (0, 1)$) and, as a starting point, use a bandwidth of 0.4, which is similar to the bandwidth in Abdulkadiroğlu et al. (2019).[10]

Table 3: Statistical Tests for Balance

	All Applicants		Grammar School Applications with General Risk	
	Non-offered mean (1)	Offer gap (2)	Non-offered mean (3)	Offer gap (4)
# Grammar schools listed first	0.089	2.421***	1.317	0.067
		(0.018)		(0.153)
# of Grammar schools listed	0.448	3.148***	2.609	-0.121**
		(0.015)		(0.056)
Sex	0.438	0.147***	0.547	-0.013
		(0.004)		(0.026)
Learning disability	0.123	-0.097***	0.016	0.007
		(0.002)		(0.006)
Math (standardized)	-0.313	0.853***	-0.249	0.059
		(0.007)		(0.054)
Reading (standardized)	-0.353	0.960***	-0.278	0.036
		(0.007)		(0.051)
N		69,906		5,204

Notes: This table reports balance statistics, computed by regressing covariates on dummies indicating a grammar school offer. Estimates in column 4 are from models with saturated grammar school propensity score and running variable controls. The sample is limited to applicants with non-missing baseline test scores. Robust standard errors are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3 reports these results together with raw differences in student characteristics for all applicants with and without a grammar school offer. Columns 1 and 2 show the results for all applicants (including those with degenerate risk). The raw differences highlight the potential for omitted variable bias resulting from student type and tie-breaker in naive comparisons between grammar and non-grammar students; students with an offer from a grammar school have a stronger preference for grammar school, listing on average three grammar schools more than other applicants. Baseline test score gaps of more than 0.85 standard deviations between students receiving a grammar school offer and those who do not also demonstrate the positive selection with respect to tie-breakers.

In columns 3 and 4 we restrict the sample to applicants with non-degenerate risk of

[10] Abdulkadiroğlu et al. (2019) chose the optimal bandwidth suggested by Imbens & Kalyanaraman (2012).

grammar school assignment. Our bandwidth of 0.4 reduces the number of observations from 69,909 students in the full population of applicants to 5,204 with non-degenerate assignment risk. Reassuringly, the results in column 4 show that all differences in covariates by offer status are substantially reduced and mostly insignificant when estimated using (12) and restricted to applicants with a non-degenerate propensity school offer. This supports the empirical validity of the conditional independence property (5), implying that local propensity score conditioning helps to eliminate omitted variable bias in estimations of the effect of school assignment on test scores. These results confirms similar findings by Abdulkadiroğlu et al. (2019) for public high schools in New York City.

Next, we investigate the effect of grammar school attendance on test score gains. As a benchmark, we first report OLS estimates in the second column of Panel B of Table 4. These estimates come from a regression of equation (12) without propensity score controls and instrumenting in a sample that includes all applicants. Since we also include baseline test scores in math and reading, these estimates correspond to traditional value-added estimates. OLS estimates suggest large and significant test score gains due to grammar school attendance around 0.23 standard deviations in math and 0.25 standard deviations in reading.

Table 4: Grammar School 2SLS Estimates

	All Applicants		Grammar School Applications with General Risk	
	Non-offered mean (1)	Offer gap (2)	Non-offered mean (3)	Offer gap (4)
	Panel A: First Stage Estimates			
Enrolled at grammar school			0.173	0.817*** (0.012)
	Panel B: Second Stage Estimates			
Math 10th grade	-0.336	0.228*** (0.006)	-0.309	0.091** (0.039)
Reading 10th grade	-0.377	0.248*** (0.006)	-0.310	0.109** (0.044)
N		54,572		5,204

Notes: This table reports estimates of the effects of grammar school enrollment. OLS estimates are from models that omit propensity score controls and include all students with non-missing test scores. 2SLS estimates are from models with a dummy for grammar school treated as endogenous, limiting the sample to students with grammar school assignment risk. All models include controls for baseline math and reading scores, sex, and learning disability. Estimates in column 4 are from models that include running variable controls. Robust standard errors are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Before turning to the 2SLS estimates, we investigate the effect of receiving a grammar school offer on actual enrollment. Importantly, even with a grammar school offer school enrollment remains probabilistic because not all offers are accepted and some applicants without offers still get a grammar school seat. This can be seen in the fourth column of Panel A in Table 4, which reports the first stage coefficient for receiving an offer on grammar school enrollment computed by estimating the second line of (12). A grammar school offer substantially increases the likelihood of any grammar school enrollment by about 82 percentage points. However, of those not offered a grammar school seat in a match around 17 percent still end up in a grammar school.

Our main 2SLS results are reported in column 4 of Table 4. Similar to the OLS estimates in column 2, the 2SLS estimates suggest substantial test score gains of grammar school attendance. However, the 2SLS estimates are only about half the size of the corresponding OLS estimates, suggesting considerable selection bias in traditional value-added estimates. The estimated test score gain is about 0.09 standard deviations in math and 0.11 in reading. These are significantly different from zero with standard errors of about 0.039 and 0.044, respectively, and represent approximately 10 percent of the achievement gap between regular students and those with a learning disability. According to the new classification by Kraft (2019) these can be considered medium sized educational intervention effect sizes. Importantly, the 2SLS effects have to be interpreted as the local average treatment effect for students with non-degenerate grammar school assignment risk. That is, they do not measure the effect for very good or very poor students who are either sure to receive a grammar school offer or never receive such an offer.

These preliminary results are very encouraging for our future research plans for three reasons. First, they demonstrate that even a relatively wide bandwidth of 0.4 allows local propensity score conditioning to achieve good balancing properties for students with and without a grammar school offer. This supports the validity of this approach and reduces concerns for omitted variable bias in estimating the effect of attending a specific school track. Second, even data for only the 2015 outcomes of the centralized assignment in Hungary are sufficient to estimate reasonably precise effects of attending the grammar school track. Standard errors are small enough to provide statistical power to detect test score gains under one-tenth of a standard deviation. With more waves of data we should be able to increase the precision of our estimates even further. This should allow us to investigate any potential heterogeneity by focusing on specific subgroups, but also to look at moderating factors which could reveal the channels driving the effects. Third, the importance of applying these new methods is illustrated by the fact that commonly estimated valued-added models appear to be substantially biased.

8 Conclusion

This paper outlines how to study the implications of school assignment rules for social segregation in the Hungarian school system, where students are tracked by ability and assigned to schools through a centralized mechanism. Our proposed research agenda is structured into three steps. First, we plan to investigate the different sources of social segregation by investigating the information provided by students and schools for the centralized assignment mechanism. In a second step, we plan to estimate the causal effect of attending a certain school type. Finally, based on the insights from the previous two steps we will try to answer the question of whether the current assignment rule increase social inequality and discuss potential outcomes of alternative rules.

We check the feasibility of our proposed estimation strategies for the estimation of student preferences and priorities at different schools (step 1) and for the identification of the causal effect of attending a certain school type (step 2). Balancing tests for the estimator in step 2 indicate that our estimation strategy can convincingly estimate causal effects with reasonable precision. Furthermore, the preliminary results reported here suggest that attending a grammar school, the highest school track in Hungary, from 8th grade onwards produces substantial achievements gains in test scores on the order of 9 to 11 percent of a standard deviations by the end of tenth grade. Of course, performance on these tests are only one facet of the potential outcomes that are likely to be influenced by schools. Attending a grammar schools might also affect, for example, students' educational aspirations, non-cognitive skills, as well as their knowledge of ideas that are not easily captured by standardized achievement test scores. Our research agenda for future research includes the exploration of these questions.

Another important feature of the Hungarian school system is that some students already switch to highly selective schools after grades 4 and 6. The empirical strategy laid out in this paper should be applicable to study this setting as well. Comparing the effects of this early tracking to that taking place after grade 8 might be particularly informative about whether the timing of tracking matters for social segregation and later SES achievement gaps.

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