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Ahmet Alkan, Sinan Sarpça

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Effects of High-Achieving Peers: Findings from a National High School Assignment System

Abstract

Recent studies of US elite exam schools have yielded the startling conclusion that such schools improve neither educational achievement nor longer-term educational outcomes. Is the same true for exam schools elsewhere? The system in Turkey is ideal for investigating this question. There, students are placed in exam schools based on a high-stakes national examination. Utilizing an exceptional database for Turkey not heretofore available, we conduct regression discontinuity analysis exploiting score discontinuities between more than 200 exam schools. We find that attending more selective exam schools yields large achievement gains and improved university placements for high achieving students.

JEL-Codes: I210, O150.

Keywords: peer effects, value added, test scores, selective schools, student outcomes.

Ahmet Alkan
Sabancı University
Turkey – 34956 Istanbul
alkan@sabanciuniv.edu

Sinan Sarpça
École Normale Supérieure de Lyon
France – 69342 Lyon
sinan.sarpca@ens-lyon.fr

1 Introduction

Much research has been devoted to investigating whether educational outcomes are improved by giving students and parents more choice over schools or by changing how students are grouped together in schools and classrooms. There is little doubt that many students and parents value schools with high-achieving peers. This is manifest in many parts of the world by the intensive study that students devote to preparing for entry examinations to selective schools, and by the investment parents make both of their time and of resources paid for coaching services. Prominent recent studies cast doubt on the benefits of attending such selective schools, suggesting that their outcomes may not improve when students attend schools with high-performing peers.

Documenting causal effects of schools or schoolmates on outcomes is challenging in situations in which students are not assigned to schools randomly. This has led researchers to seek and exploit sources of plausibly exogenous variation in school assignment, including regression discontinuity designs. Our objective in this paper is to shed additional light on the value of attending more selective schools by study of an exceptional dataset that permits us to investigate the effects on educational outcomes of a nationwide system with exam-based high school admissions. We also provide an explanation to account for the persistence over time of a hierarchy of schools even though resources are allocated to equalize educational inputs across schools.

We use national exam and high school placement data from the Turkish education system to investigate whether attending a more selective high school improves university entrance exam performance and university placement. Both the national high school exam and the national university exam are challenging high stakes tests. We find large effects for high-ability students of attending a more selective high school, but not for lower-ability students. Overall, our findings attest to very substantial value added by schools for an important segment of the ability distribution.

Placement to a large and highly sought after set of high schools in Turkey is carried out centrally, through a procedure based on students' scores from a national exam and students' preference orderings over schools. More sought-after schools end up having higher cutoff scores than less sought-after schools, which in turn gives rise to higher average peer ability in the more sought-after schools. And four years later at graduation, the students at the more highly ranked schools do better in the national university entrance exam and they have better university placement outcomes. The higher observed university exam scores from the higher ranked high schools reinforce beliefs from each cohort of applicants to the next that the higher ranked schools are better schools.

The placement system introduces sharp differences in school attributes to a subset of virtually indistinguishable students, namely those with exam scores near admission cutoffs. We exploit such variation across cutoffs to investigate the extent to which better outcomes reflect school effects and not selection. The centralized placement provides us with hundreds of cutoffs from a large number of schools throughout the country. These cutoffs cover a wide range of the exam score distribution and create heterogeneity in treatment differences across schools. The national education system holds curriculum, teacher qualifications, and other resources fixed across the exam schools. Hence, the differences in observed treatments across schools arise almost entirely from differences in peer quality. Using the variation throughout the country in the magnitude of differences in peer quality across more- vs. less-preferred schools on opposite sides of cutoffs, we document some large achievement gains and improved university placements with increases in peer quality. These effects are not detectable at parts of the ability distribution. These observations may help reconcile some seemingly contradictory findings in the literature.

The Turkish education system offers an outstanding opportunity to expand our knowledge of school effects. Although the centralized placements into high schools and universities have been in practice for several decades, research benefiting from either of them is scarce, partly because of reluctance of the state institutions to share the data. Ours is the first paper that uses nationwide student-level high school entrance exam and placement data together with the university entrance exam and placement data from Turkey. These two exams assess the same set of skills and adhere to the same format, providing us with a distinctive and precise measure of student achievement gains throughout high school.

To assess school effects, earlier studies have relied on instrumental variables approaches (e.g., Evans and Schwab, 1995; Sander, 1996; and Neal, 1997), controlled for students' being accepted by similar schools (Dale and Krueger, 2002), and exploited admission lotteries (Cullen, Jacob, and Levitt, 2005, 2006; Hastings, Kane, and Steiger, 2009). More recently, a series of papers have utilized regression discontinuity designs (Ding and Lehrer, 2007; Hoesktra, 2009; Jackson, 2010; Clark, 2010; Pop-Eleches and Urquiola, 2013; Dobbie and Fryer, 2013; Abdulkadiroglu, Angrist, and Pathak, 2014; de Janvry, Dustan, and Sadoulet, 2017; Luflade and Zaiem, 2017; Hoekstra, Mouganie, and Wang, 2018). This research, concisely summarized by de Janvry, Dustan, and Sadoulet (2017), has generally found either no effects or small effects of attending a more highly ranked school. Two studies that find significant effects (in China and Romania) are discussed below.

To place our contribution relative to prior research that investigates school assignment systems, it is useful to enumerate key features of the Turkish system and our data. We study more than 200 highly selective high schools with characteristics ideally suited to assessing the importance of peer effects. In particular:

• All are "exam" schools, with admissions governed by a nationwide, standardized, high-stakes entry examination.

• All of these schools follow a nationwide standardized curriculum.

• Resources are allocated uniformly across schools. This includes a system of teacher rotation designed to assure that no school obtains a disproportionate share of the most able teachers. This rotation system is likely to be particularly effective in equalizing teacher capabilities across schools within regions. Hence, differences in peer ability are the key differentiating factor across schools.

• There are 42 regions, each with two or more exam schools. Students in each region are restricted to attending a school in their region.

• Prior to taking the high school examination, each student lists a preference ordering across the high schools in their region.

• Virtually all students attend their assigned school.

• The treatment period is long, four years of high school.

• A nationwide, standardized, high-stakes university examination is taken by high school students four years after high school entry. Scores on this examination govern assignment across all universities in the country and serve as the key outcome measure in evaluating performance of high schools.

• Each student's score on the high school exam is matched with their score four years later on the university exam.

• A second outcome measure is the peer quality in the academic major to which the student gains admission in the university to which the student gains admission.

In the discussion that follows, our objective is to explain how the structure of the Turkish system coupled with our data enable us to build on several ably executed contributions closest to our endeavor.

Ding and Lehrer (2007) conduct a regression discontinuity analysis to investigate the impact of attending a more selective school in a large county in China. They demonstrate almost complete compliance with school assignments. Using a sharp design, they find significant and large impacts of attending a better school. While their focus on a single county limits the generality of their finding, they are the first (Lee and Lemieux, 2010, Table 5) to estimate causal effects by exploiting the discontinuities in school quality arising in an exam-based school assignment system.

We build on the work of Pop-Eleches and Urquiola (2013) who are the first to use nationwide data from a school assignment system. They obtain data from schools throughout Romania that permit them to conduct a regression discontinuity analysis with a large number of cutoffs. They report that roughly 40% of students in their data attend the highest-achieving school to which they are admitted. In their intent-to-treat analysis, they find that assignment to a school with higher achieving peers results in a significant gain in scores, ranging as high as .1 standard deviation, on the high-stakes national high school graduation exam. We follow them in using nationwide data. Relative strengths of our data and setting are that we have students' reported preferences across schools, and almost all students attend the highest-achieving school for which they are eligible in their reported preference ordering. Hence, we can implement a sharp design. In addition, because of the equalization of resources across schools, the differentiating feature of schools is peer quality. This permits us to take advantage of the magnitude of peer differences across schools in our analytic framework to estimate how the magnitudes of peer differentials impact the magnitudes of treatment effects. We find treatment effects in Turkish schools to be much higher than those found by Pop-Eleches and Urquiola (2013) for Romanian schools. One standard deviation increase in peer quality in high school results in about a .36 standard deviation increase in university exam performance.

Dobbie and Fryer (2013) undertake analysis of exam schools in New York City to investigate impacts on longer term outcomes and conclude "exposure to these higher-achieving and more homogeneous peers has little impact on college enrollment, college graduation, or college quality." We do not have college graduation data, but we find that attending a high school with higher achieving peers results in being admitted to a university academic major within higher achieving peers.

Abdulkadiroglu, Angrist, and Pathak (2014) use data from the three most prominent public exam schools in Boston, including the storied Boston Latin School, and the three most prominent public exam schools in New York City. Among the schools they study, most students attend the highest ranked school to which they are admitted, but a substantial fraction do not. Hence, they utilize a fuzzy regression discontinuity design. In an impressively comprehensive and thorough analysis, with multiple outcome variables, they find no significant elite-school payoff or peer quality effect. In their conclusion, they observe that "Every context is different, and the absence of peer effects in one setting does not prove that such effects are unimportant elsewhere." This is particularly prescient in light of our findings of large and statistically significant effects for exam schools in Turkey. We build also on their contributions, utilizing data from a nationwide system in which assignments to exam schools within every region of the country are based on a standardized nationwide exam. The exam schools in our setting are very highly sought after. Indeed, it would be hard to overstate the intensity of preparation by Turkish students prior to taking the exam. We have 203 school achievement boundaries dividing thousands of students across the exam schools in 42 regions in Turkey. Because of the common nationwide entry exam, the running variable is the same for all schools. In the Turkish system, almost all students attend their assigned school. Hence, we can employ a sharp design to investigate the impact of treatment on outcomes.

In other work related to Turkish Exam Schools, Akyol and Krishna (2017) study value-added by the highly specialized Science High Schools on how well students do on the University Exam. The Science High Schools, in total, constitute less than 1% of high schools by enrollment, and enroll students only from the top 2% of the test score distribution. Their paper develops and estimates a discrete choice model using publicly available school-level statistics (mainly minimum and average entry and exit test scores) and find no school value-added effects. Our paper differs greatly, most importantly, using detailed student level data, and studying a substantially larger number of schools which enroll students from a significantly wider portion of the ability distribution. We consider a broader set of outcomes. Our specific focus is on the effects of peer quality.

Our paper is also related to other research on peer effects in education. Epple and Romano (2011) and Sacerdote (2011) provide two detailed reviews of this huge literature. As they detail, the key challenge is identification of peer effects when the assignment of students to schools is nonrandom, as omitted variables correlated between students who attend the same school may appear like peer effects (See Manski, 1993, and Brock and Durlauf, 2001, for discussions). As summarized above our investigation exploits the variation in peer composition introduced by school assignment to compare outcomes of virtually indistinguishable students who fall on opposite sides of admission thresholds of otherwise standardized schools.

Section 2 describes our set-up. Section 3 presents our findings. Section 4 concludes.

2 Set-up

2.1 Institutional Background

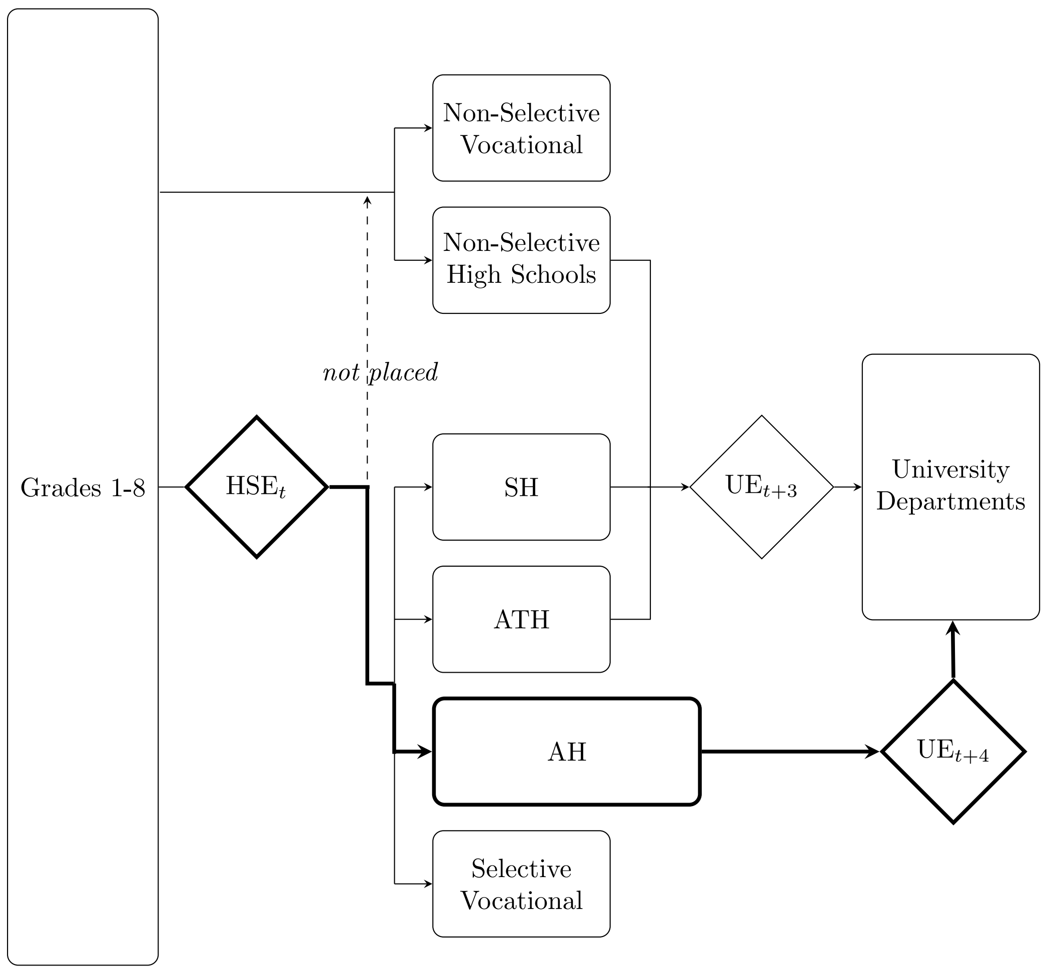
Around 96% of high school students in Turkey are in public schools, financed and run by the Ministry of Education.[1] Most of these are non-selective, welcoming all students from their feeder schools. There are also *exam schools*, which enroll about 19% of high school students. Figure 1 presents a diagram of the education system. The Ministry carries out exam school placement on the basis of students' rankings in the nationwide *High School Entrance Exam* (HSE) and students' preference orderings over exam schools. Some exam schools are vocational and their graduates are mostly precluded from higher education. There are three types of *university-track* (i.e., non-vocational) public exam schools, Anatolian High Schools (AHs), Science High Schools (SHs), and Anatolian Teacher Training High Schools (ATHs), which enroll about 8% of high school students. Table 1 presents some summary information about these schools. AHs are by far the most common, drawing 85% of the total enrollment among these three. AHs constitute the precise domain of our analysis.

The HSE is held on a particular day each spring. Students are given 120 minutes to answer 100 multiple choice questions, 25 in each of 4 subject areas—math, science, social sciences, and Turkish. A penalty is imposed for each wrong answer to discourage guessing. The preference ordering submitted by the average participant (before the exam) consists of 8.5 schools (standard deviation: 3.4, maximum allowed: 12). Among all of the participants, 86% have listed one or more AHs in their preference ordering (SHs: 45%, ATHs: 57%).[2]

[1] The figures and details in this section refer to our years of analysis, 2001 and 2005.

[2] About half of those who do not list an AH list only vocational schools. About 6% do not list an AH but list an ATH or an SH.

Figure 1: A Diagram of the Turkish Education System, 1994-2015



Note: AHs are four year schools. All other high schools are three year schools. Students who are placed in an AH take the UE a year later than the students placed in one of the other schools. *The thicker lines highlight the path we study, which is enabled by the HSE 2001 and UE 2005 data that we have.*

Table 1: University-Track Public Exam Schools

Type of School	Number of Schools	Enrollment Number	Enrollment % test takers	Cutoffs (Percentiles) Mean	Cutoffs (Percentiles) Min	Cutoffs (Percentiles) Max
AH	442	35,269	6.39%	85	21	99
SH	48	3,983	0.72%	99	98	99
ATH	91	5,153	0.93%	95	85	99
Total	581	44,405	8.04%			

Notes: The figures refer to our year of analysis, 2001. AHs: Anatolian High School, SHs: Science High School, ATHs: Anatolian Teacher Training High School. *In addition, 59,348 students (about 10.75% of test takers) are placed in 1,214 vocational (non-academic) public exam schools.*

The alternatives to the university-track public exam schools are either much inferior public schools or very expensive private schools. Not surprisingly, students spend long off-school hours preparing for the HSE, both on their own and in *dershanes* (cram schools) or with private tutors, and placement in public exam schools almost always results in actual enrollment. Students are restricted to include AHs only from their city or town of residence in their preference lists.[3]

The Turkish secondary education system assigns a single curriculum and allocates equal resources to all (public) schools of the same type. The teachers are employed by the Ministry and appointed to (and periodically rotated between) schools under uniform criteria and in a centralized fashion. The Ministry regularly inspects compliance with the curriculum and its delivery. For our purposes, the important point to note is that AHs in a city or town are extremely uniform in their school inputs except for the composition of their students.

Most students then compete for placement in one of the more sought-after university departments. All students seeking higher education in Turkey take, in their last year of high school or after graduation, the *University Entrance Exam* (UE), which, like the HSE, is held once a year. University placement is entirely centralized[4] according to UE scores (and, to a smaller degree, standardized high school grade point averages) and students' preference orderings over university departments. Unit of choice, "department," is a combination of a major and a university, e.g., chemistry, at Istanbul University. Every spring, 1.8 million people (including 600,000 high school seniors and

[3]For localities without an AH, AH(s) in a designated nearby locality. In contrast, SHs and ATHs anywhere can be listed.

[4]Carried out by the Center for Student Selection and Placement of the national Higher Education Board.

repeat takers who graduated in previous years) take the UE. Only 190,000 of them are placed.[5] The UE is also a multiple choice exam, with 180 minutes for 180 questions, equally divided into the same four subject areas as the HSE. The UE requires even more intensive preparation than the HSE, and households across the nation devote precious resources to that end.

These two placement institutions for high school and university, thus have much in common, with similarly structured national exams for determining student rankings and centralized procedures for placing students based on their submitted preference orderings. For university-track students, high school and university placement can be seen as two stages of a single contest, with the HSE and the UE seen as two incarnations of one exam. In that sense, preparation for the HSE can be viewed as initial preparation for the UE. Not surprisingly, UE scores are highly correlated with HSE scores. Students who are placed in more selective high schools generally do better on the UE. For the schools in our analysis, the correlation between school averages of HSE and UE scores is about .78.

2.2 Data Sources

Our two data sources are the administrative data for the HSE in 2001 and for the UE in 2005. These are obtained from the Ministry of Education and the Center for Student Selection and Placement, respectively. The HSE data include name, birthdate, gender, father's name, town of residence, submitted preference orderings for HSE-placement schools, exam score, and eventual placement information (if it is an HSE-placement school) for 568,494 students that took the exam in 2001. We use the information on preference orderings and scores to determine our quasi-experiments and construct their corresponding analysis samples, as described in the next section.

We then link the HSE data to the UE data to obtain the outcomes of interest. (The two files are matched by name-surname, father's name, birthdate, town, and school information.) The UE data include name, birthdate, father's name, preference orderings over university departments, number of correct/incorrect/blank answers for each section of the test, a series of major-specific exam scores, and the university department placed (if any) for the 1,851,618 people who took the UE in 2001, of which 691,893 were high school seniors (first-time test takers).

[5] In addition, about 400,000 are placed in so-called *open-university* departments that offer distance education.

2.3 Centralized Placement and RD Design

Given students' submitted preference orderings over schools, students' rankings according to their HSE scores, and the capacity of each school, the Ministry of Education places students using a *deferred acceptance* procedure (Gale and Shapley, 1962), which works as follows. Schools pick the students with the highest scores among those who listed them as their first choice and reject those exceeding their quota, if any. Schools then repeat this with all of the unassigned students from the previous round who listed them as their second choice *and* the students they picked before, and so forth. The placement is obtained when there are no more rejections and the procedure ends. (Students are not allowed to decline this placement and attend another exam school, even if their scores are adequate for the latter.) The *cutoff* score of a school s, which plays an essential role in our design, is precisely the lowest score of all of the students placed in s. It is worth noting that the cutoff of a school is endogenous and co-determined with all of the other school cutoffs, with its exact value depending on all student scores, preference orderings, and school quotas.

Our strategy in assessing school effects is to focus on students who are virtually indistinguishable but for school assignment. Each cutoff provides us with such a set of students. We interpret the centralized placement as an experiment that introduces variation in school quality. To formalize, consider students indexed by i with exam scores H_i near the cutoff $\underline{H}_s$ of school s. Define an indicator variable IN_i for a given student i's placement status: If the *assignment variable*, $H_i - \underline{H}_s$, is greater than or equal to zero then i is placed in s and $IN_i = 1$. If $H_i - \underline{H}_s < 0$, i is not placed in s and $IN_i = 0$. Suppose then that i is placed in another school s' with a lower cutoff. Let Q_{is} and $Q_{is'}$ denote the qualities of these schools. To estimate the effect of school quality on a subsequent outcome y_i our model is

$$y_i = \eta + \alpha[IN_i Q_{is} + (1 - IN_i)Q_{is'}] + f(H_i) + \epsilon_i, \tag{1}$$

where η is the intercept, $f(.)$ includes controls for H_i, and ϵ_i is the error term. Students whose scores fall near the cutoff lack the ability to choose IN_i and self-select into s or s' (we discuss this in detail in the last paragraph of this section). We use the variation in the magnitude of difference in quality between schools, across and within regions, to identify α. Note that our model is a generalization of the prototypical RD specification[6]

$$y_i = \nu + \beta\ IN_i + f(H_i) + \varepsilon_i. \tag{2}$$

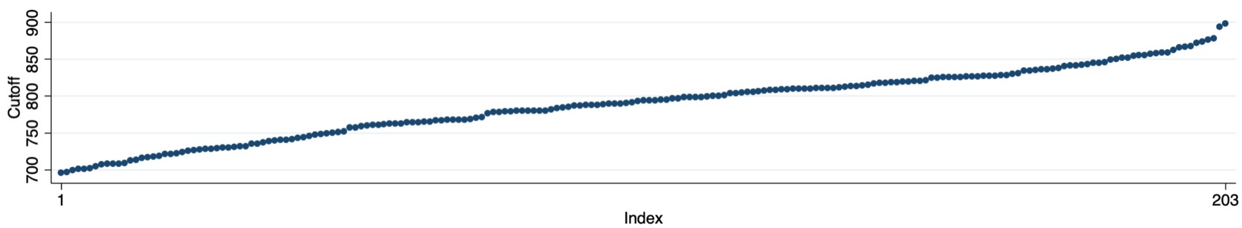
The specification in (2) treats large and small changes in quality the same, and the resulting estimates may mask some important heterogeneous effects. Our generalization takes into account the magnitude of differences in quality between schools, and also allows us to inspect if marginal effects of quality differ along the ability/quality distribution, a common finding of several prominent studies (we elaborate on this in the findings section).

Given a quasi-experiment s, we construct a virtual "*applicants*" set for each school s, by identifying the test takers who have included s in their preference lists and have not been placed in a school they listed above s. This pinpoints the students whom the actual procedure scans for placement in s. This includes two groups: Those with scores above the cutoff for s (they are placed in s) and those who would have been placed in s if their scores had not fallen short.

All of our quasi-experiments are AHs. Take a quasi-experiment s. The applicants placed in s in 2001 take the UE four years later, in 2005, as do the applicants who were rejected at s but were placed in *another* AH. Among the rejected, however, are some who were not placed in an AH. Such students typically end up in three-year schools (non-exam or exam) and hence take the UE in 2004, a year earlier—and when they are a year younger—than those placed in s. (We do not have the UE data for 2004, which precludes us from providing AH vs. non-AH comparisons.) They are also exposed to different curricula, teacher quality, etc. Some among the rejected get placed in vocational schools and thereby lose most university prospects. The effects of such dramatic changes at the cutoff are beyond the scope of our analysis. We therefore focus on applicants for whom university prospects or exam timing do not change with placement in s. Specifically, we focus on those applicants of s who (i) (among the rejected) are placed in another AH; and (ii) (among the placed) who would have been placed in another AH had they been rejected. In the analysis that follows, "school" refers to an AH.

[6]Our specification is $y_{is} = \eta_s + \alpha[IN_{is}Q_{is} + (1 - IN_{is})Q_{is'}] + \epsilon_{is}$ where i indexes the student and s, s' denote schools. Suppose that the difference in quality between the schools at each threshold is a constant, Δ: $Q_{is'} = Q_{is} - \Delta$, for all s, s'. Rewrite our equation as $y_{is} = \alpha[IN_{is}Q_{is} + (1 - IN_{is})(Q_{is} - \Delta)] + \eta_s + \epsilon_{is} = \alpha IN_{is}\Delta - \alpha\Delta + \alpha Q_{is} + \eta_s + \epsilon_{is}$. Let $\beta = \alpha\Delta$ and $v_s = -\alpha\Delta + \alpha Q_{is} + \eta_s$. Note that v_s is a school fixed effect. We then get the prototypical RD specification: $y_{is} = v_s + \beta\ IN_{is} + \epsilon_{is}$.

Figure 2: Distribution of Cutoff Scores



Schools (AHs) that constitute our quasi-experiments are ordered by their cutoff score levels. The cutoff scores are plotted on the vertical axis. The HSE score distribution has a mean of 600 and a standard deviation of 100. The analysis sample at each cutoff is obtained using students within 10 points away from the cutoff.

This setup yields 203 quasi-experiments and their corresponding analysis samples as detailed above. We pool these in our analysis to summarize information and improve the precision of our estimates. Table 2 provides some summary statistics of our pooled sample. Figure 2 plots the cutoff scores for the schools that constitute our set of quasi-experiments.

In our setting, students' lack of ability to manipulate the assignment variable $H_i - \underline{H}_s$ around zero arises from multiple sources. As noted at the beginning of this section, cutoffs $\underline{H}_s$ are endogenous and cannot be predicted precisely. In addition, a test-taker cannot predict his own score H_i with precision, as question weights in the score calculation are determined endogenously, the weight of a given question decreasing in proportion to students who answered it correctly. In addition, students form and submit their school preference orderings before the exam, yet their performance might be very different from what they expect. The fact that the average test-taker lists 8.5 schools in their submitted preference ordering is consistent with the high level of uncertainty regarding the realization of the assignment variable for a given school. The consequence of all of this is that the variation in IN_i near the cutoffs is "as good as random" as Lee and Lemieux (2010) put it. We verify that the pre-placement student observables we have (age, gender, whether the student has requested an SH, whether the student has requested an ATH) or the distribution of the assignment variable $H_i - \underline{H}_s$ do not exhibit any discontinuity at the cutoff. We present a graphical analysis in Figures 3 and 4, and also estimate the magnitudes of the discontinuities in Figure 3 using alternative specifications. In Table 3 we present findings using the specification

$$y_{is} = \beta \; IN_{is} + \gamma \; (H_i - \underline{H}_s) + \phi \; IN_{is} \; (H_i - \underline{H}_s) + \; \eta_s + \; \epsilon_{is} \tag{3}$$

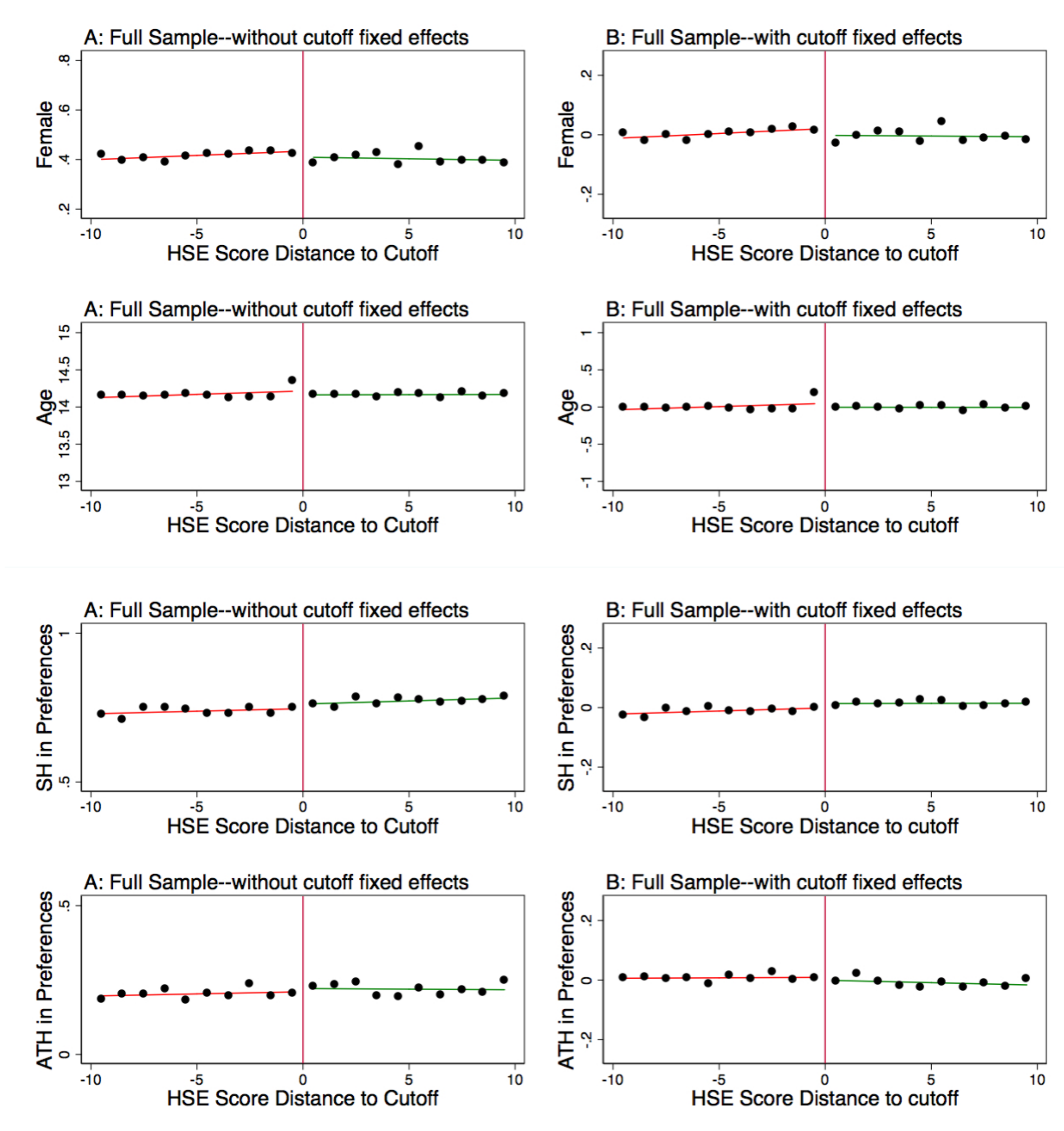
Table 2: Summary Statistics

	Mean	Std. Dev.	Min	Max
Students (N=7,392)				
HSE Score	822.9	34.5	689.6	906.8
Placed ($IN = 1$)	0.45	0.50	0	1
HS Quality	825.7	33.6	606.3	906.0
Listed a SH	0.78	0.42	0	1
Listed an ATH	0.23	0.42	0	1
UE Performance	113.6	26.8	0	176.3
UE Placement	0.76	0.42	0	1
Schools (N=203)				
Cutoff Score	789.0	46.4	695.9	898.3
Quota	97.1	41.9	30	300

Notes: This table reports summary statistics from our pooled sample. Students are "applicants" as defined in Section 2.3, whose exam scores are within 10 points (about 0.10 standard deviations) of the cutoffs of their associated quasi-experiments. Schools are our quasi-experiments. *The exam schools we study, admitting 97 students on average each year, are small compared with those in other studies.*

with cutoff fixed effects (η_s) and report estimates in from samples which consist of applicants at each cutoff whose HSE scores are within 10 points (about 0.10 standard deviations) of the associated cutoff score.

Figure 3: Baseline Variables



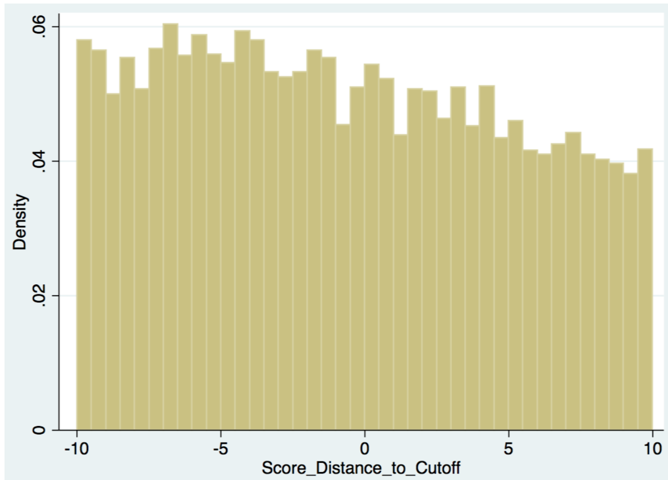
The horizontal axis displays the assignment variable, students' HSE score distance to the cutoff of the school (AH) to which they are "applicants," as defined in Section 2.3. Applicants with scores above the cutoffs are placed and enrolled, whereas those with scores below the cutoffs are rejected (and are placed in a marginally less preferred AH). We gather the applicants into 1-point bins (about 0.01 standard deviations) with respect to the assignment variable $H_i - \underline{H}_s$ and plot the bin averages of the baseline variable of interest on the vertical axis in left panels. We also plot a predicted line from a local linear regression on each side of the cutoff. We then regress the variable of interest at the placed school for each applicant on cutoff fixed effects, and plot the bin averages of *residuals*, along with the fitted lines, in right panels.

Table 3: Baseline Variables

	(1) Age	(2) Female	(3) SH in Preferences	(4) ATH in Preferences
A. Full Sample				
$IN_{is} = 1$	-0.070	-0.037	0.017	-0.009
	(0.086)	(0.023)	(0.019)	(0.018)
Distance to Cutoff	0.012	0.003	0.001	-0.003
	(0.013)	(0.003)	(0.002)	(0.002)
Positive Dist. to Cutoff	-0.014	-0.003	-0.001	0.003
	(0.014)	(0.004)	(0.003)	(0.003)
Sample Average and S.D. of Dep.Variable	14.09 (1.13)	0.416 (0.493)	0.776 (0.417)	0.225 (0.418)
N	7,317	7,317	7,317	7,317
R^2	0.01	0.03	0.13	0.23
B. Higher Cutoffs				
$IN_{is} = 1$	-0.090	-0.036	0.017	-0.007
	(0.103)	(0.026)	(0.021)	(0.019)
Distance to Cutoff	0.014	0.003	0.001	-0.000
	(0.016)	(0.003)	(0.002)	(0.002)
Positive Dist. to Cutoff	-0.014	-0.004	-0.001	0.002
	(0.017)	(0.004)	(0.004)	(0.003)
Sample Average and S.D. of Dep.Variable	14.09 (1.23)	0.420 (0.494)	0.761 (0.426)	0.182 (0.386)
N	5,939	5,946	5,946	5,946
R^2	0.01	0.02	0.12	0.15

Notes: This table reports estimates of possible discontinuities caused by high school placement in students' baseline variables (age, gender, whether the student has included an SH/ATH in their preferences). We use the specification in (3) with y_{is} as the baseline variable of interest and report estimates from samples which consist of applicants at each cutoff whose HSE scores are within 10 points (about 0.10 standard deviations) of the associated cutoff score. Robust standard errors are shown in parentheses. The regressions are clustered at the student level and include cutoff fixed effects. The estimates are small in magnitude and statistically insignificant. Alternative specifications (available from the authors) yield similar results. In the analysis that follows we sometimes focus on a subsample which consists of cutoffs above a certain level. Panel B provides the counterpart of the findings in Panel A for this subsample. * significant at 10%, ** significant at 5%, *** significant at 1%.

Figure 4: Histogram of the Assignment Variable.

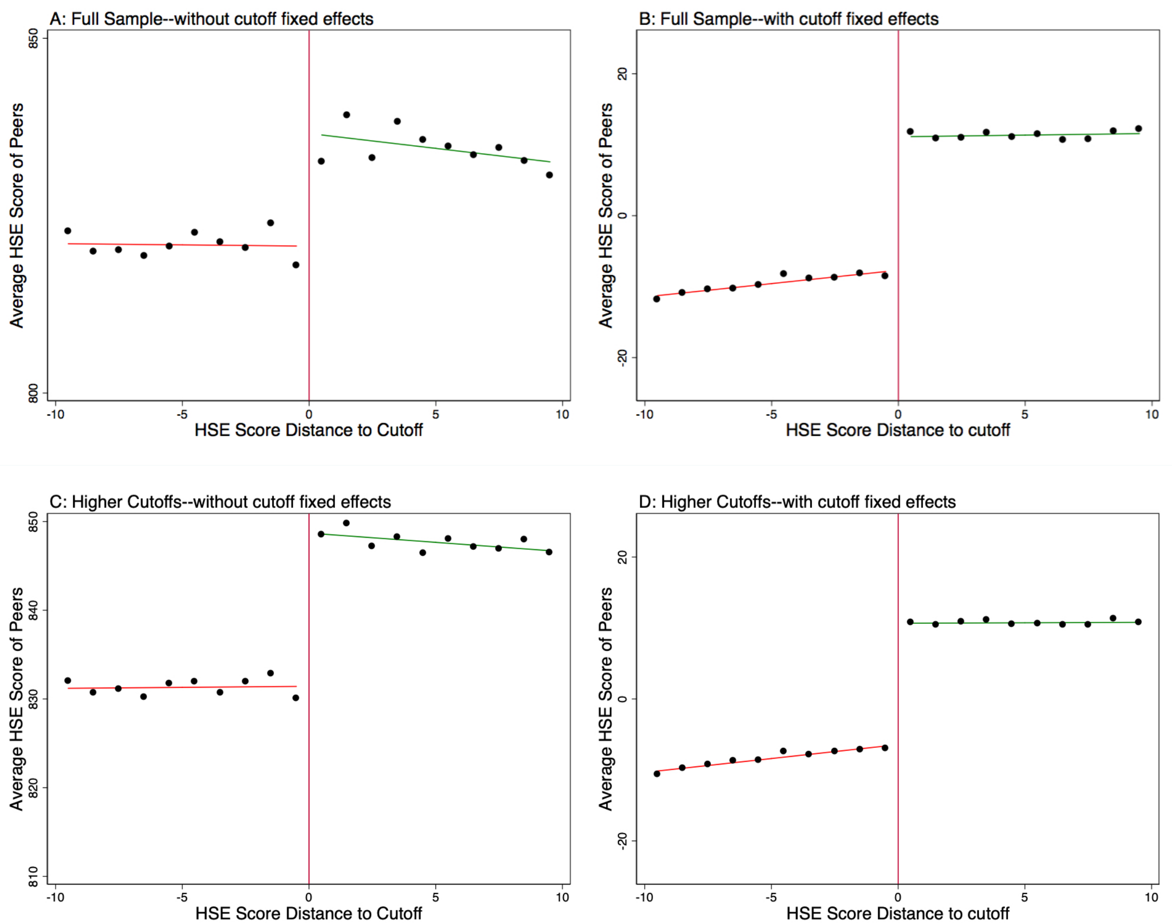


3 Findings

Using the administrative data, for every student we calculate the peer quality at the school they are placed at and attend, as the leave-out mean of HSE scores of all students that start the same school that year. (Ability tracking is not permitted in public exam schools.) Among applicants around cutoffs, placement in the more preferred school results in, on average, about a 20-point (about 0.20 standard deviations) jump in the average ability of peers (as measured by their HSE scores), as illustrated in Figure 5. We present estimates of this jump in Table 4, using the specification in (3) with peer quality as the outcome y_{is} and samples which consist of applicants at each cutoff whose HSE scores are within 12, 10 (with and without linear splines), and 8 points of the associated cutoff score. The differences arising from endogenous sorting of students by ability across our schools are comparable to those in related studies. The uniformity of schools in other dimensions (curriculum, teacher qualifications, etc.) provides us with a unique and valuable setting.

We do not document a statistically significant effect of HSE placement or high school quality on UE participation of the students in our analysis sample. Figure 6 and the first three columns of Table 5 presents our findings. We use the specification in (3) with $y_{is} = 1$ if we observe the student in the UE, and 0 otherwise. We also conduct this analysis using our main specification based on (1) (and introduced below in (4)) and present the results of this in columns 4-6 of Table 5.

Figure 5: High School Placement and Change in Peer Quality



The average peer quality students experience increases significantly and discontinuously when placed in schools ordered higher in their submitted preferences. The horizontal axis displays the assignment variable, students' HSE score distance to the cutoff of the school (AH) to which they are "applicants," as defined in Section 2.3. Applicants with scores above the cutoffs are placed and enrolled, whereas those with scores below the cutoffs are rejected (and are placed at another AH, one they ordered lower in their preference lists). The vertical axis displays the peer quality students experience, as measured by the average HSE score of their actual schoolmates. We first calculate for each applicant the peer quality, leave-out mean of HSE scores, at the school they are placed at and attend. We then gather the applicants into 1-point bins with respect to the assignment variable $H_i - \underline{H}_s$ and plot the bin averages of calculated peer qualities on the vertical axis in Panels A and C. We also plot a predicted line from a local linear regression on each side of the cutoff. The left panel plots the peer quality collapsed into bins containing applicants who are within 1 point HSE score (about 0.01 standard deviation) from each other. We then regress the peer quality at the placed school for each applicant on cutoff fixed effects, and plot the bin averages of *residuals*, along with the fitted lines, in Panels B and D.

Table 4: HSE Placement and Peer Quality

	(1) *Bandwidth=12*	(2) *Bandwidth=10*	(3) *Bandwidth=10, without splines*	(4) *Bandwidth=8*
A. Full Sample				
$IN_{is} = 1$	19.59***	19.86***	22.09***	20.37***
	(0.39)	(0.40)	(0.21)	(0.45)
Distance to Cutoff	0.448***	0.332***		0.261***
	(0.039)	(0.050)		(0.073)
Positive Dist. to Cutoff	-0.365***	-0.216***		-0.272***
	(0.049)	(0.065)		(0.091)
Sample Average and	825.1	825.7	825.7	825.8
S.D. of Dep.Variable	(33.75)	(33.62)	(33.62)	(33.58)
N	8,687	7,317	7,317	5,930
R^2	0.94	0.95	0.94	0.95
B. Higher Cutoffs				
$IN_{is} = 1$	17.17***	17.43***	19.64***	17.80***
	(0.37)	(0.34)	(0.18)	(0.38)
Distance to Cutoff	0.464***	0.366***		0.286***
	(0.037)	(0.047)		(0.068)
Positive Dist. to Cutoff	-0.412***	-0.294***		-0.265***
	(0.043)	(0.056)		(0.079)
Sample Average and	835.5	835.9	835.9	836.0
S.D. of Dep.Variable	(23.75)	(23.73)	(23.73)	(23.68)
N	7,190	6,072	6,072	4,919
R^2	0.92	0.92	0.92	0.92

Notes: This table reports estimates of the effects of high school placement on students' peer quality in high school. The samples cover students within 12, 10, and 8 points of (endogenous) placement cutoffs. Robust standard errors are shown in parentheses. The regressions are clustered at the student level and include cutoff fixed effects. In the analysis that follows we sometimes focus on a subsample which consists of cutoffs above a certain level. Panel B provides the counterpart of the findings in Panel A for this subsample.
* significant at 10%, ** significant at 5%, *** significant at 1%.

Figure 6: High School Placement and University Exam Participation

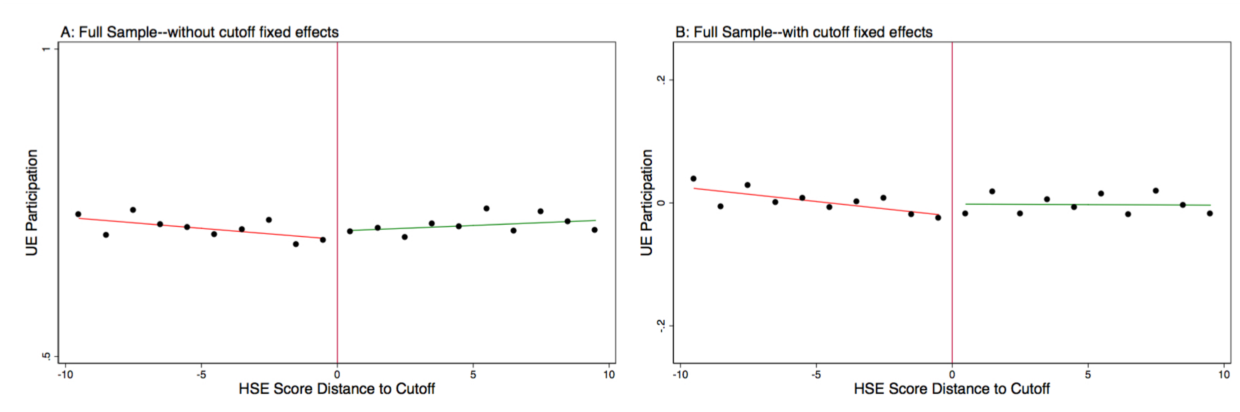


Table 5: UE Participation *(Percentage points)*

	Full Sample (1)	*Higher Cutoffs* (2)	*Lower Cutoffs* (3)	*Full Sample* (4)
$IN_{is} = 1$	2.015	2.918	-2.520	
	(1.730)	(1.929)	(3.860)	
HS Quality				0.055
				(0.049)
Distance to Cutoff	-0.518**	-0.660***	0.377	-0.471***
	(0.202)	(0.220)	(0.500)	(0.178)
Positive Dist. to Cutoff	0.490	0.509	0.0678	0.515*
	(0.305)	(0.336)	(0.715)	(0.304)
Sample Average and S.D. of Dep.Variable	71.02 (45.37)	69.12 (46.20)	80.60 (39.55)	71.02 (45.37)
N	10,303	8,602	1,701	10,303
R^2	0.08	0.07	0.08	0.08

Notes: This table reports estimates of the effects of high school placement and those of high school quality on students' participation on the university exam, for the students in our analysis sample described in section 2.3 of the paper (and also for two subsamples used in the analysis later in the paper). The sample covers students within 10 points of (endogenous) placement cutoffs. Robust standard errors are shown in parentheses. The regressions are clustered at the student level and include cutoff fixed effects. * significant at 10%, ** significant at 5%, *** significant at 1%.

3.1 University Exam Performance

Our main outcome of interest is UE performance, which we measure by the number of *net-correct answers*—correct answers net of penalties for wrong answers. As we noted in Section 2, the centralized system entails a standard curriculum, a common budget, and an assignment system for teachers that is designed to equalize teacher qualifications across schools. This system is likely to be particularly effective in standardizing our quasi-experiments, each of which consists of two schools in the same district and one is only marginally preferred over the other. Hence, the primary source of variation in quality across our schools is peer quality, which we denote by $\overline{H}$. If applicant i is placed in s, the quality she experiences is $\overline{H}_{is}$, defined as the average HSE score of applicants placed in school s, excluding i herself.[7] Let s' denote the school in which i would be placed if her score is below $\underline{H}_s$ and denote the quality thereof by $\overline{H}_{is'}$. We modify (1) as follows:

$$y_{is} = \alpha \; [IN_{is} \; \overline{H}_{is} + (1 - IN_{is}) \; \overline{H}_{is'}] + \gamma \; (H_i - \underline{H}_s) + \phi \; IN_{is} \; (H_i - \underline{H}_s) + \; \eta_s + \; \epsilon_{is} \tag{4}$$

We control the possible effects of the variation in HSE scores H_i on the outcome with linear splines separately on two sides of the cutoff. We also include cutoff fixed effects η_s (common to all applicants of s, placed and rejected). Here and below, we report nonparametric estimates from samples that consist of applicants at each cutoff whose HSE scores are within 10 points (about 0.10 standard deviations) of the associated cutoff score. There is a trade-off regarding the choice of bandwidth. A wider bandwidth increases the number of observations around a cutoff, in turn resulting in more precise estimates, but the additional observations are further from the cutoff and thus are less relevant for a quasi-experimental comparison. We have experimented with different bandwidths, including those proposed by Imbens and Kalyanaraman (2012) and Cattaneo and Escanciano (2017), and found 10 points (about 0.10 standard deviations) to strike a good balance between sample size and comparability of observations, while allowing us to maintain the same sample across many outcomes and specifications in our analysis. Additional analyses with different bandwidths and functional forms, some of which are included below, confirm that our findings are not sensitive to these specifications. Most students occur in a single quasi-experiment, but some in more than one (e.g., as barely placed in one and barely missed in another). We therefore cluster standard errors at the student level.

[7] Ability tracking is prohibited in public exam schools.

Table 6: School Effect Estimates - UE Performance

	Full Sample (1)	*Higher Cutoffs* (2)	*Lower Cutoffs* (3)	*Higher Cutoffs* (4)
A. UE Performance				
HS Quality	0.057*	0.108***	0.027	
	(0.031)	(0.041)	(0.053)	
$IN_{is} = 1$				0.150
				(1.226)
Distance to Cutoff	0.317***	0.291**	0.058	0.452***
	(0.117)	(0.128)	(0.326)	(0.141)
Positive Dist. to Cutoff	-0.070	-0.055	0.002	-0.065
	(0.197)	(0.215)	(0.507)	(0.217)
Sample Average and	113.6	117.0	96.97	117.0
S.D. of Dep.Variable	(26.82)	(26.27)	(23.35)	(26.27)
N	7,317	6,072	1,200	6,072
R^2	0.226	0.168	0.128	0.167

Notes: This table reports estimates of the effects of high school quality on students' performance on the university exam. It also presents in column 4 the reduced form estimates for the higher cutoffs sample, for which we document some statistically significant quality effects in column 2. Each sample covers students within 10 points of (endogenous) placement cutoffs. Robust standard errors are shown in parentheses. The regressions are clustered at the student level and include cutoff fixed effects. * significant at 10%, ** significant at 5%, *** significant at 1%.

We present the estimates in column 1 of Table 6 (Panel A). The coefficient of school quality is statistically significant. It turns out that this result originates from the upper part of the ability/quality distribution. The median cutoff among our schools is 793.6 points. We repeat the analysis separately for the cutoffs above and below this median. Columns 2 and 3 of Table 6 present the estimates. Using observations only from cutoffs above the median, the estimate of school quality is almost twice as large and more precise. In contrast, we do not document a significant quality effect in the lower-cutoffs sample. Alternative specifications (discussed below) yield similar results.

Our separate analyses for the upper and lower parts of the ability/quality distribution is motivated by (and our differential findings are in fact aligned with) a prevalent finding in the peer effects literature: Hoxby and Weingarth (2005) document that having more high-ability students as peers has strong positive effects for the highest- (top decile) and lowest-ability students but not for those in between. The students in our lower-cutoffs sample are in fact from above the 48th percentile of the ability distribution, and those in the higher-ability sample are in the top decile. Lavy, Paserman, and Schlosser (2012) find that high-ability students in Israeli high schools benefit from the presence of other high-ability students. Ding and Lehrer (2007), Burke and Sass (2013), and Imberman, Kugler, and Sacerdote (2012) likewise provide evidence that peer effects are stronger for higher-ability students. Hoekstra, Mouganie, and Wang (2018) document positive effects in a district of China, from attending most-selective high schools, but not from attending moderately selective ones.

Our findings thus suggest that schools make a difference on the UE, at least in the upper part of the ability/quality distribution. What we document are large effects: For the average quality jump at the cutoff of 17.4 points in this segment (standard deviation: 12.4), this estimate corresponds to about 1.9 more net correct answers on the UE on average. For the average student in this segment, we estimate this increase puts them ahead of 3,160 to 4,800 contenders, depending on students' major choices. These students are competing for the most sought-after individual departments, which have relatively small quotas (average: 48, standard deviation: 35). Thus, despite the heterogeneity in student preferences for university departments (arising from major preferences, location preferences, etc.), the documented effects are likely to affect one's university placement significantly. A unit increase in school quality has the same effect on UE performance as about a 0.40 unit increase in one's own ability. This is on the larger side compared with the estimates in the literature.[8]

We present results from *more alternative specifications* in Tables 7 through 9. These analyses provide additional support for our findings discussed above. Columns 1 and 2 of Table 7 present the counterparts of our main estimates on UE performance (presented in Table 6) with alternative bandwidth specifications of 12 and 8 points, separately for the upper and lower segments in panels A and B. In column 3 we drop the cutoff fixed effects and control for the cutoff levels by a quartic polynomial. In all cases we get results similar to those presented earlier. We also conduct our original analysis with the full sample, this time letting the slope of school quality differ between higher and lower cutoffs (Table 8). The difference between the two segments persist. Some quasi-experiments provide as treatment a larger change in peer quality than others in the case of placement. To see if this has an implication for our analysis, we divide our sample into two groups of equal size, according to the magnitude of the quality change that a cutoff introduces to a subject, and conduct separate analyses. We do not document a statistically significant effect of peer quality in either segment (Table 9). This analysis suggests our findings reflect the importance of where in the ability/quality distribution the treatment is given rather than the magnitude of the change in peer quality.

3.2 University Placement

In addition to studying UE performance, it is of interest to study university placements. Some academic majors (departments) within a university are more highly sought after than others. Besides, different majors use different weights to calculate (field-specific) UE scores to rank students. For example, the relative weights of math questions are smaller for a sociology department than they are for a math department. In addition, compatibility between a student's high school type and the targeted major may further affect these weights. Of course, some universities are more sought after than others in general. As a consequence, students can increase their likelihood of university placement by listing in their preferences majors and universities to which demand is lower. Nonetheless, the number of net correct answers on the overall UE continues to be a very strong

[8] The estimates of studies that exploit randomization in assignment range between 0.20 and 0.60 (Duflo, Dupas, and Kremer, 2009; Kang, 2007; Whitmore, 2005; Graham, 2008) and are larger than findings of studies that exploit other identification strategies (Hoxby, 2000; Hanushek et al., 2003; Vigdor and Nechyba, 2004; Zabel, 2008).

Table 7: The Effects of School Quality: Alternative Specifications - 1

	(1) *Bandwidth=12* *Cutoff FE=Yes*	(2) *Bandwidth=8* *Cutoff FE=Yes*	(3) *Bandwidth=10* *Cutoff FE=No*
A. Higher Cutoffs			
HS Quality	0.108***	0.101**	0.114***
	(0.041)	(0.044)	(0.040)
Distance to Cutoff	0.291**	0.303*	0.281**
	(0.128)	(0.176)	(0.125)
Positive Dist. to Cutoff	-0.055	-0.216	-0.063
	(0.215)	(0.297)	(0.215)
Sample Average and	117.0	117.2	117.0
S.D. of Dep.Variable	(26.13)	(26.14)	(26.27)
N	7,036	4,818	6,072
R^2	0.16	0.17	0.13
B. Lower Cutoffs			
HS Quality	0.027	0.018	0.016
	(0.053)	(0.058)	(0.043)
Distance to Cutoff	0.058	-0.371	0.171
	(0.326)	(0.453)	(0.292)
Positive Dist. to Cutoff	-0.002	0.736	-0.140
	(0.507)	(0.704)	(0.463)
Sample Average and	96.97	97.49	96.97
S.D. of Dep.Variable	(23.35)	(23.57)	(23.35)
N	1,200	973	1,200
R^2	0.13	0.14	0.04

Notes: This table reports estimates of the effects of high school quality on students' performance on the university exam under alternative specifications. Robust standard errors are shown in parentheses. The regressions are clustered at the student level and include cutoff fixed effects (except column 3, where they are replaced by a quartic polynomial of cutoff levels). * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8: The Effects of School Quality: Alternative Specifications - 2

	(1)	(2)	(3)
Full Sample, Bandwidth=	*8*	*10*	*12*
HS Quality	0.004	-0.011	-0.004
	(0.042)	(0.037)	(0.034)
HS Quality*1{Higher Cutoff}	0.099*	0.131***	0.119***
	(0.052)	(0.045)	(0.041)
Distance to Cutoff	0.195	0.253**	0.272***
	(0.163)	(0.118)	(0.090)
Positive Dist. to Cutoff	-0.008	-0.046	-0.005
	(0.271)	(0.197)	(0.147)
Sample Average and	113.6	113.6	113.5
S.D. of Dep.Variable	(26.79)	(26.82)	(26.78)
N	5,930	7,317	8,687
R^2	0.23	0.23	0.22

Notes: This table reports estimates of the effects of high school quality on students' performance on the university exam under alternative specifications. Robust standard errors are shown in parentheses. The regressions are clustered at the student level and include cutoff fixed effects. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 9: The Effects of School Quality: Alternative Specifications - 3

	Full Sample (1)	*Larger Jump* (2)	*Smaller Jump* (3)
HS Quality	0.055*	0.029	-0.018
	(0.030)	(0.036)	(0.116)
$IN_{is} = 1$			
Distance to Cutoff	0.322***	0.445***	0.293
	(0.116)	(0.171)	(0.195)
Positive Dist. to Cutoff	-0.069	-0.064	-0.035
	(0.197)	(0.291)	(0.276)
Sample Average and	113.1	111.6	114.5
S.D. of Dep.Variable	(27.12)	(27.65)	(26.50)
N	7,430	3,716	3,714
R^2	0.25	0.28	0.23

Notes: This table reports estimates of the effects of high school quality on students' performance on the university exam under an alternative partition of our sample. The students are grouped according to whether the quasi-experiment provides as treatment a larger or a smaller jump (than the sample median) in peer quality. The regressions are clustered at the student level and include cutoff fixed effects. The sample covers students within 10 points of (endogenous) placement cutoffs. Robust standard errors are shown in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 10: School Effect Estimates - University Placement *(percent)*

	Full Sample (1)	*Higher Cutoffs* (2)	*Lower Cutoffs* (3)
HS Quality	0.046	0.069	0.062
	(0.055)	(0.066)	(0.109)
Distance to Cutoff	0.316	0.396*	-0.256
	(0.197)	(0.207)	(0.680)
Positive Dist. to Cutoff	-0.225	-0.346	0.312
	(0.339)	(0.347)	(1.109)
Sample Average and	76.40	80.14	58.0
S.D. of Dep.Variable	(42.47)	(39.90)	(49.38)
N	7,317	6,072	1,200
R^2	0.117	0.067	0.141

Notes: This table reports estimates of the effects of high school quality and of HSE placement on students' placement rates in a university department. The sample covers students within 10 points of (endogenous) placement cutoffs. Robust standard errors are shown in parentheses. The regressions are clustered at the student level and include cutoff fixed effects. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 11: School Effect Estimates - UE Placement

	Full Sample (1)	*Higher Cutoffs* (2)	*Lower Cutoffs* (3)
A. Min. UE performance of Peers at the Assigned University Department (Net correct answers)			
HS Quality	0.066*	0.096***	0.026
	(0.035)	(0.040)	(0.079)
Distance to Cutoff	0.243**	0.178	0.485
	(0.119)	(0.124)	(0.451)
Positive Dist. to Cutoff	-0.074	0.026	-0.763
	(0.200)	(0.211)	(0.566)
Sample Average and	96.78	98.55	84.87
S.D. of Dep.Variable	(22.97)	(22.54)	(22.62)
N	5,590	4,866	696
R^2	0.19	0.15	0.16
B. Ave. UE performance of Peers at the Assigned University Department (Net correct answers)			
HS Quality	0.050	0.081**	-0.007
	(0.034)	(0.039)	(0.076)
Distance to Cutoff	0.290**	0.224*	0.554
	(0.116)	(0.122)	(0.415)
Positive Dist. to Cutoff	-0.089	-0.007	-0.582
	(0.194)	(0.205)	(0.625)
Sample Average and	119.4	121.3	106.5
S.D. of Dep.Variable	(22.49)	(22.10)	(21.18)
N	5,590	4,866	696
R^2	0.21	0.18	0.13

Notes: This table reports estimates of the effects of high school quality on the selectivity and peer quality of students' university departments (for those placed). The sample covers students within 10 points of (endogenous) placement cutoffs. Robust standard errors are shown in parentheses. The regressions are clustered at the student level and include cutoff fixed effects. * significant at 10%, ** significant at 5%, *** significant at 1%.

indicator of student ability, and we use average UE performance within university departments as a measure of the peer quality of students in the department. We do not document a significant effect of high school quality (or of placement in a more-preferred high school) on the probability of university attendance (Table 10). By contrast, we document statistically significant effects on the peer quality in the university department assigned to a student in the upper-segment (Table 11), with magnitudes similar to those on student's own performance, consistent with a strong sorting by UE performance across university departments.

4 Concluding Remarks

Using novel national exam and centralized placement data from Turkey, we document that attending more selective high schools has large, significant effects on students' university entrance exam performance and university placement. Our estimates vary in magnitude along the ability/quality distribution, which calls for more work to better understand the nonlinearities in school effects. A student's relative position among peers could be an important component of the school effect (Light and Strayer, 2000; Pop-Eleches and Urquiola, 2013; Elsner and Isphording, 2017; Cicala, Fryer, and Spenkuch, 2018), something we plan to investigate in future work. Findings of research can guide education policy and practice to better assign students to schools and classrooms, to improve outcomes such as total learning or income mobility. Our findings suggest that gathering the highest-ability students together has potential benefits, but forming policies to take advantage of peer and school effects comes with challenges (see Carell, Sacerdote, and West, 2013).

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