

Randomizing Elite School Admissions*

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Abstract

What happens if students are randomized to elite schools? This paper studies the causal effects of attending several highly sought-after public high schools in New York City (termed the “screened select”) under two starkly different admissions regimes: a traditional screening process based on test scores and grades, and a partial lottery introduced in 2021. Leveraging this policy reform, I compare the impacts of screened select attendance before and after the admissions change using an IV strategy. While the screened select schools boosted overall SAT scores by approximately 30 points under traditional screened admissions, they had no positive impact in the post-reform lottery era. However, these effects are heterogeneous: students who would not be admitted under full screening saw positive SAT Math impacts, and students who would have been admitted under screening saw negative SAT Math impacts. I provide evidence that these effects are primarily driven by changes in the school peer composition rather than shifts in school resources or curricula. Using a stylized theoretical model of admissions, I formalize the tradeoff between equity and testing outcomes and derive conditions for when a partial lottery would be optimal.

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1 Introduction

Admissions to elite educational institutions are highly competitive and face increasing scrutiny. Colleges use unique and often opaque methods to evaluate students, incorporating standardized test scores, grades, and other factors. A similar pattern is also present in elite high schools, often in large urban districts where the demand for seats far exceeds the supply. The admissions structure at selective public high schools, like that of colleges, plays a critical role in shaping the incoming class and is also subject to public debate. Supporters of selective admissions argue that screening preserves a rigorous academic environment by admitting students best equipped to benefit from it (Shapiro, 2014). Critics contend that screening mechanisms contribute to inequity and segregation. In response to these issues, Michael Sandel argues that an obsession with meritocracy damages both schools and society as a whole. His proposal for allocating spots at elite schools is to use a lottery for well-qualified students (Sandel, 2020). How would such a policy change affect school impacts on student achievement?

Despite the debates surrounding popular elite schools and their admission policies, it is not clear that they have strong impacts on grades, graduation, and college attendance: While these schools typically enroll higher-achieving and more advantaged peers, there is mixed evidence on whether such environments translate into improved academic or longer-term outcomes for individual students (e.g., Dobbie and Fryer (2014)). Additionally, it is empirically challenging to evaluate whether any observed benefits of schools are driven by schools themselves or by peers (e.g., due to the endogeneity of peer group formation, as raised in Manski (1993)).

This paper analyzes the impacts of elite school attendance at five highly-sought New York City high schools under two distinct admission policies, which effectively caused a large-scale peer experiment. I first evaluate the school enrollment effects in the pre-reform (fully screened admissions) and compare them to the school effects in the post-reform (partially randomized admissions). The difference in impacts between the pre-reform and post-reform

periods highlights the significance of admission rules in shaping school effectiveness. I then show evidence that the difference in effects is fueled by the change in peer composition that was caused by the admission reforms. Given my results, I introduce a stylized theoretical model of lottery admissions and derive conditions for the optimal admission regime under varying levels of peer effects and Pareto weights on equity.

In 2021, several screened high schools in New York City (which I refer to as the “screened select”) transitioned to using a partial lottery process to admit students. This was a dramatic change from the pre-reform fully screened admissions, where schools ranked students in priority order based on school-specific criteria. I first evaluate the impact of the reforms on the demographics and other characteristics of the admitted and enrolled class. The reforms dramatically increased income and racial diversity, and reduced the average baseline scores of the incoming class.

With the DA-propensity score methodology from Abdulkadiroğlu et al. (2017), I use an instrumental variables strategy to estimate and compare the enrollment effects of the screened select schools in the pre-reform era and in the post-reform era, focusing on the impact on SAT scores. Pre-reform, the screened select schools boosted SAT scores by approximately 30 points in total. Post-reform, the net effects on SAT scores are null. These effects are heterogeneous across students. Using the published criteria used by schools to score students, I infer which students are likely to be admitted under non-lottery admissions (screening-qualified) and students who would not be admitted (lottery beneficiaries). School impacts for SAT Math achievement are negative for the screening-qualified, but positive for the lottery beneficiaries, emphasizing a tradeoff between access and achievement.

What caused the school impacts to change? I present evidence that the differences in peer composition drove the shift in school effects. I use an instrumental variables setup to isolate the causal effect of enrollment on peer composition across admission regimes, and a visual instrumental variables approach to show that SAT impacts are increasing in peer baseline scores. I also rule out potential alternative explanations for the changing school

effects (e.g., shifts in school quality, funding, and curriculum).

More broadly, I formalize the access-enrollment effect tradeoff that shapes the optimal assignment. The question of whether these reforms were overall optimal depends on the Pareto weight that is placed on increasing access. Given my estimates of SAT Math impacts, the partial lottery is optimal if the Pareto weight on the lottery-beneficiaries exceeds twice that of the weight placed on screening-eligible students. With a stylized model of lottery admissions, I derive the conditions under which partial random assignment boosts overall weighted average achievement. In this model, I allow for school effects to vary by both student type and peer composition, and consider the optimal lottery cutoff rule depending on the extent of peer impacts and the Pareto weight placed on access. Under analogous school effects to my empirical estimates, a partial lottery may be optimal in the setting of my model.

The rest of the paper is organized as follows. The remainder of this section reviews related literature and the contribution of this paper. Section 2 describes the screened select schools, the NYC high school admissions landscape and details of the admissions reforms. Section 3 discusses the impacts of the admission reforms on school composition and enrollment effects. Section 4 presents evidence in favor of the peer mediation hypothesis, and then rules out other candidate explanations for the changes in school effects. Section 5 models the optimal admissions scheme depending on peer effects and the societal benefit of improving access. Section 6 concludes.

Related Literature

This paper contributes to three main areas. First, there is a wide literature that evaluates how peer composition affects academic performance. Manski (1993) documents the empirical challenges of measuring peer effects. Several papers measure peer effects in higher education at using variation in composition at different levels of student groups, e.g., roommate, class, or squadron level (Sacerdote, 2001; Arcidiacono and Nicholson, 2005; Carrell et al.,

2013). Lazear (2001) provides a theoretical model in which classroom productivity depends on student behavior and ability. Other papers show the impact on student outcomes of being exposed to peers with specific characteristics using different sources of variation across cohorts (Hoxby, 2000; Carrell and Hoekstra, 2010; Lavy and Schlosser, 2011; Setren, 2025). In my setting, the lottery admission policy reform resulted in a unique large-scale peer experiment. I exploit the large variation in peers across cohorts to evaluate the impacts of exposure to peers with varying baseline scores on student outcomes.

This paper also relates to the literature studying whether elite schools improve student outcomes and the causes of elite school impacts. There is mixed evidence on the enrollment effects at selective high schools on test scores, college attendance, or long-run earnings (Dobbie and Fryer, 2014; Abdulkadiroğlu et al., 2014; Angrist and Rokkanen, 2015). Recent work by Ellison and Pathak (2025) suggests that elite schools implement optimal curricula in a way that would not show positive achievement effects at selective school admission cut-offs. School effects may also be driven by where students would otherwise attend (Idoux, 2022; Angrist et al., 2023). I use the methodology developed Abdulkadiroğlu et al. (2017) and Abdulkadiroğlu et al. (2022) to show additional evidence on the impacts of elite school attendance under multiple admission regimes.

Finally, this paper relates to the literature that examines how assignment mechanisms affect access, and the impacts on students of changing assignment policies. A wide literature studies the effects of geography-based policies to reduce segregation (Barrow et al., 2020; Angrist et al., 2023; Laverde, 2024; Setren, 2024). Urban school assignment systems may distinguish students with lottery tie-breaking rules, which are used by researchers to estimate school enrollment effects across different school types (Cullen et al., 2006; Hastings et al., 2012; Deming et al., 2014; Chabrier et al., 2016; Angrist et al., 2017; Angrist et al., 2016). Other papers evaluate the effects of modifications to admission policies at the K-12 to post-secondary level (Corcoran and Baker-Smith, 2018; Bianchi, 2020; Idoux, 2023; Avery et al., 2025). In my setting, I contribute to this literature by evaluating the impacts of a dramatic

shift in elite school admission policy on access and outcomes. I then characterize the optimal admissions policy under varying assumptions on the tradeoff between access and enrollment effects implied by my estimates.

2 Background

Institutional context and data

The NYC high school match

The New York City public school system is the largest American school district, containing more than 400 high schools. Every fall, approximately 80,000 students in the New York City public school system enroll in ninth grade. In the previous year, current 8th graders apply to high schools via a centralized application system which is run by the New York City Public Schools (NYCPS). In their applications, students are asked to submit rankings over programs across New York City in order of preference. Since 2004, students can rank up to 12 programs. The programs rank applicants based on school-specific rubrics¹.

After the applications have been submitted, the deferred acceptance (DA) algorithm² is used to match students to programs, based on the submitted preferences by students and the school rankings over students. Students receive a single offer in the spring of their 8th grade. Over 90% of students receive an offer in the main round. For those who are unmatched, there is a supplementary round, where students can submit rankings over schools with open seats. Any students who are still unmatched after this are matched with a school close to their residence.

Schools prioritize students based on different factors, which depend on the school's admission method. There are 4 main types of schools in the high school match. Screened programs evaluate students based on some school specific rubric, which might include grades,

¹Some schools offer multiple programs, but the schools of focus in this paper (the “screened select”) offer only one program each. For the purposes of this paper, I use the term programs and schools interchangeably.

²Details of the Deferred Acceptance algorithm are described in Appendix C

test scores, attendance, and location. Zoned schools give high or guaranteed priority to students who live in the residential area zoned to that school. Educational Option (Ed. Opt) programs admit students with a range of grades and test scores: Half of their seats are evaluated by lottery, and the other half are evaluated by a program specific rubric, such that 16 percent are allocated to students who were rated top performers in a standardized English Language Arts exam, 68 percent to middle performers, and 16 percent to low performers. Unscreened programs admit students using a random lottery.

The NYCPS publishes a guide containing information about the general high school application process and specific programs. The NYCPS website also has a school specific information page for each school. The school information includes a statement of the school's mission; admission method and general priorities; courses and extracurricular activities; graduation rates; school environment; and the number of applicants per seat in previous years. Additionally, the NYCPS also releases yearly reports that cover teacher characteristics, funding, and other school information. In this paper, I focus on screened schools. Screened programs are distinct and have a separate admissions process from New York City's nine specialized high schools, including Stuyvesant High School and the Bronx High School of Science³. Screened programs make up about 20 percent of the total number of high school programs offered, and approximately 13,000 seats were classified as screened according to the 2020 NYC High School Admissions Guide.

Data

I use two main sources of data. The first is publicly accessible information from the school directories, school quality reports, funding information published by NYCPS between the 2017 and 2022 school years⁴. School quality reports include teacher information, such as

³For tested specialized schools, students are admitted based on their scores on the Specialized High Schools Admissions Test (SHSAT). There is one specialized arts school that scores students based on auditions. Specialized admissions operate separately from the standard high school application process. A student who enters the high school match and also applies to the specialized schools would receive a non-specialized offer via the match, and would receive an additional specialized offer if their SHSAT score is high enough.

⁴The 2017 school year refers to the year starting in the Fall of 2017, i.e., the 2017-2018 school year.

the average student-teacher ratio, proportion of teachers who are licensed, and proportion of teachers with over 3 years of experience.

The second source consists of administrative data provided by NYCPS. This includes detailed school enrollment records, educational outcomes, and student application information. The school enrollment records are collected each June, and include information about student grade levels, school attended, poverty status which proxies for free/reduced-price lunch (FRPL) eligibility, English language learner status (ELL), and ethnicity. Educational outcomes includes performance on New York State English Language Arts (ELA) and math standardized tests administered to students in grades 3 through 8, Regents exams, SATs, and course enrollment and grades. Course enrollment and grade is at the classroom level. The application files contain detailed records on students' school choices, priorities, rankings, main round offers, manual placements, and final offers. The match files also include student residential information, such as district and borough. The dataset also includes information about the applicant's disability status, as students with disabilities are matched to specific seats. I use this information to replicate the high school match⁵. Application data can be linked to the student-level enrollment, demographics, test scores, and residential location using a unique identifier. This administrative data covers all 9th grade cohorts from 2018 to 2021⁶, with information on their outcomes and other variables through 11th grade⁷.

2.1 Broader Context

In the early 2020s, a growing number of educational institutions at the K-12 and post-secondary levels undertook substantial admissions reforms. This reflected a broader reevaluation of standardized testing and merit-based selection in light of equity considerations, pandemic related logistics, and institutional priorities. During the COVID-19 pandemic, many colleges dropped the SAT/ACT requirement for 2021 admissions. By 2025, most

⁵More details on the match replication is in Table A14 in Appendix A

⁶The 2018 ninth grade cohort refers to students who enroll in ninth grade in the 2018 school year.

⁷12th grade outcomes are also available for the 2018-2020 cohorts.

schools have since reinstated the standardized testing requirement. Parallel reforms also occurred in selective K–12 public schools nationwide, which also transitioned away from purely screened admissions. Lowell High School in San Francisco previously used a combination of student middle school GPA, 7th grade test scores, and essay responses to rank and admit students, but switched to lottery admissions in 2020. In 2021, top high schools in Philadelphia also stopped using school specific screening admissions in favor of a lottery process. Other schools also relaxed their usage of traditional screening in admissions. Thomas Jefferson High School began using holistic admissions in 2020, instead of standardized testing. Exam schools in Boston began using census tract based affirmative action in 2021, instead of standardized testing. These policy shifts all reflect a reconsideration of traditional selective admissions frameworks that were characteristic of the era.

In New York City, the move to reform high school admissions were motivated by a similar combination of factors. The COVID-19 pandemic disrupted the reliability and availability of traditional student evaluation metrics, such as standardized test scores, course grades, and class attendance. Meanwhile, screened applications had been under increasing scrutiny for having opaque rubrics, and not being widely available: According to a policy brief by the Fordham Law School’s Feerick Center for Social Justice, after several rounds of outreach to the 157 screened programs in the 2018-19 school year, only 20 rubrics were ultimately able to be accessed. Additionally, this came during a time where there were growing calls for equity. Other district reforms (e.g., Diversity in Admissions) were being concurrently implemented to decrease segregation. Integration measures and increased transparency in high school admissions was a signature component of the de Blasio administration (Chalkbeat, 2021). Overall, the selective admissions processes were increasingly informed by debates over racial and socioeconomic equity, local political pressures, and the usage of strict screening in admissions.

2.2 The Screened Select Schools and Admissions

I focus on the following five screened high schools, which I term the “screened select”.

1. Townsend Harris High School
2. Eleanor Roosevelt High School
3. Baruch College Campus High School
4. N.Y.C. Lab School for Collaborative Studies
5. N.Y.C. Museum School

The screened select schools are among the highest performing, and most sought-after in New York City (NYC). Table 2 shows the average SAT scores at the screened select schools compared to other schools in NYC between 2015 and 2019. The average SAT score at the screened select is 1283, which is more than one standard deviation above the mean score in NYC. On average, these schools are well above the 95th percentile across the district. The second entry shows the average SAT scores at the specialized high schools, which operate under a separate admissions procedure. Figure 3 shows a histogram of average SAT scores at each high school in NYC. This illustrates that the screened select schools are near the top of the SAT score distribution of NYC high schools. Figure 2 shows the top 20 most oversubscribed screened programs in NYC in 2020. Oversubscription is measured as the proportion of total applicants to the total number of general admission seats. All of the screened select are present in this list, and each one has 25 or more applicants per available seat. These school characteristics further motivate our focus on these schools and their effects on students.

Table 3 describes the typical incoming 9th grade class at the screened select schools in the pre-reform period, compared with the 9th grade population of NYC. The student population in NYC is majority underrepresented minority (i.e., Black or Hispanic) and FRPL qualifying. Meanwhile, the admitted and enrolled population at the screened select schools have

a majority White and Asian students, and less than 40% of students are FRPL qualifying. The average Math and ELA baseline scores are over 1 standard deviation above the NYC mean. Overall, the screened select student population is higher income, higher scoring, and higher non-minority than NYC overall.

Figure 1 shows the geographic location of the screened select schools. Townsend Harris is located in Queens, and the other four schools are located in Manhattan. The four Manhattan schools are all in District 2, an affluent district including the Upper East Side and Chelsea neighborhoods. Traditionally, screened schools in District 2 would prioritize District 2 residents for seats, e.g., in 2020, according to the NYC High School Admissions Guide, all admission offers to Eleanor Roosevelt High School went to District 2 residents.

Pre-reform, the screened select ranked students according to school-specific rubrics. These rubrics used some combination of applicant grades, test scores, attendance, and location. Students who did not fulfill certain criteria would not be eligible to be ranked. District 2 schools used location-based priority. Figure B16 in Appendix B shows an example screened rubric. Panel A of Figure 4 demonstrates the sharp cutoff rule at Eleanor Roosevelt High School. This figure consists of all applicants within the sharp sample at Eleanor Roosevelt, i.e., all applicants who did not receive an offer from a school that they ranked higher than Eleanor Roosevelt High School in their application.

In 2021, the screened select schools no longer used full screening to prioritize students. Instead, the screened select schools used a partial lottery format⁸. As in the pre-reform, applicants were scored according to some school-specific rubric. However, under the partial lottery, all applicants who scored above some cutoff were all in the same priority group. Within priority group, ties were broken by lottery numbers. Figure B17 shows an example

⁸Baruch College Campus High School (BCCHS) and N.Y.C. Lab School for Collaborative Studies (LSCS) began using a form of lottery admissions in 2020 (one year before the other schools switched to lottery admissions). However, in 2021, they removed District 2 priority (as did N.Y.C. Museum School and Eleanor Roosevelt), and increased priority for FRPL qualifying students. The admitted 2020 cohort at BCCHS and the LSCS has similarities to both the 2019 (final fully screened) cohort and the 2021 lottery cohort (see Table A15 in Appendix A). For my main results, I treat the 2020 cohort at these schools as pre-reform. Results are robust to removing the 2020 cohort at these two schools from the pre-reform sample. More details on this are given in Section 3 and Appendix A

2021 admissions rubric with a partial lottery. Here, all students with an average score of 91% or above were eligible for the top priority group. Figure 5 shows the post-reform analog to Figure 4. The school-specific ranking variables are different across years, so the scale of the axis are not comparable. Under partial lottery admissions, all students in the sharp sample at Eleanor Roosevelt who fall above a certain cutoff have a positive probability of admission. This probability stays below 10%, and does not increase in applicant rank. Table 4 shows the number of applicants who are eligible for the top priority group at Eleanor Roosevelt, and the number of seats allocated. There are 2337 applicants who qualify for the top priority group, and each of them have a positive chance of admission at 116 seats. Similar patterns also emerged at other schools; the number of students that qualified for the highest priority group far exceeded the number of available seats.

There were two other changes to admissions implemented at the screened select that also altered the incoming class:

1. District priorities were removed citywide. In particular, the screened select schools in District 2 (Eleanor Roosevelt High School, Baruch College Campus High School, N.Y.C. Lab School for Collaborative Studies, and the N.Y.C. Museum School) could no longer prioritize in-district residents.
2. Expansion of Diversity in Admissions: Priority was given to applicants eligible for FRPL qualifying students. At all screened select schools, the first 50% of all available seats were reserved for FRPL qualifying students⁹. The other remaining offers were open to all other students.

Overall, these reforms represented a substantial shift in the admissions landscape at screened select schools, introducing a new priority system aimed at increasing equity and access. These schools, among the most oversubscribed, and academically competitive in the city, provide a compelling setting for evaluating the causal impacts of descreening in elite

⁹At BCCHS and the LSCS, the first 66% were reserved for FRPL students (an increase from 34% in 2020)

admissions. By relaxing previously restrictive geographic and performance-based admissions priorities, the policy change created a quasi-experimental environment that facilitates the evaluation of changes in school composition, student outcomes, and peer impacts.

3 Results

3.1 Reform Impacts on Student Composition

How did the lottery admissions impact the student population at the screened select? Figure 6 illustrates that there were substantial shifts in the demographic and academic composition of students enrolled at screened select schools following the implementation of lottery-based admissions. Prior to the reform, less than 40% of enrolled students were FRPL eligible; in 2021, this proportion had increased to approximately 60%. The change in racial composition was more modest but still apparent: the total share of Black and Hispanic students rose by roughly 10 percentage points (from approximately 15% to over 25%), accompanied by a corresponding decline in the representation of White and Asian students. The proportion of admitted and enrolled students residing within the residential zones of the screened select schools declined by 20 percentage points. In terms of academic preparedness, the average baseline performance on 6th grade standardized Mathematics and ELA assessments declined by approximately 0.3 standard deviations. Collectively, these compositional changes show that the admissions reforms significantly altered the student body, aligning the screened select school composition more closely with NYC overall. Similar patterns are also present in Figure 7, which shows the offered class of students. These results indicate that the lottery-based admissions were effective in broadening access to the screened select schools, leading to higher levels of income and racial diversity, and decreased baseline test scores.

3.2 School Effects on Student Outcomes

Empirical Approach

I estimate the achievement effect of attending the screened select in the pre-reform (full screening) regime as compared with the post-reform (partial lottery) regime using a 2SLS setup. Solely using a screened select offer as an instrument for screened select 9th grade enrollment does not fully account for correlation between an applicant's type and their outcomes. Instead, I control for the DA-propensity score, i.e., conditional probability of admission, as detailed in Abdulkadiroğlu et al. (2022), which is calculated using the information from the match replication process. A summary of the details of computing the DA-propensity score follows.

The New York City high school match process induces a stratified randomized trial. Recall that student-proposing Deferred Acceptance is used to match students to programs in NYC. In the assignment process, each student can rank up to 12 programs in order of preference. Schools prioritize students according to program-specific rubrics, and have tiebreakers to distinguish students with the same priority.

There are two key sources of variation in this setting:

1. (Non-lottery) Tiebreaking at schools within priority groups.
2. Higher ranked rejection: a student who does not receive an offer at a more-preferred program will propose to their next choice.

Each applicant i has type $\theta_i = (\succ_i, \rho_i)$, consisting of applicant preference rankings over schools as denoted \succ_i , and priority vector $\rho_i = (\rho_{i1}, \dots, \rho_{iS})$ which describes applicant i 's priorities at each school. Each school s has some tiebreaker rule $v(s)$. Applicant position at school s can be represented as π_{is} , which is a composite of their type and the school tiebreaker. A lower π_{is} indicates a better position at school s .

In a large market setting, with DA-generated offers, each school has some marginal priority ρ_s (which indicates that all applicants rejected by their higher-ranked schools who

have school priority $\rho_{is} < \rho_s$ receive offers) and tiebreaker cutoff τ_s (which distinguishes applicants with school priority $\rho_{is} = \rho_s$).

The most informative disqualification ($MID_{\theta_s}^v$) for type θ_s and tiebreaker v is:

$$MID_{\theta_s}^v = \begin{cases} 0 & \text{if } B_{\theta_s}^v = \emptyset \text{ or if } \rho_{\theta b} > \rho_b \forall b \in B_{\theta_s}^v, \\ 1 & \text{if } \rho_{\theta b} < \rho_b \text{ for some } b \in B_{\theta_s}^v, \\ \max\{\tau_b | b \in B_{\theta_s}^v \text{ \& } \rho_{\theta b} = \rho_b\} & \text{otherwise.} \end{cases}$$

In this definition, $B_{\theta_s}^v = \{s' \in S_v | s' \succ_\theta s\}$, i.e., the set of schools that a type θ prefers to s for each tie-breaker v . In words, the $MID_{\theta_s}^v$ measures the extent to which eligibility for seats in the set of schools that are preferred to s by θ -type applicants and use the tie-breaker rule $v(s)$ affects the distribution of tie-breaker values among applicants competing for those seats. In particular, the tie-breaker process influences the allocation of seats by truncating the distribution of tie-breaker scores for applicants in the pool.

A type θ applicant's DA-propensity score at school s depends on their disqualification rates at the schools that they prefer to s . The $MID_{\theta_s}^v$ and the tiebreaker cutoff τ_s create regression discontinuity-type experiments around relevant cutoffs. Applicants within a bandwidth around a school's cutoff would be subject to positive risk, while applicants far from all relevant cutoffs would not. These bandwidths are computed using the Imbens and Kalyanaraman (2011) procedure with a uniform kernel, and are estimated separately for each outcome variable, as in Abdulkadiroğlu et al. (2022)¹⁰. Overall, these disqualification rates combined with the qualification rate at s the determines conditional assignment probability for an applicant at school s . Theorem 1 of Abdulkadiroğlu et al. (2022) states the exact formula for the DA propensity score at a school s .

Because DA is a single-offer system, the local DA propensity score p_i for applicant i for

¹⁰The positive risk sample consists of all applicants within a quarter of the calculated bandwidth from the cutoff.

receiving an offer to any of the screened select schools is the sum of their propensity scores p_{is} at any of the screened select schools¹¹:

$$p_i = \sum_{s \in S_{scr}} p_{is}$$

Table 5 displays summary statistics of the applicants with positive risk in the pre-reform and post-reform. Approximately 1200 applicants have positive risk at the screened select schools in the pre-reform admissions regime, and 5000 applicants have positive risk at the screened select schools under lottery admissions. It is natural for more applicants to have positive risk under lottery admissions than full screening. The pre-reform and post-reform positive risk samples have similar baselines and gender composition. In the post-reform, there are more students with positive risk who are FRPL qualifying, due to the reserve structure that prioritized low income students.

Columns 1-2 of Table 6 shows that in the risk sample, covariates are generally balanced when conditioned on the DA propensity score, thus eliminating omitted variables bias. Balance is estimated by regressing each covariate on a dummy for receiving a screened select offer (real or simulated), controlling for screened select propensity score and a dummy variable for the fall year. The covariate balance is similar when offers are the true offer versus the simulated offer. This sample consists of the 2018-2021 cohorts. There is some imbalance on the FRPL covariate, again since these students were prioritized for half of the available seats in 2021. Columns 3-6 of Table 6 shows that the sample split between the pre-reform and post-reform cohorts is also balanced across covariates.

School Impacts

What are the enrollment impacts of the screened select schools on standardized test scores? To calculate the pre-reform and post-reform effects, I use the following 2SLS estimator which controls for the probability that applicant i is offered a seat at one of the screened select

¹¹Here, S_{scr} denotes the set of screened select schools.

schools and other baseline covariates.

$$Y_i = \beta C_i + \nu_1 p_i + \gamma'_1 X_i + \epsilon_i \quad (1)$$

$$C_i = \delta D_i + \nu_2 p_i + \gamma'_2 X_i + \xi_i \quad (2)$$

The main outcome of interest Y_i is student i 's highest SAT score taken in high school during or before 11th grade¹². D_i indicates if student i receives an offer from any screened select school, and C_i indicates if student i enrolls in 9th grade at a screened select school. β is the causal effect of interest. δ is the first-stage effect of D_i on screened select exposure, C_i . I control linearly for p_i , the DA-propensity score at the screened select schools. Covariate vector X_i includes ethnicity, gender, FRPL status, ELL status, and baseline math and reading scores. Equations 1 and 2 are computed on the sample of applicants in the risk set at the screened select schools, i.e., applicants with propensity score $p_i \in (0, 1)$.

Columns 1-2 of Panel A of Table 7 reports the 2SLS estimates of β from equation 1 for the pre-reform risk sample. Estimated screened select effects on both SAT Math and English are positive and significant, with estimated gains of 15 points on Math and 18 points on English. As a benchmark, the average SAT score in the United States in 2018 was approximately 530 on each section, with a standard deviation of 100. These estimates imply that attendance at the screened select schools would boost SAT scores on each section by 0.15 standard deviations on average. Columns 1-2 of Panel D report the OLS estimates of β for the pre-reform risk sample, excluding the propensity score controls. These effects are also positive and significant but smaller in magnitude.

Columns 3-4 of Panel A of Table 7 report 2SLS estimates of equation for the positive risk sample for the post-reform (lottery regime) sample. The net effects are null for both SAT Math and English scores.

These estimates imply that the school effects under the lottery regime were no longer

¹²I compute estimates of β for both the Mathematics section and the English section.

positive, which suggests that the admission reforms did not improve student outcomes. Recall that increasing access was a key goal of the reforms, and that the school composition was dramatically altered in the post-reform era. Are school effects heterogeneous across students? To address this, I divide students into two categories, and estimate the school effects separately for each group.

1. Screening-qualified: These students would have been admitted under a non-lottery regime according to the calculated student scores (based on the published school rubrics), and the next top 10% of applicants.
2. Lottery-beneficiaries: All remaining applicants (these students would not be admitted under a non-lottery regime).

Columns 5-8 of Panel A of Table 7 show 2SLS estimates when the students in the risk sample is split into the lottery beneficiaries and screening-qualified. SAT Math enrollment effects are positive and significant for lottery beneficiaries, while SAT Math enrollment effects are negative and marginally significant for screening-qualified students. The difference in SAT Math effects for the screening-qualified compared to the pre-reform is significant at the 0.01 level. Taken together, attendance at the screened select yields positive enrollment impacts for lottery beneficiaries. But, these come at the expense of the screening-qualified students. The positive school impacts for lottery beneficiaries and negative school impacts for the screening-qualified are also observed with 9th grade Math Regents scores in Table A17 in Appendix A. The enrollment effects for SAT English remain null across groups.

Estimates of first-stage equation 2 (seen in Panel B of Table 7) show that, on average, receiving an offer from the screened select increases the probability of enrolling at the screened select by 63 percentage points in the pre-reform admissions regime. In the post-reform, the offer effect on enrollment increases to 77%. However, this increase is concentrated among the lottery beneficiaries. For this group, receiving an offer from the screened select schools has an increased enrollment probability of 85 percentage points. Meanwhile, for the screening-

qualified students, receiving an offer from the screened select also increases their screened select enrollment probability by 62 percentage points in the post-reform admissions regime. This implies that screening-qualified students did not flee the screened select schools in the lottery regime.

Table A16 in Appendix A shows that these effects are robust to the staggered timing of the admission reforms. In this table, the pre-reform sample excludes the 2020 cohort from Baruch College Campus High School and N.Y.C. Lab School for Collaborative Studies. The enrollment impacts on both SAT Math and SAT English remain positive and significant.

Table 8 enhances the precision of the analysis by disaggregating the full risk sample into two distinct groups: those exposed to RD risk and those exposed to lottery risk. The RD risk category includes applicants from the pre-reform regime¹³. By the screening structure, these applicants are close to the school-specific cutoffs by a regression discontinuity (RD) design. The RD risk category also includes applicants from the 2021 cohort who are near the threshold for the top lottery group, meaning that they are also subject to RD risk. Note that these applicants have lower baseline scores, so the non-offered RD risk category means in Table 8 are lower than the non-offered pre-reform means in Table 7. The lottery risk group consists of 2021 positive risk applicants who are eligible for the school lotteries but are not included in the RD sample. The school effect for applicants subject to RD risk is approximately 20 points for SAT Math at 0.01 significance, and 15 points for SAT English at 0.05 significance. These estimates of school effects are close in magnitude to those in Table 7, but more precise.

Taken together, the results in this section indicate that the screened select schools were effective in the pre-reform period, when these schools fully screened their applicants. In the post-reform regime, school effects are heterogeneous by group: lottery beneficiaries have positive SAT Math effects, while the screening qualified have negative SAT Math effects. These impacts are similar in magnitude and cancel out, leading to a null effect for the

¹³The positive risk sample excludes BCCHS and LSCS in 2020, since these applicants were subject to lottery risk.

sample overall under lottery admissions.

4 Why Did School Effects Change?

Under lottery-based admissions, screened select enrollment effects for screening-qualified students are significantly worse than for comparable students in pre-reform screened era. What caused the difference in school effects between admission regimes? I consider whether there were other significant changes at the screened select schools that could have caused the changes in attendance effects. To answer this, I focus on school characteristics across years. The school quality measures are student-teacher ratio, proportion of teachers with a license, the proportion of teachers with 3 or more years of experience, school funding per pupil. I also consider whether the course offerings at the screened select were different in the pre-reform period versus the post-reform period. Across these indicators, there are no significant differences in these school characteristics across years that seem likely to drive the change in school impacts.

4.1 School Quality

I first compare the average difference in school quality measures for screened select schools versus other schools by year. Figure 8 plots the average differences in school quality measures with 95% confidence intervals around the average difference. At the screened select schools, the student-teacher ratio is consistently 2 students-per-teacher higher than all other high schools across years. The difference in the percentage of teachers who are licensed is noisy across years. The screened select schools have a consistently higher proportion of licensed teachers. The percentage of teachers with 3 or more years of experience is between 0-10% higher than other schools, and is not significantly different before and after the reforms. The funding per pupil is consistently lower at the schools of interest than the rest of the district¹⁴.

¹⁴Schools with higher concentrations of ELL and FRPL qualifying students are typically allocated higher funding.

Overall, the differences in the measures of school quality between the schools of interest and other high schools in the district are generally consistent across years.

I also estimate whether there was change in relative school characteristics between the pre-reform and post-reform years using a DiD event study regression. These results also indicate that there is no difference in school quality compared with the rest of the district before and after the reform. I use the following specification (with the estimator from Callaway and Sant'Anna, 2021):

$$Y_{st} = \alpha_s + \delta_t + \sum_{j=-2}^2 \gamma_j \text{Reform}_{s,t}^j + \pi X_{st} + \epsilon_{st} \quad (3)$$

The term $\text{Reform}_{s,t}^j = \mathbf{1}\{t - T_s = k\}$ is the event study indicator, where T_s denotes the time of treatment. The time of treatment T_s for the screened select is 2021; all other schools are untreated. α_s and δ_t are school and year fixed effects. Y_{st} is the outcome of interest for school s in time t (such as school quality measures and funding). X_{st} are controls for proportion of ELL and FRPL qualifying students by school in each year. The DiD estimates in Figure 9 suggest that there is no significant change in school characteristics pre-reform and post-reform. There appears to be a slight (but insignificant) increase in the proportion of teachers with more than 3 years of experience, and the proportion of teachers with a license. It is unlikely that a relative increase in more experienced or better trained teachers would cause schools to become less effective. Overall, there is no evidence that changes in school quality drove the change in enrollment effects.

Curriculum Offerings

Another potential change in school characteristics would be if schools changed their course offerings, or if students were enrolled in different courses. Since the main changes in school effects were in math scores on standardized tests, I focus on the math course offerings and the proportion of students taking each course. Approximately 97% of students in the risk

sample are enrolled in either an Algebra I or Geometry class in ninth grade (for both offered and non-offered applicants). Figure 10 shows the number of Algebra and Geometry courses offered at the screened select schools across years. The average number of Algebra I courses offered at each school is between 2 and 4 each year. The average number of Geometry courses offered at each school is between 6 and 8 each year. Neither of these figures show a significant change in the average number of math courses of each type offered in 2021. Figure B15 in Appendix B likewise show similar course offerings across years in other math courses. Figure 11 shows the average proportion of ninth grade students taking Geometry and Algebra I in the pre-reform cohorts versus the post-reform cohort. Approximately 60% of students at the screened select are enrolled in Geometry in ninth grade (for both the pre-reform and post-reform cohorts), and most of the remaining students are enrolled in Algebra I. These figures indicate that the course offerings and course enrollment patterns at the screened select schools remained consistent across years.

Counterfactual Destinies

The estimates of the screened select enrollment effects in Table 7 are relative to where students would otherwise attend. Did the counterfactual destinies change for the screening-qualified students? Tables 9 and 10 address this question. I examine the types of schools students who do not receive an offer attend instead. I group the program types in New York City into 3 categories: Unscreened (includes zoned and open programs), Mixed (includes Ed. Opt. schools), and Elite (includes Screened and Specialized programs).

Column 1 in both tables shows that just under half of all non-offered students attend a different elite school. Column 3 in Table 9 shows the counterfactual school destinies for students with positive propensity scores, which can be thought of as analogous to the screening-eligible (column 3 in Table 10). In both cases, 80% of non-offered students attend an elite school. In both tables, columns 4-6 restrict the sample to compliers, i.e., applicants who attend the screened select schools if offered, and do not attend otherwise. The school

destiny patterns for the full sample and compliers look virtually identical. As seen in Panel B of Table 7, pre-reform students and screening-qualified students who receive screened select offers are similarly likely to accept their offer. Since the partial lottery admissions were only implemented at a few screened schools, other NYC screened and specialized schools are unlikely to have different impacts between the pre-reform and post-reform.. These counterfactual destinies are consistent across cohorts. This indicates that non-offered students are not attending significantly different schools in the post-reform. Changes in outside options are therefore also unlikely to drive the differences in school effects across years.

4.2 Peer Effects

The previous subsection demonstrates that there is no evidence of changes in characteristics at the screened select schools that would cause their enrollment effects to change. Instead, the primary change at the screened select schools was the diversification of peers. In this section, I show that this change in peer composition can explain the changes in enrollment effects. I evaluate this by comparing peer exposure in the pre-reform to the post-reform, and show how the change in peers varied by student groups. I also use a visual instrumental variables approach to show the effect of the peer mediator on SAT effects.

First, I consider peer exposure in 9th, 10th, and 11th grade math classes as an outcome of enrollment in Equations 1 and 2. Student i 's peer exposure is defined as the average math baseline score in their math class in each grade. Estimates are shown in Table 11. The estimates in Panel A show that pre-reform peer baselines at the screened select schools are over 0.2 standard deviations higher than the peer baselines at schools where students would otherwise attend. Overall estimates from the post-reform era in Panel B are also similarly positive. However, this post-reform increase in peer baseline is concentrated among the lottery beneficiaries (as shown in Panel C). Panel D shows that post-reform, the screened-qualified no longer have improved peer baselines from attending the screened select, and have marginally lower baseline-scoring peers. This is a significant decrease in peer baselines

in comparison with the pre-reform.

Next, I formally test for peer effects on student outcomes using a visual instrumental variables approach, which demonstrates that SAT Math achievement is increasing in peer exposure. The previous results have shown that enrollment at the screened select significantly impacts peer exposure, and that peer baselines changed between the pre-reform and post-reform era. Additionally, the school effects on SAT Math scores are different between the pre-reform and the post-reform. Given the changes in peer exposure before and after the admission reforms, it appears that the peer mediator is a key causal channel for school effects.

To test this, I consider the effect of peer exposure M_i on SAT math score Y_i . Peer exposure M_i is the average math baseline in student i 's math classes from 9th grade to 11th grade overall. I use the following 2SLS model in the style of Angrist et al. (2021). In this model, receiving an offer D_i from a screened select school impacts peer exposure M_i , which in turn affects SAT math score Y_i :

$$Y_i = \mu_1 M_i + \gamma'_1 X_i + \nu_1 p_i + \varepsilon_{1i} \quad (4)$$

$$M_i = \pi'_{10} X_i + \pi_{11} D_i + \pi'_{12} X_i D_i + \nu_2 p_i + \epsilon_i \quad (5)$$

I calculate the covariate specific first stage coefficients $\pi(X_i) = \pi_{11} + \pi'_{12} X_i$, and plot them against the covariate specific reduced form coefficients in Figure 12. Specifically, the figure shows reduced form estimates for SAT math plotted against the corresponding first stage estimates for peer baseline, conditional on each covariate and computed separately for (1) the pre-reform students (in magenta) (2) the post-reform lottery-beneficiaries (in blue) and (3) the post-reform screening-qualified students (in green). The covariate-specific points are calculated using a model with joint estimates for reduced-form and first-stage interactions, and all second-order terms. The solid points represent estimates for any-offer instruments, pooled across covariate cells.

In the pre-reform era, the offer instrument yields large positive first stage increases in

peer baselines across all covariates (as in the Table 11 estimates). This is also true for the lottery beneficiaries in the post-reform era, while the offer instrument generates a marginally negative overall first stage impact on peer baseline. In the reduced form, the SAT impacts are generally positive and significant across covariate groups for the pre-reform cohorts and the post-reform lottery beneficiaries. Meanwhile, across covariate cells the reduced form impacts are generally negative for the screening-qualified. The point estimates across the pre-reform and the two post reform groups generally fall on a fitted line: The slope of the line through these points is an IV estimate of μ_1 . The line has a slope of 39.6 when estimated with no intercept¹⁵.

Other Evidence of Peer Effects

Section 4.1 showed that there was no change in the number of math courses offered or the number of students taking each ninth grade math class. The math class that a student takes in ninth grade largely determines which math classes they take later in high school¹⁶, and which peers are also enrolled in their math classes. Across the two main math course trajectories, the variation in baseline scores across admission regimes demonstrates further evidence in favor of peer effects on shool impacts.

The student math trajectory is primarily determined by their middle school curriculum: approximately 97% of screened select students who passed Algebra I in middle school enroll in Geometry in high school. Students are characterized as on the Geometry track (accelerated) if they received high school credit for Algebra I in middle school, and the Algebra I track (regular) if they did not.

Figure 13 shows the classroom baseline scores for students taking Algebra I in 9th grade and the classroom baseline scores for students taking Geometry in 9th grade. The distribution of baseline scores for those taking Algebra I in 9th grade is similar between the

¹⁵This fitted line is calculated according to a sample-size weighted regression as in Angrist et al. (2023), excluding the pooled estimates.

¹⁶The standard order of math courses taken in New York City is:

1. Algebra I; 2. Geometry; 3. Algebra II (Trigonometry); 4. Precalculus; 5. Calculus

pre-reform and the post-reform. Meanwhile, the distribution of baseline scores of students enrolled in Geometry in 9th grade is shifted down 0.3 standard deviations post reform¹⁷.

Table 12 reports the change in peer baseline split by math track. The outcome of interest is the average peer baseline score across math classes from 9th to 11th grade for each student. Column 1 shows estimates for students on the regular track, and Column 2 shows estimates for students on the accelerated track. As in the descriptive histogram, there is no significant change in peer baseline for students on the regular track, but a significant decrease in peer baselines for students on the accelerated track.

Finally, Table 13 reports SAT Math enrollment effects split by math track. While the estimates are somewhat noisy, there is suggestive evidence that the decrease in enrollment effects is concentrated among students on the accelerated track. Taken together, these results also indicate that changes in peer exposure were a key mechanism for the changes in school impacts.

5 Optimal Lottery Design

5.1 Equity vs Efficiency: Pareto Weights for the Partial Lottery

From the perspective of policymakers, it could be more valuable to boost the achievement of certain students, i.e., perhaps there is some additional welfare weight for specific groups, or a 10 point gain in SAT score might have more impact on students at various points on the score distribution. Given the estimated impact on school effects, under what conditions would the partial lottery reform be optimal overall?

To calculate this, I compare the average screened select enrollment effect pre-reform β_{pre} to the weighted average enrollment effect post-reform:

¹⁷Recall that in Figure 6, the decrease in baseline score of enrolled students was also 0.3 standard deviations.

$$\beta_{pre} = (\beta_{post}^s(1 - p_{post}^l)) + (1 + \delta^l)(\beta_{post}^l p_{post}^l) \quad (6)$$

In Equation 6, β_{post}^s denotes the post-reform enrollment effect for the screening-qualified students, and β_{post}^l denotes the post-reform enrollment effect on the lottery beneficiaries. The proportion of lottery-beneficiaries is p_{post}^l . I allow for some societal weight δ^l on improving access, so the term $(1 + \delta^l)$ scales the enrollment effect for the lottery beneficiaries. If $\delta^l = 0$, the enrollment effects for the lottery beneficiaries and the screening-qualified are weighted equally. I use the SAT Math estimates of coefficient β to calculate δ^l .

$$\delta^l = \frac{\beta_{pre} - \beta_{post}^s(1 - p_{post}^l) - \beta_{post}^l p_{post}^l}{\beta_{post}^l p_{post}^l} \approx 1.48$$

In words, the impact on the lottery beneficiaries needs to be over twice as much as the screening-eligible for the same weighted effect as the pre-reform.

5.2 The Optimal General Lottery

The results in the previous section highlight that despite differences in school impacts, lottery admissions might be seen as beneficial due to the expansion of school access, depending on the weight δ^l . I characterize the conditions under which no lottery, a partial lottery, or a full lottery is the optimal admissions method, as a function of peer effects and the relative weights assigned to different student groups. Suppose that there is a unit mass of students with scores distributed $s \sim Unif[0, 1]$. There is one elite school which has capacity c , and must fill all seats. A planner would like to assign a mass c of students to this elite school. All other students attend some non-elite school. Students receive some benefit b depending on what school they attend. The benefit b of not attending the elite school is $b = 0$ for all students.

Under fully screened admissions, all students with scores $s \in (1 - c, 1)$ are admitted to the elite school. In this case, the benefit of attending the elite school is normalized to $b = 1$.

Instead of fully screened admissions, schools can run a partial lottery, for some parameter $\varepsilon \in (0, 1 - c)$. A partial lottery has cutoff $1 - c - \varepsilon$. Under a partial lottery, all students with score s above cutoff $1 - c - \varepsilon$ have an equal chance of being admitted. This is a full lottery (i.e., all students have a positive chance of admission) when $\varepsilon = 1 - c$.

The benefit b from attending the elite school depends on the admitted class of students. The school benefit for students with baseline score $s > 1 - c$ is $1 - l\varepsilon$, for $l \in \mathbb{R}^+$. These students are analogous to the screening-qualified. l is assumed to be positive because the empirical screened select impacts are significantly worse for the screening qualified in the post-reform. Students with $s < 1 - c$ (the cutoff for fully screened admissions) receive school benefit $1 + k\varepsilon$ for some $k \in \mathbb{R}$. These students are analogous to the lottery beneficiaries in the prior analysis. We can think of $1 + k\varepsilon$ and $1 + l\varepsilon$ as more general averages of some function that determines school effect for students on either side of the cutoff¹⁸. Finally, the social planner puts additional weight δ on the benefit of those below cutoff $(1 - c)$. This parameter δ is the benefit from improving access¹⁹.

The average benefit under lottery parameter ε is:

$$\mathbb{E}(b|\varepsilon) = (1 + \delta) \frac{\varepsilon}{c + \varepsilon} (1 + k\varepsilon) + \frac{c}{c + \varepsilon} (1 - l\varepsilon) \quad (7)$$

A planner chooses the optimal lottery parameter ε to maximize Equation 7.

Proposition 1 *The optimal lottery depends on the relationship between c, δ, l and k* ²⁰:

1. *No lottery (i.e. $\varepsilon = 0$) is optimal when $\delta < cl$ and $k \in (-\infty, \bar{k})$.*

¹⁸The equations for the school effects under (partial) lottery admissions $1 - l\varepsilon$ and $1 + k\varepsilon$ can be equivalently written in terms of the change in the average baseline score at the elite school. In particular:

$$\Delta \bar{s} = \bar{s}^{post} - \bar{s}^{pre} = (1 - \frac{c}{2}) - (1 - \frac{c}{2} - \frac{\varepsilon}{2}) = -\frac{\varepsilon}{2}$$

For example, the school benefit to the screening qualified students is $1 + L(\Delta \bar{s}) = 1 - l\varepsilon$ for $L = 2l$. This highlights the fact that the change in school impacts is directly related to the change in peer composition.

¹⁹Similar results hold if $b = \bar{s}^{pre}$ under full screening, and the benefit is $\bar{s}^{post} + k\varepsilon$ and $\bar{s}^{post} - l\varepsilon$ in the post-reform for lottery beneficiaries and screening-qualified respectively.

²⁰Details on solving the model are found in Appendix C.2

2. A partial lottery is optimal (i.e. $0 < \varepsilon_+^* < 1 - c$) when $\delta \geq cl$ and $k \in (-\infty, \underline{k})$.

3. Otherwise, a full lottery is optimal (i.e. $\varepsilon = 1 - c$).

$$\varepsilon_+^* = c \sqrt{1 + \frac{cl - \delta}{(1 + \delta)ck}} - c$$

$$\bar{k} = \frac{cl - \delta}{(1 + \delta)(1 - c)} \quad \underline{k} = \bar{k} \frac{c}{c + 1}$$

The following table sums up the preceding cases:

Table 1: Optimal Lottery Conditions

δ	k	optimal $\varepsilon \in [0, 1 - c]$
$\delta < cl$	$k \in (-\infty, \bar{k})$	0
$\delta < cl$	$k \in [\bar{k}, \infty)$	$1 - c$
$\delta \geq cl$	$k \in (-\infty, \underline{k})$	ε_+^*
$\delta \geq cl$	$k \in [\underline{k}, \infty)$	$1 - c$

Figure 14 shows these optimal lottery regions, plotted for $l = 1.5, c = 0.5, k \in [-1, 1]$, and $\delta \in [0, 3]$. In the lower right quadrant of Figure 14 (where $\delta \geq cl, k \leq 0$), a partial lottery with ε_+^* is optimal when $k < \underline{k}$. Intuitively, the weight δ placed on improving access is high enough to balance out the negative impacts on the screening-qualified (cl), so some lottery is optimal. As the impact on lottery beneficiaries improves (k increases) and the overall weighted benefit increases, it becomes optimal to lower the eligibility cutoff by increasing ε ²¹. Ultimately, when k is high enough, a full lottery is optimal, which corresponds to Row 4 in Table 1. Meanwhile, when $k \leq 0$ and $\delta < cl$, no lottery is optimal. In this case, the benefit from increasing access does not outweigh the cost to the screening-qualified.

²¹Figure B18 in Appendix B depicts the optimal value of ε on this region. The optimal ε value is increasing in δ and in k .

Given the empirical estimates in Table 7, the pre-reform school impact is greater than the post-reform school impact on the lottery beneficiaries (i.e., $\beta_{pre} > \beta_{post}^l$). In the context of this model, the results imply that $k < 0$. Therefore, the region below $k = 0$ is of particular practical relevance for determining whether a partial lottery is optimal.

If $k > 0$, a partial lottery is never optimal (as seen in Figure 14). As δ decreases, there must be increasing benefit for the lottery beneficiaries for a lottery to be optimal. In this region, if k is sufficiently high, then the improved school impact for the lottery beneficiaries outweighs the negative impacts on the screening eligible, and a full lottery is optimal.

This model of lottery admissions highlights the tradeoff between expanding access and peer effects. Because the enrollment effect depends on peer composition, the unweighted school effects might be lower overall. Despite this fact, given a high enough value placed on increasing access, it may be optimal to open up the school to some additional applicants using a (partial) lottery format.

6 Conclusion

This paper evaluates and compares the causal impacts of attending elite, oversubscribed high schools in New York City under two distinct admissions regimes: fully screened selection and partial lottery. By leveraging a 2021 policy reform that dramatically altered admissions procedures at the screened select schools, I identify how school attendance effects shift when the composition of admitted students changes. My findings indicate that the screened select schools did provide net positive impacts on SAT scores under full screening. Meanwhile, under lottery admissions, students who would have been admitted under screening saw declines in school effects post-reform, while students who gained access through the lottery saw improvements. A key mechanism behind these results is peer composition. These results underscore the tradeoffs inherent in balancing access with enrollment effects. To formalize these tradeoffs, I introduce a stylized model of admissions that incorporates peer effects and

heterogeneous Pareto weights across student groups. The model yields policy-relevant implications: under plausible parameter ranges that reflect my empirical findings, a partial lottery can be socially optimal if there is a sufficiently high Pareto weight on increasing access for lower-baseline students.

Overall, these results show that there are potential benefits of more equity-oriented admissions as implemented in New York City. However, the overall social impact depends on the value of increasing access, as well as the interplay between peer effects and student outcomes. As the admission processes to elite high schools schools continue to evolve, it is important to account for the tradeoffs between access and academic outcomes to avoid unintended consequences.

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7 Tables

Table 2: Average SAT Scores (2015-2020)

Screened Select Schools	1283 (161)
Specialized High Schools	1407 (101)
High Schools (other)	961 (186)
25th percentile (other)	841
75th percentile (other)	969
95th percentile (other)	1180

Notes: This table reports the average SAT scores at the screened select schools, specialized high schools, and all other NYC public high schools from the 2015 to 2020 school year. The final three rows show the 25th, 75th, 95th percentile scores at all other high schools (not including screened select or specialized schools).

Table 3: School Characteristics (2018-2020)

	Screened Select Schools			
	NYC (1)	Apply (2)	Offered (3)	Enroll (4)
URM	0.68	0.40	0.17	0.24
Non-URM	0.32	0.60	0.83	0.76
Free/Reduced Lunch	0.75	0.59	0.36	0.37
Same District	0.07	0.23	0.51	0.56
Female	0.50	0.56	0.57	0.57
Student with Disability	0.15	0.06	0.09	0.14
Math Baseline	0.00	0.82	1.49	1.19
ELA Baseline	0.00	0.76	1.30	1.05
Observations	187491	25853	3385	2241

Notes: This table reports the average school composition for different student characteristics for applicants from 2018-2020. Column (1) reports characteristics among all high school applicants in NYC. Column (2) reports the share of each group among all ninth grade applicants to the screened select schools. Column (3) reports the group characteristics for all offered students. Column (4) reports the group characteristics for all enrolled students. The final two rows show the average baseline scores on standardized tests taken by 6th graders in New York.

Table 4: Number of Applicants By Tier (Lottery Admissions, 2021)

Tier	Tier 1	Tier 2	Tier 3	Tier 4	Tier 5	Tier 6
Offered	116	0	0	0	0	0
Total number of applicants	2,337	213	110	46	17	16

Notes: This table shows the number of applicants in the sharp sample (applicants who did not receive an offer from a school that they ranked higher) who qualify for each tier at Eleanor Roosevelt High School under the 2021 partial lottery admissions, and the number of offers made within each tier group.

Table 5: Positive Risk Set

	Pre-Reform (2018-2020)			Lottery Admissions (2021)		
	Apply (1)	Risk Set (2)	Offered (3)	Apply (4)	Risk Set (5)	Offered (6)
URM	0.40	0.19	0.17	0.42	0.26	0.26
Non-URM	0.60	0.81	0.83	0.58	0.74	0.74
FRPL	0.59	0.38	0.36	0.63	0.56	0.60
Same District	0.23	0.55	0.51	0.20	0.26	0.30
Female	0.56	0.56	0.57	0.56	0.56	0.53
Student w/ Disability	0.06	0.06	0.09	0.07	0.03	0.15
Math Baseline	0.82	1.23	1.49	0.69	1.15	1.03
ELA Baseline	0.76	1.07	1.30	0.65	1.06	0.91
Observations	25853	1235	3385	12962	5055	1009

Notes: This table reports baseline characteristics for the population of applicants to the screened select schools in the pre-reform era and the post-reform era. Columns 1 and 4 show characteristics for applicants to the screened select schools. Columns 2 and 5 show characteristics for applicants who are in the risk set (i.e., their propensity score is between 0 and 1). Columns 3 and 6 show characteristics for applicants who receive an offer from a screened select school.

Table 6: Covariate Balance

	2018-2021		Pre-Reform (2018-2020)			Post-Reform (Lottery) (2021)					
						All		Lottery Beneficiaries		Screening-Qualified	
	Offered (1)	Sim. Offer (2)	Offered (3)	Sim. Offer (4)	Offered (5)	Sim. Offer (6)	Offered (7)	Sim. Offer (8)	Offered (9)	Sim. Offer (10)	
Asian	-0.006 (0.016)	-0.001 (0.015)	0.009 (0.029)	-0.008 (0.030)	-0.004 (0.019)	-0.007 (0.017)	-0.004 (0.023)	-0.006 (0.021)	-0.001 (0.033)	-0.005 (0.029)	
Black	-0.014** (0.007)	0.002 (0.006)	-0.028** (0.012)	-0.019 (0.012)	-0.006 (0.008)	0.008 (0.008)	-0.010 (0.012)	0.003 (0.010)	-0.002 (0.010)	0.016* (0.008)	
Hispanic	0.015 (0.012)	0.009 (0.011)	0.007 (0.018)	0.013 (0.018)	0.019 (0.014)	0.007 (0.013)	0.033* (0.019)	0.012 (0.017)	-0.017 (0.019)	-0.006 (0.016)	
ELL	0.000 (0.003)	0.004 (0.003)	-0.003 (0.003)	0.000 (0.003)	0.002 (0.004)	0.005 (0.003)	0.003 (0.005)	0.008 (0.005)	0.000 (.)	0.000 (.)	
Student w/ Disability	0.009 (0.006)	0.009* (0.005)	0.013 (0.014)	0.012 (0.014)	0.008 (0.006)	0.009* (0.005)	0.011 (0.008)	0.012 (0.007)	0.002 (0.007)	0.004 (0.006)	
Female	0.002 (0.016)	0.018 (0.015)	0.012 (0.030)	0.065** (0.030)	-0.004 (0.019)	0.004 (0.017)	-0.012 (0.023)	0.004 (0.021)	0.017 (0.033)	0.006 (0.029)	
FRPL	0.044*** (0.016)	-0.001 (0.015)	0.031 (0.029)	-0.008 (0.029)	0.057*** (0.019)	-0.008 (0.017)	0.066*** (0.023)	-0.005 (0.021)	0.028 (0.033)	-0.018 (0.029)	
ELA Baseline	0.002 (0.024)	0.025 (0.022)	-0.005 (0.044)	0.044 (0.044)	-0.001 (0.028)	0.015 (0.025)	-0.001 (0.033)	0.022 (0.030)	0.036 (0.044)	0.024 (0.039)	
Math Baseline	-0.035 (0.026)	0.017 (0.024)	0.013 (0.044)	-0.001 (0.045)	-0.055* (0.031)	0.011 (0.028)	-0.030 (0.037)	0.026 (0.033)	-0.065 (0.047)	0.010 (0.041)	
Observations	5818	5818	1133	1133	4685	4685	3062	3062	1623	1623	

Notes: This table reports coefficients from regressing the covariate in each row on a dummy for a screened select school offer while controlling for the DA-propensity score and year. Columns 1, 3, 5, 7, and 9 are computed from a regression on the dummy for the actual offer, and Columns 2, 4, 6, 8, and 10 are computed from a regression on the dummy for a simulated offer (based on my match replication). The sample is restricted to applicants who are enrolled in the district in 8th grade. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 7: Screened Select Enrollment Effects across Years

	Pre-Reform				Post-Reform (Lottery)			
			All		Lottery Beneficiaries		Screening-Qualified	
	SAT Math (1)	SAT English (2)	SAT Math (3)	SAT English (4)	SAT Math (5)	SAT English (6)	SAT Math (7)	SAT English (8)
Panel A: 2SLS								
Enrollment Effect	15.674** (7.196)	17.997*** (6.562)	4.482 (3.897)	5.650 (3.571)	11.544** (4.521)	5.584 (4.088)	-12.564* (7.216)	7.393 (6.765)
Non-offered Mean	648	639	639	630	608	605	696	678
p-value ($\beta = \beta_{pre}$)			0.196	0.105			0.006	0.248
Observations	995	995	4242	4242	2742	2742	1500	1500
Panel B: First Stage								
Offer	0.634*** (0.023)		0.769*** (0.015)		0.847*** (0.016)		0.624*** (0.030)	
Observations	995		4242		2742		1500	
Panel C: Attrition								
Takes SAT	-0.020 (0.031)		0.030** (0.013)		0.031* (0.016)		0.031 (0.026)	
Observations	1127		4653		3041		1612	
Panel D: OLS								
Enrollment Effect	9.308*** (1.923)	9.321*** (1.709)	12.551*** (3.013)	9.963*** (2.797)	20.960*** (3.503)	13.094*** (3.270)	-17.220*** (5.476)	-2.082 (5.014)
Non-offered Mean	569	564	568	570	539	544	682	672
Observations	19912	19912	9944	9944	7699	7699	2245	2245

Notes: This table computes screened select school SAT effects across admission regimes. The Pre-Reform columns (1-2) include 2018-2020 applicants. The Post-Reform columns (3-8) include the 2021 applicants. Panel A reports the 2SLS estimates, where receiving an offer is the instrument for enrollment. Panel B reports the first stage impacts of receiving an offer on enrollment. Panel C reports the 2SLS estimates of attrition, where the outcome variable is whether the student takes the SAT during or before 11th grade. Panel D reports OLS estimates. All 2SLS and OLS estimates include controls for gender, race, English Language Learner status, disability status, Free/Reduced Price Lunch eligibility, district information, and baseline test scores. Panels A, B, and D also include controls for the year of SAT. Panels A-C linearly control for propensity score. $\beta = \beta_{pre}$ in Columns 7 and 8 reports the p-value for the t-test comparing the post-reform screening-qualified school effect to the pre-reform school effect (Columns 3 and 4 are analogous). Robust standard errors are reported in parentheses. The sample is restricted to applicants who are enrolled in the district in 8th grade. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 8: School Effects under RD Risk vs Lottery Risk

	SAT Math (1)	SAT English (2)
RD Risk		
Enrollment Effect (β_{pre})	20.316*** (7.033)	14.653** (6.166)
Non-offered Mean	598	591
Observations	1708	1708
Lottery Risk		
Enrollment Effect	4.731 (4.087)	5.002 (3.817)
Non-offered Mean	645	636
Observations	3799	3799
Lottery Beneficiaries		
Enrollment Effect	13.002*** (4.808)	4.585 (4.495)
Non-offered Mean	613	610
Observations	2312	2312
Screening-Eligible		
Enrollment Effect (β_{se})	-12.805* (7.201)	6.758 (6.739)
Non-offered Mean	697	680
Observations	1487	1487
p-value ($\beta_{pre} = \beta_{se}$)	0.0010	0.3874

Notes: This table computes the 2SLS estimates of the screened select enrollment effects on SAT scores students who are subject to RD risk as opposed to lottery risk. The RD risk category includes all applicants with positive risk under full screening (in the 2018-2020 sample) in addition to 2021 applicants who are within the RD bandwidth of the top tier group cutoff. Bandwidths use the formulas in Imbens and Kalyanaraman (2011). Controls are the same as Panel A of Table 7.
 *significant at 10%; **significant at 5%; ***significant at 1%.

Table 9: Counterfactual School Destinies (2020)

	All Applicants	$p_i < 0.005$	$p_i \in (0.005, 1]$	Compliers		
	(1)	(2)	(3)	All	$p_i < 0.005$	$p_i \in (0.005, 1]$
Panel A: Offered						
Unscreened	0.00	-	0.00	0.00	0.00	0.00
Mixed	0.00	0.00	-	0.00	0.00	0.00
Elite	1.00	0.97	1.00	1.00	1.00	1.00
Observations	1,112	53	1,059	716	47	669
Panel B: Non-Offered						
Unscreened	0.05	0.05	-	0.05	0.05	-
Mixed	0.52	0.53	0.17	0.52	0.53	0.17
Elite	0.44	0.42	0.82	0.44	0.42	0.81
Observations	10,565	10,141	424	10,550	10,133	417

Table 10: Counterfactual School Destinies (2021)

	All Applicants	Lottery Beneficiaries	Screening Qualified	Compliers		
	(1)	(2)	(3)	All	Lottery Beneficiaries	Screening Qualified
Panel A: Offered						
Unscreened	0.00	-	0.00	0.00	0.00	0.00
Mixed	0.01	-	0.00	0.00	0.00	0.00
Elite	0.99	0.99	1.00	1.00	1.00	1.00
Observations	4,321	683	298	2,732	581	188
Panel B: Non-Offered						
Unscreened	0.05	0.06	0.02	0.05	0.06	0.02
Mixed	0.48	0.58	0.16	0.48	0.58	0.16
Elite	0.47	0.36	0.82	0.47	0.36	0.82
Observations	34,484	9,723	2,256	34,373	9,718	2,256

Notes: These tables show the destiny school type for screened select applicants in 2020 (Table 9) and in the post-reform sample (10). School types are divided into the following categories: The Unscreened category consists of Zoned and Open enrollment programs. The Mixed category consists of Ed. Opt programs. The Elite category includes fully screened and specialized schools. Table 9 shows the enrollment patterns in 2020 (the final pre-reform year), and 2021 shows the enrollment patterns in 2021. Panel A reports the destiny type for offered applicants, and Panel B reports the destiny type for non-offered applicants.

Table 11: Peer Math Composition

	Grade 9 (1)	Grade 10 (2)	Grade 11 (3)
Test-Based Admissions (pre-reform)			
Enrollment Effect (β_{pre})	0.274*** (0.043)	0.311*** (0.043)	0.230*** (0.039)
Non-offered Mean	0.91	0.94	0.99
Observations	1084	1073	1053
Lottery Admissions (post-reform)			
Enrollment Effect	0.247*** (0.025)	0.149*** (0.024)	0.161*** (0.022)
Non-offered Mean	0.83	0.90	0.96
Observations	4565	4467	4375
Lottery Beneficiaries			
Enrollment Effect	0.344*** (0.028)	0.251*** (0.027)	0.249*** (0.025)
Non-offered Mean	0.63	0.71	0.78
Observations	2972	2901	2830
Screening-Qualified			
Enrollment Effect (β_{se})	0.003 (0.049)	-0.105** (0.045)	-0.054 (0.040)
Non-offered Mean	1.20	1.26	1.30
Observations	1593	1566	1545
p-value ($\beta_{pre} = \beta_{se}$)	0.0000	0.0000	0.0000

Notes: This table computes 2SLS estimates of screened select school effects on peer baseline composition across admission regimes. The Test-Based Admissions panel consists of 2018-2020 applicants with propensity scores $\in (0, 1)$. The Lottery Admissions panel consists of 2021 applicants with propensity scores $\in (0, 1)$. In Column 1, a student's average baseline exposure in 9th grade is measured as the average baseline math score in their math class in 9th grade (and likewise for Columns 2 and 3). Estimates include controls for gender, race, English Language Learner status, disability status, Free/Reduced Price Lunch eligibility, district information, and baseline test scores. All columns linearly control for propensity score. Robust standard errors are reported in parentheses. The sample is restricted to applicants who are enrolled in the district in 8th grade. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 12: Peer Math Composition Split by Math Track

	Algebra Track	Geometry Track
Test-Based Admissions (pre-reform)		
Enrollment Effect β_{pre}	0.367*** (0.071)	0.251*** (0.047)
Non-offered Mean	0.32	1.07
Observations	205	854
Lottery Admissions (post-reform)		
Enrollment Effect	0.334*** (0.042)	0.202*** (0.029)
Non-offered Mean	0.34	0.95
Observations	985	3480
Lottery Beneficiaries		
Enrollment Effect	0.349*** (0.044)	0.310*** (0.032)
Non-offered Mean	0.23	0.77
Observations	778	2141
Screening-Qualified		
Enrollment Effect (β_{se})	0.214** (0.107)	-0.026 (0.055)
Non-offered Mean	0.80	1.25
Observations	207	1339
p-value ($\beta_{pre} = \beta_{se}$)	0.2363	0.0001

Notes: This table computes 2SLS estimates of screened select school effects on peer baseline composition across admission regimes split by math track. The Test-Based Admissions panel consists of 2018-2020 applicants with propensity scores $\in (0, 1)$. The Lottery Admissions panel consists of 2021 applicants with propensity scores $\in (0, 1)$. Column 1 restricts to students who take Algebra in 9th grade (the normal track). Column 2 restricts to students who take Geometry in 9th grade (the accelerated track). A student's average baseline exposure is measured as the average baseline math score in their math classes from grades 9-11. Estimates include controls for gender, race, English Language Learner status, disability status, Free/Reduced Price Lunch eligibility, district information, and baseline test scores. All columns linearly control for propensity score. Robust standard errors are reported in parentheses. The sample is restricted to applicants who are enrolled in the district in 8th grade. *significant at 10%; **significant at 5%; ***significant at 1%.

Table 13: SAT Math Achievement Split by Math Track

	Algebra Track (1)	Geometry Track (2)
Test-Based Admissions (pre-reform)		
Enrollment Effect (β_{pre})	17.34 (19.75)	24.11** (10.53)
Non-offered Mean	569	669
Observations	128	666
Lottery Admissions (post-reform)		
Enrollment Effect	16.02** (7.82)	2.80 (4.92)
Non-offered Mean	576	655
Observations	899	3255
Lottery Beneficiaries		
Enrollment Effect	14.00* (8.20)	11.24* (5.06)
Non-offered Mean	558	626
Observations	706	1989
Screening-Qualified		
Enrollment Effect (β_{se})	7.30 (18.78)	-12.02 (8.21)
Non-offered Mean	653	701
Observations	193	1266
p-value ($\beta_{pre} = \beta_{se}$)	0.744	0.0221

Notes: This table computes 2SLS estimates of screened select school SAT effects across admission regimes split by student math trajectory. The Test-Based Admissions panel consists of 2018-2020 applicants with positive propensity scores. The Lottery Admissions panel consists of 2021 applicants with positive propensity scores. Column 1 restricts to students who take Algebra in 9th grade (the normal track). Column 2 restricts to students who take Geometry in 9th grade (the accelerated track). Estimates include controls for Free/Reduced Price Lunch eligibility, and baseline test scores. All columns linearly control for propensity score. Robust standard errors are reported in parentheses. The sample is restricted to applicants who are enrolled in the district in 8th grade. *significant at 10%; **significant at 5%; ***significant at 1%.

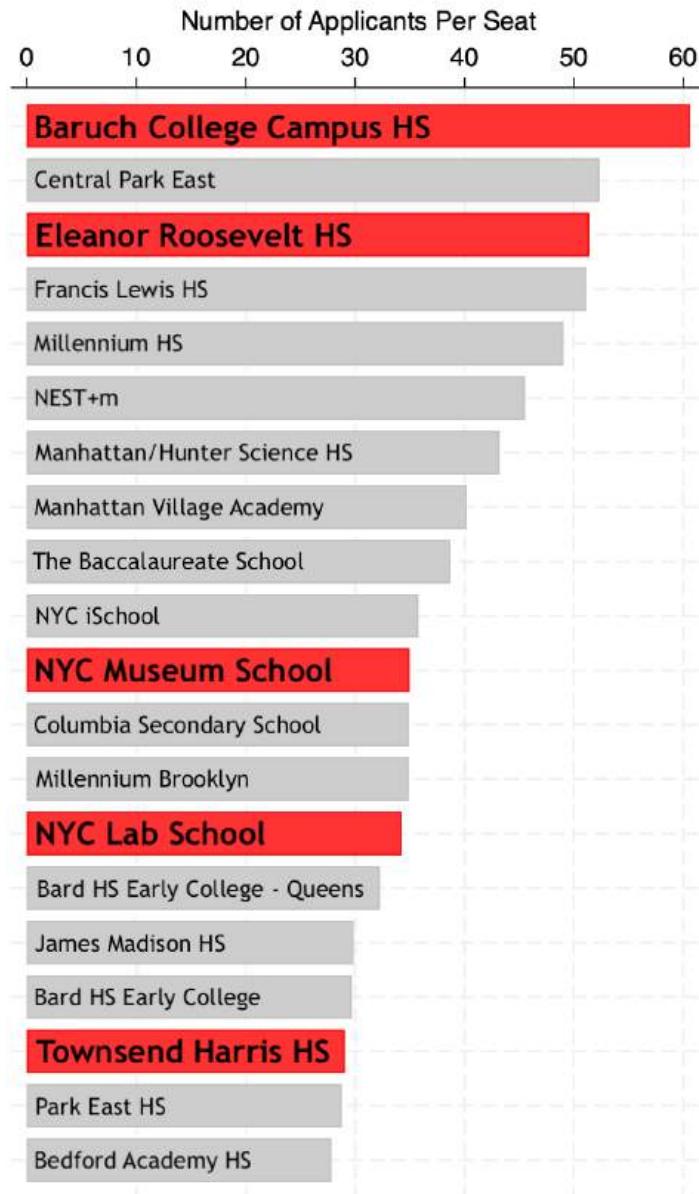
8 Figures

Figure 1: Screened Select School Locations



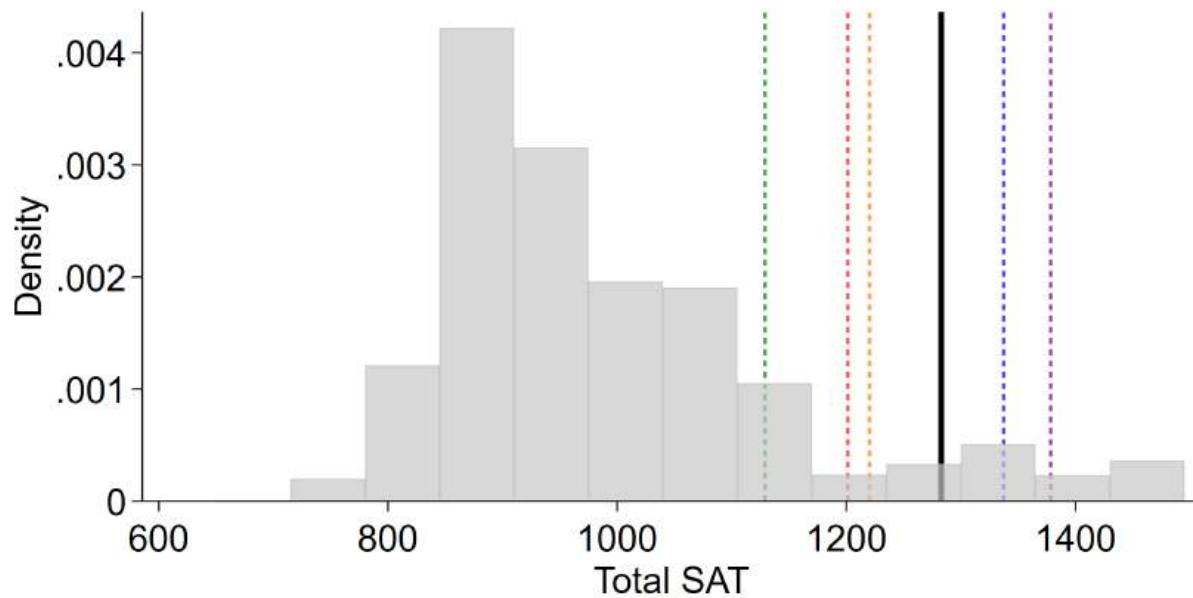
Notes: This figure labels the locations of the screened select high schools with stars. Each borough is labeled in a different color. The schools are 1) Townsend Harris; 2) Eleanor Roosevelt High School; 3) Baruch College Campus High School; 4) N.Y.C. Lab School for Collaborative Studies; 5) N.Y.C. Museum School.

Figure 2: Oversubscription at Screened High Schools



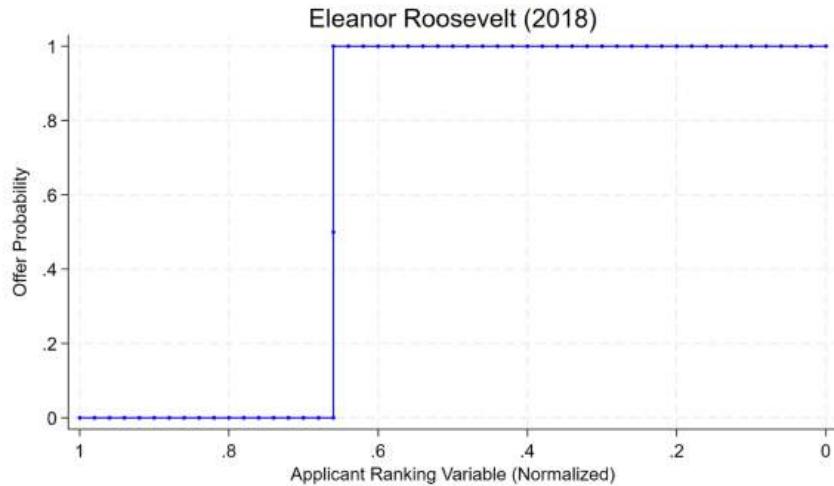
Notes: This figure plots the screened programs with the top 20 number of applicants per seats in the Fall 2020 application cycle. The figure was created using publicly available data on the number of total seats and the number of total applicants by program published by the NYCPS in the 2021 High School Directory (this is publicly available).

Figure 3: Average SAT Scores in NYC



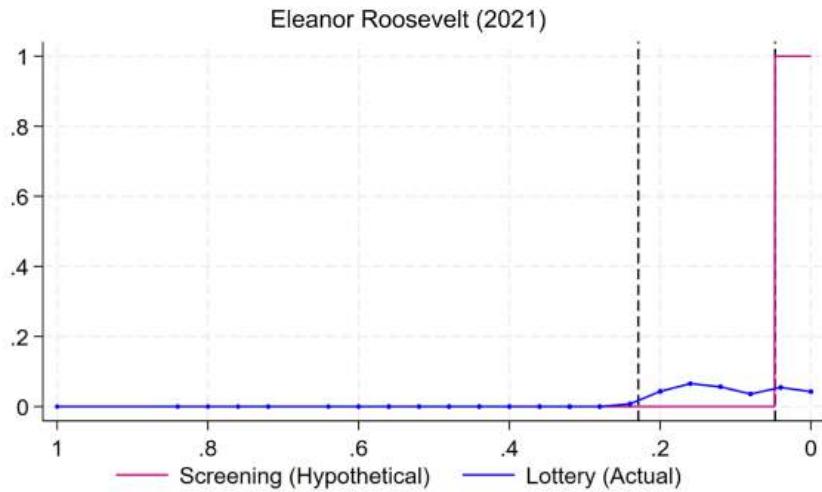
Notes: This histogram plots the average SAT score from 2015-2020 at each school in New York City. The lines denote the SAT average at each screened select school: Red = Baruch College Campus High School; Orange = NYC Lab School; Green = NYC Museum School; Blue = Eleanor Roosevelt High School; Purple = Townsend Harris High School; Black = Average across all screened select schools.

Figure 4: Screened Admissions



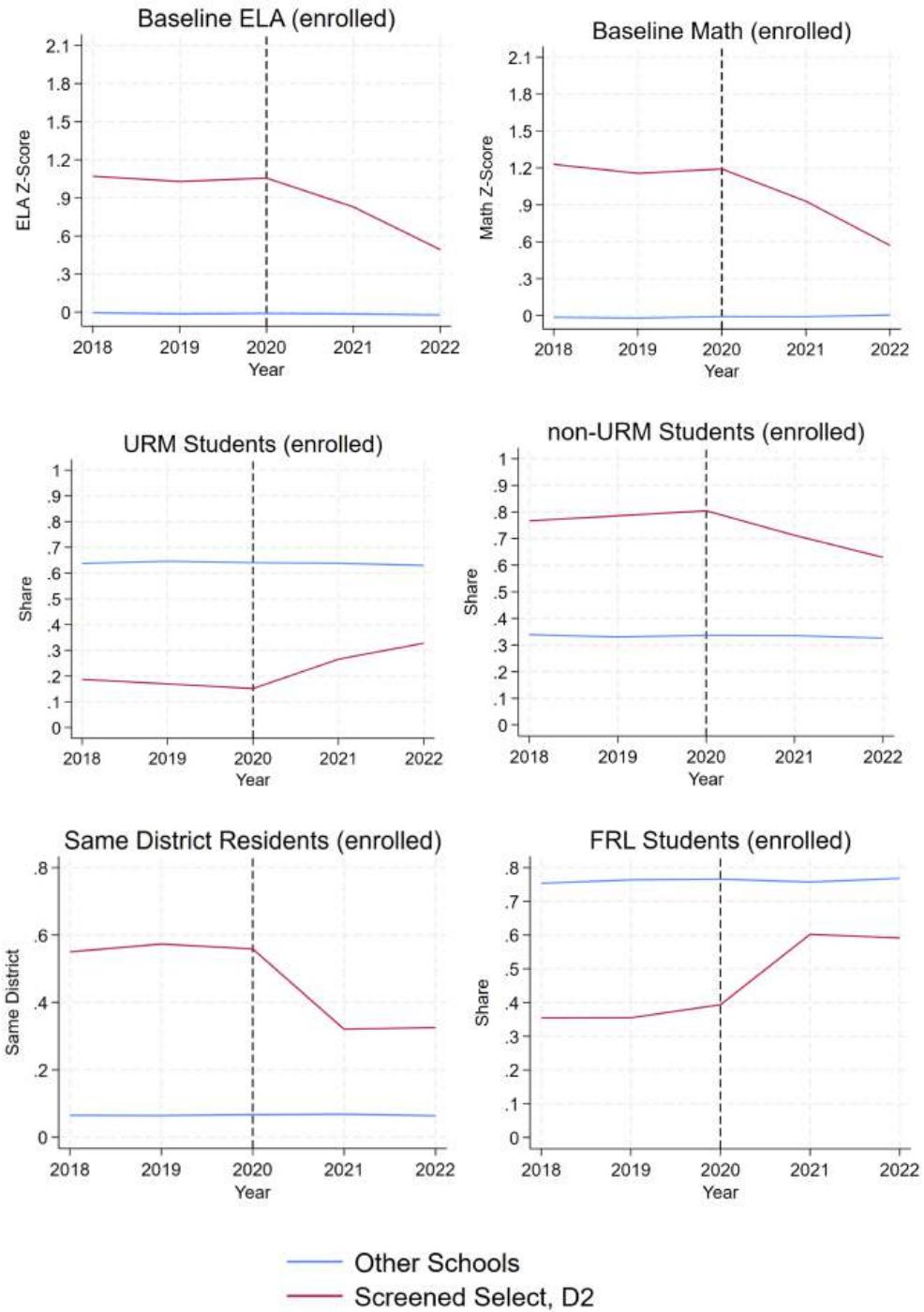
Notes: This figure plots the probability of receiving an offer to Eleanor Roosevelt High School in 2018, binned by the normalized applicant rank (lower normalized numerical rank implies higher priority). This is limited to the sharp sample (applicants who have not received a higher offer).

Figure 5: Lottery Admissions



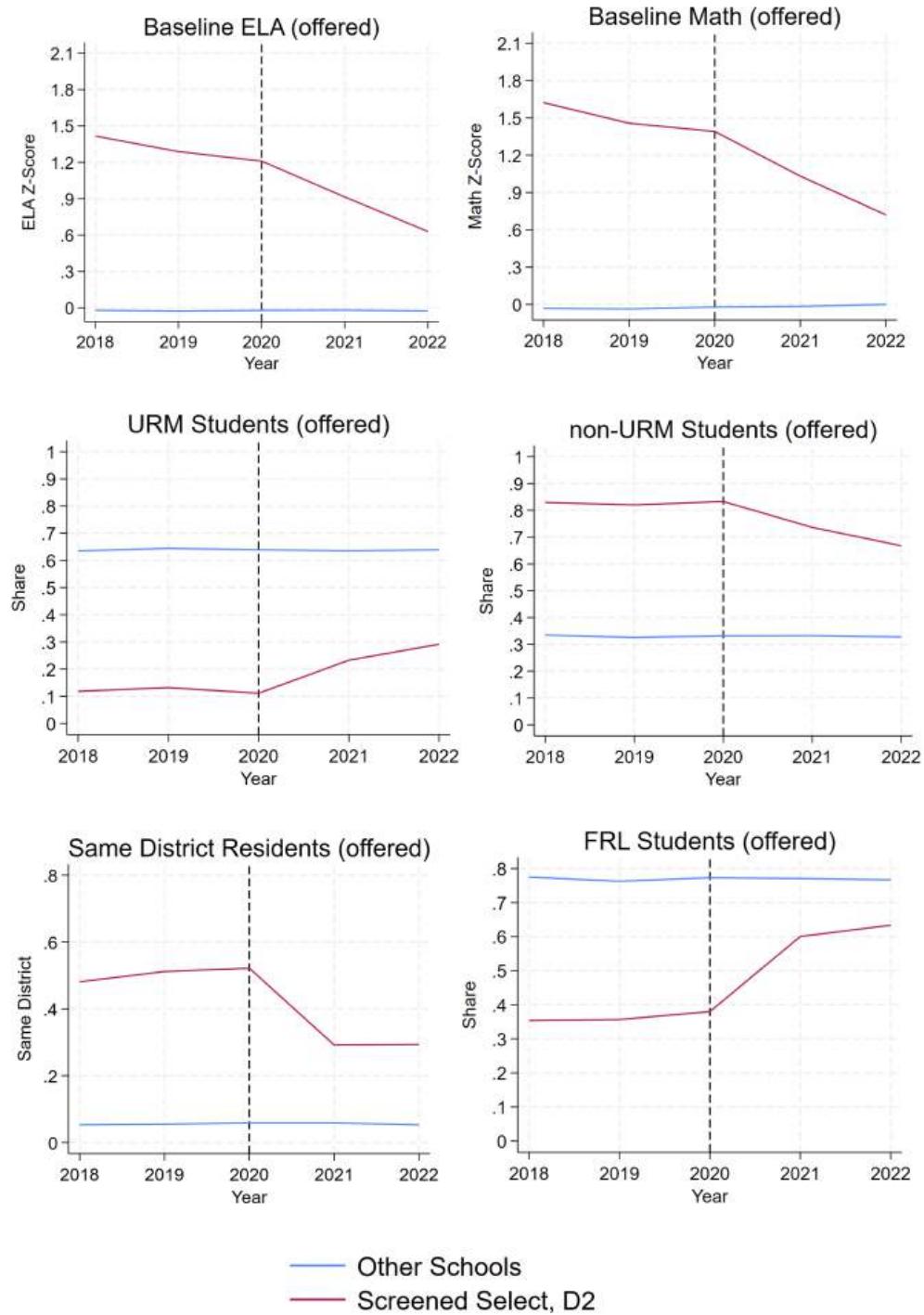
Notes: This figure plots in blue the probability of receiving an offer to Eleanor Roosevelt High School in 2021, binned by the normalized calculated applicant rank, calculated according to the published school rubric. The pink line indicates the hypothetical rank cutoff if only the top scoring applicants were admitted. This is limited to the sharp sample (applicants who have not received a higher offer). The ranking variable in this graph is not comparable to the x-axis in Figure 4.

Figure 6: Enrolled Student Composition



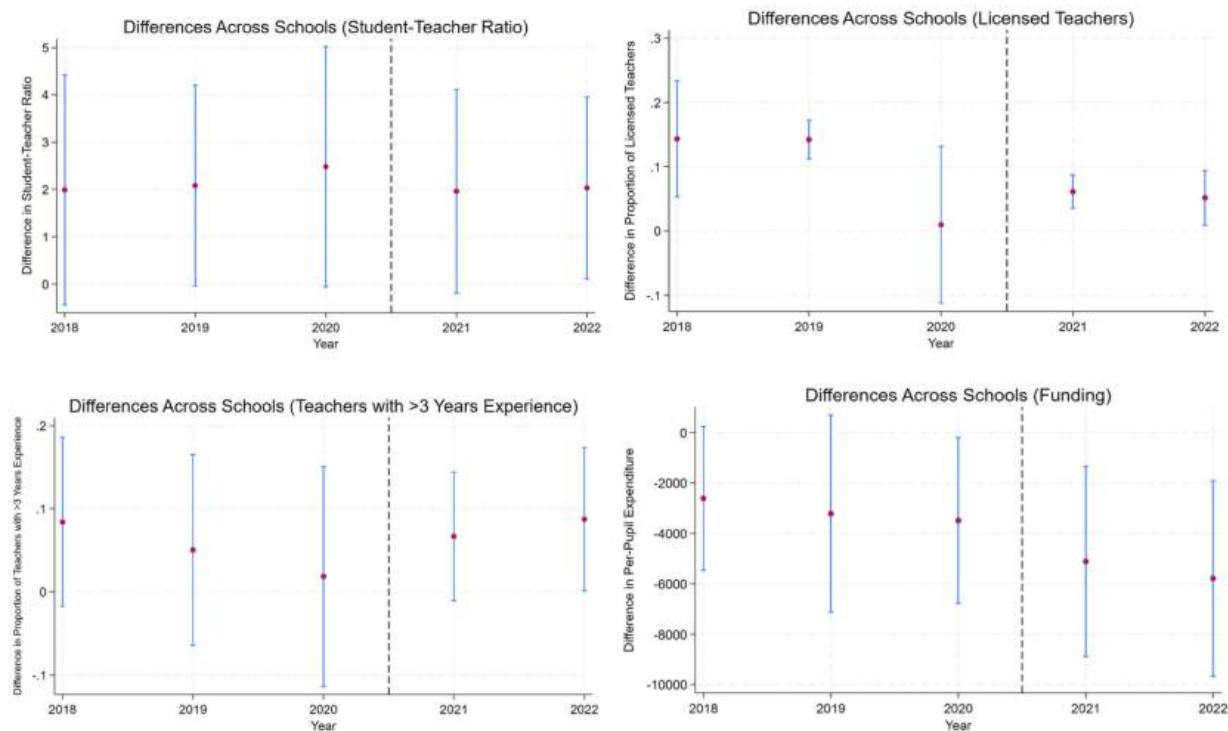
Notes: This figure shows the average characteristics of enrolled students at the screened select schools, and the average characteristics at all other high schools. Baseline test scores denote tests taken by students in 6th grade. The blue lines indicate the average characteristics of students who are enrolled at all other high schools, and the red lines indicate the average characteristics of students who enroll at the screened select schools. The sample is restricted to applicants who are enrolled in the district in 8th grade.

Figure 7: Offered Student Composition



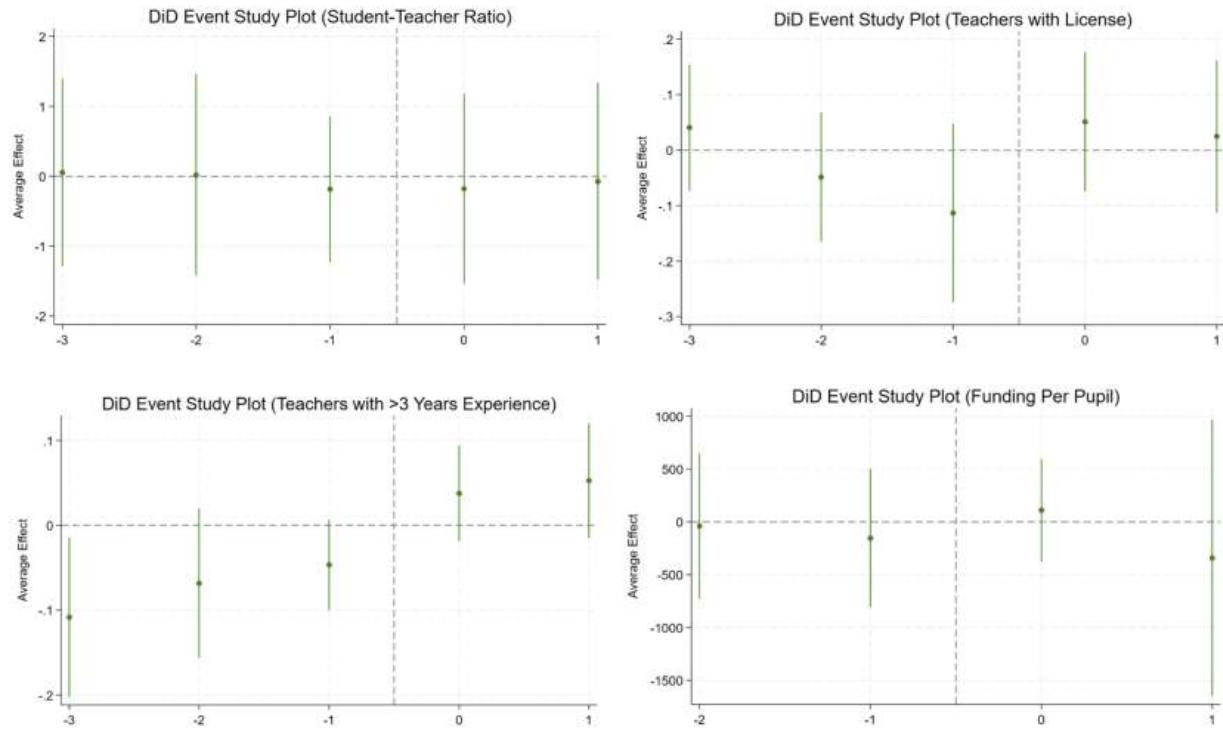
Notes: This figure shows the average characteristics of students who receive an offer at the screened select schools compared with all other high schools. The blue lines indicate the average characteristics across students who receive an offer to any other high schools, and the red lines indicate the average characteristics across students who receive an offer to one of the screened select schools. The sample is restricted to applicants who are enrolled in the district in 8th grade.

Figure 8: School Characteristics



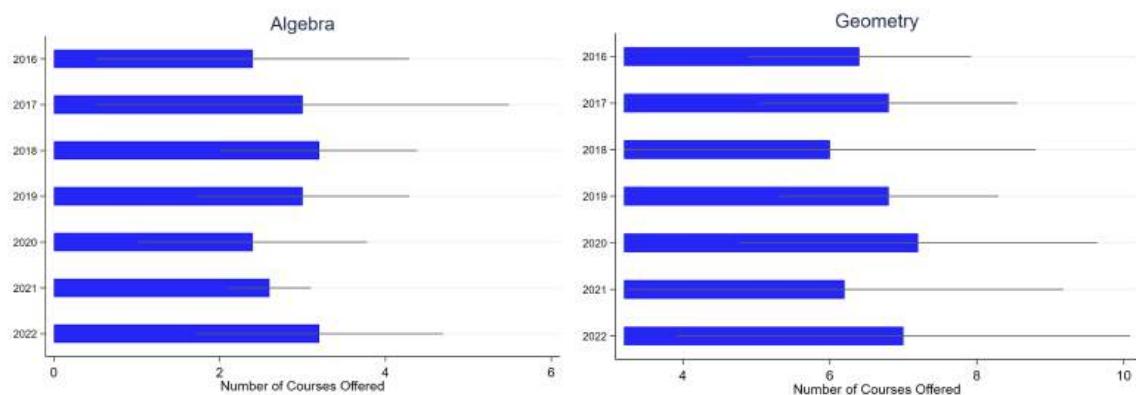
Notes: This figure shows the average difference in school characteristics across years (the student-to-teacher ratio, the proportion of teachers with a license, the proportion of teachers with more than 3 years of experience, and the funding per pupil). The center point indicates the difference between the average at the screened select school and the average across all other districts. The blue lines indicate 95% confidence intervals around the mean difference.

Figure 9: Event Studies for School Characteristics



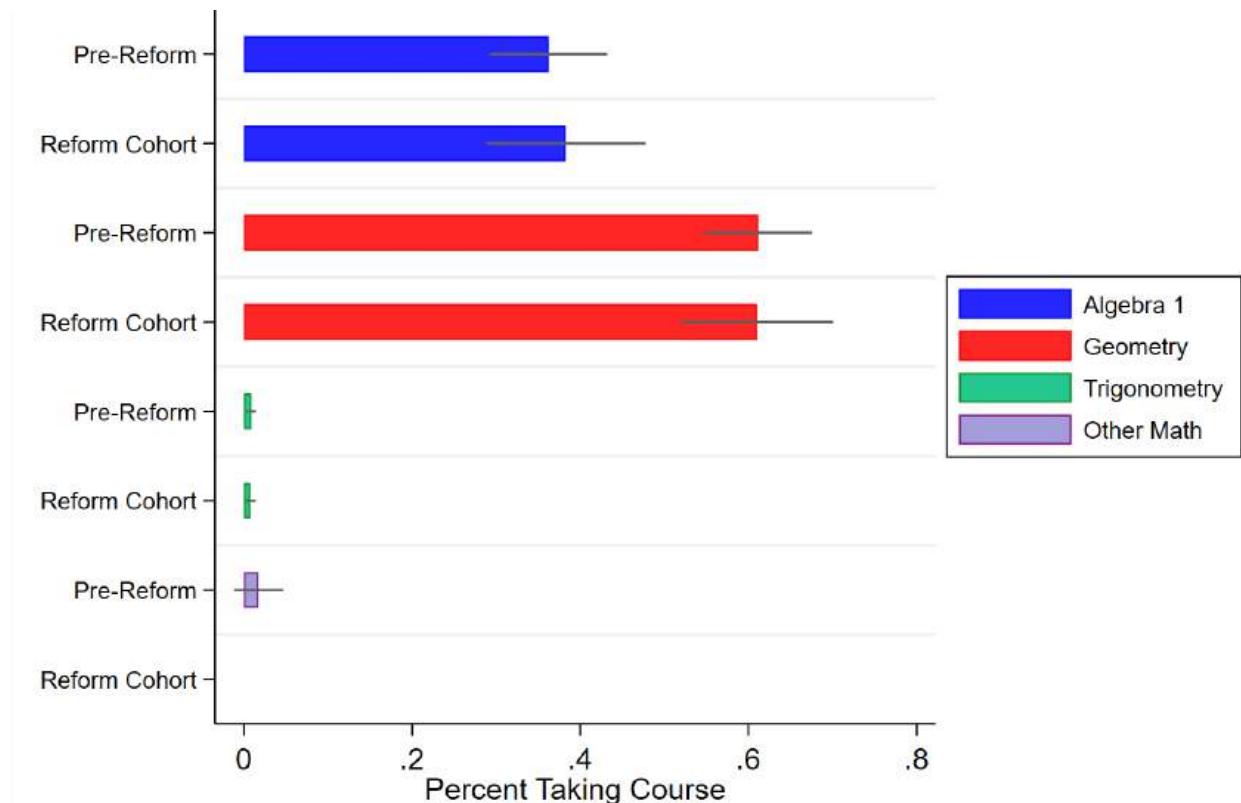
Notes: These figures plot event studies for the changes in school characteristics comparing screened select schools to other NYC high schools, for school characteristics corresponding to those in Figure 8. The x-axis denotes years until 2021 (the year of the lottery reform). Observations are at the school level. The sample includes schools that only serve students from grades 9-12. Regressions follow equation 4.1, and include controls for the proportion of ELL students and FRPL-qualifying students at the school.

Figure 10: Number of Algebra and Geometry Courses Offered



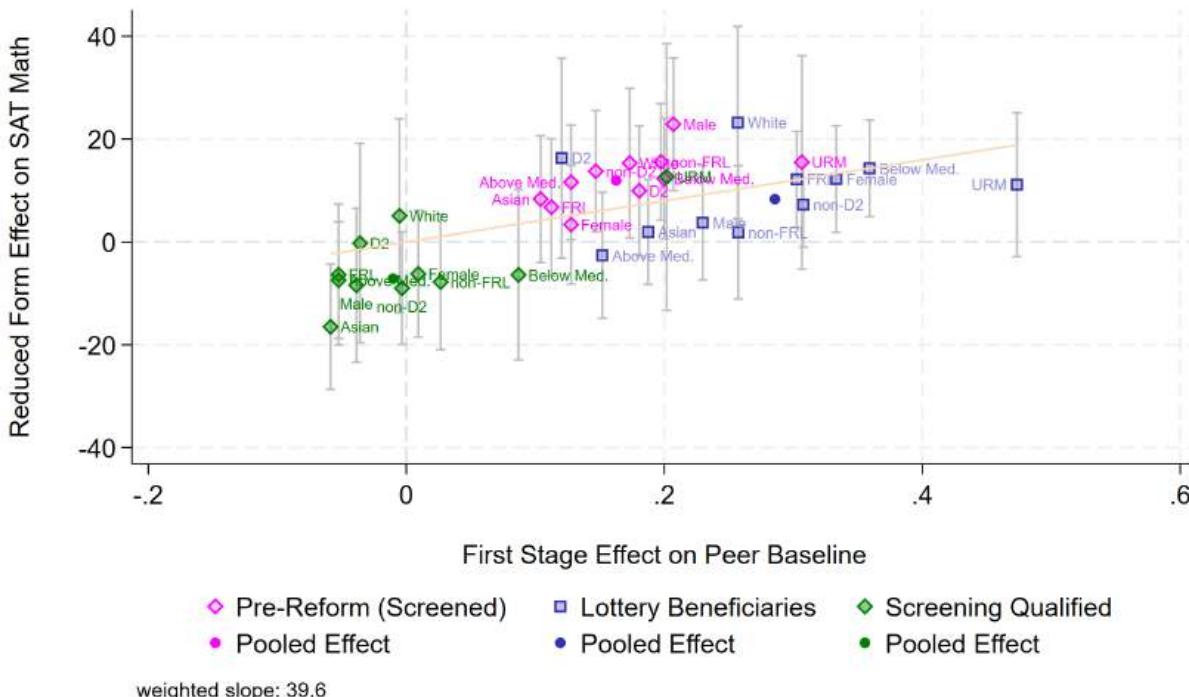
Notes: These plots show the average number of math courses of each type offered at the screened select schools. The lines indicate 95% confidence intervals for the mean.

Figure 11: 9th Grade Math Course



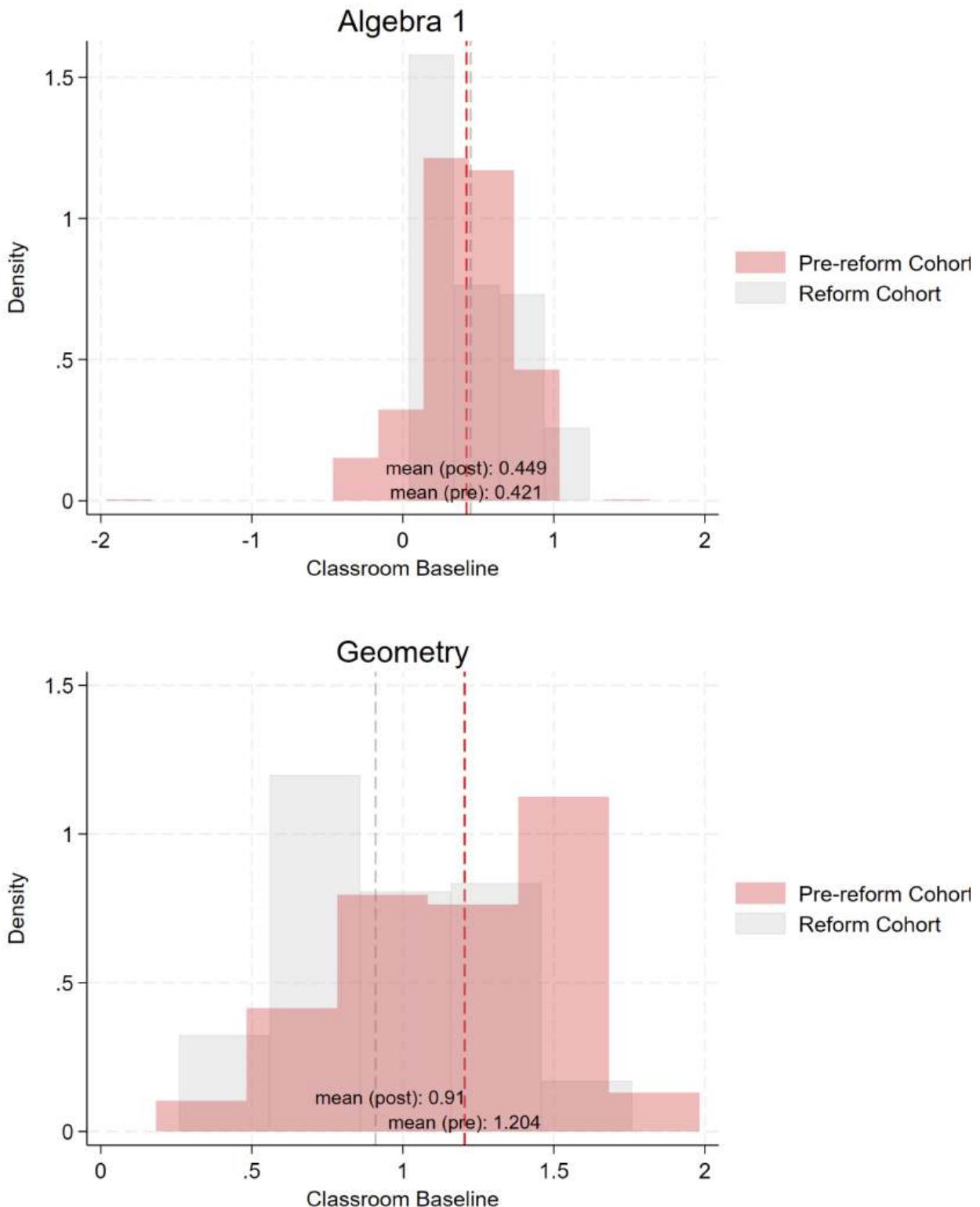
Notes: This plot shows the proportion of ninth graders taking each math course at the screened select schools out of the population of ninth graders taking any math course. The lines indicate 95% confidence intervals for the mean.

Figure 12: Visual IV estimates of peer composition on SAT Math outcomes



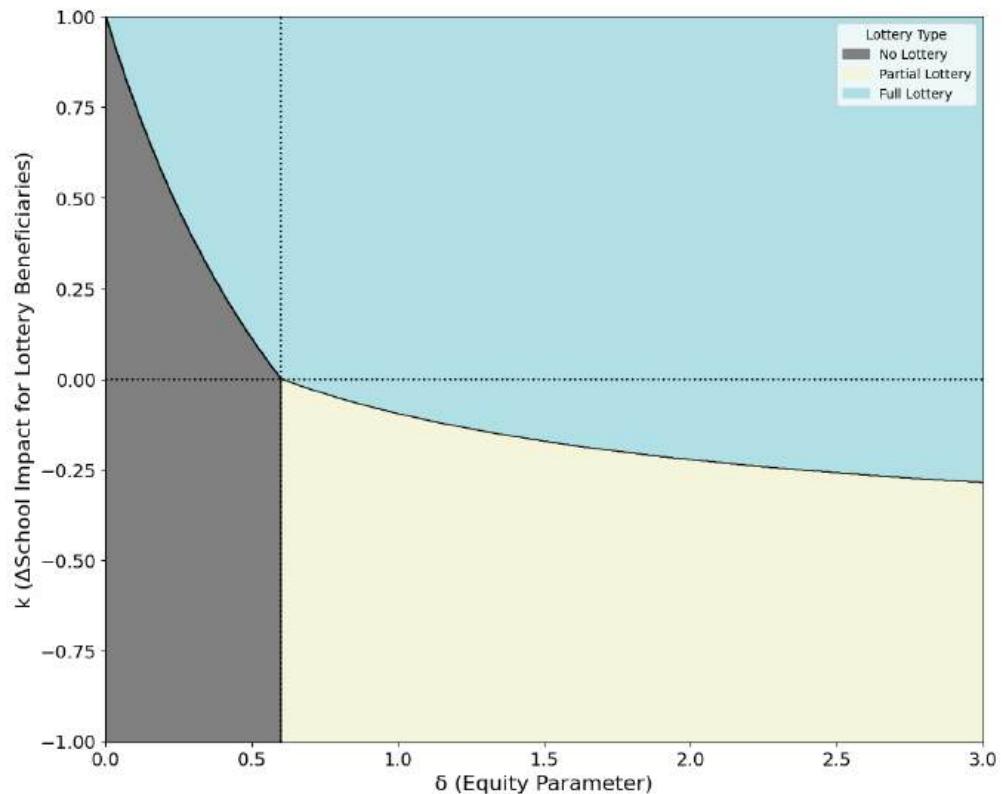
Notes: This is a visual instrumental variable plot of reduced form effects of screened select offers on SAT Math scores against the corresponding first stage effects of screened select offers on peer exposure, computed separately for a set of 11 covariate groups. Peer exposure is measured as the average math baseline score in a student's math classes from grades 9-11. Pre-reform effects are plotted in pink, post-reform effects for lottery beneficiaries are plotted in indigo, and post-reform effects for screening qualified students are plotted in green. The regression line is constrained to run through the origin. Whiskers mark 95 percent confidence intervals.

Figure 13: Baseline Scores by Math Class



Notes: These plots shows the distribution of baseline scores within Algebra I classes and Geometry classes at the screened select schools. The vertical red lines indicate the pre-reform mean classroom baselines, and the vertical gray lines indicate the post-reform mean classroom baselines.

Figure 14: Optimal Admissions Scheme



Notes: This figure shows the regions each different admission scheme is optimal according to the model in Section 5.2. The regions correspond to the results in Proposition 1 and Table 1. This is plotted for parameter values $l = 1.5$, $c = 0.4$, $k \in (-1, 1)$, and $\delta \in (0, 3)$.

A Appendix: Additional Tables

Table A14: Match Replication Rate

Year	Replication Rate	Replication Rate (excluding unmatched)
2018	98.4%	96.7%
2019	84.8%	92.4%
2020	84.8%	92.8%
2021	79.1%	85.0%

Notes: This table shows the match replication rate of the simulated match that I run. The second column indicates the percentage of students who are matched with the same school in the actual match as under the simulated match. The third column indicates the percentage of students who are matched with the same school as in the simulated match, excluding students who are unmatched in the main round of the actual match.

Table A15: BCCHS and LSCS School Characteristics (2018-2020)

	2019			2020			2021		
	Apply (1)	$p \in (0, 1)$ (2)	Offered (3)	Apply (4)	$p \in (0, 1)$ (5)	Offered (6)	Apply (7)	$p \in (0, 1)$ (8)	Offered (9)
URM	0.40	0.17	0.22	0.41	0.13	0.17	0.40	0.22	0.32
Non-URM	0.60	0.83	0.78	0.59	0.87	0.83	0.60	0.78	0.68
FRPL	0.54	0.27	0.25	0.58	0.28	0.46	0.54	0.42	0.69
D2	0.29	0.84	0.84	0.30	0.85	0.82	0.23	0.55	0.40
Female	0.57	0.63	0.54	0.56	0.55	0.46	0.56	0.49	0.51
Math	0.82	1.15	1.03	0.75	1.06	0.87	0.79	1.01	0.77
ELA	0.76	0.95	0.93	0.69	0.89	0.71	0.77	0.89	0.65
Obs	4207	115	306	4226	500	248	4919	767	243

Notes: This table reports baseline characteristics for the population of applicants in 2019, 2020, and 2021 to Baruch College Campus High School and the NYC Lab School for Collaborative Studies. Columns 1, 4, and 7 show characteristics for all applicants to these schools. Columns 2, 5, and 8 show characteristics for applicants who are in the risk set for these schools (i.e., their propensity score is between 0 and 1). Columns 3, 6, and 9 show characteristics for applicants who receive an offer from these schools.

Table A16: Pre-Reform SAT Effects (without BCCHS and LSCS in 2020)

	SAT Math (1)	SAT English (2)
SAT Attrition		
Takes SAT	0.001 (0.037)	0.001 (0.037)
Non-offered Mean	0.86	0.86
Observations	937	937
RD Risk		
Enrollment Effect (β_{pre})	22.147*** (8.435)	13.649* (7.736)
Non-offered Mean	662	653
Observations	817	817

Notes: These tables report results analogous to columns 1-2 of Panel A and Panel C of Table 7, with Baruch College Campus High School and the NYC Lab School Collaborative Studies excluded from the risk sample in 2020 (i.e., the positive risk sample in 2020 only includes applicants with positive risk at the other 3 screened select schools). The sample is otherwise equivalent to the pre-reform sample in Panels A-C of Table 7.

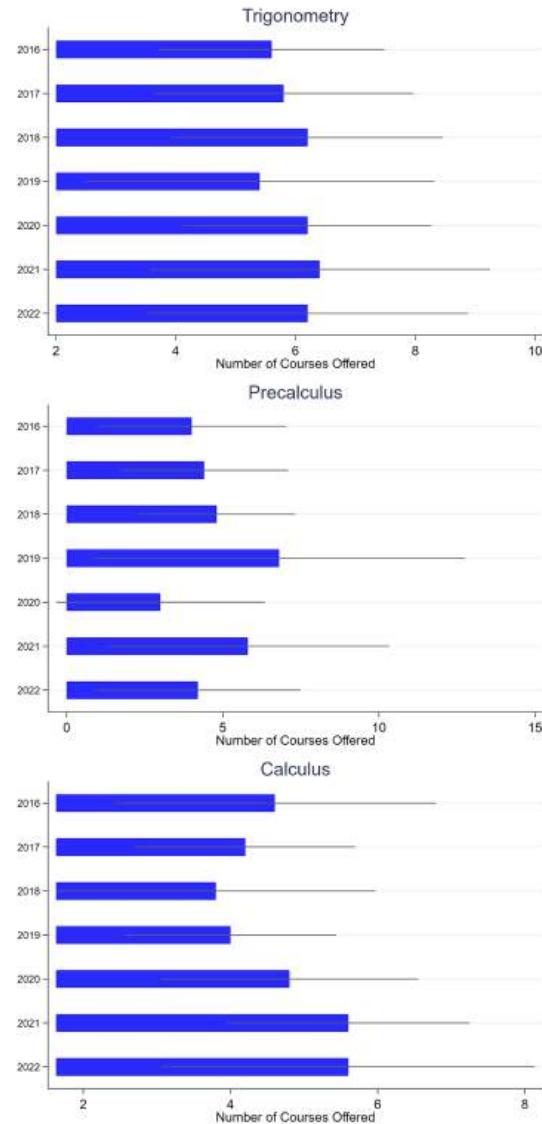
Table A17: 9th Grade Math Regents Outcomes

	Math (any) (1)	Algebra (2)	Geometry (3)
Attrition (Test-Based Admissions)			
Takes Exam	0.224*** (0.042)	-0.001 (0.019)	0.219*** (0.039)
Non-offered Mean	0.25	0.05	0.20
Observations	1127	1127	1127
Attrition (Lottery Admissions)			
Takes Exam	0.102*** (0.012)	0.054*** (0.020)	0.149*** (0.022)
Non-offered Mean	0.87	0.19	0.63
Observations	4653	4653	4653
Test Based Admissions (pre-reform)			
Enrollment Effect (β_{pre})	0.357*** (0.101)	0.236 (0.148)	0.400*** (0.115)
Non-offered Mean	1.15	0.99	1.19
Observations	297	49	241
Lottery Admissions (post-reform)			
Enrollment Effect	0.044* (0.024)	0.069* (0.041)	0.004 (0.030)
Non-offered Mean	1.15	0.81	1.29
Observations	4156	926	2994
Lottery Beneficiaries			
Enrollment Effect	0.067** (0.029)	0.087* (0.047)	0.067* (0.037)
Non-offered Mean	0.96	0.72	1.11
Observations	2646	727	1724
Screening-Qualified			
Enrollment Effect (β_{se})	-0.075* (0.039)	0.010 (0.084)	-0.100** (0.045)
Non-offered Mean	1.45	1.14	1.50
Observations	1510	199	1270
p-value ($\beta_{pre} = \beta_{se}$)	0.0001	0.1704	0.0001

Notes: This table computes screened select school effects on 9th Grade Math Regents exams across admission regimes. The Test-Based Admissions panel consists of 2018-2020 applicants in the risk set. The Lottery Admissions panel consists of 2021 applicants in the risk set. All 2SLS estimates include controls for gender, race, English Language Learner status, disability status, Free/Reduced Price Lunch eligibility, district information, propensity score, and baseline test scores. Robust standard errors are reported in parentheses. The sample is restricted to applicants who are enrolled in the district in 8th grade. *significant at 10%; **significant at 5%; ***significant at 1%.

B Appendix: Additional Figures

Figure B15: Number of Other Math Courses Offered



Notes: These plots show the average number of math courses offered at the screened select schools in Trigonometry, Precalculus, and Calculus. The lines indicate 95% confidence intervals for the mean number of courses.

Figure B16: Example Screened Rubric (2019)



All NYCDOE schools with a screened or audition program are asked to develop and submit a rubric that forms the basis by which students are reviewed and ranked by that program. These rubrics must detail the criteria that are taken into consideration and the weights applied to those criteria when determining a student's rank; e.g. academic performance, standardized test scores, attendance, audition, etc.

2019 THHS Admission Rubric

Review each student's data as follows:

9th grade and 10th grade

- Standardized test scores will count for 40% of the THHS average.
- Math test score 20%
 - Reading test score 20%

Core Academic Grade Average will count for 45% of the THHS average.

- Average (not weighted average) must be greater than or equal to 90

Attendance will count for 15% of the THHS average.

- Days absent must not be greater than 10
- Extenuating Circumstances will be taken into consideration

Public Schools

- Separate: 1) In Queens, rank by high school zone in descending order
2) In other boroughs, rank in descending order

Private Schools

Sort in descending THHS average, and rank in descending order

For 10th grade:

After computing averages, sort all eligible students in descending order without high school zone.

For incoming 9th graders, we use 7th grade core academic grade averages and standardized test scores.

For incoming 10th graders, we use 8th grade core academic grade averages and standardized test scores.

Notes: This shows an example screened rubric under the pre-reform (non-lottery) admissions at one of the screened select schools.

Figure B17: Example Lottery Rubric (2021)

[Admissions](#) » [Admissions Criteria](#)

Admissions Criteria

Welcome to High School Admissions

COVID-19 has had a profound impact across every aspect of our school system, including the admissions process. As a result, NYC is updating our pre-pandemic admissions timeline to ensure ample time for families to explore schools and apply once the application launches. Please note:

- The high school application will open the week of **January 18, 2021**. The deadline to apply is **March 1, 2021**.

We are working closely with the DOE Enrollment office to solidify our screening process. We will continue to update this page, we appreciate your patience.

Virtual Open House
Admissions Criteria
FAQs and Waitlist Information
Open House and events
High School Fairs
Staff

Our Selection Criteria 2021

Admission to THHS Video

URL: <https://drive.google.com/file/d/1nUCoMvjeOV-nXVWKKzadspNEI2xYvys/view?usp=sharing>

Townsend Harris students are life-long achievers. Their dedication to personal development and academic discipline begins long before they reach our school and continues long after.

Students from all five boroughs of New York City are eligible to apply to Townsend Harris and are selected through a competitive screening process rather than by a single entrance exam. Admission for entering ninth-grade students is based on 6th-grade standardized-test scores, final report card grades, and 7th grade marking period grades. Admission for entering tenth-grade students is based on 7th-grade standardized-test scores, final report card grades, and 8th grade marking period grades.

9th Grade Admission	Percentage	10th Grade Admission	Percentage
6th Grade ELA/Math State Test Scores	40%	7th Grade ELA/Math State Test Scores	40%
6th Grade Final Grades	50%	7th Grade Final Grades	50%
7th Grade Marking Period Grades	10%	8th Grade Marking Period Grades	10%

All applicants will be ranked according to their composite rubric score, as follows:

Rank of 1 = 91 - 100%
 Rank of 2 = 86 - 90%
 Rank of 3 = 81 - 85%
 Rank of 4 = 76 - 80%
 Rank of 5 = 71 - 75%
 Rank of 6 = 66 - 70%
 Rank of 7 = Below 66%

Among all applicants with the same rank, starting with the rank of 1, low-income applicants (students who qualify for Free or Reduced Lunch) are considered equally and randomly for the first 50% of offers.

Then, the remaining applicants with the same rank (both low-income and non-low-income) are considered equally and randomly for the remaining 50% of offers. This is done separately for general education students and students with IEPs.

How to Apply

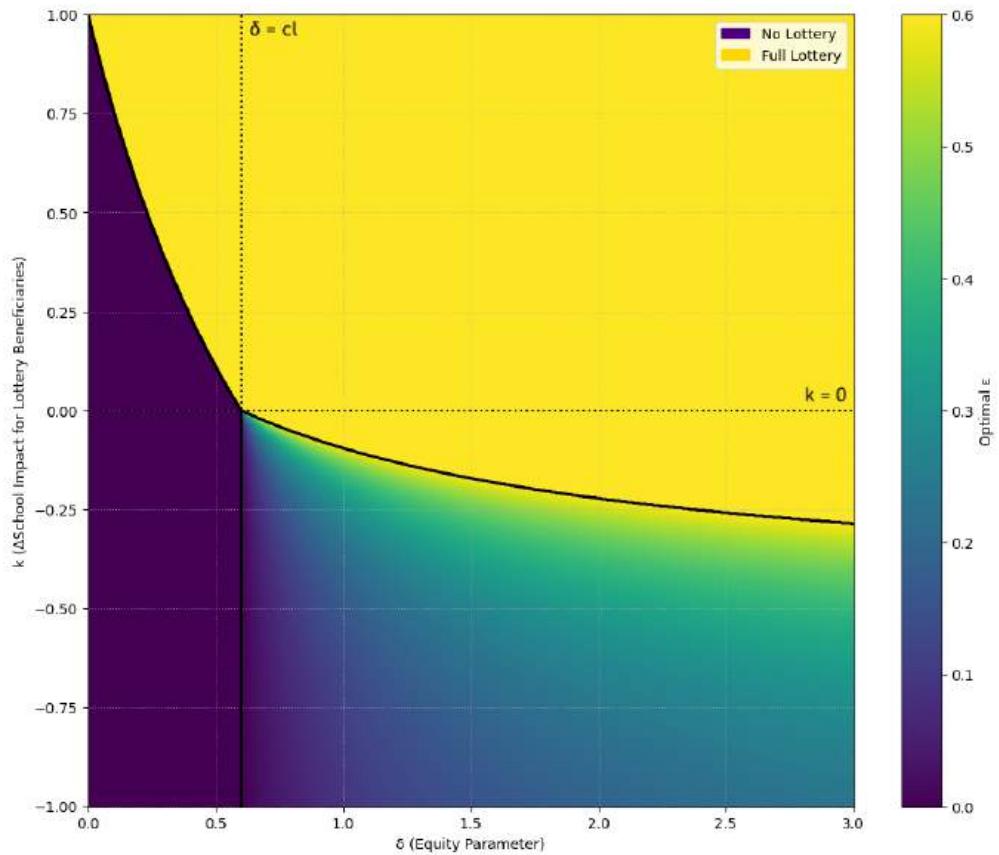
Students apply to Townsend Harris through their home school, whether it is public or private. For more information on the admission process, see your current counselor and/or use this link: <https://www.schools.nyc.gov/enrollment/enroll-grade-by-grade/high-school/high-school-admissions-video-series>

<https://www.schools.nyc.gov/enrollment/enroll-grade-by-grade/high-school>

NYCDOE Tip: For a more detailed look at how offers are made to screened and audition programs, watch the video "How Students Get Offers to Screened Schools and the Specialized High Schools" on our website.

Notes: This shows an example screened rubric under the post-reform (partial lottery) admissions at one of the screened select schools.

Figure B18: Optimal ε values



Notes: This figure shows the optimal values of ε according to the model in Section 5.2. These regions are analogous to those in Figure 14, but the colors in the partial lottery region correspond to the value of the optimal ε . This is plotted for parameter values $l = 1.5, c = 0.4, k \in (-1, 1)$, and $\delta \in (0, 3)$

C Appendix: Other Details

C.1 Deferred Acceptance

Each applicant proposes to his or her most preferred school. Each school ranks these proposals, first by priority, then by tie-breaker within priority groups, provisionally admitting the highest-ranked applicants in this order up to its capacity. Other applicants are rejected. Each rejected applicant proposes to his or her next most preferred school. Each school ranks these new proposals together with applicants admitted provisionally in the previous round, first by priority and then by tie-breaker. From this pool, the school again provisionally admits those ranked highest up to capacity, rejecting the rest. The algorithm terminates when there are no new proposals (some applicants may remain unassigned).

- Description of DA from Abdulkadiroğlu et al. (2022)

C.2 Model Solution Derivation

We solve for the optimal lottery by maximizing the planner's objective:

$$\frac{d}{d\varepsilon}(1 + \delta)\frac{\varepsilon}{c + \varepsilon}(1 + k\varepsilon) + \frac{c}{c + \varepsilon}(1 - l\varepsilon) = \frac{(1 + \delta)k\varepsilon^2 + (1 + \delta)2ck\varepsilon + \delta c - lc^2}{(c + \varepsilon)^2}$$

This is zero at:

$$\varepsilon = -c \pm c\sqrt{1 + \frac{cl - \delta}{(1 + \delta)ck}}$$

The second derivative is:

$$\frac{d^2}{d\varepsilon^2}(1 + \delta)\frac{\varepsilon}{c + \varepsilon}(1 + k\varepsilon) + \frac{c}{c + \varepsilon}(1 - l\varepsilon) = 2c\frac{(cl - \delta) + ck(\delta + 1)}{(c + \varepsilon)^3}$$

The optimal ε depends on the relative δ , c , l , and k . Proceed by cases:

Case 1: Suppose that $\delta < cl$, $k < 0$.

$$cl - \delta > 0; (1 + \delta)ck < 0 \implies 1 + \frac{cl - \delta}{(1 + \delta)ck} < 1$$

First, suppose that $0 < 1 + \frac{cl - \delta}{(1 + \delta)ck} < 1$. Then:

$$-c \pm c\sqrt{1 + \frac{cl - \delta}{(1 + \delta)ck}} < 0$$

Suppose instead that $1 + \frac{cl-\delta}{(1+\delta)ck} < 0$. Then $-c \pm c\sqrt{1 + \frac{cl-\delta}{(1+\delta)ck}}$ is imaginary.

Either way, there are no interior solutions $\varepsilon \in [0, 1 - c]$, so $\varepsilon \in \{0, 1 - c\}$. Consider the benefit function on the interval of interest.

$$\frac{d}{d\varepsilon} \mathbb{E}(b|0) = \frac{c(\delta - lc)}{c^2} < 0 \implies \varepsilon = 0$$

The slope of the benefit function is decreasing on the interval from $[0, 1 - c]$, the maximizing ε is at 0.

Case 2: Suppose that $\delta < cl, k > 0$

$$cl - \delta > 0; (1 + \delta)ck > 0 \implies \sqrt{1 + \frac{cl - \delta}{(1 + \delta)ck}} > 1$$

In this case, there is one negative root and one positive root.

Consider the concavity of the benefit function with the second derivative, assuming $\delta < cl, k > 0$:

$$2c \frac{(cl - \delta) + ck(\delta + 1)}{(c + \varepsilon)^3} > 0$$

The maximizing ε will be at one of the endpoints.

A full lottery is optimal compared to no lottery (complete screening) when:

$$(1 + \delta) \frac{1 - c}{c + (1 - c)} (1 + k(1 - c)) + \frac{c}{c + (1 - c)} (1 - l(1 - c)) < 1$$

$$\implies k < \frac{cl - \delta}{(1 + \delta)(1 - c)} = \bar{k}$$

Case 3: Suppose that $\delta \geq cl, k < 0$

$$cl - \delta < 0; (1 + \delta)ck < 0 \implies \sqrt{1 + \frac{cl - \delta}{(1 + \delta)ck}} > 1$$

In this case, there is one negative root and one positive root ε_+^* :

$$\varepsilon_+^* = -c + c\sqrt{1 + \frac{cl - \delta}{(1 + \delta)ck}}$$

There is $\varepsilon_+^* < 1 - c$ when the following condition (written in terms of k) holds:

$$-c + c\sqrt{1 + \frac{cl - \delta}{(1 + \delta)ck}} < 1 - c \iff k < \frac{(cl - \delta)c}{(1 + \delta)(1 - c)(1 + c)} = \underline{k}$$

Consider the second derivative of the benefit function:

$$= 2c \frac{\delta(kc - 1) + ck + cl}{(c + \varepsilon)^3} = 2c \frac{(cl - \delta) + ck(\delta + 1)}{(c + \varepsilon)^3}$$

This is negative for $\delta > cl$, $k < 0$, so ε_+^* is a maximum.

What would happen if $\varepsilon_+^* > 1 - c$? This occurs when:

$$-c + c \sqrt{1 + \frac{cl - \delta}{(1 + \delta)ck}} < 1 - c \iff k > \frac{cl - \delta}{(1 + \delta)(1 - c)} \frac{c}{1 + c}$$

If $\varepsilon_+^* > 1 - c$, then the maximizing ε must be at one of the two boundary points.

A full lottery (i.e., $\varepsilon^* = 1 - c$) will be optimal compared to no lottery if the following condition is fulfilled:

$$k > \frac{cl - \delta}{(1 + \delta)(1 - c)}$$

Consider k in this case. Note that $\frac{c}{1+c} < 1$, and $\frac{cl-\delta}{(1+\delta)(1-c)} < 0$. So:

$$k > \frac{cl - \delta}{(1 + \delta)(1 - c)} \frac{c}{1 + c} > \frac{cl - \delta}{(1 + \delta)(1 - c)}$$

Case 4: Suppose that $\delta \geq cl$, $k > 0$.

Under these conditions:

$$cl - \delta < 0; (1 + \delta)ck > 0 \implies \sqrt{1 + \frac{cl - \delta}{(1 + \delta)ck}} < 1$$

As in **Case 1**, there are no interior solutions $\varepsilon \in [0, 1 - c]$. Again, the optimal ε must be at one of the endpoints. For these conditions:

$$\frac{d}{d\varepsilon} \mathbb{E}(b|0) = \frac{c(\delta - lc)}{c^2} > 0 \implies \varepsilon = 1 - c$$

The benefit function is increasing in ε , so a full lottery is optimal in this case.

This concludes the cases covered by Proposition 1