The Mirage of Elite Schools: Evidence from Lottery-based School Admissions in China*

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Abstract

In this paper we use school admission lotteries to estimate the effect of elite school attendance on student achievement in China. The empirical assessment requires combining lottery records with administrative Middle School Exit Exam (MSEE) records, in which we encounter an imperfect matching problem arising from the lack of a common unique identifier. To address this problem, we develop a data combination procedure and extend the existing local average treatment effect (LATE) framework to analyze treatment effects in contexts with imperfect matching following data combination. Despite the large observed superiority of elite schools in student achievement, we find little evidence that three-year attendance at an elite school improves students' MSEE scores or secondary school admission outcomes. We also find that the most sought-after elite schools are those with the highest student achievement level, rather than those with the largest value-added effect on test scores. This finding suggests that parents may choose schools primarily on the basis of their observed superiority in student outcomes rather than their academic value-added, which casts doubt on parents' ability to identify schools that are better suited to their children's learning needs.

Keywords: elite schools, student achievement, school admission lotteries, treatment effect analysis, school choice

JEL Classifications: I21 I28 J13

1 Introduction

The question of whether elite schools add value to their already high-achieving students is of practical importance to parents who are concerned about their children's academic progress as the perceived superiority of such schools may be masking the true nature of academic performance. At the same time, this question is also of great value to researchers and policymakers because the answer not only contributes to the existing understanding on the determinants of student achievement but also has important policy implications for the efficient organization of students into schools and the effective allocation of school resources.

Clear evidence on the effects of better schooling is scarce, however, largely because school attendance is an endogenous decision and thus may be correlated with unobserved family and individual characteristics that affect student achievement. Several recent studies adopt compelling research designs to address the issue of endogeneity, albeit with mixed results. One strand of such research relies on regression discontinuity (RD) designs to exploit the sharp changes in the probability of attending selective schools around their admission cutoffs. For example, Jackson (2010) discovers large test score gains for students attending better secondary schools in Trinidad and Tobago, and Pop-Eleches and Urquiola (2011) find that students benefit from attending higher-achieving schools in Romania. In contrast, Clark (2010) finds selective school attendance to have no impact on test scores in the UK, and Abdulkadiroglu, Angrist, and Pathak (2012) show exam school education to have little effect on student achievement in Boston and New York. The second strand of this research uses school admission lotteries to identify exogenous variation in school attendance induced by lottery assignment. Cullen, Jacob, and Levitt (2006), for example, find no test score gains among students attending high-achieving public schools in Chicago, whereas Hastings and Weinstein (2008) report substantial achievement gains for students attending higher-performing schools in Charlotte-Mecklenburg. It is thus apparent that whether better schools improve the achievement of their attendees remains unclear and requires further investigation.

This paper presents new evidence on the effect of elite school attendance on student achievement by exploiting exogenous variation in access to elite schools in China generated by school admission lotteries. Students in China are assigned to primary schools (grades 1-6) and neighborhood middle schools (grades 7-9) based on their area of residence. Elite middle schools (hereafter, elite schools) are considered superior to neighborhood middle schools (hereafter, neighborhood schools) and open their enrollment to all interested students within their school districts. Lotteries are often used by the oversubscribed elite schools to determine the allocation of seats, thereby generating exogenous variation in access to elite schools among students with a variety of academic backgrounds. In

addition to this natural experimental setting, a number of distinct features of the Chinese education system render extension of the investigation to the Chinese context particularly informative and interesting. First, a number of institutional factors, such as the uniform curriculum, textbooks and high-stakes exit exams adopted in both elite and neighborhood schools and the rare occurrences of grade retention and dropping out during the nine-year compulsory schooling stage,[1] make middle schools in China a very ideal setting to evaluate the effect of attending better schools on student achievement. Second, secondary school (grades 10-12) and university admissions in China are almost solely determined by students' test scores in entrance exams, which is in contrast to the situation in many Western countries, where such admissions are based on multi-dimensional criteria going beyond standardized test scores (e.g., teacher recommendations, extracurricular activities, leadership potential, etc.). Thus, if attending a better school adds any value to students' academic outcomes, the gains are likely to be more salient in China than in the West. Third, unlike the most studied US school admission lotteries where exercising school choice is free of charge (e.g., Abdulkadiroglu *et al.*, 2011; Dobbie and Fryer, 2011), attending an elite school in China usually incurs an additional tuition cost, as is the case in the city under study in this paper (see Section 2 for a detailed discussion). With the additional cost that parents pay to enroll their children in elite schools, the perceived difference between elite and neighborhood schools must be substantial in China, rendering it a good context to compare parental perceptions of what constitutes a "good school" to evidence of effective value-added.

In this paper, we examine the effect of elite school attendance on student achievement using data from a provincial capital city in China. Our data come from two sources: school admission lottery records and administrative records from the Middle School Exit Exam (MSEE), which is compulsory for all students at the end of middle school and also serves as the entrance exam for secondary school admissions. The former contain information on school choices and lottery assignments for elite school applicants and the latter contain information on the middle schools attended, MSEE scores and secondary school admission outcomes for all students taking the MSEE. Program evaluation employing random assignment with imperfect compliance often resorts to the benchmark local average treatment effect (LATE) framework, which assumes perfect observation of the lottery assignments, treatment intakes and outcomes of all individuals in the target population or sample (e.g., Imbens and Angrist, 1994; Angrist, Imbens, and Rubin, 1996). However, we encounter an observational obstacle when combining student records in the two data sources. Because the common variables available for use in data combination (i.e., name, gender and cohort) do not

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[1]Dropping out during the compulsory schooling stage almost never happens in the urban areas under study in this paper, although it may occur occasionally in some rural areas.

constitute a unique identifier, we sometimes falsely link lottery and MSEE records pertaining to different individuals, resulting in *imperfect matching* in the combined data set. To address this problem, we first develop a data combination procedure that forms all pairwise links between the lottery and MSEE records, and then extend the benchmark LATE framework to analyze treatment effects in contexts with imperfect matching by employing all linked lottery-MSEE pairs, regardless of whether link is correct or false. We show that, if sample attrition is independent of lottery assignment, our extended LATE framework can identify the average treatment effect for the admission lottery compliers who are retained in the MSEE data set. We also consider the general case in which sample attrition depends on lottery assignment and analyze the extent of the bias that may result from differential attrition between winners and losers. Because both imperfect matching and sample attrition are commonly confronted problems in randomized evaluations involving the combination of information from different data sets, the extended LATE framework presented herein is widely applicable to a variety of contexts in which similar observational problems arise following data combination.

In the city investigated in this paper, elite schools are far superior to neighborhood schools with regard to student achievement. Students graduating from elite schools score, on average, about two-thirds of a standard deviation (hereafter, σ) above those from neighborhood schools on the MSEE. However, similar to Pischke and Manning's (2006) finding in the case of selective schools in the UK, much of the achievement advantage associated with elite school attendance is attributable to the ability sorting of students and is not caused by attendance per se. Using exogenous variation in access to elite schools generated by school admission lotteries, we find little evidence that three-year attendance at an elite school improves students' MSEE scores or secondary school admission outcomes. The instrumental variables (IV) point estimate of the effect of elite school attendance on MSEE scores is -0.016 and can reject a relatively modest gain of 0.15σ in test scores over the three-year period. Moreover, although we find evidence of a small degree of differential attrition between winners and losers, we show that the resulting bias is small in magnitude and that its sign is in favor of finding a positive elite school attendance effect.

The results of this paper are also relevant to the broader debate over school choice, as the magnet-type elite schools investigated herein provide schooling alternatives for students to opt out of their assigned neighborhood schools.[2] The popularity of school choice is based largely upon the belief that increasing parental choice can yield improved efficiency in education production

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[2]Recent years have seen a surge in the empirical literature investigating the effects of school choice on student outcomes, and the debate has permeated various cultural contexts, including Chile (Hsieh and Urquiola, 2006), China (Lai, Sadoulet, and Janvry, 2011; He, 2012), Columbia (Angrist *et al.*, 2002; Angrist, Bettinger, and Kremer, 2006; Bettinger, Kremer, and Saavedra, 2010), Israel (Lavy, 2010) and Norway (Machin and Salvanes, 2010).

through enhanced competition or better matches between students and schools (e.g., Friedman, 1962; Chub and Moe, 1990; Hoxby, 2000). The ability of the choice mechanism to improve educational outcomes, however, depends in part on the extent to which parents express their preference for achievement gains in selecting schools for their children. As Rothstein (2006) notes, any factors that inhibit parents from choosing the most effective schools will tend to dilute the incentives for efficiency improvement that the choice mechanism might otherwise create. However, several studies find that parents do not necessarily place the greatest weight on academic outcomes (Hastings, Kane, and Staiger, 2005, 2006 ; Jacob and Lefgren, 2007) or know which schools are likely to benefit their children the most academically (Figlio and Lucas, 2004; Mizala and Urquiola, 2007). Despite the lack of evidence of any academic value-added, all of the elite schools in our sample were oversubscribed throughout the study period and had very competitive admission lotteries, of which even the highest winning rate was still below 50 percent. Moreover, the most popular elite schools in our sample are those with the highest average student achievement level, not those estimated to have the largest value-added effect on test scores. Our finding that schools are sought after primarily for their observed superiority rather than their academic value-added casts doubt on parents' ability to identify schools that are better suited to their children's learning needs and, consequently, on the potential of school choice to improve student achievement.

The remainder of this paper is organized as follows. Section 2 provides the background on China's middle school system and elite school admission procedures, and introduces the admission lottery data. Section 3 introduces our data combination procedure and describes the data combination outcomes. Section 4 presents our empirical framework, which extends the benchmark LATE framework to treatment effect analysis in contexts with imperfect matching encountered in data combination. Section 5 presents our empirical results and discusses the possible reasons for elite schools' persistent popularity and yet lack of evident academic value-added. Section 6 provides some concluding remarks.

2   Background

2.1   Middle School System

The middle school system in the provincial capital city investigated in this paper is typical of those in most Chinese cities. Upon graduation from primary school, students are assigned to a neighborhood school through an assignment mechanism that works at the neighborhood level. Elite schools exist outside the neighborhood middle school system and provide schooling alternatives for students to opt out of their assigned neighborhood schools. Unlike neighborhood schools, which are

entirely publicly funded and tuition-free, elite schools rely on public funding only for basic personnel expenses and charge tuition fees to cover operating and benefit expenses.[3] Historically, these elite schools were exam schools that admitted students primarily on the basis of their entrance exam scores. However, the central government adopted country-wide educational reform in the late 1990s under the banner *Cross Century Quality Education Project,*[4] of which an important educational ideal was to reduce the excesses of exam-based assessment. In response to the central government's call to ease exam pressure, local education authorities across the country adopted directives banning the use of any form of entrance exam in admissions during the nine-year compulsory schooling stage, although entrance exams are still used in secondary school and university admissions. Consequently, elite schools resorted to alternative admission schemes, such as the use of admission lotteries, for allocation of spots. Attending a private school is another option available to students. In the city under study here, private schools, all of which were founded after the 1990s, are much less popular than elite schools because of the lack of a long established reputation. Nonetheless, they are still an option for students who are interested in attending an elite school but fail to gain a place.

Table 1 summarizes enrollment and outcomes by school type using the city's MSEE takers in 2005, the first cohort examined in this paper. The city had 181 middle schools, including 160 neighborhood schools, 16 elite schools, and five private schools. The average school-grade size was 591 for elite schools, 223 for neighborhood schools and 114 for private schools. Accordingly, the 16 elite schools and five private schools accounted for 20.7 and 1.3 percent,[5] respectively, of the city's total enrollment in middle school. Elite schools, with a mean MSEE score of 0.52σ, were far more advantageous than neighborhood schools (-0.14σ) and private schools (-0.09σ) in student achievement, whereas the latter two were largely comparable. After taking the MSEE, middle school graduates were tracked into three types of secondary schools based on their MSEE scores: top-echelon high schools, regular high schools and vocational secondary schools, ranked in descending order of entrance score requirements. The superiority of elite schools was also reflected by its greater proportion of graduates admitted to top-echelon high schools (40 vs. 17 percent) and regular high schools (41 vs. 28 percent) compared with neighborhood schools.

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[3]The basic salaries of elite school teachers are paid out of the local government's education budget. However, these schools provide a higher level of overall compensation to their teachers through school-funded benefits.

[4]See Dello-Iacovo (2009) for a review of the quality education reform in China. Although quality education, or so called "*suzhi jiaoyu,*" has inspired a number of innovative reforms and received considerable support in principle, wider-scale implementation has been hindered by a lack of resources, conceptual ambiguity, and cultural resistance, leaving the problems in the country's education system largely unresolved.

[5]Because private schools located outside the city's boundaries, most of which are boarding schools, are not included in the sample, the actual enrollment share of private schools is larger than the reported 1.3 percent; nonetheless, the increase in that share would be marginal even if the these schools were included.

2.2 Elite School Admissions and Lottery Data

The shift away from excessive exam orientation in the late 1990s, as advocated by the country's quality education reform, has engendered a new admission process of elite schools. In the city under study in this paper, all of the elite schools adopted a two-tier admission process, with the total admission quota divided between advance and general admissions. The former are reserved exclusively for gifted and talented students who are awarded in city- or district-level academic, artistic, and athletic contests.[6] Following advance admissions, application becomes open to all students who are willing to pay elite school tuition. During the period considered in this paper, all of the city's elite schools set their tuition at the price ceiling allowed by the city education council, that is, RMB3,000 (approximately US$400) per year, or about one-tenth the average annual disposable income of a three-person family. All of the elite schools were nonetheless oversubscribed, as the demand at the regulated tuition far exceeded the general admission quota.

Unlike lottery-based magnet or charter school admissions in the US, where students can apply to as many schools as they want and participate in multiple lotteries (e.g., Cullen, Jacob, and Levitt, 2006; Abdulkadiroglu *et al.*, 2011), in the Chinese city under study, students can apply to only one elite school and will be disqualified from enrolling in any elite school if caught submitting multiple applications. Every year, all of the city's elite schools conduct their general admission lotteries on the same day using the same computer program designated by the city education council. In each lottery, the program randomly assigns a lottery number to each applicant and enrolls students with the lowest numbers first, until the school's quota is filled. To prevent tampering, all admission lotteries are certified by notaries public. Within a few weeks of the lottery, winners are required to pay off the entire three-year tuition, which is nonrefundable even if they later switch schools. Those who do not pay their tuition by the deadline are considered to have declined their admission offer. The nonrefundable nature of the tuition payment means that students rarely switch out of any elite school once enrolled. However, the lottery assignments are not completely binding. A significant portion of applicants who lose out in the lottery still gain admission through back door channels.[7] The final enrollment number in an elite school is therefore larger than its announced admission quota. During the period under study, a typical elite school in the city admitted approximately one-third of its students through advance admissions, one-third through the lottery, and the remaining

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[6]Advance admission recipients are also offered with some tuition waivers, with the amount waived varying by school and award rank.

[7]The most important factor determining a lottery loser's chances of being admitted through back door channels is whether he/she has been referred by someone (for example, a government official that the student's parents have found through their personal network) who can exert an influence over the principal. The student's academic performance in primary school is another factor, albeit of secondary importance. These factors also affect the tuition paid by students admitted through back door channels, with the ceiling reaching twice as much as regular tuition.

one-third through the "back door."

The city investigated in this paper has seven municipal districts whose boundaries coincide with the school district boundaries. The Yangtze River runs through the city and divides it into two parts: North Bank (Districts 1-3) and South Bank (Districts 4-7). These two parts can be considered as independent enrollment areas, as students rarely commute across the Yangtze River for schooling. With the cooperation of the notary public office, we obtained the lottery records of three cohorts of students who participated in the 21 admission lotteries run by all of the eight elite schools in the North Bank during the 2002-2004 period. Three lotteries conducted in 2003 are excluded because the notary public office's records contain only winners' information. We also exclude the approximately four percent of applicants who attended primary schools outside the North Bank at the time of application. Our final lottery sample comprises 13,768 applicants who were enrolled in North Bank primary schools at the time of application.[8] The lottery records include each applicant's name, gender, primary school attended and lottery assignment, but contain no information on his/her family background or baseline scores. Working with School District 3, we obtained students' test scores on a district-wide uniform exam taken in grade 6, the final grade of primary school, and matched the scores to the applicants in our lottery sample by name, gender, primary school and year. However, no baseline scores are available for applicants from the other two districts.

Columns 1-3 in Table 2 report the descriptive statistics of the lottery assignments and applicants' predetermined characteristics at the time of application. These admission lotteries are quite competitive, with an average of only three out of ten applicants winning their lottery. The subsample of applicants in District 3 where baseline scores are available has even a lower lottery winning rate of 22 percent. Among the 3,483 applicants in District 3, the match rate of baseline scores is 85 percent, whereas non-matching is largely a result of name misspellings or gender misidentifications in either the lottery or baseline score records. Figure 1A plots the kernel density curves of the 6th-grade combined math and Chinese scores (standardized to have zero mean and unit variance for each cohort) for District 3 students by elite school enrollment status. There is clear evidence of "cream-skimming": students enrolled in elite schools have a mean 6th-grade score 0.42σ above the district average, whereas the mean 6th-grade score of neighborhood school students is 0.08σ below the district average. Two sources of cream-skimming can be identified in Figure 1B: advance admission recipients and general admission applicants have mean 6th-grade scores that are 0.57σ and 0.29σ above the district average, respectively, leaving non-applicants a mean score that is 0.12σ

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[8]Students in the final lottery sample accounted for 23 percent of all North Bank students who transitioned from primary school to middle school in the 2002-2004 period.

below the district average. The "cream-skimming" evidence indicates that the superiority of elite schools in student achievement on the MSEE is at least in part the result of their ability to lure a disproportionate number of already high-achieving students from neighborhood schools.

2.2.1 Validity of the Randomization

If these admission lotteries are indeed random as certified by notaries public, the winners and losers of a given lottery would be expected, on average, to have the same predetermined individual characteristics. Accordingly, we check the validity of the randomization by testing whether applicants' predetermined individual characteristics are associated with their win/loss status. In Column 4 of Table 2, we regress the dummy indicator of winning a lottery on gender, the set of dummy indicators for primary schools attended, and lottery fixed effects for all of the applicants in the North Bank. Neither the coefficient on the female indicator nor any of the coefficients on the 175 primary school dummies (omitted in the table) is statistically significant. The F-test of the joint significance of these coefficients (excluding lottery fixed effects) is very small and insignificant (F=0.78, p-value=0.986), suggesting little evidence that applicants' gender and primary school attended are correlated with their odds of winning a lottery. For applicants from District 3, we further include the availability of baseline scores and, if available, the score level as additional regressors. The results, reported in Columns 5 and 6 of Table 2, show no evidence that either this availability or the baseline score level is associated with lottery outcomes. We therefore take the fact that these lotteries did not favor applicants with higher baseline scores as compelling evidence for the validity of their randomness.

3 Data Combination

The lottery data set contains only applicants' choice of elite school and lottery assignment. To examine the lotteries' effects on students' elite school enrollment status and subsequent educational outcomes, we need to link the lottery data set with the MSEE data set, which contains information on students' middle schools attended, MSEE scores and secondary school admission outcomes upon graduation from middle school. The linkage between the two data sets is through the set of common variables C that are observed in both data sets, i.e., name and gender. As skipping or repeating a grade rarely occurs and few students commute across the Yangtze River to attend middle school, we restrict the target universe in the MSEE data set to students who finished middle school in the North Bank three years after each lottery.[9] But even conditional on gender and cohort, it is still

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[9]Expanding the target universe to middle school graduates from the entire city increases the overall match rate by only two percentage points, which suggests that very few applicants opted to attend middle school in the South

possible to falsely link some lottery and MSEE records which have the same names but pertain to different individuals. As we are unable to distinguish between correct and false links, our empirical analysis has to employ all pairwise links between the lottery and MSEE records, regardless of whether the link is correct or false.

Our data combination procedure can be formalized as follows. Assume that there is a super-population of (unobserved) identifiers from which the identifiers (also unobserved) of individuals from both the lottery data set and MSEE data set are drawn. The lottery data set contains (C_i, Z_i) for $i \in I$ and the MSEE data set contains (C_j, D_j, Y_j) for $j \in J$, where Z is a dummy indicator for lottery assignments, D is a dummy indicator for elite school enrollments, and Y denotes student outcomes (e.g., test scores) in the MSEE. In this notation, because i and j are drawn from the same superpopulation of identifiers, $i = j$ if the lottery record and MSEE record pertain to the same individual and $i \neq j$ if the two records pertain to different individuals. Our data combination procedure involves forming *all pairwise links* of records between the two data sets that are matched by the common variables C. In other words, for each record i in the lottery data set and each record j in the MSEE data set such that $C_j = C_i$, we construct a linked pair $(C_i, Z_i, D_{i(j)}, Y_{i(j)})$ in which $D_{i(j)}/Y_{i(j)}$ indicates that D_i/Y_i is imputed by the value of D_j/Y_j. Therefore, our combined data set can be written as

$$\Psi = \{(C_i, Z_i, D_{i(j)}, Y_{i(j)}), \forall\, C_j = C_i, i \in I, j \in J\}.$$

The total number of record pairs in the combined data set is $\sum_{i \in I} n_i$, where $n_i = \sum_{j \in J} \mathbf{1}(C_j = C_i)$, the number of MSEE records that are linked to individual i in the lottery data set by C.

Table 3 reports matching statistics by lottery assignment separately for all applicants, District 3 applicants and a subsample of District 3 applicants with baseline scores. As the results are qualitatively the same across different samples, we discuss in the following only the results for the full sample of all applicants in Columns 1-2. Each lottery loser is, on average, matched to 1.58 MSEE records, demonstrating that duplicate names are quite common in our context. Because the number of false matches is independent of random lottery assignments, the average number of false matches should be the same for winners and losers. Therefore, the difference in the average number of total matches between winners and losers should reflect the extent of the differential attrition caused by lottery assignment. Column 2 presents the regression-adjusted win/loss difference controlling for lottery fixed effects, showing winners to have, on average, 0.027 more matches than losers.

Bank. However, such expansion of the target universe significantly increases the probability of multiple matches. In this paper, we report only the results using the matched records in the North Bank, although the main results are similar when all matched records from the entire city are used.

However, the difference is insignificant as the standard error of the coefficient (0.039) corresponds with a minimum detectable difference of 0.075. Even if differential attrition does exist, its degree is likely to be small and muted by the large variation in the number of total matches in our context, which ranges from 0 to 23 and has a standard deviation over 2.0.

To address the imprecision of the foregoing exercise, we further examine the win/loss difference in the matching probability without distinguishing between single and multiple matches. Note that if an applicant's name is also used by some other student(s) in the MSEE data set conditional on gender and cohort, he/she will be matched regardless of whether his/her own record is contained in the MSEE data set. Thus, differential attrition will lead to a difference in the matching probability between winners and losers only among the subsample of applicants with no false matches. It follows that the win/loss difference in the matching probability among all applicants is equal to the product of this difference among the subsample of applicants with no false matches and their proportion in the entire sample. The overall match rate, regardless of whether the match is single or multiple, is 89.2 percent for lottery losers. The coefficient on the lottery winner dummy in Column 2 shows a win/loss difference of 2.2 percentage points in the overall match rate, significant at the one percent level, suggesting that differential attrition does exist, though not detected at a level of statistical significance in the previous exercise. The sharp increase in precision is the result of the large reduction in the standard deviation of the dependent variable – from over 2.0 for the number of total matches to only 0.3 for the dummy indicator of being matched. To quantify the extent of the differential attrition, we also need to know the proportion of applicants with no false matches. However, direct observation of this proportion is not possible, as we cannot distinguish between false and correct matches. We instead employ an indirect strategy to proxy for this information. To do so, we search for the names of the 2004 elite school applicants in the 2005 and 2006 MSEE records conditional on gender. As applicants entering the 2004 lotteries would not have graduated from middle school until 2007, none of them were expected to be observed in the 2005 or 2006 MSEE data set. Therefore, any such name match would be a false match. In this exercise, we find matched MSEE records for only about a quarter of the applicants in the 2004 lotteries, suggesting that about three-quarters of the applicants have no false matches. As this proportion is likely to remain stable across cohorts, we take 75 percent as a proxy for the proportion of applicants with no false matches. Dividing the win/loss difference in all applicants' matching probability (2.2 percentage points) by the proportion of applicants with no false matches (75 percent) gives us the extent of differential attrition: 2.9 percentage points, very close to the point estimate of a 2.7-percentage-point difference in the average number of total matches between winners and losers. Therefore, we take the results of both exercises in Table 3 as evidence suggesting that differential

attrition exists, but with a very small magnitude.

4 Empirical Framework

The employment of random assignment, albeit with imperfect compliance, in elite school admissions leads us to consider utilizing the LATE framework to evaluate the average treatment effect for the assignment compliers among applicants. However, application of this framework to our context encounters an observational challenge, *imperfect matching*, which arises because of the lack of a common unique identifier between the lottery and MSEE data sets. In this section, we start with a brief review of the benchmark LATE framework and then extend it to address the imperfect matching problem encountered in our context.

4.1 Review of the Benchmark LATE Framework

In this subsection, we follow Imbens and Angrist (1994) and Angrist, Imbens, and Rubin (1996) to review the benchmark LATE framework in a heterogeneous treatment effect model. In the setup of this framework, researchers are interested in the effect of a treatment D (e.g., attending an elite school) on an outcome Y (e.g., test scores) and there exists an instrument Z (e.g., lottery assignment) for D. Following the conventions of the prior literature, we adopt a generalized potential treatment and outcome notation, in which $D_i(z)$ denotes the potential treatment status of individual i were the individual to have instrument value $Z_i = z$ and $Y_i(d, z)$ denotes the potential outcome of individual i were the individual to have treatment status $D_i = d$ and instrument value $Z_i = z$. The benchmark LATE framework assumes perfect observation of $(Z_i, D_i(Z_i), Y_i(D_i(Z_i), Z_i))$ for every individual i.

Proposition 1 *The LATE Theorem. Suppose*
(A1 Exclusion) $Y_i(d, z) = Y_i(d)$ for $d \in \{0, 1\}$, $z \in \{0, 1\}$;
(A2 Independence I) $\{D_i(0), D_i(1), Y_i(0), Y_i(1)\} \perp Z_i$;
(A3 First stage) $E[D_i(1) - D_i(0)] > 0$ and $0 < P(Z_i = 1) < 1$;
(A4 Monotonicity) $D_i(1) \geq D_i(0)$, $\forall i$.
Then, the IV estimand without covariates is

$$\gamma^{IV} = \frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[D_i|Z_i = 1] - E[D_i|Z_i = 0]} = E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0)].$$

4.2 Extended LATE Framework under Imperfect Matching

Although the settings of the benchmark LATE framework assume perfect observation of (Z, D, Y) for every individual in the target population or sample, very often there is no single data set that contains all variables of interest. When these variables are contained in two or more separate data

sets, economists are confronted with the problem of estimating models by combining different data sets (see Ridder and Moffit [2007] for a survey of data combination). A prominent example is the two-sample instrumental variables (TSIV) estimation proposed independently by Angrist and Krueger (1992) and Arellano and Meghir (1992), in which instrument Z is common to both data sets but endogenous regressor D and dependent variable Y are included in only one or the other. The nature of the data combination problem encountered in our study, as discussed in Section 3, is different to that in applications of TSIV estimation. In our context, instrument Z is observed in one (i.e., lottery) data set while treatment status D and dependent variable Y are observed in the other (i.e., MSEE) data set. The two data sets are linked through the set of common variables C contained in both data sets. However, C does not constitute a unique identifier in either data set, leading to imperfect matching between the lottery and MSEE records. In this subsection, we present our extended LATE framework to analyze treatment effects in contexts with imperfect matching following data combination.

To begin with, we first assume that all individuals in the lottery data set are observed in the MSEE data set, i.e. no sample attrition. As shown in Proposition 1, with a binary instrument and treatment, the IV estimand can be expressed as the ratio of the intent-to-treat (ITT) effect of Z on Y and that of Z on D. In contexts with imperfect matching but no sample attrition, these two ITT estimands can be constructed using the combined data set Ψ as follows:

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\begin{aligned}
& E[D_{i(j)}|Z_i = 1, C_j = C_i] - E[D_{i(j)}|Z_i = 0, C_j = C_i] \\
= \quad & p(E[D_i|Z_i = 1] - E[D_i|Z_i = 0]) + (1-p)(E[D_j|Z_i = 1, C_j = C_i, j \neq i] - E[D_j|Z_i = 0, C_j = C_i, j \neq i])
\end{aligned} \tag{1}
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and

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\begin{aligned}
& E[Y_{i(j)}|Z_i = 1, C_j = C_i] - E[Y_{i(j)}|Z_i = 0, C_j = C_i] \\
= \quad & p(E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]) + (1-p)(E[Y_j|Z_i = 1, C_j = C_i, j \neq i] - E[Y_j|Z_i = 0, C_j = C_i, j \neq i]),
\end{aligned} \tag{2}
$$

where $p = P[j = i|C_j = C_i]$, the proportion of correct matches among all matches in Ψ. Equation (1)/(2) shows that the ITT estimand of Z's effect on D/Y using the combined data set is equal to a weighted average of the mean difference in D/Y by Z of correct and false matches, with the weights equal to their corresponding proportions in Ψ. We further impose an additional independence assumption that lottery assignment Z_i is independent of falsely matched treatments D_j and outcomes Y_j.

Assumption (A5) *Independence II: $\{D_j, Y_j\} \perp Z_i \; \forall \; C_j = C_i, j \neq i$.*

Note that implicit in our potential treatment and outcome notation is the assumption of no interference between individuals: an individual's potential treatments and outcomes are unaffected by the instrument value and treatment status of any other individual (Cox, 1958). With no interference between individuals, (A5) is immediately satisfied with the random assignment of Z_i. As the second terms in both Equations (1) and (2) can be eliminated under Assumption (A5), the two ITT estimands are attenuated to the same extent by the proportion of false matches in Ψ. However, the biases are canceled out when their ratio is taken in calculating the IV estimand. Therefore, despite the contamination of falsely matched pairs, the IV estimand constructed using the combined data set still identifies the same population parameter as that in Proposition 1 under perfect observation, that is, the LATE on compliers who change their treatment status according to the instrument. We summarize this formally in Proposition 2 as follows.

Proposition 2 *In the presence of imperfect matching, if Assumptions (A1)-(A5) hold and there is no sample attrition, then the IV estimand without covariates using the combined data set is*

$$
\begin{aligned}
\gamma^{IV} &= \frac{E[Y_{i(j)}|Z_i = 1, C_j = C_i] - E[Y_{i(j)}|Z_i = 0, C_j = C_i]}{E[D_{i(j)}|Z_i = 1, C_j = C_i] - E[D_{i(j)}|Z_i = 0, C_j = C_i]} \\
&= \frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[D_i|Z_i = 1] - E[D_i|Z_i = 0]} = E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0)].
\end{aligned}
$$

Proposition 2 assumes complete, although imperfect, observation of (Z_i, D_i, Y_i) in Ψ for all of the individuals in the lottery data set. However, as previously discussed, the combined data set is subject to sample attrition as some individuals in the lottery data set are not observed in the MSEE data set. Let T_i denote a binary indicator that equals 1 if lottery participant i is also observed in the MSEE data set, and 0 otherwise, i.e., $T_i = \mathbf{1}(i \in J)$. The following proposition shows that, if sample attrition is independent of lottery assignment, the LATE theorem still holds for lottery compliers who are retained in the MSEE data set.

Proposition 3 *In the presence of imperfect matching, if Assumptions (A1)-(A5) hold and $T_i \perp Z_i$, then the IV estimand without covariates using the combined data set is*

$$
\begin{aligned}
\gamma^{IV} &= \frac{E[Y_{i(j)}|Z_i = 1, C_j = C_i] - E[Y_{i(j)}|Z_i = 0, C_j = C_i]}{E[D_{i(j)}|Z_i = 1, C_j = C_i] - E[D_{i(j)}|Z_i = 0, C_j = C_i]} \\
&= \frac{E[Y_i|Z_i = 1, T_i = 1] - E[Y_i|Z_i = 0, T_i = 1]}{E[D_i|Z_i = 1, T_i = 1] - E[D_i|Z_i = 0, T_i = 1]} = E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0), T_i = 1].
\end{aligned}
$$

In addition to imperfect matching, application of the LATE framework to our context faces two additional empirical challenges. First, sample attrition is not independent of lottery assignment as assumed in Proposition 3. Instead, winning a lottery has a monotone effect on an individual's observability in the MSEE data set: it induces some students who would otherwise opt for schooling outside the North Bank middle school system to remain in the system, but not vice versa. Hence, $T_i(1) \geq T_i(0) \ \forall \ i \in I$, where $T_i(1)$ and $T_i(0)$ are the latent indicators for whether individual i

would be retained in the MSEE data set when $Z_i = 1$ and $Z_i = 0$, respectively. With a monotone effect of lottery assignment on an individual's observability in the MSEE data set, the linked record pairs $(Z_i, D_{i(j)}, Y_{i(j)})$ in Ψ can be classified into three categories: (i) the correctly linked pairs that pertain to *always retained individuals* $(T_i(0) = T_i(1) = 1)$, (ii) the correctly linked pairs that pertain to *marginally retained individuals* $(T_i(0) = 0, T_i(1) = 1)$, and (iii) *falsely linked pairs* $(C_j = C_i, j \neq i)$. Let Ψ_1 and Ψ_0 denote the subset of linked lottery-MSEE pairs in Ψ that are matched to winners and losers, respectively. Lottery winners and losers are subject to differential degrees of attrition because category (ii) pairs are contained in Ψ_1 only (but not Ψ_0). As the degree of differential attrition, only about 2.7 percent, is quite small in practical terms in our context, we relegate the analysis of differential attrition bias to Appendix A. More specifically, Corollary 1 in Appendix A shows that, in the presence of both imperfect matching and differential attrition, the IV estimand in Proposition 3 corresponds to the sum of the LATE parameter of interest, $E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0), T_i(0) = 1]$, and two additional bias terms. The first/second bias term arises if the average ability/treatment effect of marginally retained individuals differs from the weighted average ability/treatment effect of the counterparts to which they are compared (i.e., always retained individuals and falsely matched pairs), and the magnitude of both bias terms are proportional to the ratio of the share of marginally retained individuals in Ψ_1 and the difference in treatment status between Ψ_1 and Ψ_0.

Second, the randomized elite school admissions investigated in this paper constitute a stratified randomized experiment as each school runs an independent admission lottery every year with a varying winning rate. This is different to the simple randomization setup in Proposition 3 in which the assignment probability $P(Z_i = 1)$ is the same for all individuals in the sample. Under stratified randomization with varying assignment probabilities, it is essential to control for lottery membership in the IV procedures, as assignment is random only within each lottery. In Appendix B, we further extend the LATE framework to apply to stratified randomized experiments and show in Corollary 2 that all of the properties in Proposition 3 can be carried over to stratified randomized experiments. More specifically, the IV estimand of a stratified randomized experiment controlling for lottery fixed effects is a weighted average of the simple IV estimands of the various lotteries.

5   Empirical Results

5.1   Lottery Impact on Elite School Enrollment

We first examine the impact of winning a lottery on the likelihood that an applicant would enroll in his/her selected elite school. Panel A of Table 4 presents the results of the first-stage regressions

corresponding to the IV estimations using all linked lottery-MSEE pairs in Ψ. We begin with the results for the full sample in Columns 1-2. The coefficients on the lottery winner dummy show that the enrollment probability at the selected elite school is 19.7 percentage points higher among the MSEE records matched to winners (i.e, Ψ_1) than among those matched to losers (i.e., Ψ_0). As illustrated in Equation (1), compared to the actual lottery impact on students' elite school enrollment, this first-stage estimand is attenuated because of the presence of falsely linked lottery-MSEE pairs in Ψ. Although the consistency property of our IV estimations is unaffected by this attenuation in the first-stage relationship as shown in Proposition 3, it is still interesting and informative if we can estimate the actual, unattenuated, lottery impact on elite school enrollment for applicants. We perform this estimation in Panel B. The unit of analysis is each elite school applicant retained in Ψ and the dependent variable is the total number of matched MSEE records from his/her selected elite school (i.e., $\sum_{j:C_j=C_i} D_j$). As the number of falsely matched MSEE takers who happened to enroll in an applicant's selected elite school is independent of his/her lottery assignment, the win/loss difference in the total number of matches from an applicant's selected elite school has the same expectation as the win/loss difference in the actual enrollment probability among applicants retained in Ψ, i.e., $E[\sum_{j:C_j=C_i} D_j | Z_i = 1] - E[\sum_{j:C_j=C_i} D_j | Z_i = 0] = E[D_i | T_i = 1, Z_i = 1] - E[D_i | T_i = 1, Z_i = 0]$. The coefficient on the lottery winner dummy in this regression shows that winning a lottery increases an applicant's probability of enrolling in his/her selected elite school by 34.0 percentage points. If there were no differential attrition, the ratio of the two coefficients in Panels A and B ($19.7/34.0 = 0.579$) would correspond to the proportion of correct matches among all of the matches in Ψ.

If all of the applicants retained in Ψ complied with their lottery assignments, then the coefficient on the lottery winner dummy would be 1 in Panel B. However, the estimated coefficient is only one-third of that, thus suggesting a high degree of noncompliance in our context, which occurs when lottery losers enrolled in their selected elite school through back door admissions or lottery winners declined their admission offers. Quantifying these two types of non-compliance requires information on the enrollment status of each applicant in his/her selected elite school. However, perfect identification of such information is not possible, as we cannot distinguish correctly linked pairs from falsely linked ones in Ψ due to imperfect matching. Nevertheless, we conduct an exercise in which an applicant's enrollment status in his/her selected elite school is inferred by whether his/her name appears in the school's list of MSEE records (conditional on gender and cohort). Although false matching by name and gender is quite common among approximately 20,000 MSEE records from all of the middle schools in the North Bank in a given year as discussed in Section 3, the chances

of it occurring become very rare, albeit not entirely zero, when the target universe is restricted to a few hundred students graduating from a particular elite school. Therefore, the probability of mistakenly inferring an unenrolled applicant as enrolled is quite small, and the win/loss difference in the inferred enrollment status should be close to that in the actual enrollment status. As shown in Panel C of Table 4, 52.8 percent of the lottery losers are inferred to have enrolled in his/her selected elite school, whereas only 10.5 percent of the lottery winners are inferred to have declined their admission offers. The regression-adjusted win/loss difference in the inferred enrollment status is 33.2 percentage points, very close to the 34.0-percentage-point difference reported in Panel B.

Columns 3-4 in Table 4 report separate results for the subsample of District 3 applicants with baseline scores. Based on the inferred elite school enrollment status, this subsample has a lower noncompliance rate than the full sample: only 39.6 percent of the lottery losers are inferred to have enrolled in their selected elite school and only 6.4 percent of the lottery winners are inferred not to have enrolled, compared to 52.8 and 10.5 percent, respectively, of those in the full sample. Consequently, the marginal effect of winning a lottery on an applicant's probability of enrolling in his/her selected elite school for this subsample, an estimated 51.9 percentage points, is much larger than that for the full sample (34.0 percentage points). The addition of baseline scores in Column 4 has almost no effect on the coefficients on the lottery winner dummy in any of the three panels in Table 4. However, the coefficient on the baseline scores itself is always positive and significant, thus suggesting a positive association between lottery losers' baseline scores and their enrollment probability through back door channels. Figure 2A further illustrates the selection in back door admissions by plotting the kernel density curves of the baseline scores for lottery losers in District 3 by their inferred enrollment status in the elite school of their choice. The difference between the two distribution curves confirms that lottery losers who attended their selected elite school through back door admissions had substantially higher average baseline scores than those who did not (0.42σ vs. 0.22σ). We next examine whether lottery winners who gave up the option to attend an elite school differ from those who exercised this option in terms of baseline scores. Figure 2B plots the kernel density curves of the baseline scores lottery winners in District 3 by their inferred enrollment status in the elite school of their choice. The two-sample Kolmogorov-Smirnov test (with a p-value of 0.948) cannot reject the equality of the two distributions.

5.2 IV Estimates of the Elite School Attendance Effects

In this subsection, we examine the effects of attending an elite school on students' academic outcomes in three years' time. Based on the MSEE scores and secondary school admission outcomes, we construct five measures of students' ex post academic outcomes: total scores on the MSEE, a

dummy for admission to a top-echelon high school, a dummy for scoring above the threshold for top-echelon high school admissions, a dummy for admission to any high school (including both top-echelon and regular high schools), and a dummy for scoring above the threshold for regular high school admissions. Every year, the city's education council announces the minimum score requirements for top-echelon and regular high school admissions. Students with MSEE scores below the minimum requirement for regular high school admission either drop out of school after completion of nine years of compulsory education or attend a vocational secondary school to obtain job-oriented training. The reason for including the dummy indicators for scoring above the two thresholds in addition to secondary school admission status is to examine whether elite school attendance increases the likelihood of admission to a top-echelon or regular high school through channels beyond entrance exam scores.

Table 5 reports our regression results employing all linked lottery-MSEE pairs in Ψ. Each row uses one of the aforementioned five outcome measures as the dependent variable, and each cell corresponds to a separate regression. Columns 1-3 show the OLS, reduced-form and IV estimates, respectively, for the full sample. Because the OLS regressions employ only information on middle school enrollment and student outcomes contained in the MSEE data set, they correspond to cross-sectional regressions of student outcomes on elite school enrollment status in the subsample of MSEE takers whose names appear in the list of lottery participants (conditional on gender and cohort). The OLS coefficients on the five outcome measures in Column 1 are all positive and significant. Those from the regressions using total MSEE scores and top-echelon high school admission status as the dependent variables show that elite school attendance is associated with 0.4σ higher total scores on the MSEE and a 12.4 percentage points higher probability of admission to a top-echelon high school, a more than 50 percent increase over the citywide average admission rate of 21.7 percent. The large and significant OLS coefficients, however, are at least in part attributable to selective lottery participation and applicants' selective noncompliance with their lottery assignments. These two sources of selection are illustrated previously in Figures 1B and 2A, respectively. The former contributes to the OLS coefficients because Ψ contains a large number of falsely matched MSEE records that pertain to non-applicants (most of whom did not enroll in an elite school), whereas the latter contributes to the OLS coefficients by allowing more able applicants to have a greater chance of gaining access to elite schools after losing the lottery.

To circumvent the spurious cross-sectional relationship between elite school enrollment and student outcomes, we examine in Column 2 the reduced-form relationship between lottery assignments and student outcomes in Ψ. Similar to our earlier exposition on the first-stage relationship, the reduced-form coefficients reflect the win/loss difference in academic outcomes among lottery partic-

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ipants but are attenuated by the existence of falsely matched pairs. Despite such attenuation, we would still expect a positive reduced-form relationship in Ψ if winners outperformed losers in academic outcomes upon graduation from middle school. On the contrary, the reduced-form coefficients are insignificant and virtually zero for all five outcome measures, thus providing little evidence of any difference between winners and losers in ex post academic outcomes. Compared to the attendance effects, the reduced-form coefficients are attenuated by the extent of both the contamination of falsely matched pairs in Ψ and the imperfect compliance of lottery participants. To estimate the attendance effects that are not attenuated by these two factors, we use the applicant's lottery assignment as an instrument for the matched MSEE taker's enrollment status at the elite school of the applicant's choice for every linked lottery-MSEE pair in Ψ. The IV coefficients, presented in Column 3, are never significant and often negative,[10] thus providing little evidence of any positive academic gains from elite school attendance once selection in enrollment is accounted for. The point estimate of the effect of elite school attendance on MSEE scores is -0.016 and allows rejection of a relatively modest achievement gain of 0.15σ at the ten percent level. Columns 4-6 of Table 5 replicate the regressions in Columns 1-3 using the subsample of applicants from District 3 with baseline scores. All of the IV coefficients for this subsample are insignificant, showing qualitatively the same results as those for the full sample.

Our empirical analysis so far has ignored the impact of differential attrition between winners and losers in Ψ. However, results in Table 3 show evidence for a small degree of differential attrition. As illustrated in Corollary 1, differences in the ability and treatment effect between marginally retained individuals and the counterparts to which they are compared to (i.e., always retained individuals and falsely matched MSEE takers) can lead to biases in the IV estimates. We employ Corollary 1 in Appendix C to discuss in detail the sign and magnitude of the potential bias that may arise from differential attrition. In sum, this exercise shows that differential attrition is likely to bias our IV estimates upward, although the size of the bias, estimated to be bounded by a few percent of a standard deviation in MSEE scores, is quite small in practical terms. Nonetheless, despite of the potentially positive differential attrition bias in favor of elite schools, our estimates still provide little evidence that elite schools confer any clear academic benefits to compliers who gain access through admission lotteries.

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[10] The only positive IV coefficient is obtained when the dummy indicator for admission to a top-echelon high school is used as the dependent variable, but it is not statistically significant.

5.3 Possible Pathways of Elite School Exposure

Despite of the large observed superiority of elite schools, our IV estimates show little evidence that elite school attendance improves students' MSEE scores or secondary school admission outcomes. To understand this obscurity, we now turn to the possible pathways through which differences in the schooling environment between elite and neighborhood schools may affect student achievement. First, elite school attendees are exposed to higher-achieving peers. If better peers facilitate learning, then elite schools' large peer advantage should benefit their attendees. At the same time, however, the admission lottery compliers we examine in this study are, on average, relatively weak students in elite schools owing to the existence of advance and back door admissions. If lower ranking is demoralizing, then exposure to higher-achieving peers may diminish student performance. In addition, peer effects may be heterogeneous and depend on a student's initial achievement or ranking. For example, in the context of secondary schools in China, Ding and Lehrer (2007) find that students who score in the top quantiles benefit much more from having high-achieving peers than those who score in the lowest quantiles. Given the limited and mixed evidence on the mechanisms of peer effects in the existing literature,[11] it is unclear whether exposure to higher-achieving peers in elite schools has an overall positive effect on the admission lottery compliers.

Second, elite schools typically employ better qualified teachers than neighborhood schools, which should benefit all their attendees. At the same time, because of the exam school tradition and over-representation of high-achieving students, classroom instruction in these schools tends to emphasize more advanced materials and to be more challenging. In an experimental evaluation in Kenya, Duflo, Dupas, and Kremer (2011) find that low-achieving students benefit from tracking because the teachers assigned to the lower-achieving classrooms adjust their teaching level toward these students' ability. Thus, the more advanced classroom instruction in elite schools may have adverse impact on the relatively weak admission lottery compliers, offsetting the positive effect of teacher quality, if any. Accordingly, similar to the reasoning of Bui, Craig, and Imberman (2011) concerning the lack of achievement benefits for marginal students admitted to gifted and talented programs in the US, the students who gained access to elite schools through admission lotteries in our study may also have been ill-positioned to benefit from such experience. Lastly, elite schools typically have higher per-pupil spending and better facilities, such as language learning centers and science laboratories. On the other hand, they also have much larger class sizes due to strong parental demand, thus making it difficult to determine whether the overall resource effect is positive. In sum, for each pathway considered (i.e., peer quality, teacher quality and focus, and school resources),

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[11]Examples include Carman and Zhang (2012), Hoxby and Weingarth (2006), Imberman, Kugler, and Sacerdote (2012), Lavy, Paserman, and Schlosser (2012), and Lavy, Silva, and Weinhardt (2012)

elite schools expose their attendees to elements that may have opposing effects on achievement. Our failure to find score improvement most likely reflects the combined workings of the various forces that shape the performance of elite school attendees, some of which may be value-adding and others value-reducing, although we are unable to distinguish the extent of these forces.[12]

5.4 Parental Perception of "Better" Schools and Effective Value-Added

The persistent popularity of elite schools despite the lack of score improvement suggests a discrepancy between parental perceptions of "better" schools and effective value-added education. To further understand the rationale behind parental school choice, we construct two popularity measures for elite schools – the oversubscription rate (ratio of the total number of applicants to the general admission quota) and winner take-up rate (proportion of lottery winners enrolled) – and examine their relationship to average student achievement and estimated value-added. Figures 3A and 3B plot the two popularity measures against a school's average MSEE scores.[13] Both measures are found to be positively associated with the average MSEE scores, thus suggesting that the most popular elite schools are those with highest average student achievement. Figures 4A and 4B plot the two popularity measures against the estimated value-added effect. If parents indeed prefer and are able to identify high value-added schools, we would expect to see higher value-added elite schools possessing a heavier oversubscription rate and a higher take-up rate among lottery winners. Contrary to our expectation, Figure 4A shows a negative correlation between a school's oversubscription rate and value-added and Figure 4B suggests no relationship between a school's winner take-up rate and value-added. The patterns in Figures 3 and 4 are confirmed by the regression analysis results in Table A2 of the Appendix. Columns 3 and 6, in particular, show that the positive effects of average student achievement on both popularity measures remain significant even after controlling for the estimated value-added effect.

The lack of evidence of any achievement benefits conferred by elite schools, together with the positive association between school popularity and average student achievement, indicates that elite schools may be sought after primarily for their observed superiority in student outcomes. One

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[12] In their study on exam schools in Boston and New York, Abdulkadiroglu, Angrist, and Pathak (2012) interpret their RD estimates as either the upper bound of peer effects assuming all other exam school inputs are beneficial to student achievement (Proposition 1) or the sum of direct and indirect effects of peers assuming all other inputs are causally downstream to peer characteristics (Proposition 2). However, in the context of elite schools in China, some elements of elite school exposure, as discussed in the text, may not necessarily be value-adding or downstream to peer chacteristics.

[13] More specifically, Figures 3A and 3B plot the popularity measures of each elite school in each year against the average MSEE achievement of the school's attendees over the study period except for the cohort used in calculating the popularity measures after controlling for district-year-specific fixed effects. Note that the corresponding cohort of attendees is excluded in calculating each school's average MSEE scores because a school's popularity in a given year may affect the ex post MSEE achievement of attendees in that cohort through its effect on their composition.

explanation is that parents choose elite schools for reasons other than their impact on learning, for example, for nonacademic attributes such as school facilities and peer quality (beyond its effect on achievement). Another explanation is that parents may confuse student outcomes with achievement gains and therefore use the former to proxy for the latter. Figure 5 plots schools' estimated value-added effects against average MSEE scores and shows that the two measures are largely uncorrelated, echoing previous findings of a weak correlation between school grades and value-added in the US school accountability literature (see Kane and Staiger [2002] for a survey). Thus, when student outcomes constitute a poor proxy for achievement gains (as in the case investigated herein), parents are likely to misidentify schools with effective value-added. A third explanation is that, because of the large differences in the accuracy between value-added and peer quality measurements, parents may choose a school based primarily on peer quality rather than value-added, even though they indeed value achievement gains the most. For example, parents may place a high intrinsic weight on achievement gains and a low intrinsic weight on peer quality (beyond its effect on achievement) in choosing a school. However, because value-added is very imprecisely measured whereas peer quality can be observed directly with accuracy, the high intrinsic weight on value-added is swamped by its noisy measurement, resulting in schools being chosen mainly for their observed peer quality. Although we cannot differentiate between these three underlying reasons from the existing empirical evidence, any of them being true would lead to elite schools being sought after predominantly for their observed superiority in student achievement rather than their academic value-added, thus reducing the potential of school choice to improve student achievement.

6   Conclusion

The empirical evidence on whether students benefit from attending "better" (i.e., selective, elite, or high-achieving) schools is mixed in the existing literature. In this paper, we present new evidence on this question by exploiting exogenous variation in elite school attendance induced by school admission lotteries in China. In addition to the natural experimental setting, the use of uniform curriculum across schools and the rigid entrance exam-based secondary and university admissions render the Chinese context very clean and ideal for evaluating the effects of superior schooling on student achievement and comparing parental perceptions of "better" schools to evidence of value-added advantage.

Although winning a lottery substantially increases students' chances to enroll in their selected elite schools that are far advantageous in peer achievement compared to neighborhood schools, we find little evidence that elite school experience improves students' MSEE scores or their secondary

school admission outcomes. We show that it is unlikely that our failure to establish evidence of a positive achievement gain from elite school attendance is driven by biases that arise from lottery assignment-induced differential attrition. We also find that the most sought-after elite schools are those with the highest observed student achievement on the MSEE rather than those with the largest value-added effect on test scores, thus suggesting that parental choice may be based primarily on a school's observed superiority in student outcomes. Our finding that schools are chosen for reasons other than their achievement benefits casts doubt on the potential of school choice to improve student achievement in the Chinese context.

This paper also makes an important methodological contribution to the program evaluation literature by extending the benchmark LATE framework to treatment effect analysis in contexts with imperfect matching, encountered when combining an assignment data set and a treatment/outcome data set in the lack of a common unique identifier. We develop a data combination procedure that forms all pairwise links between records in the two data sets that are matched by the common variables, and show that the IV estimate constructed employing all linked record pairs in the combined data set identifies the same causal parameter as in the case under perfect observation. As imperfect matching is commonly confronted in program evaluations involving the combination of information from different data sets, our extended LATE framework is widely applicable to a variety of contexts in which similar observational problems arise following data combination.

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Table 1 Enrollment and Student Outcomes by School Type

	Elite schools (1)	Neighborhood schools (2)	Private schools (3)	Total (4)
Number of schools	16	160	5	181
Average number of 9th-grade students	591	223	114	252
Enrollment share (%)	20.7	78.0	1.3	100.0
Mean MSEE score (in s.d.)	0.523	-0.137	-0.093	0
% of students admitted to top-echelon high schools	40.0	16.9	10.5	21.7
% of students admittted to regular high schools	40.9	27.5	26.3	30.5

Notes: All statistics are calculated from a random sample of MSEE takers in the study city in 2005.

Table 2 Predetermined Individual Characteristics and Lottery Assignments

	Means			Coefficients		
	Full sample	District 3 subsample	District 3 subsample w/ baseline scores	Full sample	District 3 subsample	District 3 subsample w/ baseline scores
	(1)	(2)	(3)	(4)	(5)	(6)
				Dependent variable: lottery winner		
Lottery winner	0.301	0.216	0.215			
	(0.459)	(0.412)	(0.411)			
Female	0.469	0.493	0.489	-0.008	-0.013	-0.021
	(0.499)	(0.500)	(0.500)	(0.008)	(0.014)	(0.015)
6th-grade baseline score available	-	0.854	1.000	-	-0.002	-
		(0.354)	(0.000)		(0.020)	
6th-grade baseline score (in s.d.)	-	-	0.292	-	-	0.011
			(0.759)			(0.010)
F-statistics				F(176,13,591)=0.7	F(35,3447)=0.58	F(34,2,939)=0.68
Prob > F				0.985	0.979	0.919
Number of observations	13,768	3,483	2,973	13,768	3,483	2,973

Notes: Columns (1)-(3) report the mean of each variable indicated by the row heading for each sample. Columns (4)-(6) report the coefficients of a linear regression of the lottery winner dummy on the independent variables indicated by the row headings and full set of primary school and lottery dummies. The F-statistics and Prob > F report, respectively, the F-test statistic and p-value for a two-tailed test of the hypothesis that the coefficients on all of the predetermined individual characteristics, including primary school dummies but excluding lottery dummies, are zero. The numbers reported in parentheses are standard deviations in Columns (1)-(3) and standard errors in Columns (4)-(6).

Table 3 Matching Outcomes and Lottery Assignments

	Full sample		District 3		District 3 w/ baseline scores	
	Losers' mean	Win/loss difference	Losers' mean	Win/loss difference	Losers' mean	Win/loss difference
	(1)	(2)	(3)	(4)	(5)	(6)
Number of matches (n_i)	1.580	0.027	1.779	0.013	1.889	0.027
	(2.059)	(0.039)	(2.364)	(0.098)	(2.416)	(0.148)
Overall match rate ($n_i>=1$)	0.892	0.022 ***	0.904	0.018	0.940	0.018 *
	(0.311)	(0.006)	(0.295)	(0.012)	(0.238)	(0.011)
Number of observations	9,630	13,768	2,730	3,483	2,335	2,973

Notes: Odd columns report the mean for lottery losers, and even columns report the regression-adjusted win/loss difference after controlling for lottery fixed effects. The numbers reported in parentheses are standard deviations in odd columns and standard errors in even columns.

*significant at 10%; *** significant at 1%.

Table 4 Effect of Winning an Admission Lottery on Elite School Enrollment

	Full sample		District 3 subsample w/ baseline scores	
	no controls	w/ controls	no controls	w/ controls
	(1)	(2)	(3)	(4)
Panel A				
Unit of analysis: matched record pairs in the combined data set				
Dependent variable: enrollment status at the elite school of an applicant's choice				
Lottery winner	0.197 ***	0.196 ***	0.258 ***	0.257 ***
	(0.007)	(0.007)	(0.014)	(0.014)
Female	-	-0.020		0.002
		(0.006)		(0.011)
Baseline score	-	-		0.034 ***
				(0.008)
Number of observations	21,676		5,606	
Panel B				
Unit of analysis: applicants retained in the combined data set				
Dependent variable: the number of matched MSEE records from the elite school of an applicant's choice				
Lottery winner	0.340 ***	0.340 ***	0.519 ***	0.517 ***
	(0.009)	(0.009)	(0.021)	(0.021)
Female	-	-0.019		0.001
		(0.008)		(0.017)
Baseline score	-	-		0.055 ***
				(0.012)
Number of observations	12,347		2,803	
Panel C				
Unit of analysis: applicants retained in the combined data set				
Dependent variable: the inferred enrollment status at the elite school of an applicant's choice				
Losers' mean in the inferred enrollment status	0.528		0.396	
Winners' mean in the inferred enrollment status	0.895		0.936	
Lottery winner	0.332 ***	0.332 ***	0.498 ***	0.496 ***
	(0.009)	(0.009)	(0.020)	(0.020)
Female	-	-0.014		-0.003
		(0.008)		(0.016)
Baseline score	-	-		0.056 ***
				(0.011)
Number of observations	12,347		2,803	

Notes: This table reports the coefficients of regressions of the dependent variables (specified in each panel) on the lottery winner dummy and a set of lottery fixed effects. The even columns further include a female dummy and baseline scores (if available) as covariates. The unit of analysis is the matched record pairs in the combined data set for Panel A and the applicants contained in the combined data set for Panels B and C. Standard errors are reported in parentheses. *** significant at 1%.

Table 5 OLS, Reduced-form, and IV Estimation Results

	Full sample			District 3 subsample w/ baseline scores		
	OLS	Reduced-form	IV	OLS	Reduced-form	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable						
MSEE scores (in s.d.)	0.400	-0.003	-0.016	0.353	0.010	0.038
	(0.041)	(0.019)	(0.100)	(0.105)	(0.047)	(0.182)
Admission to a top-echelon high school	0.124	0.006	0.031	0.107	0.008	0.029
	(0.014)	(0.007)	(0.035)	(0.049)	(0.013)	(0.053)
Scoring above the threshold for top-echelon high school admission	0.087	-0.001	-0.005	0.088	0.000	0.001
	(0.016)	(0.006)	(0.033)	(0.046)	(0.011)	(0.043)
Admission to any high school	0.181	-0.003	-0.015	0.149	0.002	0.008
	(0.016)	(0.009)	(0.045)	(0.041)	(0.022)	(0.086)
Scoring above the threshold for high school admission	0.087	-0.001	-0.008	0.088	0.000	0.067
	(0.016)	(0.006)	(0.042)	(0.046)	(0.011)	(0.091)
Number of observations	21,676			5,606		

Notes: This table reports the OLS, reduced-form, and IV estimation results. Each cell corresponds to a separate estimation. Columns (1) and (4) report the OLS estimates of elite school attendance effect on each dependent variable indicated by the row heading for the full sample and District 3 subsample with baseline scores, respectively. Columns (2) and (5) report the reduced-form estimates of the effect of winning an admission lottery. Columns (3) and (6) report the IV estimates of the elite school attendance effect using lottery assignments as an instrument. Robust standard errors clustered by middle school attended interacted with graduation year are reported in parentheses.
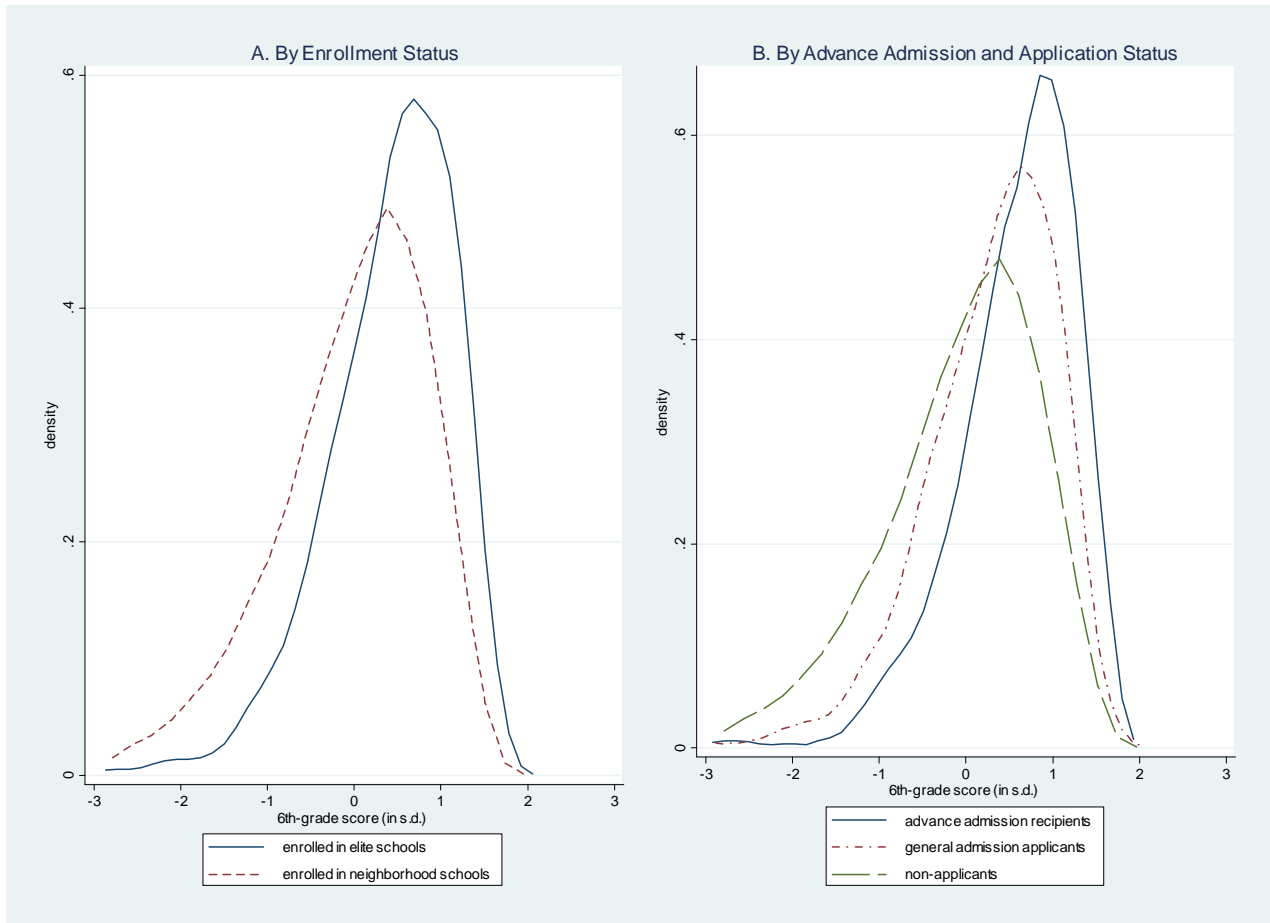
Figure 1 Baseline Score Distributions, District 3 Students

Notes: Panel A plots the Kernel density curve of 6th-grade scores of District 3 students by their elite school enrollment status. The Kolmogorov-Smirnov two-sample test has a p-value of 0.000, showing that elite school and neighborhood school students are different in terms of baseline scores. Panel B plots the Kernel density curve of 6th-grade scores for District 3's advance admission recipients, general admission applicants, and non-applicants, respectively. The Kolmogorov-Smirnov two-sample test results show that the three distributions are all different from one another with a p-value of 0.000.
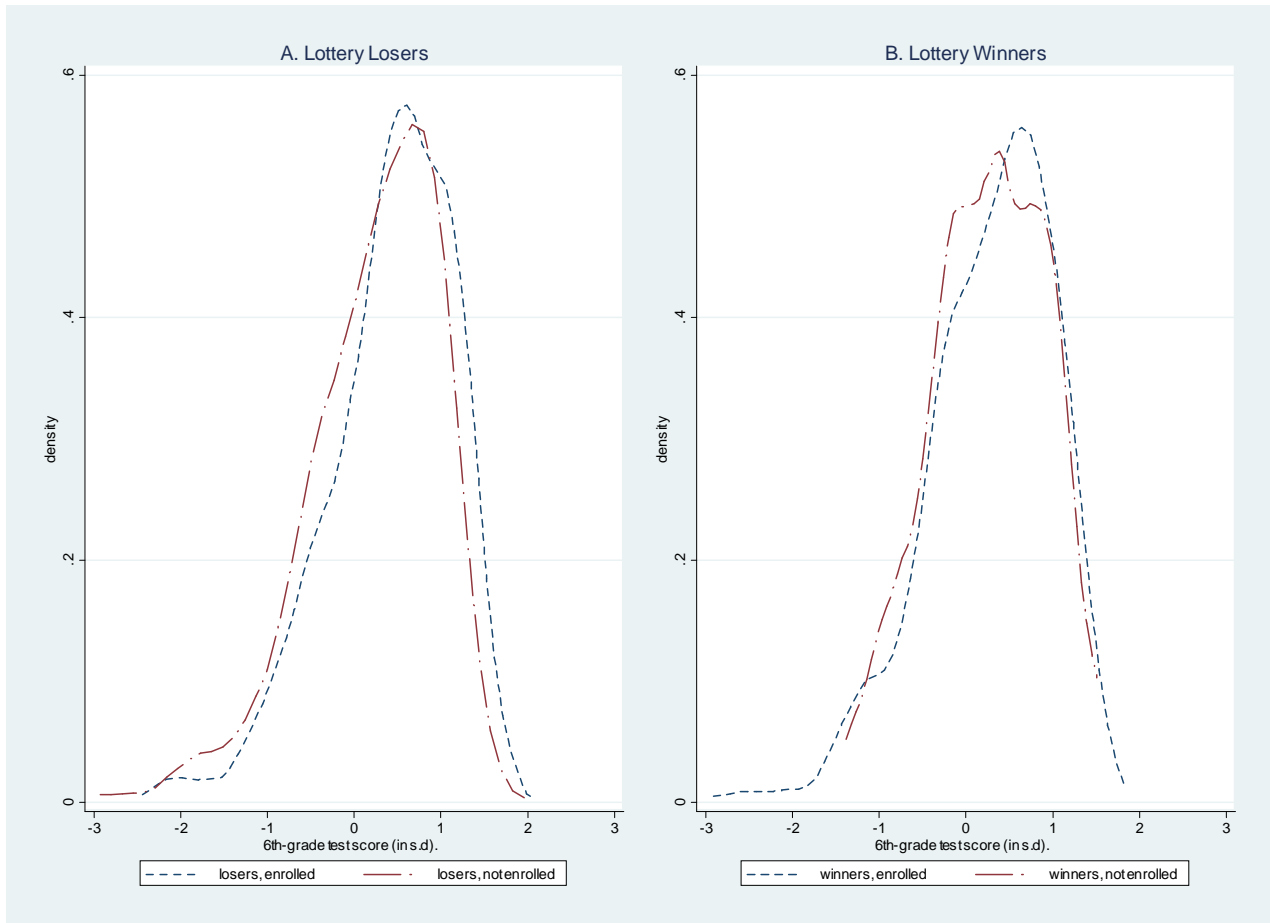
Figure 2 Baseline Score Distributions by Lottery and Enrollment Status, District 3 Applicants

Notes: Panel A plots the Kernel density curve of the 6th-grade scores of lottery losers in District 3 by their inferred elite school enrollment status. The Kolmogorov-Smirnov two-sample test has a p-value of 0.000, which rejects the equality of the two distributions. Panel B plots the Kernel density curve of the 6th–grade scores of lottery winners in district 3 by their inferred elite school enrollment status. The Kolmogorov-Smirnov two-sample test has a p-value of 0.948, which cannot reject the equality of the two distributions.
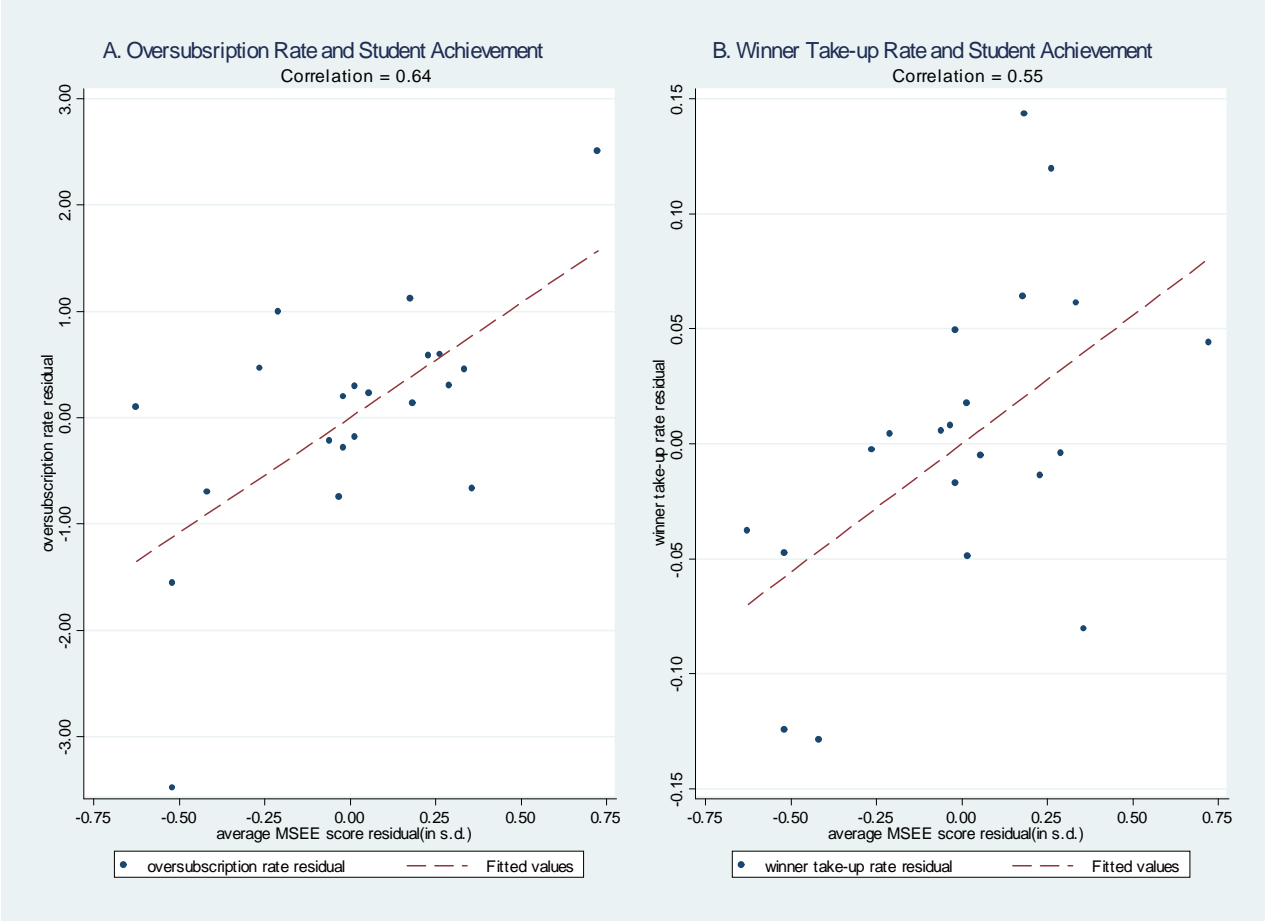
Figure 3 School Popularity and Student Achievement

Notes: Panel A plots the oversubscription rate residual against the value-added effect residual after controlling for district-year-specific fixed effects. Panel B plots the winner take-up rate residual and the value-added effect residual after controlling for district-year-specific fixed effects.

Figure 4 School Popularity and Value-Added Effect

Notes: : Panel A plots the oversubscription rate residual against the value-added effect residual after controlling for district-year-specific fixed effects. Panel B plots the winner take-up rate residual and the value-added effect residual after controlling for district-year-specific fixed effects.
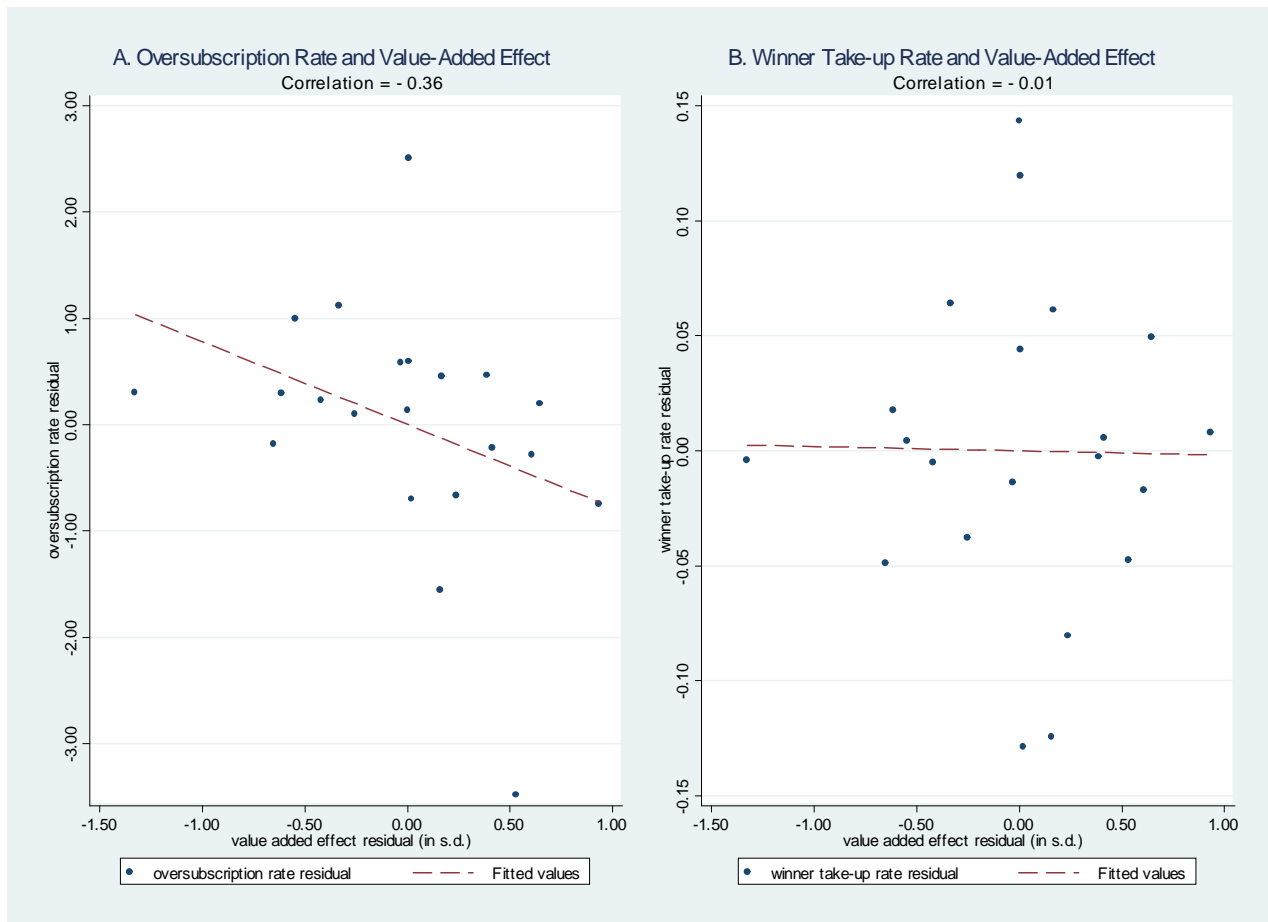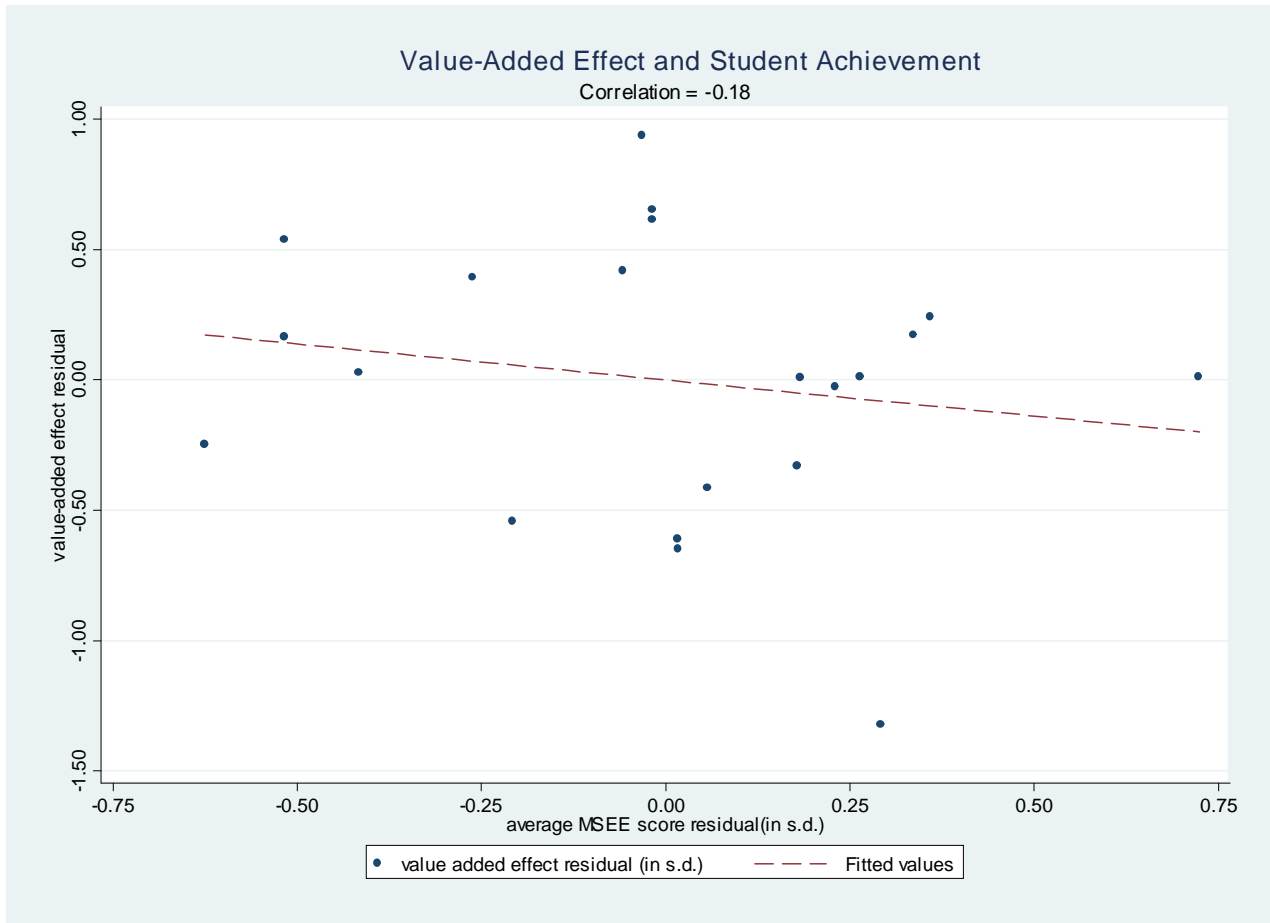
Figure 5 Value-Added Effect and Student Achievement

Notes: : This graph plots the value-added effect residual against the average MSEE score residual after controlling for district-year-specific fixed effects. In calculating a school's average MSEE score, we exclude the corresponding cohort of attendees used in estimating the school-cohort-specific value-added effect.

Appendix

A. Extended LATE Framework under Monotone Sample Attrition

The independent sample attrition assumption in Proposition 3 does not hold in practice as winning a lottery has a monotone effect on an individual's observability in the MSEE data set, resulting in differential attrition between winners and losers in Ψ. Figure 1A illustrates the partition of the linked lottery-MSEE pairs in Ψ in a more generalized setting with both imperfect matching and differential attrition. The subset of linked lottery-MSEE pairs to losers, Ψ_0, consists of *always retained individuals* (with a proportion of p_0) and *falsely matched pairs* (with a proportion of $1 - p_0$), and the subset of linked lottery-MSEE pairs to winners, Ψ_1, is comprised of *always retained individuals* (with a proportion of p_1), *marginally retained individuals* (with a proportion of p_m), and *falsely matched pairs* (with a proportion of $1 - p_1 - p_m$). These three fractions are linked in such a way that $p_1 = p_0(1 - p_m)$. Following Angrist, Imbens, and Rubin (1996), we further partition always retained individuals into *always takers* ($D_i(0) = D_i(1) = 1$, with a proportion of d_a), *compliers* ($D_i(0) = 0, D_i(1) = 1$, with a proportion of d_c), and *never takers* ($D_i(0) = D_i(1) = 0$, with a proportion of $1 - d_a - d_c$). In addition, we also divide falsely matched pairs by treatment status into *treatment takers* ($D_j = 1$, with a proportion of d_f) and *treatment non-takers* ($D_j = 0$, with a proportion of $1 - d_f$), and assume that all marginally retained individuals in Ψ_1 are indeed treated, i.e., $D_i(1) = 1 \ \forall \ i : T_i(1) > T_i(0)$.

With the foregoing partitions and notations, the ITT estimand of Z's effect on D, constructed by comparing the means in the treatment status between Ψ_1 and Ψ_0, can be expressed as

$$
\begin{aligned}
&E[D_{i(j)}|Z_i = 1, C_j = C_i] - E[D_{i(j)}|Z_i = 0, C_j = C_i] \\
&= p_1 E[D_i(1)|T_i(0) = 1] + p_m E[D_i(1)|T_i(1) > T_i(0)] + (1 - p_1 - p_m)E[D_j|C_j = C_i, j \neq i] \\
&\quad - p_0 E[D_i(0)|T_i(0) = 1] - (1 - p_0)E[D_j|C_j = C_i, j \neq i] \\
&= p_1(d_a + d_c) + p_m \cdot 1 + (1 - p_1 - p_m)d_f - p_0 d_a - (1 - p_0)d_f \\
&= p_0 d_c + p_m[1 - p_0(d_c + d_a) - (1 - p_0)d_f].
\end{aligned}
\tag{1'}
$$

The first term, $p_0 d_c$, corresponds to the potential ITT effect of Z on D in Ψ_0, whereas the second term is the bias arising from the existence of marginally retained individuals in Ψ_1 only, whose treatment status differs from that of the always retained individuals and falsely matched pairs in Ψ_0 to which they are compared. Analogously, the ITT estimand of Z's effect on Y in Ψ can be written as

$$E[Y_{i(j)}|Z_i = 1, C_j = C_i] - E[Y_{i(j)}|Z_i = 0, C_j = C_i]$$

$$= p_1 E[Y_i(0) + \gamma_i D_i(1)|T_i(0) = 1] + p_m E[Y_i(0) + \gamma_i D_i(1)|T_i(1) > T_i(0)] + (1 - p_1 - p_m)E[Y_j(0) + \gamma_j D_j|C_j = C_i, j \neq i]$$

$$- p_0 E[Y_i(0) + \gamma_i D_i(0)|T_i(0) = 1] - (1 - p_0)E[Y_j(0) + \gamma_j D_j|C_j = C_i, j \neq i]$$

$$= p_1(y_a + \gamma_a d_a + \gamma_c d_c) + p_m(y_m + \gamma_m) + (1 - p_1 - p_m)(y_f + \gamma_f d_f) - p_0(y_a + \gamma_a d_a) - (1 - p_0)(y_f + \gamma_f d_f)$$

$$= p_0 \gamma_c d_c + p_m[(y_m + \gamma_m) - p_0(y_a + \gamma_c d_c + \gamma_a d_a) - (1 - p_0)(y_f + \gamma_f d_f)], \tag{2'}$$

where y_a, y_m, and y_f denote the average potential outcome **without treatment** for always retained individuals, marginally retained individuals, and falsely matched MSEE takers, respectively;[1] and γ_c, γ_a, γ_m, and γ_f denote the average treatment effect for compliers, always takers, marginally retained individuals, and falsely matched treatment takers, respectively.[2] Similar to Equation (1'), the first term, $p_0 \gamma_c d_c$, in Equation (2') corresponds to the potential ITT effect of Z on Y in Ψ_0, whereas the second term is the bias owing to the contamination of marginally retained individuals in Ψ_1, whose average outcome $(y_m + \gamma_m)$ may differ from that of always retained individuals $(y_a + \gamma_c d_c + \gamma_a d_a)$ and falsely matched MSEE takers $(y_f + \gamma_f d_f)$. Taking the ratio of Equations (2') and (1'), we can derive the extended LATE theorem under both imperfect matching and differential attrition as follows.

Corollary 1 *In the presence of imperfect matching, if Assumptions (A1)-(A5) hold and $T_i(1) \geqslant T_i(0) \; \forall \; i \in I$, then the IV estimand without covariates using the combined data set is*

$$\gamma^{IV} = \frac{E[Y_{i(j)}|Z_i = 1, C_j = C_i] - E[Y_{i(j)}|Z_i = 0, C_j = C_i]}{E[D_{i(j)}|Z_i = 1, C_j = C_i] - E[D_{i(j)}|Z_i = 0, C_j = C_i]}$$

$$= \gamma_c + (p_m/\delta)\{\underbrace{y_m - p_0 y_a - (1 - p_0)y_f}_{\eta_1} + \underbrace{\gamma_m - p_0 d_a \gamma_a - (1 - p_0)d_f \gamma_f - [1 - p_0 d_a - (1 - p_0)d_f]\gamma_c}_{\eta_2}\},$$

where all notations are as previously defined except for δ, which denotes the ITT estimand of the effect of Z on D in the combined data set, i.e., $E[D_{i(j)}|Z_i = 1, C_j = C_i] - E[D_{i(j)}|Z_i = 0, C_j = C_i]$.

Remark 1. The magnitude of the bias is proportional to the ratio of the share of marginally retained individuals in Ψ_1 (p_m) and the ITT estimand of $Z's$ effect on D in Ψ (δ). The smaller the size of p_m/δ, the smaller the bias. Proposition 3 can be considered as a special case of Corollary 1 in which the bias term is eliminated in the absence of differential attrition (i.e., $p_m = 0$).

Remark 2. The first bias component in the curly bracket, η_1, arises if the extent of the ability selection of marginally retained individuals differs from that of the counterparts to which they are compared, i.e., always retained individuals and falsely matched MSEE takers. The size of this bias component is determined by the extent to which the average potential outcome without treatment

---

[1] More specifically, $y_a = E[Y_i(0)|T_i(0) = 1]$, $y_m = E[Y_i(0)|T_i(1) > T_i(0)]$, and $y_f = E[Y_j(0)|C_j = C_i, j \neq i]$.

[2] More specifically, $\gamma_c = E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0), T_i(0) = 1]$, $\gamma_a = E[Y_i(1) - Y_i(0)|D_i(0) = 1, T_i(0) = 1]$, $\gamma_m = E[Y_i(1) - Y_i(0)|T_i(1) > T_i(0)]$, and $\gamma_f = E[Y_i(1) - Y_i(0)|C_j = C_i, j \neq i]$.

of marginally retained individuals, y_m, differs from a weighted average of that of always retained individuals and falsely matched MSEE takers, $p_0 y_a + (1 - p_0) y_f$.

Remark 3. The second bias component in the curly bracket, η_2, arises if the average treatment effect (ATE) of marginally retained individuals, γ_m, differs from a weighted average of the ATE of always takers, falsely matched treatment takers, and compliers, $p_0 d_a \gamma_a + (1 - p_0) d_f \gamma_f + [1 - p_0 d_a - (1 - p_0) d_f] \gamma_c$. It is important to note that in the case of homogeneous treatment effects, η_2 becomes zero, as the ATEs are the same for all subgroups.

B. Extended LATE Framework under Stratified Randomization

The randomized elite school admissions investigated in this paper constitute a stratified randomized experiment as each lottery has a varying winning rate. In this appendix, we consider the further extension of the LATE framework to stratified randomization in which the population is partitioned into randomization blocks (strata) and an independent lottery with varying assignment probability is conducted within each randomization block. Let B_i indicate the lottery (i.e., randomization block) that individual i enters, indexed by $k \in \{1, ..., K\}$, where K is the total number of lotteries. Note that when the assignment probability, $P(Z_i = 1)$, varies across lotteries, assignment Z_i is correlated with lottery membership B_i. Therefore, if lottery membership B_i is associated with individuals' potential treatments and outcomes, Z_i is correlated with $\{D_i(0), D_i(1), Y_i(0), Y_i(1)\}$ through its correlation with B_i, which is in violation of Assumption (A2). Nevertheless, a conditional version of (A2) still holds as Z_i is randomly assigned conditional on B_i. Analogously, Assumptions (3) and (5) also hold only in conditional forms.

Assumptions (A2'), (A3') and (A5')

(A2') *Conditional Independence I* : $\{D_i(0), D_i(1), Y_i(0), Y_i(1)\} \perp Z_i | B_i$;

(A3') *Conditional First Stage*: $E[D_i(1) - D_i(0) | B_i] > 0$ and $0 < P(Z_i = 1 | B_i) < 1$;

(A5') *Conditional Independence II*: $\{D_j, Y_j\} \perp Z_i | B_i \; \forall \; C_j = C_i, \; j \neq i$.

When these assumptions hold in their conditional forms in the presence of multiple independently conducted lotteries, it is essential to control for lottery membership in the IV procedures. The following Corollary shows that, controlling for lottery fixed effects, the IV estimand of a stratified randomized experiment is a weighted average of the simple IV estimands of the various lotteries.

Corollary 2 *In the case of stratified randomization with imperfect matching, if Assumptions (A1), (A2'), (A3'), (A4), and (A5') hold and $T_i(1) \geqslant T_i(0) \; \forall \; i \in I$, then the IV estimand with no covariates except the lottery dummies is a weighted average of the simple IV estimands of the various lotteries:*

$$\gamma^{IV} = \sum_{k=1}^{K} \omega_k \gamma_k,$$

where γ_k denotes the simple IV estimand of lottery k, $\frac{E[Y_{i(j)}|Z_i=1,B_i=k,C_j=C_i] - E[Y_{i(j)}|Z_i=0,B_i=k,C_j=C_i]}{E[D_{i(j)}|Z_i=1,B_i=k,C_j=C_i] - E[D_{i(j)}|Z_i=0,B_i=k,C_j=C_i]}$, and ω_k represents the weight for lottery k that equals $\frac{N_k \pi_k (1-\pi_k) \delta_k}{\sum_{k=1}^{K} N_k \pi_k (1-\pi_k) \delta_k}$. In the weight formula, N_k is the total

number of matched pairs for lottery k, $\sum\limits_{i:B_i=k}\left(\sum\limits_{j:C_j=C_i}1\right)$; π_k is the proportion of matched pairs for lottery k that are for winners, $P(Z_i=1|B_i=k,C_j=C_i)$; and δ_k is the win/loss difference in treatment status in all matched pairs for lottery k, $E[D_{i(j)}|Z_i=1,B_i=k,C_j=C_i]-E[D_{i(j)}|Z_i=0,B_i=k,C_j=C_i]$.

Proof. Let $\overline{D_k}$ and $\overline{Y_k}$ denote the average treatment status and outcome of all matched pairs for lottery k, i.e., $\overline{D_k}=\frac{1}{N_k}\sum\limits_{i:B_i=k}\left(\sum\limits_{j:C_j=C_i}D_{i(j)}\right)$ and $\overline{Y_k}=\frac{1}{N_k}\sum\limits_{i:B_i=k}\left(\sum\limits_{j:C_j=C_i}Y_{i(j)}\right)$. The IV estimator, controlling for the set of lottery dummies, is

$$\widehat{\gamma}^{IV}=\frac{\sum\limits_{k=1}^{K}\left\{\sum\limits_{i:B_i=k}\left[\sum\limits_{j:C_j=C_i}Z_i(Y_{i(j)}-\overline{Y_k})\right]\right\}}{\sum\limits_{k=1}^{K}\left\{\sum\limits_{i:B_i=k}\left[\sum\limits_{j:C_j=C_i}Z_i(D_{i(j)}-\overline{D_k})\right]\right\}}=\frac{\sum\limits_{k=1}^{K}\left\{\sum\limits_{i:B_i=k,Z_i=1}\left[\sum\limits_{j:C_j=C_i}(Y_{i(j)}-\overline{Y_k})\right]\right\}}{\sum\limits_{k=1}^{K}\left\{\sum\limits_{i:B_i=k,Z_i=1}\left[\sum\limits_{j:C_j=C_i}(D_{i(j)}-\overline{D_k})\right]\right\}}.$$

The population analog of the IV estimator can be written as

$$\gamma^{IV}=p\lim\widehat{\gamma}^{IV}=\frac{\sum\limits_{k=1}^{K}\left\{N_k\pi_k([E[Y_{i(j)}|Z_i=1,B_i=k,C_j=C_i]-E[Y_{i(j)}|B_i=k,C_j=C_i])\right\}}{\sum\limits_{k=1}^{K}\left\{N_k\pi_k(E[D_{i(j)}|Z_i=1,B_i=k,C_j=C_i]-E[D_{i(j)}|B_i=k,C_j=C_i])\right\}}$$

$$=\frac{\sum\limits_{k=1}^{K}\left\{\begin{array}{c}N_k\pi_k(E[Y_{i(j)}|Z_i=1,B_i=k,C_j=C_i]-\pi_kE[Y_{i(j)}|Z_i=1,B_i=k,C_j=C_i]\\-(1-\pi_k)E[Y_{i(j)}|Z_i=0,B_i=k,C_j=C_i])\end{array}\right\}}{\sum\limits_{k=1}^{K}\left\{\begin{array}{c}N_k\pi_k(E[D_{i(j)}|Z_i=1,B_i=k,C_j=C_i]-\pi_kE[D_{i(j)}|Z_i=1,B_i=k,C_j=C_i]\\-(1-\pi_k)E[D_{i(j)}|Z_i=0,B_i=k,C_j=C_i])\end{array}\right\}}$$

$$=\frac{\sum\limits_{k=1}^{K}\left\{N_k\pi_k(1-\pi_k)(E[Y_{i(j)}|Z_i=1,B_i=k,C_j=C_i]-E[Y_{i(j)}|Z_i=0,B_i=k,C_j=C_i])\right\}}{\sum\limits_{k=1}^{K}\left\{N_k\pi_k(1-\pi_k)(E[D_{i(j)}|Z_i=1,B_i=k,C_j=C_i]-E[D_{i(j)}|Z_i=0,B_i=k,C_j=C_i])\right\}}$$

$$=\frac{1}{\sum\limits_{k=1}^{K}N_k\pi_k(1-\pi_k)\delta_k}\sum\limits_{k=1}^{K}\left\{N_k\pi_k(1-\pi_k)\delta_k\frac{E[Y_{i(j)}|Z_i=1,B_i=k,C_j=C_i]-E[D_{i(j)}|Z_i=0,B_i=k,C_j=C_i]}{E[D_{i(j)}|Z_i=1,B_i=k,C_j=C_i]-E[D_{i(j)}|Z_i=0,B_i=k,C_j=C_i]}\right\}$$

$$=\frac{1}{\sum\limits_{k=1}^{K}N_k\pi_k(1-\pi_k)\delta_k}\sum\limits_{k=1}^{K}\left\{N_k\pi_k(1-\pi_k)\delta_k\gamma_k\right\}=\sum\limits_{k=1}^{K}\omega_k\gamma_k.$$

∎

C. Accounting for Differential Attrition Bias

Our analysis in Section 3 suggests a small degree of differential attrition between lottery winners and losers in Ψ, raising concerns over bias from differential attrition in our IV estimates. The prior literature on treatment effect analysis of randomized experiments with missing outcomes resorts to the construction of bounds for the treatment effect by either inferring the missing outcomes with population maximums/minimums (Manski, 1990; Horowitz and Manski, 2000) or trimming the lower/upper tail of the outcome distribution (Lee, 2009). Because our point estimates are close to zero, application of such strategies to our data yields treatment bounds that always include zero and thus cannot help to sign the treatment effect. Moreover, the constructed treatment bounds are

also too wide to be informative. We thus employ an alternative approach to investigate directly the sign and size of the bias arising from differential attrition.

As Corollary 1 shows the magnitude of the bias to be proportional to the ratio of the share of marginally retained individuals in Ψ_1 (p_m) and the first-stage effect (δ), we begin with investigating the size of p_m/δ. The matching statistics in Table 3 indicate an average of 1.58 matches for lottery losers and a win/loss difference of about 0.03. Thus, p_m can be estimated as $0.03/(1.58 + 0.03) = 0.019$. Also, Panel A of Table 4 shows the first-stage coefficient on the lottery winner dummy, δ, to be 0.197. Therefore, the ratio of p_m/δ in the bias formula is close to 0.1.

We now consider the first bias component, η_1, i.e., $y_m - p_0 y_a - (1 - p_0)y_f$, which arises if the extent of the ability selection in terms of potential outcomes without treatment of marginally retained individuals differs from those of always retained individuals and falsely matched MSEE takers. Because of the unobservable nature of potential outcomes without treatment, we are unable to analyze this difference directly. However, for a subsample of District 3 applicants with baseline scores, the extent of this ability selection can be gauged by the difference in the average baseline scores between marginally retained individuals (x_m) and always retained individuals (x_a) and falsely matched MSEE takers (x_f). Let θ_L and θ_W denote the proportion of losers and winners who are unmatched in Ψ, respectively. The former contains both (i) never retained individuals without false matches (i.e., $T_i(1) = 0$ and $F_i = 0$) and (ii) marginally retained individuals without false matches (i.e., $T_i(0) = 0$, $T_i(0) = 1$ and $F_i = 0$); and the latter contains type (ii) applicants only, where F_i is a dummy indicator for whether applicant i has any false match in Ψ. Columns (5)-(6) of Table 3 show $\theta_L = 0.060$ and $\theta_W = 0.042$. It follows that θ_W (0.042) corresponds to the size of type (i) applicants, $P(T_i(1) = 0, F_i = 0)$, and $\theta_L - \theta_W$ (0.018) corresponds to the size of type (ii) applicants, $P(T_i(0) = 0, T_i(1) = 1, F_i = 0)$. Thus, the ratio of type (i) and type (ii) applicants in the unmatched losers is $7 : 3(0.042 : 0.018)$. If these two types of applicants were to have the same ex ante ability, we would expect the unmatched losers and winners to be balanced in their baseline scores. However, Row (a) of Table A1 shows that the former outperform the latter by a large margin of 0.174σ. Because of the small size of unmatched applicants (141 losers and 29 winners) in this subsample, this difference is not statistically significant. However, as the 0.174σ difference is entirely owing to 30 percent unmatched losers pertaining to type (ii), it indicates a difference of 0.582σ in the average baseline scores between type (ii) and type (i) applicants. Based on these coefficients, the average baseline scores of type (ii) applicants are estimated to be 0.636σ. Assuming that false matching is independent of baseline scores, we can proxy x_m by the average baseline scores of type (ii) applicants, who account for three-quarters of all marginally retained individuals given our prior knowledge of a false matching rate of 0.25. Row (b) of Table A1 shows the average baseline scores of matched losers to be 0.303σ. However, in addition to always retained individuals, the matched losers are contaminated by unretained losers with false matches, i.e., $T_i(0) = 0$ and

$F_i = 1$. If we assume the same false matching rate also applies to the subgroup of applicants with $T_i(0) = 0$, i.e., $P(F_i = 1 | T_i(0) = 0) = 0.25$, then the proportion of unretained losers with false matches $P(T_i(0) = 0, F_i = 1) = P(T_i(0) = 0, F_i = 0) \times \frac{P(F_i=1|T_i(0)=0)}{P(F_i=0|T_i(0)=0)} = 0.06 \times \frac{0.25}{1-0.25} = 0.02$. Given this small representation of unretained losers with false matches (0.02) among matched losers (0.94), we thus use the average baseline scores of unmatched losers (0.303σ) to proxy for x_a. Finally, we further assume that the average baseline scores of falsely matched MSEE records (x_f) are 0 and proxy p_0 by the ratio of the two first-stage estimates in Panels A and B of Table 4, $0.197/0.340 = 0.579$. Then, the extent of ability selection in baseline scores, $x_m - p_0 x_a - (1 - p_0)x_f$, is estimated to be 0.461σ. Given our estimate of the marginal effect of baseline scores on MSEE scores, that is, approximately 0.7, η_1 is estimated to be approximately 0.322σ. With our prior estimate of the p_m/δ ratio, 0.1, the magnitude of the ability selection bias due to η_1 appears to be around 0.032σ in the IV estimate.

We next consider the second bias component, η_2, i.e., $\gamma_m - p_0 d_a \gamma_a - (1-p_0)d_f \gamma_f - [1 - p_0 d_a - (1-p_0)d_f]\gamma_c$. As previously noted, η_2 vanishes if treatment effects are homogeneous. In contexts with heterogeneous treatment effects, η_2 depends on the extent to which the ATE of marginally retained applicants (γ_m) differs from that of always takers (γ_a), falsely matched MSEE takers (γ_f), and compliers (γ_c). As we cannot quantify the ATEs of different subgroups, η_2 can be either positive or negative. However, it seems implausible that the extent of heterogeneous treatment effects would outweigh that of ability selection, which would require η_2 to exceed 0.32σ in magnitude. Therefore, the overall differential attrition bias $(p_m/\delta)(\eta_1 + \eta_2)$ in the IV estimate should still be positive, with its magnitude bounded by a few percent of a standard deviation. Nonetheless, despite the potentially positive differential attrition bias, our IV estimates still provide no evidence that elite schools confer positive academic benefits to admission lottery compliers.

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Table A1 Ability Selection in Baseline Scores of District 3 Applicants

	Losers' mean	Win/loss difference
	(1)	(2)
(a) Unmatched applicants	0.229	-0.174
	(0.694)	(0.141)
(b) Matched applicants	0.303	0.034
	(0.770)	(0.033)
(c) Marginally retained individuals w/o false matches	0.636	
$(a1)+\theta_W/(\theta_L-\theta_W)*(a2)$		
Number of observations	2,335	2,973

Notes: Rows (a) and (b) report the average baseline scores for lottery losers and the win/loss difference for matched and unmatched applicants, respectively. Row (c) reports the estimated average baseline score of marginally retained individuals without false matches, calculated as $(a1)+\theta_W/(\theta_L-\theta_W)*(a2)$. θ_w denotes the proportion of winners, estimated to be 0.042, and θ_w denotes the proportion of losers who are unmatched, estimated to be 0.060. The numbers reported in parentheses are standard deviations in odd columns and standard errors in even columns. The number of observations reported in each column is the maximum number of observations used in that column.

Table A2 School Popularity, Average Achievement, and Value-Added

	Dependent variable					
	Oversubscription rate			Winner take-up rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Average MSEE scores (in s.d.)	2.163 **		2.010 **	0.112 **		0.115 **
	(0.757)		(0.759)	(0.049)		(0.051)
Estimated value-added effect (in s.d.)		-0.780	-0.555		-0.002	0.011
		(0.582)	(0.483)		(0.037)	(0.033)
Number of observations	21	21	21	21	21	21

Notes: This table reports the OLS coefficients of the regressions of the dependent variables, indicated by the column headings, on the independent variables, indicated by the row headings, and district-year fixed effects. Note that the oversubscription rate, winner take-up rate, and estimated value-added effect are all measured at the school-cohort level, whereas the average MSEE is calculated using the school's attendees over the study period except for the cohort used in computing other measures. Standard errors are reported in parentheses.
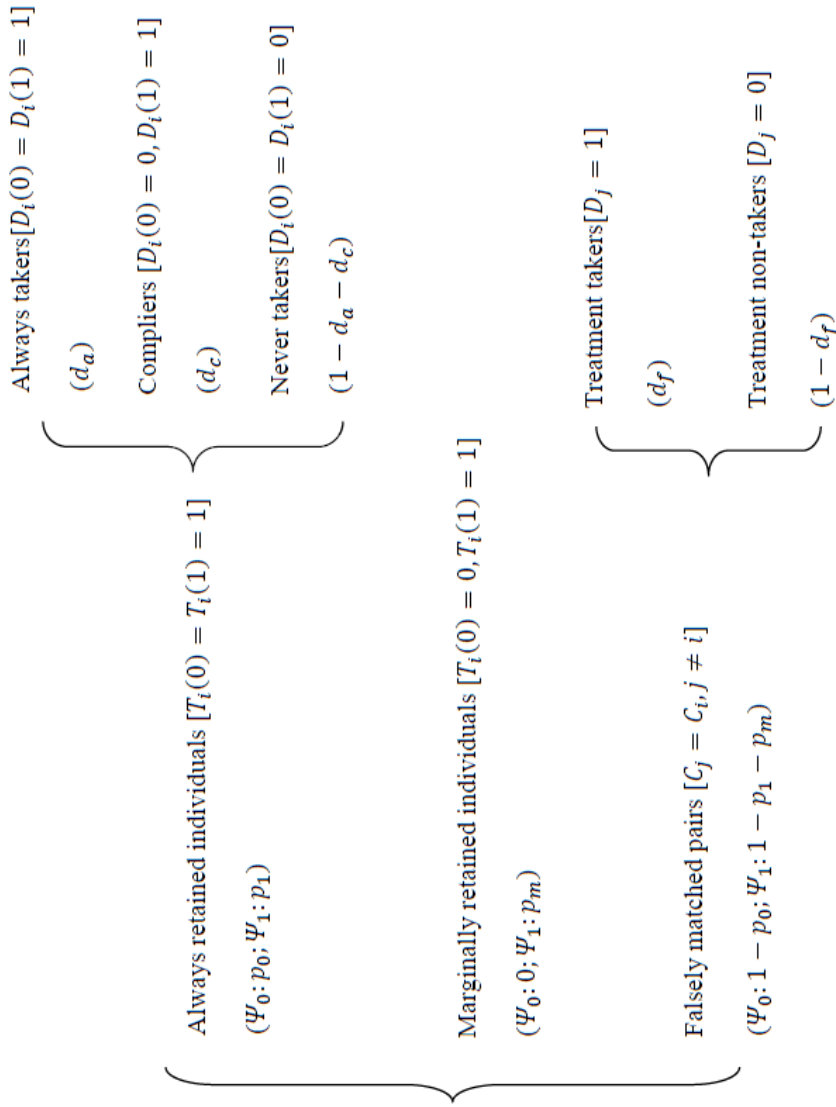**Significant at the 5% level.

Always takers [$D_i(0) = D_i(1) = 1$]

(d_a)

Compliers [$D_i(0) = 0, D_i(1) = 1$]

(d_c)

Never takers [$D_i(0) = D_i(1) = 0$]

$(1 - d_a - d_c)$

Treatment takers [$D_j = 1$]

(d_f)

Treatment non-takers [$D_j = 0$]

$(1 - d_f)$

Always retained individuals [$T_i(0) = T_i(1) = 1$]

$(\Psi_0: p_0; \Psi_1: p_1)$

Marginally retained individuals [$T_i(0) = 0, T_i(1) = 1$]

$(\Psi_0: 0; \Psi_1: p_m)$

Falsely matched pairs [$C_j = C_i, j \neq i$]

$(\Psi_0: 1 - p_0; \Psi_1: 1 - p_1 - p_m)$

Figure A1 Partition of Matched Lottery-MSEE Pairs in Combined Data Set Ψ