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. When combining students’ lottery records and Middle School Exit Exam (MSEE) records, we encounter an imperfect matching problem due to the lack of a common unique identifier. We develop a data combination procedure under imperfect matching and demonstrate that applying the standard IV estimand to our combined data set can still identify the local average treatment effect for lottery compliers. 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 a school’s academic value-added and achievement level are largely uncorrelated in our context and that parents seem to choose schools based primarily on the latter. The fact that parents do not place high expressed weights on value-added in choosing schools also casts doubt on the potential of school choice to increase demand-side pressure for schools to improve effectiveness.

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

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1 Introduction

Parents are willing to pay sizable sums for the opportunity to send their children to what they perceive to be better schools, often largely (if not solely) on the basis of school average test scores. One strand of economic literature has sought to understand the value of better schools to parents by looking at the relationship between school quality and housing prices, and has found unambiguous evidence for a significant positive relationship in a wide range of international contexts.¹ Recent empirical evidence relying on quasi-experimental differences in school quality, driven by reasons such as discontinuity or redrawing of attendance areas, suggests that house prices increase on average by 3-4% for one standard deviation increase in school average test scores, a significant and sizable capitalization effect (for a survey, see Black and Machin, 2011). However, much less is known about the payoffs of attending a better school. While better schools are almost always associated with higher student achievement, such advantage is often at least in part the result of the self-selection of students in school attendance. In empirical work that employs compelling research designs to account for the endogenous school attendance decisions, evidence is ambiguous regarding the value-added of attending a better school. One strand of such research exploits discontinuities in selective school attendance around the admission cutoffs. While Jackson (2010) and Pop-Eleches and Urquiola (2013) discover large test score gains for students attending selective secondary schools in Trinidad and Tobago and Romania, Clark (2010) finds grammar school attendance in the UK, and Abdulkadiroglu, Angrist, and Pathak (2014) and Dobbie and Fryer (2014) also find exam school attendance in the US, to have little effect on student achievement. The other strand of this research employs school admission lotteries to identify exogenous variation in access to better schools, but also yields mixed findings. For example, Cullen, Jacob, and Levitt (2005) find no test score gains among students attending higher-achieving public schools in Chicago, whereas Deming, Hastings, Kane, and Staiger (2014), in contrast, 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 an open question 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 district boundaries. 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, the uniform curriculum, textbooks and high-stakes exit exams adopted in both elite and neighborhood schools make middle schools in China an ideal setting to evaluate the achievement effect of attending better schools. Second, secondary school (grades 10-12) and university admissions in China are almost solely determined by students’ test scores in entrance exams. Thus, compared with many Western contexts where such admissions are based on multi-dimensional criteria going beyond standardized test scores (e.g., teacher recommendations, extracurricular activities, leadership potential, etc.), if better schools add any value to their attendees academically, the test score 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., Hastings, Kane, and Staiger, 2009; Dobbie and Fryer, 2011), attending an elite school in China usually incurs extra cost (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 perception 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 in-
formation 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 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 the 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. As imperfect matching in data combination is not unique to our setting, the extended LATE framework presented herein is also applicable to other contexts in which similar observational problems exist.

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, $\sigma$) above those from neighborhood schools on the MSEE. However, this achievement advantage is likely to be mostly due to cream skimming. Using exogenous variation in access to elite schools generated by school admission lotteries, we find little evidence that attending an elite school improves students’ MSEE scores or secondary school admission outcomes. While there is some evidence of a small degree of differential sample 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 achievement effect of elite school attendance. Our failure to establish evidence of any positive achievement effect of elite school attendance is somewhat in contrast to a previous study by Ding and Lehrer (2007) who exploit the regression discontinuities created by the entrance exam cutoff rules for entry into differently ranked secondary schools in a county.
in China’s Jiangsu Province and find that attending a higher-ranked secondary school with higher-achieving peers increases student achievement in the College Entrance Exam. The differences in the results between this paper and Ding and Lehrer (2007) may be related to our differences in both the targeted schooling levels – i.e., middle school (grades 7-9) vs. secondary school (grades 10-12) – and the sources of exogenous variations in access to better schools. It is possible that students who score above the admission cutoffs for top-ranked secondary schools in their context are better positioned to benefit from high-achieving learning environments than those gaining access to elite middle school through admission lotteries in ours.

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. Proponents of school choice argue that increasing parental choice can improve educational outcomes by raising the demand for effective schools. This argument relies on the assumption that parents place high expressed weights on value-added in choosing schools, i.e., they place high weights on academics and can also identify effective schools. However, this assumption has been challenged on several grounds. First, some parents may base school choice decisions on factors other than academics, such as racial composition, school facilities, and student satisfaction (e.g., Jacob and Lefgren, 2007; Hastings, Kane, and Staiger, 2009; Cellini, Ferreira, and Rothstein, 2010), which will dilute the incentives for efficiency competition that the choice mechanism might otherwise create. Second, parents may lack information or face high decision-making costs to act on their preference for academics, as suggested by studies showing significant impacts of receiving information on school/housing markets. For example, Hastings and Weinstein (2008) find that providing simplified information on school performance leads more parents to choose higher-achieving schools in Charlotte-Mecklenburg, and Figlio and Lucas (2004) show that both residential location choices and house prices in Florida respond to the assignment of school letter grades even though the informational content used to determine school grades is public prior to the introduction of the grading system. Third, while solid research yields consistent evidence that parents value schools’ absolute achievement in mak-

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2Recent 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), and Israel (Lavy, 2010).
ing school choice and residential location decisions, there is less evidence that such decisions are influenced by schools’ value-added. For example, Downes and Zabel (2002) show that the values of neighborhood schools in Chicago, as capitalized in house prices, are based on schools’ mean contemporaneous test scores but not on the value-added they produce, and Mizala and Urquiola (2013) also fail to find consistent evidence that information on school effectiveness as delivered by the SNED program in Chile affects schools’ market outcomes, including enrollment, tuition levels, and socioeconomic composition. By showing that the largely oversubscribed Chinese elite schools do not raise test scores, this paper also challenges the view that increasing parental choice can improve student achievement. The most popular elite schools in our sample are those with the highest average student achievement, not those found to have the largest treatment effect on test scores, suggesting that schools may be rewarded for who they enroll rather than for their academic value-added, which casts doubt on the extent to which school choice can yield effective demand-side pressure for efficiency competition.

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 matching 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 main empirical results. Section 6 discusses the possible reasons for elite schools’ lack of academic value-added and yet persistent popularity. Section 7 concludes.

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 school system and provide schooling alternatives for students to opt out of their assigned neighborhood schools. Unlike neigh-
borhood 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. Historically, these elite schools were exam schools that admitted students primarily on the basis of their entrance exam scores. However, the central government adopted a nationwide educational reform in the late 1990s under the banner *Cross-Century Quality Education Project*, in 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 educational authorities across the nation 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 for secondary school and university admissions. Consequently, elite (middle) schools resorted to alternative admission schemes, including the use of admission lotteries, for the allocation of spots. Attending a private school is another option available to students. In the city under study here, private schools, most of which were boarding schools 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%, 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

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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%; nonetheless, the increase in that share would be marginal even if the these schools were included.
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%) and regular high schools (41% vs. 28%) compared with neighborhood schools.

### 2.2 Elite School Admissions and Lottery Data

The shift away from excessive exam orientation in the late 1990s 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. 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 over-subscribed, despite that each student can only apply to one elite school. (A student would 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. The final enrollment number in an elite school

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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
is therefore much 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 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 applied to all of the eight elite schools in the North Bank during the 2002-2004 period. After excluding three lotteries conducted in 2003 for which the notary public office’s records contain only winners’ information and the approximately 4% of applicants who were enrolled in primary schools outside the North Bank at the time of application, our final lottery sample comprises 13,768 applicants from North Bank primary schools for 21 admission lotteries.\(^8\) 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

\(^8\) 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.
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 $0.42\sigma$ above the district average, whereas the mean 6th-grade score of neighborhood school students is $0.08\sigma$ 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 $0.57\sigma$ and $0.29\sigma$ above the district average, respectively, leaving non-applicants a mean score that is $0.12\sigma$ 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 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 during the nine-year compulsory schooling stage\textsuperscript{9} 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.\textsuperscript{10} But even conditional on gender and cohort, it is still 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 superpopulation of (unobserved) identifiers from which the identifiers (also unobserved) of individuals in 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

\textsuperscript{9}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.

\textsuperscript{10}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 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.
\( D_{i(j)}/Y_{i(j)} \) 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 \( n_i = \sum_{j \in J} 1(C_j = C_i) \), 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 only, 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 degree of differential sample selection caused by lottery assignment.

Column 2 presents the regression-adjusted win/loss difference controlling for lottery fixed effects, and the point estimate of the coefficient on the lottery winner dummy suggests winners to have, on average, 0.027 more matches than losers. However, the standard error of the coefficient (0.039) indicates that the statistical power of the test is rather weak and is unable to detect the existence of a small degree of differential sample attrition, which is likely to be the case in our context.

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 sample 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 representation in the full sample. The overall match rate, regardless of whether the match is single or
multiple, is 89.2% 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 sample 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 degree of differential sample 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 in $\Psi$. We instead employ an indirect approach 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% 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%) gives us the extent of differential sample 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 sample 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 treatment effect analysis. 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 as follows:

\[
E[D_i|Z_i = 1, C_j = C_i] - E[D_i|Z_i = 0, C_j = C_i] = p(E[D_i|Z_i = 1] - E[D_i|Z_i = 0]) + (1 - p)(E[D_j|Z_i = 1, C_j = C_i, j \neq i] - E[D_j|Z_i = 0, C_j = C_i, j \neq i])
\]  

(1)

and

\[
E[Y_i|Z_i = 1, C_j = C_i] - E[Y_i|Z_i = 0, C_j = C_i] = p(E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]) + (1 - p)(E[Y_j|Z_i = 1, C_j = C_i, j \neq i] - E[Y_j|Z_i = 0, C_j = C_i, j \neq i]).
\]  

(2)

where $p = P[j = i|C_j = C_i]$, the proportion of correct matches among all matches in $\Psi$. 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 equaling to their corresponding proportions in $\Psi$. 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 $\Psi$. 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

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

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

Proposition 2 assumes complete, although imperfect, observation of $(Z_i, D_i, Y_i)$ in $\Psi$ 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., $T_i = 1(i \in J)$. 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

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

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

Note that 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, which is different to the simple randomization setup in Proposition 3 assuming a constant assignment probability \( P(Z_i = 1) \) 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 A, we further extend the LATE framework to apply to stratified randomized experiments and show in Corollary 1 that all of the properties in Proposition 3 can be carried over to stratified randomized experiments. 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.

4.3 Ability Selection Bias under Differential Sample Attrition

Proposition 3 shows that, if sample attrition is independent of treatment assignment, the LATE theorem still holds for compliers who are retained in the MSEE data set. However, the independent sample attrition assumption may not hold in practice as the process determining the observability of an individual may be related to that individual’s treatment assignment. Specific to the context investigated in this paper, as winning an elite school admission lottery expands a student’s choice set, it may induce some students who would otherwise opt for schooling outside the system to remain in the system. Therefore, a more plausible assumption concerning the sample selection process is to assume monotonicity in the effect of treatment assignment on sample selection as invoked in Lee (2009). Let \( T_i(0) \) and \( T_i(1) \) denote the latent indicators for whether individual \( i \) would be retained in the MSEE data set when \( Z_i = 0 \) and \( Z_i = 1 \), respectively. The monotone sample selection assumption implies that some individuals will select into the sample because of being assigned to treatment (\( \exists i : T_i(0) = 0, T_i(1) = 1 \)), whereas no one will ever drop out of the sample owing to such assignment (\( \nexists i : T_i(0) = 1, T_i(1) = 0 \)).

Under the monotone sample selection assumption, we can classify the matched lottery-MSEE pairs in the combined data set \( \Psi \) into three categories: (i) the correctly matched pairs that pertain to the always retained individuals \( (T_i(0) = T_i(1) = 1) \), (ii) the falsely matched pairs \( (C_j = C_i, j \neq i) \), and (iii) the correctly matched pairs that pertain to the marginally retained individuals \( (T_i(0) = 0, T_i(1) = 1) \).\(^{11}\) Let \( \Psi_0 \) and \( \Psi_1 \) denote two mutually exclusive

\(^{11}\) Note that individuals with \( T_i(0) = T_i(1) = 0 \) are never contained in \( \Psi \).
and collectively exhaustive subsets of $\Psi$ matched to losers and winners, respectively. Let $p_0$ denote the proportion of the matched lottery-MSEE pairs in $\Psi_0$ pertaining to (i) and $p_m$ denote the proportion of the matched lottery-MSEE pairs in $\Psi_1$ belonging to (iii). Then, the composition of $\Psi_0$ and $\Psi_1$ can be illustrated as the following:

<table>
<thead>
<tr>
<th></th>
<th>$\Psi_0$</th>
<th>$\Psi_1$</th>
</tr>
</thead>
<tbody>
<tr>
<td>(i) always retained individuals</td>
<td>$p_0$</td>
<td>$(1 - p_m)p_0$</td>
</tr>
<tr>
<td>(ii) falsely matched pairs</td>
<td>$1 - p_0$</td>
<td>$(1 - p_m)(1 - p_0)$</td>
</tr>
<tr>
<td>(iii) marginally retained individuals</td>
<td>$0$</td>
<td>$p_m$</td>
</tr>
</tbody>
</table>

With the foregoing partitions, the ITT estimand of the effect of $Z$ on $D$, constructed by comparing the means in the treatment status between $\Psi_1$ and $\Psi_0$, can be expressed as

$$
E[D_{i(j)}|Z_i = 1, C_j = C_i] - E[D_{i(j)}|Z_i = 0, C_j = C_i] \\
= (1 - p_m)p_0E[D_i(1)|T_i(0) = 1] + (1 - p_m)(1 - p_0)E[D_j|C_j = C_i, j \neq i] + p_mE[D_i(1)|T_i(1) > T_i(0)] \\
- p_0E[D_i(0)|T_i(0) = 1] - (1 - p_0)E[D_j|C_j = C_i, j \neq i] \\
= (1 - p_m)p_0d_{1a}^1 + (1 - p_m)(1 - p_0)d_f + p_md_{1m}^1 - p_0d_{1a}^0 - (1 - p_0)d_f \\
= p_0(d_{1a}^1 - d_{1a}^0) + p_m[d_{1m}^1 - p_0d_{1a}^0 - (1 - p_0)d_f],
$$

(3)

where $d_{1a}^1/d_{1a}^0$ denotes the proportion of the always retained individuals who would be treated had they won/lost their lottery, $d_f$ denotes the proportion of the falsely matched MSEE takers who happen to be treated, and $d_{1m}^1$ denotes the proportion of the marginally retained individuals who would be treated had they won their lottery. The first term, $p_0(d_{1a}^1 - d_{1a}^0)$, corresponds to the ITT effect of $Z$ on $D$ in the absence of differential sample attrition (i.e., $p_m = 0$), whereas the second term is the bias owing to the contamination of the marginally retained individuals in $\Psi_1$.

Analogously, the ITT estimand of $Z$’s effect on $Y$ in $\Psi$ can be written as

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

\[= (1 - p_m)p_0E[D_i(1)|T_i(0) = 1] + (1 - p_m)(1 - p_0)E[D_j|C_j = C_i, j \neq i] + p_mE[D_i(1)|T_i(1) > T_i(0)] \\
- p_0E[D_i(0)|T_i(0) = 1] - (1 - p_0)E[D_j|C_j = C_i, j \neq i] \\
= (1 - p_m)p_0d_{1a}^1 + (1 - p_m)(1 - p_0)d_f + p_md_{1m}^1 - (1 - p_0)d_f \\
= p_0(d_{1a}^1 - d_{1a}^0) + p_m[d_{1m}^1 - (1 - p_0)d_f].\]

(3)
\[ \mathbb{E}[Y_{i(j)} | Z_i = 1, C_j = C_i] - \mathbb{E}[Y_{i(j)} | Z_i = 0, C_j = C_i] \\
= (1 - p_m) p_0 \mathbb{E}[Y_i(0) + \gamma D_i(1) | T_i(0) = 1] + (1 - p_m) (1 - p_0) \mathbb{E}[Y_i(0) + \gamma D_i | C_j = C_i, j \neq i] + p_m \mathbb{E}[Y_i(0) + \gamma D_i(1) | T_i(1) > T_i(0)] \\
- p_0 \mathbb{E}[Y_i(0) + \gamma D_i(0) | T_i(0) = 1] - (1 - p_0) \mathbb{E}[Y_j(0) + \gamma D_j | C_j = C_i, j \neq i] \\
= (1 - p_m) p_0 (y_a + d^1 a) + (1 - p_m) (1 - p_0) (y_f + d_f) + p_m (y_m + d^1 m) - p_0 (y_a + d^0 a) - (1 - p_0) (y_f + d_f) \\
= p_0 (d^1 a - d^0 a) + p_m [(y_m + d^1 m) - p_0 (y_a + d^0 a) - (1 - p_0) (y_f + d_f)] , \tag{4} \]

where \( y_a, y_m, \) and \( y_f \) denote the average potential outcome without treatment for the always retained individuals, marginally retained individuals, and falsely matched MSEE takers, respectively; and \( \gamma \) denotes the treatment effect that is assumed to be constant across all individuals (see Remark 3 below for a discussion). Similar to Equation (3), the first term in Equation (4), \( p_0 (d^1 a - d^0 a) \gamma, \) corresponds to the ITT effect of \( Z \) on \( Y \) in the absence of differential sample attrition, whereas the second term is the bias owing to the contamination of the marginally retained individuals in \( \Psi_1, \) whose average outcome \( (y_m + d^1 m) \gamma \) may differ from that of the always retained individuals \( (y_a + d^1 a) \gamma \) and the falsely matched MSEE takers \( (y_f + d_f) \gamma \) in \( \Psi_1. \)

Taking the ratio of Equations (4) and (3), our IV estimand under both imperfect matching and differential sample attrition can be expressed as follows:

\[ \gamma^{IV} = \gamma + (p_m / \delta)[y_m - p_0 y_a - (1 - p_0) y_f], \tag{5} \]

where all notations are as previously defined except for \( \delta, \) which denotes the ITT estimand of \( Z \)'s effect on \( D \) in \( \Psi \) as defined in Equation (3).

**Remark 1.** The magnitude of the bias is proportional to the ratio of the share of the marginally retained individuals in \( \Psi_1 \) \( (p_m) \) and the ITT estimand of \( Z \)'s effect on \( D \) in \( \Psi \) \( (\delta) \). The smaller the size of \( p_m / \delta, \) the smaller the bias. Proposition 3 can be considered as a special case of Equation (5) in which the bias term is eliminated in the absence of differential sample attrition (i.e., \( p_m = 0 \)).

**Remark 2.** The ability selection component in the bracket, \( y_m - p_0 y_a - (1 - p_0) y_f, \) arises if the extent of ability selection of the marginally retained individuals in \( \Psi_1 \) differs from that of the counterparts in \( \Psi_0 \) to which they are compared. Specifically, the size of this ability selection component is determined by the extent to which the average potential outcome
without treatment of the marginally retained individuals \( (y_m) \) differs from a weighted average of that of the always retained individuals and the falsely matched MSEE takers with the weights equaling to their respective representations in \( \Psi_0 \left( p_0 y_a + (1 - p_0) y_f \right) \).

**Remark 3.** It is worth noting that the fact that the bias comes from the ability selection only is a result of our restriction to the constant treatment effect framework in Equation (4). In a more generalized framework with heterogeneous treatment effects, as presented in Appendix B, another component of the bias could arise from the difference in the average treatment effect across subgroups. However, as the bias component from the treatment heterogeneity is likely to be of secondary order importance compared with that from the ability selection, we focus on the latter only in the main body of the paper to simplify the notation and illustration, and relegate the analysis of the differential sample attrition bias under the generalized heterogeneous treatment effect framework to the appendix.

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 \( \Psi \). 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., \( \Psi_1 \)) than among those matched to losers (i.e., \( \Psi_0 \)). 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 \( \Psi \). 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 \( \Psi \) 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 $\Psi$, 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.\textsuperscript{13}

If all of the applicants retained in $\Psi$ 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, 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 $\Psi$ 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), i.e., $\max_{j:C_j=C_i} D_j$. 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, where the unit of analysis is each applicant and the dependent variable is his/her inferred enrollment status in the selected elite school, 52.8% of the lottery losers are inferred to have enrolled in his/her selected elite school, whereas only 10.5% of the lottery winners are inferred to have declined their admission offers. The regression-adjusted win/loss difference in the inferred enrollment

\textsuperscript{13}If there were no differential sample attrition, the ratio of the two coefficients in Panels A and B (19.7/34.0 = 0.58) would correspond to the proportion of correct matches among all of the matches in $\Psi$. 

21
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% of the lottery losers are inferred to have enrolled in their selected elite school and only 6.4% of the lottery winners are inferred not to have enrolled. 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-year 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 outcomes 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 \( \Psi \). 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 0.4\( \sigma \) 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% increase over the citywide average admission rate of 21.7%. The large and significant OLS coefficients, however, are at least in part attributable to the selective lottery participation and noncompliance with lottery assignments of applicants. These two sources of selection are illustrated previously in Figures 1B and 2A, respectively. The former contributes to the OLS coefficients because \( \Psi \) 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 $\Psi$. Similar to our earlier exposition on the first-stage relationship, the reduced-form coefficients reflect the win/loss difference in academic outcomes among lottery participants but are attenuated by the existence of falsely matched pairs. Despite such attenuation, we would still expect a positive reduced-form relationship in $\Psi$ 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 \textit{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 $\Psi$ 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 $\Psi$. The IV coefficients, presented in Column 3, are never significant and often negative,\textsuperscript{14} 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 0.15$\sigma$ (for three-year attendance) 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.

\section*{5.3 Accounting for Ability Selection Bias from Differential Sample Attrition}

Our empirical analysis so far has ignored the small degree of differential sample attrition between winners and losers suggested by the matching statistics in Table 3. As illustrated in Section 4.3, the existence of the marginally retained individuals in the matched lottery-MSEE pairs for winners ($\Psi_1$) but not for losers ($\Psi_0$) could bias our IV estimates if the extent of ability selection, as measured in terms of the average potential outcome without treatment, of

\textsuperscript{14}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.
the marginally retained individuals \((y_m)\) differs from that of the always retained individuals \((y_a)\) and falsely matched MSEE takers \((y_f)\). However, because of the unobservable nature of the potential outcomes without treatment for elite school attendees, we are unable to evaluate the ability selection bias term in Equation (5) directly. Nonetheless, for a subsample of District 3 applicants whose baseline scores are available, we could still evaluate the extent of ability selection for this subsample in terms of their baseline scores. In this subsection, we conduct such an exercise to informally gauge the sign and magnitude of the ability selection bias by comparing the average baseline scores of the marginally retained individuals (denoted by \(x_m\)) with those of the always retained individuals (denoted by \(x_a\)) and falsely matched MSEE takers (denoted by \(x_f\)).

As the magnitude of the bias is shown to be proportional to the ratio of the share of the marginally retained individuals in \(\Psi_1 (p_m)\) and the first-stage effect \((\delta)\) in Equation (5), we begin with investigating the size of \(p_m/\delta\) from the matching results for the subsample of District 3 applicants with baseline scores. Table 3 shows an average of 1.889 matches for lottery losers (Column 5) and a win/loss difference of 0.027 (Column 6). Thus, \(p_m\) is 0.027/(1.889 + 0.027) = 0.014. With an estimated first-stage effect \((\delta)\) of 0.26 (Column 3, Panel A of Table 4), the size of \(p_m/\delta\) is estimated to be 0.054 for this subsample.

We now investigate the extent of ability selection in terms of baseline scores for the marginally retained individuals \((x_m)\). Let \(\theta_L\) and \(\theta_W\) denote, respectively, the proportion of losers and winners who are unmatched in \(\Psi\). The former contains both (i) the never retained individuals without false matches and (ii) the marginally retained individuals without false matches, whereas the latter contains (i) only. Columns 5-6 of Table 3 indicate point estimates of 0.060 and 0.042 for \(\theta_L\) and \(\theta_W\), respectively. It then follows that \(\theta_W\) (0.042) corresponds to the size of type (i) applicants and the difference \(\theta_L - \theta_W\) (0.018) corresponds to the size of type (ii) applicants. 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.

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15 The prior literature on treatment effect analysis of randomized experiments with missing outcomes often 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; Behaghel, et al., 2009). Because our point estimates are close to zero, application of such strategies to our data yields treatment bounds that always include zero and cannot help to sign the treatment effect. In addition, the constructed treatment bounds are also too wide to be informative. We thus employ an alternative strategy to gauge the sign and magnitude of the ability selection bias from differential sample attrition bias using individuals’ baseline scores.

16 For the full sample, the size of \(p_m/\delta\) is estimated to be 0.086 (0.017/0.197).
However, Row (a) of Table 6 shows that the former outperforms the latter on average by a substantial margin of 0.174σ.\textsuperscript{17} As this difference is entirely owing to the 30\% (0.018/0.060) unmatched losers pertaining to type (ii), it indicates a lead of type (ii) applicants over type (i) applicants by 0.58σ (0.174σ/0.3) in the average baseline scores. With an observed mean of 0.23σ for the unmatched losers (i.e., type (i) and type (ii) applicants pooled), the average baseline scores of type (ii) applicants are estimated to be 0.64σ in Row (c) of Table 6. Assuming that false matching is independent of baseline scores, we can proxy $x_m$ by the average baseline scores of type (ii) applicants, a subset of the marginally retained individuals without false matches.\textsuperscript{18}

We next consider the extent of ability selection in terms of baseline scores for the always retained individuals ($x_a$). Row (b) of Table 6 shows the average baseline scores of matched losers to be 0.30σ. However, in addition to the always retained individuals, the matched losers are contaminated by the unretained losers with false matches. If we assume the same false matching rate of 0.25 for the full sample (aforementioned in Section 3) also applies to the unretained losers in this subsample, then the unretained losers with false matches would be one-third ($\frac{0.25}{1-0.25}$) of the size of those without false matches. With a sample observation of 0.06 for the proportional share of the latter, that of the former is estimated to be only 0.02. Given such a small representation of the unretained losers with false matches (0.02) among all matched losers (0.94), we thus use the average baseline scores of all matched losers (0.30σ) to proxy for $x_a$.

Finally, we further assume that the average baseline scores of the falsely matched MSEE takers ($x_f$) are 0, and proxy $p_0$ by the ratio of the two first-stage estimates for the subsample of District 3 applicants in Panels A and B of Table 4, 0.26/0.52 = 0.50. Then, the extent of ability selection in baseline scores, $x_m - p_0x_a - (1 - p_0)x_f$, is estimated to be 0.49σ (0.64σ−0.5×0.30σ−0.5×0). If differences in baseline scores fully account for ability selection, with an estimated marginal effect of baseline scores on MSEE scores of approximately 0.6, the ability selection term $y_m - p_0y_a - (1 - p_0)y_f$ in Equation (5) is estimated to be 0.29σ. This can be considered as a lower-bound estimate of the ability selection term if selection

\textsuperscript{17}This difference is not statistically significant in conventional levels because of the small size of unmatched applicants (141 losers and 29 winners) in this subsample.

\textsuperscript{18}If we assume the same false matching rate also applies to the marginally retained individuals, type (ii) applicants would account for three-quarters of all marginally retained individuals given our prior knowledge of a false matching rate of 0.25 for the full sample.
on unobservables and baseline scores are positively correlated. If we instead impose equality of the extent of ability selection on unobservables and baseline scores, which Altonji, Elder, and Taber (2005) argue providing an upper bound for the selection on unobservables in their study of Catholic high school attendance in the US, we can also use 0.49σ as an upper-bound estimate of $y_m - p_0y_a - (1 - p_0)y_f$. While the ability selection term (even when the lower-bound estimate of 0.29σ is considered) seems to be sizable, the bias itself is not. With the above estimate of 0.054 for $p_m/\delta$, the ability selection bias, which is the product of $p_m/\delta$ and $y_m - p_0y_a - (1 - p_0)y_f$, appears to be only between 0.016σ and 0.026σ. Very importantly, its sign is positive, in favor of finding a positive effect of elite school attendance. Nonetheless, despite the potentially positive ability selection bias from differential sample attrition, our IV estimates still fail to establish any evidence that elite schools confer positive academic benefits to their attendees.19

6 Discussion

6.1 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 a discussion of the possible pathways through which elite school attendance may affect student achievement, although we are unable to distinguish these different pathways in our context due to data limitations. 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. Given the limited and mixed evidence on the mechanisms of peer

19In the context of heterogeneous treatment effects considered in Appendix B, another component of the differential sample attrition bias could arise from treatment heterogeneity across subgroups. Although we are unable to quantify this bias component, it seems implausible that the degree of heterogeneous treatment effects would outweigh that of ability selection (0.32 to 0.49σ). Therefore, the overall bias from differential sample attrition should still be positive, with its magnitude bounded by a few percent of a standard deviation.
effects in the existing literature, 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 (2014) 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.

Finally, the opportunity to attend a better school can result in behavioral responses from both students and parents, which may either amplify or reduce the positive effect of school quality (if any). For example, Pop-Eleches and Urquiola (2013) find that parents react to their children going to a better school by lowering their own effort. Specific to our context, parents may make compensatory investments such as tutoring if their child failed the admission lottery and did not attend an elite school, which, if true, could partly explain the lack of evidence of any achievement benefits of elite school attendance. In sum, for each pathway considered, 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, including differences in both the schooling environment and behavioral responses, 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 and quantify the extent of these forces.

---

6.2 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 perception of “better” schools and schools’ effective value-added. 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.\(^{21}\) 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 7. Columns 3 and 6, in particular, show that the relationship between schools’ average MSEE scores and popularity measures remains positive and significant even after controlling for estimated value-added effects.

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 explanation is that parents choose elite schools for reasons other than their impact on learning, such as for school facilities and peer quality (above and beyond their effect on achievement). Another explanation is that parents may confuse student outcomes

\(^{21}\) 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.
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 high-achieving schools as having 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 place higher expressed weights on peer quality than value-added in choosing a school, even though they indeed value achievement gains the most. For example, parents may place high intrinsic weights on achievement gains and low intrinsic weights on peer quality (above and 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 weights on value-added are swamped by its noisy measurement, leading parents to place higher expressed weights on information about schools’ peer quality than value-added in making their school choice decisions. Although the available empirical evidence is unable to differentiate between these three underlying reasons, each is sufficient to lead to elite schools being sought after mainly for their observed superiority in student outcomes rather than for their academic value-added, thus casting doubt on the potential of school choice to increase the demand-side pressure for schools to improve effectiveness.

7 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 school and university admissions render the Chinese context very clean and ideal for evaluating the effects of superior schooling on student achievement and comparing parental perception 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 sample 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 contributes to the program evaluation methodology 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 in data combination is not unique to our setting, the extended LATE framework presented herein could also be applied to other contexts in which similar observational problems exist.
Appendix

A. 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 \( k \in \{1, \ldots, K\} \), 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 (A3) and (A5) 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 1 In the case of stratified randomization with imperfect matching, if Assumptions (A1), (A2’), (A3’), (A4), and (A5’) hold, then the IV estimand with no covariates except for 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 \( \gamma_k \) denotes the simple IV estimand of lottery \( k \),

\[
\frac{E[Y_i(j) | Z_i=1, R_i=k, C_j=C_i] - E[Y_i(j) | Z_i=0, R_i=k, C_j=C_i]}{E[D_i(j) | Z_i=1, R_i=k, C_j=C_i] - E[D_i(j) | Z_i=0, R_i=k, C_j=C_i]},
\]

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

\[
\bar{\gamma}^{IV} = \frac{\sum_{k=1}^{K} \left\{ \sum_{i:B_i = k} \left[ \sum_{j:C_j = C_i} Z_i (Y_{i(j)} - \bar{Y}_k) \right] \right\}}{\sum_{k=1}^{K} \left\{ \sum_{i:B_i = k} \left[ \sum_{j:C_j = C_i} Z_i (D_{i(j)} - \bar{D}_k) \right] \right\}}
\]

The population analog of the IV estimator can be written as

\[
\gamma^{IV} = \lim_{N \to \infty} \bar{\gamma}^{IV} = \frac{\sum_{k=1}^{K} \{ N_k \pi_k \{ E[Y_{i(j)}|Z_i = 1, B_i = k, C_j = C_i] - E[Y_{i(j)}|B_i = k, C_j = C_i] \} \}}{\sum_{k=1}^{K} \{ N_k \pi_k \{ E[D_{i(j)}|Z_i = 1, B_i = k, C_j = C_i] - E[D_{i(j)}|B_i = k, C_j = C_i] \} \}}
\]

\[
= \frac{\sum_{k=1}^{K} \{ N_k \pi_k \{ E[Y_{i(j)}|Z_i = 1, B_i = k, C_j = C_i] - \pi_k E[Y_{i(j)}|Z_i = 1, B_i = k, C_j = C_i] \} \}}{\sum_{k=1}^{K} \{ N_k \pi_k \{ E[D_{i(j)}|Z_i = 1, B_i = k, C_j = C_i] - \pi_k E[D_{i(j)}|Z_i = 1, B_i = k, C_j = C_i] \} \}}
\]

\[
= \frac{1}{\sum_{k=1}^{K} \{ N_k \pi_k (1 - \pi_k) \{ E[Y_{i(j)}|Z_i = 1, B_i = k, C_j = C_i] - E[Y_{i(j)}|Z_i = 0, B_i = k, C_j = C_i] \} \}} \sum_{k=1}^{K} \{ N_k \pi_k (1 - \pi_k) \delta_k \gamma_k \}
\]

\[
= \frac{1}{\sum_{k=1}^{K} \{ N_k \pi_k (1 - \pi_k) \delta_k \}} \sum_{k=1}^{K} \{ N_k \pi_k (1 - \pi_k) \delta_k \gamma_k \} = \sum_{k=1}^{K} \omega_k \gamma_k.
\]

**B. Differential Sample Attrition Bias under Heterogeneous Treatment Effect Framework**

Our analysis of the differential sample attrition bias in Section 4.3 assumes constant treatment effect across all individuals. In this appendix, we extend our analysis of the differential sample attrition bias to the generalized heterogeneous treatment effect framework. Following Angrist, Imbens, and Rubin (1996), we further partition the always retained individuals into the always takers (\( D_i(0) = D_i(1) = 1 \)), compliers (\( D_i(0) = 0, D_i(1) = 1 \)), and never takers
\( (D_i(0) = D_i(1) = 0) \). With our the notations defined in Section 4.3, the proportion of the always takers, compliers, and never takers in the always retained individuals can be denoted as \( d^0_i \), \((d^1_i - d^0_i)\), and \((1 - d^1_i)\), respectively. In addition, we also divide the falsely matched pairs by treatment status into the treatment takers \((D_j = 1)\) and non-takers \((D_j = 0)\), and use \( d_f \) to denote the proportion of the treatment takers.

Under the heterogeneous treatment effect framework, the ITT estimand of \( Z\)'s effect on \( D \) in Equation (4) can be rewritten as follows:

\[
E[y_{i(j)}|Z_i = 1, C_j = C_i] - E[y_{i(j)}|Z_i = 0, C_j = C_i]
= \frac{1 - p_m}{p_m} E[y_{i(0)} + \gamma_i D_i(1)|T_i(0) = 1] + \frac{1 - (1-p_m)(1-p_0)}{p_m E[y_{i(0)} + \gamma_i D_i(1)|T_i(1) > T_i(0)]}
- \frac{1 - p_m}{p_m} E[y_{i(0)} + \gamma D_i(0)|T_i(0) = 1] + \frac{1 - (1-p_m)(1-p_0)}{p_m E[y_{i(0)} + \gamma D_i(1)|T_i(1) > T_i(0)]}
= \gamma_i + \frac{\gamma}{\delta} \delta_{\gamma} = \gamma_i + \frac{\gamma}{\delta} \delta_{\gamma} + \ldots.
\]

where \( \gamma_i \) denotes the individual-specific treatment effect, and \( \gamma_a \), \( \gamma_c \), \( \gamma_m \), and \( \gamma_f \) denote the average treatment effect for the always takers, compliers, marginally retained individuals who are treated, and falsely matched treatment takers, respectively.\(^1\) Taking the ratio of Equations (4') and (3), we can derive the IV estimand under the heterogeneous treatment effect framework with both imperfect matching and differential sample attrition as follows:

\[
\gamma_{IV} = \frac{E[y_{i(j)}|Z_i = 1, C_j = C_i] - E[y_{i(j)}|Z_i = 0, C_j = C_i]}{E[D_{i(j)}|Z_i = 1, C_j = C_i] - E[D_{i(j)}|Z_i = 0, C_j = C_i]}
= \gamma_i + \frac{\gamma_m}{\delta} \delta_{\gamma} = \gamma_i + \frac{\gamma_m}{\delta} \delta_{\gamma} + \ldots.
\]

\(^1\)Specifically, \( \gamma_a = E[y_{i(1)} - y_{i(0)}|D_i(0) = 1, T_i(0) = 1] \), \( \gamma_c = E[y_{i(1)} - y_{i(0)}|D_i(1) > D_i(0), T_i(0) = 1] \), \( \gamma_m = E[y_{i(1)} - y_{i(0)}|D_i(0) > D_i(1), T_i(0) = 1] \), and \( \gamma_f = E[y_{i(1)} - y_{i(0)}|C_j = C_i, j \neq i] \).

Compared with Equation (5) in Section 4.3, the expression in Equation (5') under the heterogeneous treatment effect framework contains a second bias component, \( \eta_2 \), which arises if the average treatment effect (ATE) of the marginally retained individuals, \( \gamma_m \), differs from a weighted average of the ATEs of always takers, falsely matched treatment takers, and compliers, \( \frac{\gamma_a}{\delta} \delta_{\gamma} - \frac{(1-\gamma_f)}{\delta} \delta_{\gamma} \). While in the case of the homogeneous treatment effect framework considered in the main body of the paper, \( \eta_2 \) becomes zero, as the ATEs are the same for all subgroups.
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<th>Private schools (3)</th>
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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

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</tbody>
</table>

**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

<table>
<thead>
<tr>
<th></th>
<th>Full sample</th>
<th>District 3</th>
<th>District 3 w/ baseline scores</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Losers’ mean</td>
<td>Losers’ mean</td>
<td>Losers’ mean</td>
</tr>
<tr>
<td>Number of matches (n_i)</td>
<td>1.580 0.027</td>
<td>1.779 0.013</td>
<td>1.889 0.027</td>
</tr>
<tr>
<td></td>
<td>(2.059) (0.039)</td>
<td>(2.364) (0.098)</td>
<td>(2.416) (0.148)</td>
</tr>
<tr>
<td>Overall match rate (n_i&gt;1)</td>
<td>0.892 0.022 ***</td>
<td>0.904 0.018</td>
<td>0.940 0.018 *</td>
</tr>
<tr>
<td></td>
<td>(0.311) (0.006)</td>
<td>(0.295) (0.012)</td>
<td>(0.238) (0.011)</td>
</tr>
<tr>
<td>Number of observations</td>
<td>9,630 13,768</td>
<td>2,730 3,483</td>
<td>2,335 2,973</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th></th>
<th>Full sample</th>
<th>District 3 subsample w/ baseline scores</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>no controls</td>
<td>w/ controls</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>Lottery winner</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.197 ***</td>
<td>0.196 ***</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.007)</td>
</tr>
<tr>
<td><strong>Female</strong></td>
<td>-</td>
<td>-0.020</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.006)</td>
</tr>
<tr>
<td><strong>Baseline score</strong></td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Number of observations</strong></td>
<td>21,676</td>
<td></td>
</tr>
</tbody>
</table>

### 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

### 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

### 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

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

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>Full sample</th>
<th>District 3 subsample w/ baseline scores</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS (1)</td>
<td>Reduced-form (2)</td>
</tr>
<tr>
<td>MSEE scores (in s.d.)</td>
<td>0.400 0.353</td>
<td>-0.003 -0.016 -0.001</td>
</tr>
<tr>
<td>Admission to a top-echelon high school</td>
<td>0.124 0.107</td>
<td>0.006 0.008</td>
</tr>
<tr>
<td>Scoring above the threshold for top-echelon high school admission</td>
<td>0.087 0.088</td>
<td>-0.001 -0.000</td>
</tr>
<tr>
<td>Admission to any high school</td>
<td>0.181 0.149</td>
<td>-0.003 -0.002</td>
</tr>
<tr>
<td>Scoring above the threshold for high school admission</td>
<td>0.087 0.088</td>
<td>-0.001 -0.000</td>
</tr>
<tr>
<td>Number of observations</td>
<td>21,676 5,606</td>
<td></td>
</tr>
</tbody>
</table>

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.
### Table 6 Ability Selection in Baseline Scores of District 3 Applicants

<table>
<thead>
<tr>
<th></th>
<th>Losers’ mean (1)</th>
<th>Win/loss difference (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>(a) Unmatched applicants</td>
<td>0.229 (0.694)</td>
<td>-0.174 (0.141)</td>
</tr>
<tr>
<td>(b) Matched applicants</td>
<td>0.303 (0.770)</td>
<td>0.034 (0.033)</td>
</tr>
<tr>
<td>(c) Marginally retained individuals w/o false matches</td>
<td>0.636</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(a1)+θ_w/(θ_L-θ_w)*(a2)</td>
<td></td>
</tr>
</tbody>
</table>

**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)+θ_w/(θ_L-θ_w)*(a2). θ_w denotes the proportion of winners, estimated to be 0.042, and θ_L 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 7 School Popularity, Average Achievement, and Value-Added

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>Oversubscription rate</th>
<th>Winner take-up rate</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Average MSEE scores (in s.d.)</td>
<td>2.163 * (1.064)</td>
<td>2.010 * (1.027)</td>
</tr>
<tr>
<td>Estimated value-added effect (in s.d.)</td>
<td>-0.780 (0.498)</td>
<td>-0.555 (0.495)</td>
</tr>
<tr>
<td>Number of observations</td>
<td>21</td>
<td>21</td>
</tr>
</tbody>
</table>

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. Robust standard errors are reported in parentheses.

*significant at 10%; **significant at 5%.
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.
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.