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Can Selective School Admission Reforms Increase Student Achievement?

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Abstract

The recent surge in admission reforms across selective US schools has been a source of much debate. The achievement consequences of these reforms hinge on which students benefit from attending selective schools. I show that Boston exam schools have heterogeneous effects on achievement, driven primarily by the quality of applicants' non-exam school alternatives rather than their demographic characteristics. Admission policies prioritizing students with weaker schooling alternatives thus have more potential to increase academic achievement than policies targeting specific demographic groups. Simulations of alternative admission criteria suggest that schemes reserving seats for such students are likely to yield the largest gains.

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1 Introduction

Exam schools are the most sought-after public high schools in the United States. The underrepresentation of minority students at these elite institutions has been at the center of the education policy debate for decades. Recently, exam schools have been under pressure to change their admission criteria in order to increase access for minority and low-income students. School boards are considering a wide range of options, from lotteries to sophisticated place-based admission schemes.[1] Yet, the consequences of these admission systems for achievement have received little attention.

The impact of admission systems on achievement depends on how different students are affected by the schools they attend. Research on exam schools focuses mostly on causal effects for marginal students, that is, students close to admission cutoffs (Dobbie and Fryer, 2014; Abdulkadiroglu et al., 2014; Angrist et al., 2019; Barrow et al., 2020). Although mostly small and not statistically significant, these estimates for average marginal students might hide substantial heterogeneity. Applicants' gains from attending exam schools depend both on their personal characteristics and on the quality of non-exam school alternatives. These two sources of differences in potential gains are key to evaluating and comparing the effects of different elite high school admission criteria on overall academic achievement.

This paper estimates the achievement consequences of counterfactual admission criteria, accounting for heterogeneity in potential gains. The paper begins with an econometric framework that isolates sources of heterogeneity. This decomposition is relevant to the exam school debate because new admission rules are likely to change the demographics of admitted students as well as the non-exam schools they substitute from. The estimates for Boston exam schools show substantial heterogeneity in exam school gains, driven by differences in the quality of students' non-exam school alternatives. I use these estimates to evaluate

[1] For instance, Fairfax County (VA) and San Francisco replaced the admission tests at their exam schools with lotteries in 2020 (Warikoo, 2021). In Boston, exam schools still screen students based on academic achievement but now give priorities to students living in disadvantaged zip codes (Barry, 2021). Philadelphia switched to lottery admission for its magnet schools and introduced some zip code priorities in 2021 (Mezzacappa, 2021). Chicago expanded access to its elite high school entrance test in 2021. Under the new policy, all public school students take the test, which is administered directly at their schools (Karp, 2021).

admission reforms and to design a scheme that increases overall academic achievement by leveraging differences in expected gains.

I estimate causal exam school effects using the IV methods introduced in Abdulkadiroğlu et al. (2017) and Abdulkadiroglu et al. (2022). To decompose exam school effects, I interact instruments for exam school enrollment with distances to non-exam Boston public schools. Distances to schools predict where applicants would enroll if not offered an exam school seat. Due to the large number of schools, I split non-exam schools into four groups according to their quality as estimated with an OLS value-added model. This split takes advantage of the fact that the OLS value-added model provides biased but indicative estimates of school quality (Angrist et al. (2017)). Hence, the classification based on OLS value-added is likely to group together schools with similar effects on achievement.

This strategy identifies the exam school treatment effect by non-exam school alternative under the assumption of constant effects within strata (Hull (2015)). In particular, marginal changes in the relative distance to each group of schools should not be correlated with the potential treatment effect of attending an exam school. The data suggests that this assumption is likely to be satisfied, as marginal changes in relative distances are not systematically associated with variations in demographic characteristics or baseline test scores. Moreover, over-identification tests do not reject homogeneous treatment effects along relative distance.

Perhaps surprisingly, my empirical analysis suggests that the least selective of Boston's exam schools (the O'Bryant School) raises achievement the most. In particular, O'Bryant increases 7^{th} and 8^{th} grade math test scores by between 0.05 and 0.20 standard deviations for applicants who would have otherwise attended a Boston public school of below-median quality. At the same time, gaining admission to the two most selective Boston exam schools (the Boston Latin school or Boston Latin Academy) appears to actually reduce math test scores with respect to enrolling in O'Bryant. As a final step to the decomposition, I try to assess the heterogeneity in exam school effects across students with different pre-treatment characteristics but same quality of non-exam school alternative. This subgroup analysis shows small positive exam school effects for Hispanic and Black applicants when substituting from low-quality non-exam alternatives, although these estimates are less precise.

I use these results to compare the achievement effects of counterfactual admission schemes. Two popular alternatives, (i) granting admission to top-ranked middle school applicants and (ii) replacing exam school entrance test (ISEE) scores with state standardized test (MCAS) scores, have little impact on achievement. On the other hand, adopting place-based reserves based on middle school quality would increase 8^{th} grade math test scores by 0.13 standard deviations for 15% of applicants. All three of these alternative admission schemes increase minority student representation at exam schools.

The rest of the paper is organized as follows. Section 2 presents a framework that distinguishes substitution effects from match effects and discusses the data and the institutional background related to Boston's exam schools. Section 3 details the empirical strategy which decomposes exam school effects by non-exam school alternative. Section 4 presents the exam school estimated effects on achievement and their decomposition by non-exam school alternative. It also discusses heterogeneous exam school effects for different groups of students. Section 5 simulates the overall achievement gains from adopting several possible admission schemes. Section 6 concludes.

2 Background

2.1 Going beyond exam school RD estimates

It is crucial to account for treatment effect heterogeneity when evaluating the impact of different admission criteria schemes on academic achievement. Changes in exam school admission criteria affect the population of students that attend those schools and, unless the benefits of attendance are homogeneous, achievement gains estimated using previous cohorts will not apply to the new wave of admits.

In a setting with multiple treatments such as school choice, heterogeneity arises not only from differences in match effects between individuals and treatments but also from differences in outside option across individuals. For instance, Angrist et al. (2019) find that negative exam school effects in Chicago are explained by diversions away from high-performing charters in the Noble Network.[2] Similarly, Chabrier et al. (2016) emphasize the importance of considering the difference in quality of urban and non-urban public schools when comparing the effectiveness of different kinds of charters. In practice, match effects and substitution effects interact, and disentangling them requires additional identification assumptions beyond those typically imposed when estimating elite school treatment effects.

To understand the significance of these sources of heterogeneity, consider a minimal set-up with individuals (students) indexed by i and treatments (schools) indexed by j. Let Y_{ij} denote the potential academic outcome (academic achievement) of student i if she attends school j. The treatment effect of attending school 0 (an exam school) instead of school j for student i can be decomposed as the mean difference in potential outcomes (the substitution effect) and student i specific difference in potential outcomes (the match effect):

$$Y_{i0} - Y_{ij} = E[Y_{i0} - Y_{ij}] + \varepsilon_{i0} - \varepsilon_{ij} \tag{1}$$

$$:= \underbrace{\bar{Y}_0 - \bar{Y}_j}_{\text{substitution effect}} + \underbrace{\varepsilon_{i0} - \varepsilon_{ij}}_{\text{match effect}}. \tag{2}$$

Suppose for simplicity that there is a single discrete student characteristic X_i (e.g. race) that affects potential outcome Y_{ij} differently depending on the school j that the student attends. This corresponds to a situation where students of different races have different benefits from attending each school, but gains are otherwise homogeneous. Formally, this setting can be expressed as $\varepsilon_{ij} = E[\varepsilon_{ij} \mid X_i = x_i] + \nu_i$. With this simplification, the match effect corresponds to the covariate specific difference in outcome when switching from school 0 to school j:

$$Y_{i0} - Y_{ij} = \underbrace{\bar{Y}_0 - \bar{Y}_j}_{\text{substitution effect}} + \underbrace{E[\varepsilon_{i0} - \varepsilon_{ij} \mid X_i = x_i]}_{\text{match effect}}. \tag{3}$$

[2] In related work, Barrow et al. (2020) show that Chicago exam schools decrease college enrollment for students from lower-SES neighborhood.

This decomposition may be used to compare students' academic achievement under different exam school admission rules. A change in exam school admission rules affects both the set of schools students are substituting from and the types of students that substitute from these schools. Academic achievement will be larger if students that enroll in exam schools substitute from non-exam school alternatives in a way that yields large positive substitution effects and large positive match effects.

Hence, comparing the performance of different student assignment to schools in terms of overall academic achievement requires knowing the substitution effect and match effect for each pair of schools and student characteristic. When trying to find the exam school assignment scheme that maximizes academic achievement, substitution effects determine from which school students should be principally reallocated, while match effects indicate which students from within each school should be reallocated. Similarly, the impact of modifying exam school admission criteria will depend on the substitution and match effects with respect to exam schools for the students being displaced by the change.

2.2 Boston exam schools

Boston is a compelling setting for studying the impact of different elite school admission criteria on achievement. Admission rules at the city's exam schools have been a contentious topic since Judge W.A. Garrity ordered in 1978 Boston Latin School, the most selective Boston exam school, to set apart 35% of seats to minority applicants. After the unsuccessful attempt of McLaughlin v. Boston Sch. Comm. (1996), Wessmann v. Boston Sch. Comm. (1998) put an end to seat reserves for minority applicants. Admissions thus went back to being solely based on grades and entrance exam results. In July 2021, the Boston School Committee adopted a new admission regime which introduced seat reserves for applicants based on the socioeconomic conditions of their neighborhoods.[3] Accurately predicting the achievement effect of a change in admission criteria is thus immediately policy-relevant.

[3]In 2020, the Boston school Committee had already approved a temporary change of admission regime, in part as a response to the challenges created by the Covid-19 pandemic. The plan suspended the entrance exam for a year and set apart seat reserves for the city's different zip codes.

Boston has three elite high schools: Boston Latin School (BLS), Boston Latin Academy (BLA) and the O'Bryant School of Mathematics and Science (OBR). Students can apply for admission either in 7^{th} grade or in 9^{th} grade. Applicants can decide to apply to all three schools or to a subset of them and may express their preferences over schools by submitting a rank-order list. Admission at each exam school is based on a school-specific weighted average of middle school GPA and of the Independent School Entrance Examination (ISEE). Each applicant receives a rank at each school she applies to.

Exam school offers, reconciling applicant preferences, rankings and school capacities, are generated using deferred acceptance. This mechanism produces admission cutoffs for each exam school that can be exploited to identify exam school achievement effects. Boston Latin, with an admission rate of 27%, is the most selective of the three schools. It is closely followed by Latin Academy, which on average admits 46 % of its applicants. Finally, O'Bryant admits 56% of its applicants, making it the most easily attainable exam school. While more than 95% of admitted students at Boston Latin and Latin Academy accept their offer, take-up is only 80% at O'Bryant.

Applicants who fail to gain entrance into any of the exam school may enroll in one of Boston public schools. The performance of these schools in raising test scores is heterogeneous, as suggested by Figure 1 which displays the distribution of the average estimated value-added of BPS schools on 8^{th} grade MCAS English and math test scores.[4] Schools at the bottom of the estimated value-added distribution increase 8^{th} grade test scores by one standard deviation less than schools at the top of the estimated value-added distribution.

Exam schools appear to perform slightly better than the median Boston public school. Boston Latin and O'Bryant appear particularly effective at raising math test scores, while Boston Latin and Latin Academy are among the best schools for increasing English test scores. Although these value-added estimates could be misleading as they are not necessarily unbiased, their dispersion suggests that changing exam school assignment could result in achievement gains. Indeed, exam school applicants with non-exam school alternative in the

[4] School value-added is computed following the same model discussed in Section 3 for each year between 2004 and 2016. This model controls for student demographics and flexible functions of baseline test scores.

lower tail of the value-added distribution could benefit from attending an exam school.

2.3 Data on Boston students

Students can apply to exam schools either in the spring of their 6$^{\text{th}}$ grade for enrollment in 7$^{\text{th}}$ grade, or in the spring of their 8$^{\text{th}}$ grade for enrollment in 9$^{\text{th}}$ grade. Each year, approximately two thousand 6$^{\text{th}}$ graders apply to one of these schools. Applicants come from both private sector schools and public sector schools (which include charters). Applicants who do not receive an offer, or who decline their offer, can choose to enroll in the public school system or in a private school. Admission to most of Boston's regular public schools and charter schools occurs in 6$^{\text{th}}$ grade. Admission in 7$^{\text{th}}$ grade is thus a peculiarity of exam schools, so the non-exam school alternative of most applicants is the school they are enrolled in when they apply. This feature, combined with the fact that it is the larger application round, motivates my focus on applications to 7$^{\text{th}}$ grade.

The analysis sample includes 7$^{\text{th}}$ grade applicants from years 2004–2016 for which both baseline and outcome test scores are available.[5] Since I am interested in studying substitutions from regular public schools, I restrict the analysis sample to students enrolled in a non-charter BPS school in the Boston area prior to application. This excludes students who enroll in a charter school after applying to an exam school, since admissions to Massachusetts charter schools occur either in 5$^{\text{th}}$ or 6$^{\text{th}}$ grade through lotteries.[6] Hence, the analysis only considers students who either enroll in an exam school or in a non-charter BPS school.

Boston Public Schools (BPS) is the source of the application files for exam schools. The Massachusetts Department of Elementary and Secondary Education (DESE) provided the enrollment files and MCAS data. Application, enrollment and test scores files are merged using the State unique identifier (SASID). The distance from each sending middle school to each potential school of enrollment is computed using the shortest road distance between the two. Only schools where at least one student enrolled after application during the 2004-2016

[5] Baseline test scores correspond to MCAS 4$^{\text{th}}$ grade test scores. Thus, students who were not enrolled in a Boston Public school in 4$^{\text{th}}$ grade are excluded from my analysis.

[6] Among applicants who do not receive an offer from any exam schools, only 3% of non-charter applicants subsequently enroll in a charter school, while 73 % of charter applicants remain in a charter school.

period are included in the set of potential non-exam school alternatives. I include a more detailed description these data sets, how I constructed the distance variable, and the sample restrictions in Appendix A.

3 Empirical strategy

3.1 Instrument for exam school enrollment

The identification of exam school achievement effects requires an instrument since exam school students are positively selected. I use exam school offer conditional on the probability of receiving an offer to instrument exam school enrollment. Receiving an exam school offer strongly predicts exam school enrollment and is as good as randomly assigned conditional on the propensity score of receiving an offer (Abdulkadiroğlu et al., 2017; Abdulkadiroglu et al., 2022). To compute the propensity score of admission at each exam school, I exploit the rank order list of applicants following the methodology in Abdulkadiroglu et al. (2022). Appendix B describes the method in more detail. Although this method is also based on discontinuities at admission cutoffs, it is more general than the approaches used in Abdulkadiroglu et al. (2014) and Dobbie and Fryer (2014) because it combines the variations at each exam school's cutoff.

In practice, since the match yields a single offer, the sum of each exam school's propensity score corresponds to the risk of being assigned at any of the exam schools.The exam school effect on any outcome Y_i can thus be estimated with a just-identified 2SLS procedure that uses an offer from any exam school $D_i = \sum_s D_{is}$ to instrument for enrollment at an exam school E_i, controlling for linear control functions $g_s(\cdot)$ and $h_s(\cdot)$ of the running variables R_{is}, and for the exam school propensity score $\hat{p}_i$. The sample is limited to applicants whose probability of receiving an exam school offer is not equal to 0 or 1. As suggested by the descriptive statistics reported in Appendix Table A1, this sample is nearly representative of the exam school applicant population. Specifically, I estimate the following regression:

$$Y_i = \beta E_i + \sum_x \alpha_x \mathbb{I}(\hat{p}_i = x) + \sum_s g_s(R_{is}) + \varepsilon_i. \qquad \text{(Second stage)}$$

Since there are three exam schools in Boston, the propensity score takes on three different values $x = \{0.895, 0.75, 0.5\}$ for applicants with non-degenerate risk of any exam school offer. For more flexibility, I allow the coefficients associated with each of these values to vary by cohort. The linear control function for each school's running variable is also allowed to vary by cohort. I parameterize these functions as

$$g_s(R_{is}) = \omega_{1s} a_{is} + \kappa_{is} \left[\omega_{2s} + \omega_{3s}(R_{is} - \tau_s) + \omega_{4s}(R_{is} - \tau_s)\mathbb{I}(R_{is} > \tau_s)\right].$$

where a_{is} indicates whether applicant i applied to school s, and $\kappa_{is} = a_{is} \times \mathbb{I}(\tau_s - \delta_s < R_{is} < \tau_s + \delta_s)$ selects applicants in a bandwidth of size δ_s around an admission cutoff τ_s.

The corresponding first stage is

$$E_i = \gamma D_i + \sum_x \delta_x \mathbb{I}(\hat{p}_i = x) + \sum_s h_s(R_{is}) + \nu_i. \qquad \text{(First stage)}$$

This approach can also identify the treatment effect of each of the exam schools separately. The multi-school specification instruments the dummy for enrollment at each exam school (E_{1i}, E_{2i} and E_{3i}) by the corresponding school offer and controls for the values of each exam school's propensity score. In this case, the sample includes applicants in the bandwidth of any of the three exam schools.[7]

Table 3 presents encouraging evidence of covariate balance by offer status. In particular, receiving an offer from any of the exam schools is not correlated with higher baseline test scores, which bolsters confidence in the validity of the instrument.

[7] Since the school-specific sample includes all applicants with assignment risk at BLS and BLA, it is more positively selected than the sample of applicants with non-degenerate risk of being offered any exam school. This is confirmed by the descriptive statistics reported in Appendix Table A1.

3.2 Estimation of exam school treatment effect heterogeneity

3.2.1 Econometric framework

The 2SLS estimators of exam school effects for all applicants and subgroups of applicants identify a mix of match effects and substitution effects, and are thus not suitable for performing counterfactual analyses.[8] The problem with these estimators is that they fail to address substitution effects, since they ignore the fact that students are substituting away from different alternatives.[9]

Substitution effects are hard to pin down as they correspond to unobserved choices that need to be inferred. Behaghel et al., 2013; Kline and Walters, 2016; Blackwell, 2017; Lee and Salanié, 2018; Mountjoy, 2022 consider non-parametric identification of multiple treatment channels and multiple treatment-specific instruments.[10] This paper follows most closely the method outlined in Hull (2015), where treatment alternatives are unobserved but vary along observable dimensions. In this case, interacting the instrument with covariates predicting individuals' outside option is an intuitive way of identifying substitution effects. Nonetheless, this approach only allows for identification of both substitution and match effects under additional assumptions. In particular, Hull (2015) establishes that covariate interactions identify treatment effect by outside option only under the assumption of constant treatment effect within strata.

Formally, let S_i denote the school in which student i enrolls. S_i takes values from 1 to J for non-exam schools and value 0 for the exam school. Assume there exists a set of $J-1$ covariates $\{W_{ik}\}_{k=2}^{J}$ satisfying Assumption 1. Then Proposition 1 states that interacting these covariates with the instrument for school 0 identifies the treatment effect by non-exam school alternative .

Assumption 1

[8] See Appendix C for details on what LATE identifies.

[9] Heckman and Urzúa (2010) underlines that IV cannot identify treatment effects for different margins of choice without additional structural assumptions.

[10] Kirkebøen et al. (2017) show how an IV strategy identifies counterfactual specific LATEs when preferred treatment alternatives are directly measured.

1. ***Relevance:*** *For all* j *and for at least some* $w_j \neq w'_j$*, holding fixed* w_{ik} $\forall k \neq j$*,* $Pr[S_i = j \mid W_{ij} = w_j, \{W_{ik}\}_{k \neq j}] \neq Pr[S_i = j \mid W_{ij} = w'_j, \{W_{ik}\}_{k \neq j}]$.

2. ***Partial unordered monotonicity:*** *For any* $w_j < w'_j$*, holding fixed* w_{ik} $\forall k \neq j$*,* $Pr[S_i = j \mid W_{ij} = w_j, \{W_{ik}\}_{k \neq j}] \geq Pr[S_i = j \mid W_{ij} = w'_j, \{W_{ik}\}_{k \neq j}]$ *and* $Pr[S_i = k \mid W_{ij} = w_j, \{W_{ik}\}_{k \neq j}] \leq Pr[S_i = k \mid W_{ij} = w'_j, \{W_{ik}\}_{k \neq j}]$.

3. ***Constant treatment effect within covariate:*** *For all* j *and vector* w*,* $E[Y_{i0} - Y_{ij} \mid W_i = w] = E[Y_{i0} - Y_{ij} \mid W_i \neq w]$ *where* W_i *is the vector of covariates* $\{W_{ik}\}_{k=2}^{J}$.

The first condition of Assumption 1 is a first stage condition: the set of covariates must predict applicant non-exam school alternative. The second condition generalizes the standard monotonicity assumption from the binary case: each covariate shift renders each treatment either weakly more attractive for all individuals or weakly less attractive for all individuals. This rules out the possibility of compliers flowing in and out of each outside option in response to a shift in the corresponding covariate. The third condition entails that the treatment effect of attending one school instead of another is independent of the covariates predicting the non-exam school alternative. In the previous framework, it implies that $E[X_i \mid W_{ik}] = E[X_i]$ $\forall k$ since X_i is the only student-specific determinant of school potential effects. This assumption guarantees that variations along interacted covariates influence outcomes only through changes in the outside option.

Proposition 1 (Identification of treatment effect by non-exam school alternative)
Suppose there exists a valid instrument Z_i *for enrollment in school* 0*, and a vector of* $J-1$ *covariates* $\{W_{ik}\}_{k=2}^{J}$ *that satisfies Assumption 1.*

1. *Conditioning on* $\{W_{ik}\}_{k=2}^{J}$ *and* Z_i*, the interaction of* Z_i *and* $(1, \{W_{ik}\}_{k=2}^{J})$ *identifies* $E[Y_{i0} - Y_{ij} \mid D_{i1} > D_{i0}]$, $\forall j = 1, ..., J$.

2. *For any covariate* X_i*, conditioning on* $\{W_{ik}\}_{k=2}^{J}$ *and* X_i*, the interaction of* Z_i*,* $(1, \{W_{ik}\}_{k=2}^{J})$ *and* X_i *identifies* $E[Y_{i0} - Y_{ij} \mid X_i, D_{i1} > D_{i0}], \forall j = 1, ..., J$.

Proposition 1 establishes that the interaction of covariates predicting applicant outside options with an instrument for enrolling in an exam school identifies exam school treatment effects with respect to each non-exam school alternative, as long as the covariates satisfy the constant treatment within covariate assumption. This proposition is along the lines of Hull (2015) with more than two outside options, its proof in Appendix D considers the case of continuous interacted covariates.

Imposing constant treatment effect within the interacted covariates is less demanding than assuming constant treatment effect in general, or assuming that interacted covariates are exogenous. Constant treatment effect would rule out any match effect between students and schools since it entails that any two students should benefit equally or lose equally from attending an exam school instead of another school. Exogeneity would limit the set of valid covariates since it requires covariates to not be correlated with potential outcomes (not only treatment effects). Constant treatment effect within interacted covariates, however, only rules out heterogeneity in treatment effect along dimensions that vary substantially with the covariates predicting outside options. It allows heterogeneous match effects between schools and different types of students as long as these types are distributed equally along values of the covariates used to predict non-exam school alternatives.

Testing for a constant treatment effect within the interacted covariates is nonetheless challenging. Over-identification tests of homogeneous treatment effects are typically conducted across covariates, since these tests compare the treatment effect induced by different covariates, not different values of the same covariate. A conclusive over-identification test would thus require all interacted covariates to induce variations of a similar type (e.g. age variations, race variations or geographic variations).

3.2.2 Estimation of exam school effect by non-exam school alternative

To identify heterogeneity in exam school achievement effect by non-exam school alternative, I interact distance to schools with the exam school instrument. A long-standing literature has used distances to predict students' school of enrollment (Card, 1995; Neal, 1997; Booker et al., 2011; Walters, 2018; Mountjoy, 2022). Nonetheless, contrary to the approach taken in these papers, I do not use relative distances as instruments for school enrollment, but instead as covariates to be interacted with an instrument for enrollment. Hence, my strategy does not require exogeneity of relative distances but rather homogeneity of treatment effect along relative distances.

Considering the large number of potential non-exam school alternatives, I need to group schools.[11] The decomposition of treatment effect aims at identifying which schools perform worse than exam schools so that students substituting from those would gain from attending an exam school instead. Hence, it is appropriate to group together schools of similar quality.[12] Sorting schools based on their estimated OLS value-added (VA) is likely to result in groups of schools with similar effects on achievement as the bias in VA models controlling for observables and past achievement is typically small (Chetty et al., 2014; Angrist et al., 2017). Moreover, Angrist et al. (2017) argues that, bias notwithstanding, policy decisions in Boston middle schools based on conventional VA models could generate substantial achievement gains.

Schools are sorted based on their estimated value-added from a "lagged score" OLS VA model. The model includes indicators for sex, race, subsidized lunch eligibility, special education status (SPED), English-language learner status (ELL) and school year, along with cubic functions of all the baseline math and ELA test scores available.[13] For each application year, I estimate the model on the two previous years' sample of BPS schools enrolling more than 25 students, which captures the value-added of each school at the time of application. As each school's estimated Value-Added is year-specific, a school may be classified in different groups across years.

Choosing the number of groups represents a trade-off between informativeness, identifi-

[11] Moreover, it is not possible to identify the treatment effect of each specific school using distances, as any other point is uniquely defined by its distance from 3 non-collinear reference points on a plane.

[12] Using this method, one may explore classification of schools according to other attributes believed to be relevant. Whichever characteristic is used to categorize schools, the 2SLS procedure should produce unbiased estimates of exam school treatment effects with respect to each group of schools.

[13] MCAS exams for English and math were progressively introduced for each grade in the 2000s. By 2006, BPS students were tested in every grade between 3rd grade and 8th grade, providing a rich set of past test scores.

cation and precision. Appendix Table A2 offers a comparison of results for different sample splitting procedures. While constant treatment seems to be satisfied to the same extent, estimates become quite imprecise when using more than five groups. The most precise and informative estimates are achieved with four groups, since exam schools appear to perform as well as the median non-exam school alternative. Schools are thus sorted according to the quartiles of the estimated VA distribution for each application year.

As described in Table 1, schools in the bottom quartile have the lowest estimated Math VA with an average of -0.22, while schools in the top quartile have the largest estimated math VA with an average of 0.19. Exam schools have an average estimated math VA of 0.05, which makes them similar to a school in the bottom of the third quartile - i.e. just above the median. According to these estimates, Boston Latin is the most effective school with a math VA of 0.20, O'Bryant is second at 0.11 and Latin academy lies behind at -0.15. Math and English VA appear to be correlated: schools with low estimated math VA also tend to have low estimated 7$^{\text{th}}$ grade English VA. Nevertheless, Latin Academy has the largest English VA of 0.16, while Boston Latin is second at 0.13 and O'Bryant is third at -0.13. Surprisingly, Table 1 does not reveal substantial heterogeneity between groups of schools in terms of ethnic composition, share of English learners, SPED, native speakers or student baseline achievement. If anything, schools in the bottom quartile appear to concentrate a larger share of Asian students than schools in other groups.

To disentangle counterfactual-specific treatment effects, I estimate the following specification where $\{S_{ik}\}_{k=1}^{4}$ are dummies indicating enrollment at a school in the k^{th} quartile of the estimated VA distribution:

$$Y_i = \beta_0 + \sum_{k=1}^{4} \beta_k S_{ik} + \varepsilon_i.$$

This specification is similar to a value-added model with exam schools as the reference group, so that β_k gives the effect of attending a school in the k^{th} quartile with respect to an exam school. This model is evaluated on the sample of applicants that have non-degenerate risk, i.e., applicants that are only marginally offered a seat at one of the three exam schools.

To construct instruments for enrollment in each group of non-exam school alternatives, I interact receiving an exam school offer with distances between applicants' middle schools and junior high schools. These distances plausibly predict applicants' non-exam school alternative, since applicants that do not receive an exam school offer are likely to either stay in the school they were enrolled in at the time of application or move to a school in the same neighborhood. Specifically, I interact the distance d_{ik} of each applicant's middle school to the closest school in the k^{th} VA quartile with a dummy for receiving an exam offer D_i. Thus, enrollment at a school in quartile $k \in \{1, ..., 4\}$ is instrumented as

$$S_{ik} = \gamma_k D_i + \sum_{j=1}^{K} \lambda_{kj}(d_{ij} \times D_i) + \sum_{j=1}^{K} \phi_{kj} d_{ij} + \sum_{x} \delta_{kx} \mathbb{I}(\hat{p}_i = x) + \sum_{s} h_{ks}(R_{is}) + \eta_{ik}.$$

Table 2 presents the estimated first stages for each group of schools. Relative distance is a good predictor of non-exam school alternative as the first stage estimates appear to be strong, with F-statistics between 63 and 78. The coefficient on the distance to the closest school of each group is systematically negative, meaning that students prefer either staying at the school they attend at the time of application or moving to a school close to it. Moreover, the interaction between the exam school offer and the minimum distance to each group is positive, meaning that an exam school offer shifts students from a school only if it is close enough to constitute one of the potential non-exam school alternatives.

3.2.3 Test of the constant treatment effect assumption

Proposition 1 states that the set of covariates interacted with the exam school instrument must satisfy the assumption of constant treatment effect within covariate to identify exam school treatment effect by non-exam school alternative. Since the model controls for the distance to the closest school of each group, the identifying variation comes from differences in relative distances to each group's closest school. The constant treatment effect assumption thus requires marginal changes in relative distance to the closest school of each group not to be correlated with changes in the potential treatment effect of attending an exam school.

Otherwise, variations in relative distance would not exclusively impact the outcome through changes in the non-exam school alternative, and would thus not identify the pure substitution effect.

To make this assumption clearer in our context, consider an example with three groups of schools and two students, A and B. These students are equidistant from a group 1 and a group 2 school, but student A lives closer to a group 3 school. The constant treatment effect assumption assumes that student A does not gain more or less than student B from attending an exam school instead of a group 3 school. This seems plausible since students A and B only differ in their relative position to a school of group 3.

As a check for the constant treatment effect assumption, Table 3 assesses whether characteristics that could influence treatment effects vary along relative distances to the closest school of each group. All the coefficients are small and only one is significant. While the probability of being a Black student appears to change along relative distances, past test scores do not vary significantly. This mitigates the concern that results will be affected by heterogeneity in treatment effect along relative distances.[14]

Finally, the constant treatment assumption can be tested through an over-identification test when distances are used as interacted covariates. The model is over-identified since the exam school enrollment dummy is omitted, but the exam school offer dummy and its interactions with distances from all groups are used as instruments. The inclusion of one additional distance in the set of instruments allows for an over-identification test of homogeneous treatment. This test is particularly powerful since it compares the estimated treatment effect values by varying the geographic definition of complier groups. It is thus a test of homogeneity along variations in the characteristic used to identify the outside option.

[14] Additionally, Appendix Table A3 shows limited differential attrition along relative distances for applicants who receive an exam school offer.

4 Empirical results

4.1 Overall exam school effect

Panel A of Table 4 reports the LATE of enrolling in any of the exam schools on academic achievement. Despite the differences in sample and in method, I find that exam schools have a null or negative effect on test scores, consistent with Abdulkadiroglu et al. (2014) and Dobbie and Fryer (2014). Overall, exam school enrollment appears to reduce English scores by -0.107 SD in 7$^{\text{th}}$ grade and by -0.058 SD in 8$^{\text{th}}$ grade, while it is associated with insignificant changes in math achievement of -0.052 SD in 7$^{\text{th}}$ grade and 0.029 SD in 8$^{\text{th}}$ grade.[15]

The estimated exam school effect is mainly driven by O'Bryant, the least selective exam school. Indeed, the estimation strategy can only pin down the effect of enrolling in any of the exam schools for applicants with non-degenerate any exam offer risk. These applicants are more likely to be offered and enroll in O'Bryant as suggested by Figure 2. Among applicants with non-degenerate exam school risk, 92 % of offered compliers enroll at O'Bryant and 8% enroll at Boston Latin.

Exam school offer compliers are more likely to enroll at O'Bryant because most applicants rank Boston Latin first, Latin Academy second and finally O'Bryant third.[16] As a result, most marginal applicants to Boston Latin or Latin Academy clear the admission cutoff at either Latin Academy or O'Bryant. This is supported by Figure 2 which reports that 80% of non-offered compliers to a Boston Latin offer enroll either at Latin Academy or O'Bryant, whereas 98% of non-offered compliers to a Latin Academy offer enroll at O'Bryant. Only compliers to an O'Bryant offer enroll a traditional public school more than 40% of the time when not admitted at O'Bryant.

Hence, any comparison between exam schools and traditional public schools is a comparison between O'Bryant and the traditional sector. Most applicants at risk of a Boston Latin

[15] In contrast, OLS estimates in Appendix Table A4 entail positive achievement effects of attending an exam school for all applicants, regardless the quality of their non-exam school alternative.

[16] 61% of applicants in my sample submit these exact preferences, an additional 14 % of applicants invert the order of Boston Latin and Latin Academy, and only 8% of applicants rank O'Bryant first.

or Latin Academy offer have a certain probability of receiving an offer from one of the three exam schools, thus they do not identify the effect of attending an exam school rather than a traditional public school. This also implies that the decomposition of exam school effect by outside option provide estimates of the effect for the different groups of schools with respect to O'Bryant.

4.2 Decomposition of exam school effect

While the estimates in Table 4 panel A confirm that enrolling at O'Bryant does not increase academic achievement on average, panel B shows substantial heterogeneity in the O'Bryant treatment effect by non-exam school alternative. Applicants with non-exam school alternatives in the bottom two quartiles of estimated VA benefit from attending O'Bryant. O'Bryant enrollment increases the 7^{th} and 8^{th} grade math test scores of these applicants by between 0.05 SD and 0.18 SD, although only the 8^{th} grade coefficient for the first quartile and 7^{th} grade coefficient for the second quartile are statistically different from zero.[17] On the other hand, applicants whose non-exam school alternatives belong to the top two quartiles of estimated VA achieve worse English and math test outcomes when they enroll at O'Bryant. For instance, a top quartile school increases 8^{th} grade English MCAS scores by 0.13 SD and 8^{th} grade math MCAS scores by 0.21 SD with respect to O'Bryant.

Overall, these estimates correspond to lower O'Bryant achievement effects than those implied by the estimated math and English VA presented in Figure 1. Likewise, schools in the bottom two quartiles of estimated VA perform similarly according to the 2SLS estimates even though their estimated average math and English VA are different. This suggests either the existence of some bias in the value-added estimation or heterogeneity in VA for marginal exam school applicants.

The over-identification test provides reassuring evidence that the assumption of a constant treatment along relative distances is likely to be satisfied. The p-values for the test

[17]The estimates in this panel correspond to the effect of enrolling in a school from each group instead of an exam school. Hence, a negative coefficient indicates that applicants would benefit from attending O'Bryant instead of a school of the corresponding group.

reported in the bottom of panel B do not reject the null of a homogeneous effect, except marginally for 7th grade math MCAS. Moreover, the implied LATE, computed by weighting each coefficient in panel B by the estimated share of compliers for the corresponding group of non-exam school alternatives, is similar to the actual LATE reported in panel A.[18] A stark difference between the two figures would have suggested that the decomposition was picking up some variation not linked to the heterogeneity in non-exam school alternative.

As a further step to the decomposition, I try to assess the heterogeneity in average effect across compliers with different pre-treatment characteristics. In particular, I explore potential match effects for applicants with a high baseline score, and for Black and Hispanic applicants. Interacting the distance and offer instruments with dummies for each group recovers match effects within each group of non-exam school alternatives. Nonetheless, the increase in the number of endogenous variables comes at the cost of decreased precision, making the results hard to interpret.

Appendix tables A5 and A6 report the results for the decomposition by baseline math score and minority status. Although the estimates are noisy, match effects appear to be smaller than substitution effects. Moreover, match effects are not consistent across non-exam school alternative groups and test scores. Only minority applicants with a group 1 non-exam school alternative appear not to gain from enrolling in an exam school while their non-minority peers do benefit. This suggests that, once accounting for differences in outside options, there is no systematic heterogeneity in treatment effect across students.

4.3 Difference in treatment effect of each exam school

Applicants whose non-exam school alternative is of below-median quality gain from attending O'Bryant, but would they be better off getting admitted to Latin Academy or Boston Latin instead? As underlined in Section 4.1, my estimation strategy can only pin down the effect of each exam school with respect to the outside option of applicants with non-degenerate offer

[18] The share of compliers with a non-exam school alternative in each group is estimated by regressing a dummy for enrollment in a given group on a dummy for not enrolling at an exam school instrumented by not receiving an exam offer. The second bar of Figure 2 summarizes these results.

risk. Given the enrollment destinies presented in Figure 2, it follows that students marginally offered Boston Latin may be used to estimate the effect of attending Boston Latin rather than Latin Academy; while students marginally seated at Latin Academy identify the effect of attending Latin Academy rather than Boston Latin or O'Bryant.

Table 5 presents the results of these comparisons by adding to the model enrollment dummies for each individual exam school.[19] These dummies are instrumented by exam school-specific offers, which are interacted with distances to groups of traditional public schools in order to account for potential differences in substitution patterns.[20] The dummy for enrolling at O'Bryant is excluded from the specification, so that each coefficient corresponds to the effect of attending that school rather than O'Bryant. As expected, the inclusion of individual exam school enrollment dummies does not substantially affect the coefficients on the non-exam school alternatives, since these were already implicitly with respect to O'Bryant in the any exam school specification.

Contrary to what applicants seem to believe, estimates reported in Table 5 suggest that it is more beneficial to enroll at O'Bryant than at Latin Academy, and that gaining access to Boston Latin from Latin Academy does not make a substantial difference. Attending Latin Academy as opposed to O'Bryant reduces MCAS math test scores by 0.12 SD in 7^{th} grade and by 0.36 SD in 8^{th} grade. The difference in 8^{th} grade MCAS English test score is not statistically different from zero, while Latin Academy appears to increase 7^{th} grade MCAS English test score by 0.15 SD. Moreover, the table shows no statistically significant gains of enrolling at Boston Latin instead of Latin Academy.[21]

[19] For all exam school specific specification, the sample includes all applicants with a non-degenerate risk of admission for at least one of the three schools, even those who receive an offer from one of the three schools with probability one.

[20] These differences are unlikely to be critical as only a small share of non-offered Boston Latin compliers enroll at a traditional public school, and they tend to enroll equally in schools of the four VA groups.

[21] Appendix table A7 explores potential heterogeneity in gains for minority students and applicants with baseline math test scores above the median. Overall, there appears to be no substantial additional gains from getting into a more selective exam school for students in these two groups.

5 Counterfactual admission criteria

The previous analysis uncovered the effects of Boston's three exam schools on educational attainment for applicants close to each school's admission cutoff. In this section, I use these estimates to evaluate which change in exam school admission criteria would lead to the highest increase in overall achievement. Estimates for marginally seated applicants are particularly relevant for this analysis, as alternative admission criteria primarily change which applicants get admitted at the margin.

The optimal admission rule would give priority to applicants whose non-exam school alternative is of low quality. Indeed, the decomposition by non-exam school alternative uncovered significant heterogeneity in achievement gains from enrolling at an exam school depending on the quality of an applicant's outside option, While it found little scope for match effects. Changing exam school admission criteria to leverage this heterogeneity in treatment effect by non-exam school alternative could thus increase overall achievement. Since non-exam school alternatives are not observable, this policy is not implementable in practice. Nonetheless, I can estimate the optimal assignment and use it to benchmark the effects of feasible changes in admission criteria.

As for feasible policies, I simulate three alternative admission schemes: granting admission to top-ranked middle school applicants, replacing ISEE scores with MCAS scoresand a place-based priority system. First, I consider the effect of implementing a "Texas top10" style rule. Specifically, I simulate the exam school selection process when granting admission to applicants whose 6^{th} grade GPA places them in the top 5 or 10% of their middle school.[22] Second, I explore the impact of replacing ISEE scores with MCAS scores in the composite score used to rank applicants. This policy was suggested by Rucinski and Goodman (2022) as a feasible alternative to race-based priorities. Finally, I implement a place-based priority system in which applicants, whose closest school belongs to the two lowest quartiles of

[22] To simulate this change in admission criteria, I assume top middle school students' application behavior is unaffected. That is, all the top students that currently apply would also apply under the new policy, and top students that currently do not apply would still not apply under the new policy.

estimated VA, have priority for 25%, 50% or 75% of reserved seats.[23]

As the non-exam school alternative for each applicant is not directly observable, I use a linear model with distances to each group of schools as regressors to predict applicants' non-exam school alternative.[24] Panel A of Table 6 explores the accuracy of the prediction model by comparing the actual distribution of non-exam school alternatives to the distribution predicted by the model, for applicants that do not receive an exam school offer. All predicted shares are less than one percentage point apart from the actual shares.

I use this model to predict the distribution of non-exam school alternatives for admitted applicants under the different admission schemes, and to estimate the assignment under an optimal admission regime. The optimal assignment is obtained by maximizing the total gain in MCAS math 8^{th} grade test scores. This policy corresponds to maximizing the shares of admitted applicants whose non-exam alternatives belong to the bottom two quartiles of estimated VA.[25]

The comparison of the three different admission schemes to the optimal assignment and the actual assignment in Panel B of Table 6 suggests that only the place-based priority system is likely to improve applicants' overall academic performance. Indeed, while the place-based priority system increases the share of offered applicants whose non-exam school alternative belongs to the first two quartiles of VA, granting seats to top middle school applicants or replacing ISEE scores with MCAS scores have almost no effect on the distribution of non-exam school alternatives among admitted applicants. As a result, the place-based scheme comes the closest to the optimal assignment. Reserving 75% seats to students with a low VA closest alternative increases 8^{th} grade math test score by 0.12 SD for 15% of applicants.

[23] The address of the school of enrollment at the time of application is used as a proxy for student's address. Reserved seats are filled after open seats, which favors applicants that qualify for the reserve (Dur et al., 2020).

[24] Any negative predicted probability of enrolling in a group is set to zero. Predicted probabilities are then normalized by setting their sum to one for each applicant.

[25] The performance of the different groups of non-exam school alternatives was gauged with respect to O'Bryant. Nonetheless, as different demographic groups do not seem to benefit more or less from gaining access to Boston Latin or Latin Academy, estimates by non-exam school alternative specify the relative gain or loss from diverting a student from a given school, even when the student enrolls at Boston Latin or Latin Academy. Hence, these estimates may be used to compute the gain from changing the distribution of non-exam school alternatives for applicants admitted at any of the three exam schools.

For comparison, the optimal assignment results in a similar increase of 0.13 SD for 20% of applicants. On the other hand, granting admission to the top students from each middle school or replacing the ISEE with MCAS affects the test scores of at most 1% of applicants, and the average gain varies between -0.04 SD and 0.02 SD.

The relative performance of each admission criteria can be explained by the correlation between the criteria used and the quality of applicants' non-exam school alternatives. Considering Table 1, the average baseline MCAS score for students does not vary systematically with the estimated quality of schools. Thus, students with MCAS scores that are higher than their ISEE scores are not more likely to have a low VA non-exam school alternative. Similarly, each VA group contains the same number of schools and is not concentrated geographically. Selecting applicants from each school of origin is thus not likely to affect the distribution of non-exam school alternatives. On the other hand, an applicant's middle school is expected to be her outside option, so giving priority to applicants with low VA middle schools is quite effective.

One of the main arguments for reforming exam school admission is that Black and Hispanic students tend to be underrepresented at these elite institutions. Interestingly, all of the alternative admission criteria increase minority students' representation at exam schools, although none targets these students directly. For instance, the 75% reserve for students with a low-quality outside option increases the share of Blacks and Hispanics among admitted applicants to 48%, that is, by 6 percentage points compared to the actual assignment.

However, the magnitude of the increase is not correlated with the effectiveness of the admission scheme. Indeed, the admission scheme resulting in the largest minority share (48%) is the priority for the top 10% of students from each middle school, which has no effect on academic achievement. This discrepancy can be explained by the fact that Black and Hispanic students do not appear to have systematically worse outside options (as shown in Table 1). Thus, only some minority applicants would actually benefit from attending an exam school instead of their non-exam school alternative. In general, Table 1 suggests that no observable characteristic is strongly correlated with the quality of a student's non-exam school alternative. Hence, targeting based on observable characteristics is unlikely to improve academic achievement.

To sum up, granting admission to top-ranked middle school applicants or replacing ISEE scores with MCAS scores does not leverage the heterogeneity in the exam school treatment effect. Thus, implementing these changes would probably not result in substantial changes in overall achievement. On the other hand, directly targeting applicants based on their middle school leverages most of the relevant heterogeneity and results in significant improvements.

6 Conclusion

Existing regression discontinuity estimates of exam school effects find zero gain from attending these schools. Nonetheless, I show that these estimates aggregate important heterogeneity in treatment effects and are thus not suitable for performing counterfactual analyses. In particular, applicants whose non-exam school alternative has an estimated math VA in the bottom two quartiles of Boston public schools benefit from attending an exam school.

It follows that changing the admission scheme for Boston exam schools could increase overall achievement. This improvement comes from identifying and targeting applicants who benefit from attending exam schools. These students are not characterized by specific demographics but rather by low-quality non-exam school alternatives. This distinction highlights the necessity of separately identifying heterogeneity in substitution effects from match effects when attempting to compare different allocation of students to schools.

More generally, in any setting with multiple treatments, an accurate counterfactual analysis should always involve a full decomposition of the treatment effect. Although identifying these different sources of heterogeneity is challenging, my analysis shows how to leverage applicants' locations. This strategy is particularly relevant since it allows for a compelling test of the identifying assumptions, and my approach may be applicable in other settings where treatment is related to spatial position.

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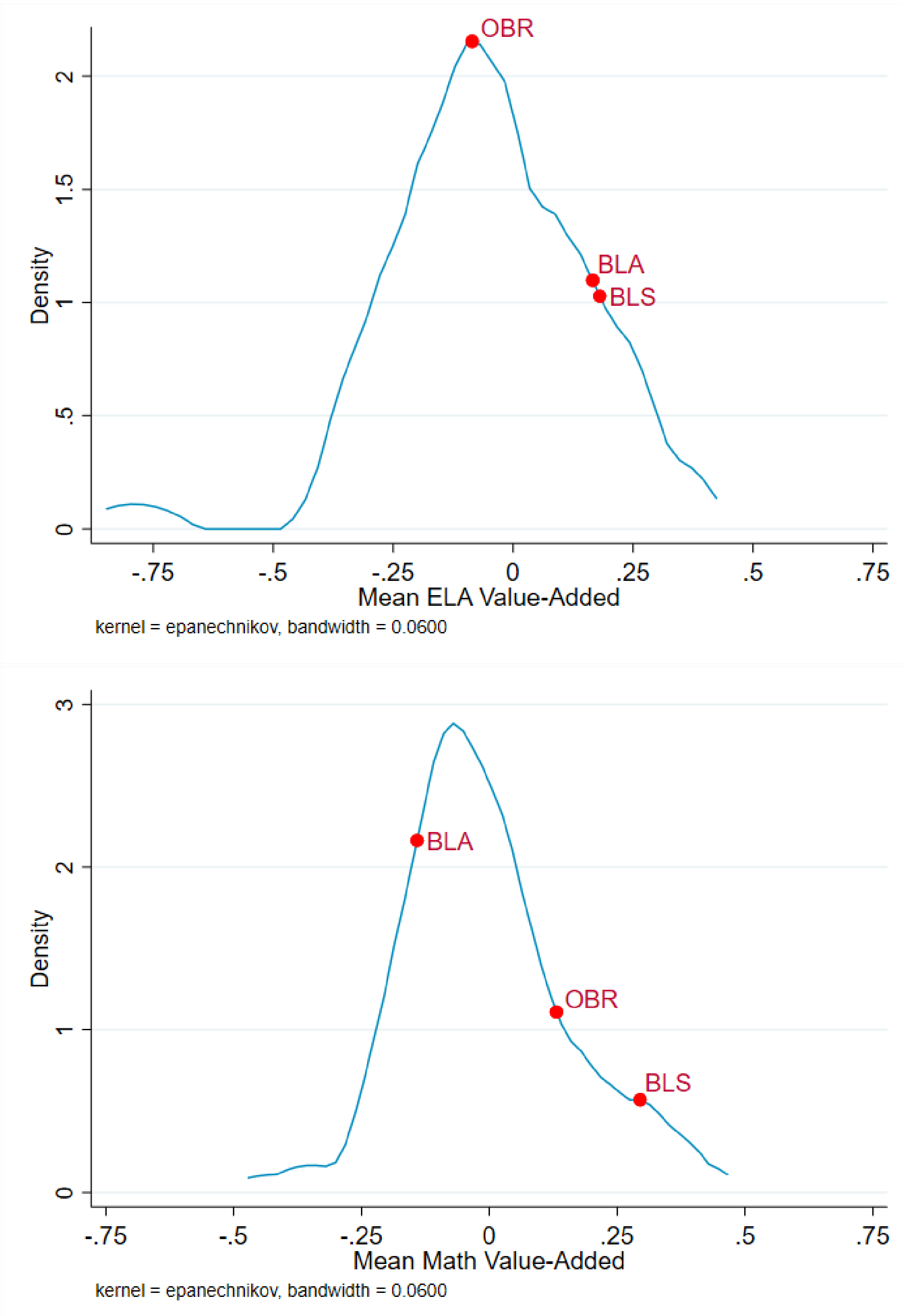
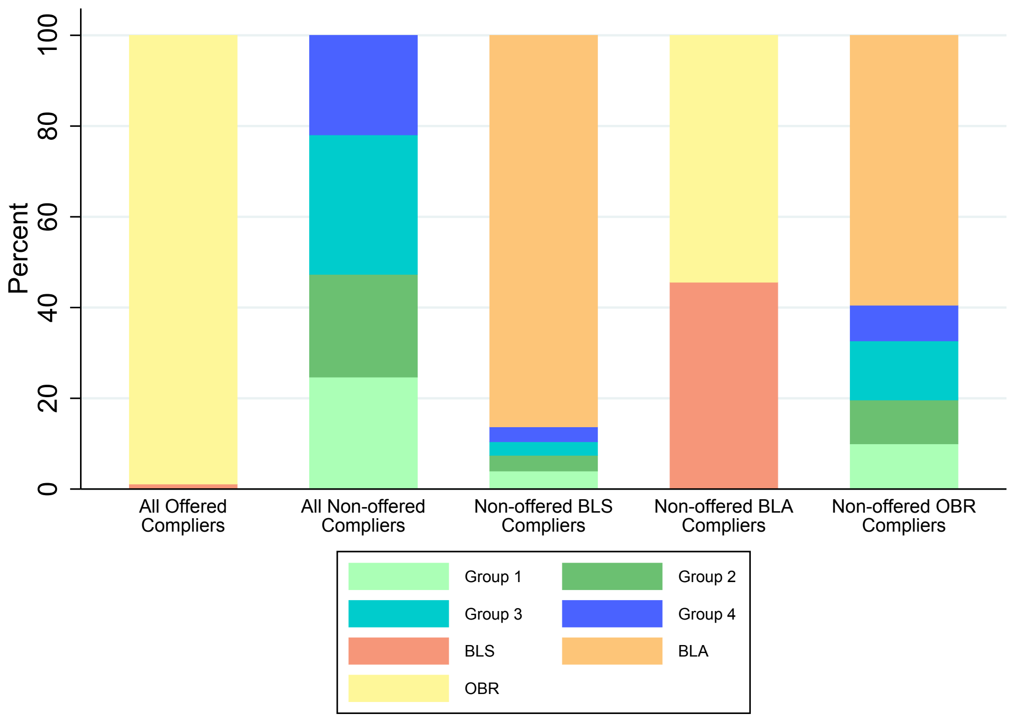
Figure 1: English and Math Value-Added of Boston Public Middle Schools



Note: This figure shows the distribution of Boston public middle schools' English and Math value-added for years 2004-2016. The plots display the mean value-added of each school over the period.

Figure 2: Enrollment Destinies by Exam School



Note: This figure shows the enrollment destinies of exam school compliers when offered and not offered an exam school seat. Enrollment compliers are applicants who attend an exam school when offered a seat but not otherwise. The 1^{st} bar plots exam destinies for applicants when accepted in any of the three exam schools. The 2^{nd} bar plots non-exam destinies for applicants rejected from all exam schools. The 3^{rd} bar plots destinies for rejected Boston Latin applicants, the 4^{th} bar for rejected Latin Academy applicants and the 5^{th} bar for rejected O'Bryant applicants. Destinies are estimated as in Abdulkadiroglu et al. (2014). Enrollment rates are measured in the fall following exam school application.

Table 1: Characteristics of Groups of Schools

	Exam schools (1)	Quartile 1 (2)	Quartile 2 (3)	Quartile 3 (4)	Quartile 4 (5)
Mean math VA	0.05	-0.22	-0.07	0.00	0.19
Mean English VA	0.09	-0.23	-0.10	-0.10	0.08
N schools	3	7	7	7	7
N students at risk	4920	861	762	867	734
% Female	0.55	0.46	0.46	0.47	0.47
% Hispanic	0.18	0.32	0.36	0.41	0.39
% Black	0.24	0.34	0.40	0.40	0.32
% White	0.31	0.08	0.06	0.08	0.10
% Asian	0.23	0.22	0.14	0.08	0.13
% Other ethnicity	0.04	0.05	0.03	0.04	0.06
% FRPL eligible	0.47	0.81	0.82	0.82	0.80
% Native speaker	0.66	0.50	0.54	0.56	0.57
% SPED	0.01	0.19	0.20	0.24	0.22
% English learners	0.03	0.22	0.23	0.20	0.20
MCAS English 4^{th}	1.18	-0.16	-0.27	-0.24	-0.17
MCAS English 6^{th}	1.18	-0.20	-0.30	-0.26	-0.08
MCAS Math 4^{th}	1.23	-0.12	-0.26	-0.23	-0.18
MCAS Math 6^{th}	1.39	-0.18	-0.34	-0.20	-0.12

Note: This table reports descriptive statistics for exam schools and the different group of schools. Statistics are computed for students enrolled in these schools between 2004-05 and 2016-17. Schools in each group are weighted by the number of students with any-exam offer risk and 8^{th} grade Math MCAS enrolling in the school. FRPL eligible students are students eligible for free or reduced-price lunch, SPED students are special education students.

Table 2: First Stage Estimates for School of Enrollment

	Quartile 1 (1)	Quartile 2 (2)	Quartile 3 (3)	Quartile 4 (4)
Exam offer	-0.137	-0.256	-0.235	-0.159
	(0.039)	(0.043)	(0.046)	(0.043)
Exam offer X distance to quartile 1	0.111	-0.036	-0.043	-0.035
	(0.008)	(0.006)	(0.006)	(0.007)
Exam offer X distance to quartile 2	-0.045	0.110	-0.024	-0.037
	(0.006)	(0.007)	(0.006)	(0.006)
Exam offer X distance to quartile 3	-0.046	-0.039	0.110	-0.023
	(0.006)	(0.007)	(0.008)	(0.007)
Exam offer X distance to quartile 4	-0.035	-0.027	-0.028	0.089
	(0.005)	(0.006)	(0.006)	(0.007)
Distance to quartile 1	-0.125	0.039	0.046	0.039
	(0.008)	(0.007)	(0.007)	(0.007)
Distance to quartile 2	0.051	-0.119	0.025	0.042
	(0.006)	(0.007)	(0.006)	(0.006)
Distance to quartile 3	0.053	0.044	-0.121	0.025
	(0.006)	(0.007)	(0.009)	(0.008)
Distance to quartile 4	0.039	0.032	0.030	-0.100
	(0.006)	(0.006)	(0.006)	(0.008)
F-statistic	63.2	74.7	78.0	69.7
N	5,978	5,978	5,978	5,978

Notes: This table reports first stage estimates for enrollment in different types of schools for 2004-2016 exam applicants with non-degenerate any-exam offer risk. The instrumental variables are receiving an exam school offer and its interactions with distances (in kms) to each group's closest school, controlling for each group's distance (in kms). The sample is limited to applicants with MCAS 8th grade math score. All models control for any-exam offer risk, school-by-year linear running variables and the set of variables listed in Table 1, except MCAS 6th grade scores. Standard errors clustered at the school of origin and application year are reported in parentheses.

Table 3: Covariate Balance

	Any Exam Offer		Distance to Quartiles			School Specific Offers		
	Offer gap	p-value	Joint F test	p-value	N	Joint F test	p-value	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	-0.031	0.376	1.565	0.197	5,978	1.242	0.294	8,144
Hispanic	0.028	0.333	1.543	0.203	5,978	0.554	0.646	8,144
Black	0.014	0.683	10.83	0.000	5,978	0.669	0.571	8,144
White	0.005	0.828	0.966	0.409	5,978	0.681	0.564	8,144
Asian	-0.038	0.172	1.958	0.413	5,978	0.665	0.574	8,144
FRPL eligible	-0.003	0.913	0.528	0.664	5,978	1.242	0.294	8,144
Native Speaker	0.056	0.125	2.649	0.048	5,978	2.238	0.083	8,144
MCAS English 4^{th}	0.059	0.321	1.985	0.116	5,978	0.984	0.400	8,144
MCAS English 6^{th}	0.028	0.547	0.294	0.830	4,897	0.501	0.682	6,772
MCAS Math 4^{th}	0.020	0.738	2.201	0.087	5,978	0.358	0.784	8,144
MCAS Math 6^{th}	-0.045	0.389	0.132	0.941	5,941	0.335	0.800	8,095

Notes: This table reports estimates of offer effect and distance to groups effects on covariates for 2004-2016 applicants. Columns 1 and 2 report any-exam offer effects; columns 3-4 show the joint F-statistics of distances to the four groups of schools, columns 5-6 show the joint F-statistics of school-specific offers. Sample sizes in column 5 count the number of observations with non degenerate risk of any-exam offer, sample sizes in column 9 count the number of observations in the bandwidth of at least one of the schools. Sample sizes are smaller for 6^{th} grade MCAS tests since they were first introduced in 2004 and 2006. Models include school-by-year linear running variable controls and either any-exam offer risk or school-specific offer risk controls, as specified in the text. Standard errors clustered at the school of origin and application year are reported in parentheses.

Table 4: 2SLS Estimates of Exam School Effects

	MCAS English		MCAS Math	
	7^{th} grade	8^{th} grade	7^{th} grade	8^{th} grade
	(1)	(2)	(3)	(4)
Panel A: Overall exam school effect				
Enrollment in exam school	-0.103	-0.058	-0.058	0.026
	(0.049)	(0.051)	(0.066)	(0.070)
First stage coefficient	0.79	0.79	0.78	0.79
Panel B: Exam school effect by non-exam school alternative				
Enrollment in quartile 1 school	-0.026	-0.026	-0.046	-0.175
	(0.063)	(0.065)	(0.085)	(0.091)
Enrollment in quartile 2 school	-0.019	0.031	-0.140	-0.131
	(0.069)	(0.066)	(0.081)	(0.087)
Enrollment in quartile 3 school	0.196	0.096	0.126	0.038
	(0.063)	(0.068)	(0.090)	(0.086)
Enrollment in quartile 4 school	0.276	0.132	0.322	0.205
	(0.063)	(0.068)	(0.090)	(0.089)
Over-id p-value	0.575	0.840	0.062	0.309
Implied LATE	-0.106	-0.057	-0.061	0.019
N	5,953	5,978	5,429	5,978

Notes: This table reports 2SLS estimates of school enrollment effects for 2004-2016 exam applicants with non-degenerate any-exam offer risk. The endogenous variable is enrollment either enrollment in 7^{th} grade at any exam school in panel A or at one of the group of schools in Panel B. Panel B coefficients correspond to the effect of enrollment at one of the groups of non-exam school alternatives with respect to enrollment at any exam school. All models control for any-exam offer risk, school-by-year linear running variables and the set of variables listed in Table 1, except MCAS 6^{th} grade scores. The implied LATE is computed by taking the negative of the sum of panel B coefficients weighted by the share of compliers going to each group of non-exam school alternatives. Standard errors clustered at the school of origin and application year are reported in parentheses.

Table 5: 2SLS Estimates of Each Exam School Effects

	MCAS English		MCAS Math	
	7^{th} grade	8^{th} grade	7^{th} grade	8^{th} grade
	(1)	(2)	(3)	(4)
Enrollment in BLS	-0.066	-0.009	-0.127	-0.343
	(0.062)	(0.063)	(0.073)	(0.072)
Enrollment in BLA	0.145	-0.041	-0.120	-0.359
	(0.049)	(0.049)	(0.058)	(0.059)
Enrollment in quartile 1 school	-0.039	-0.019	-0.069	-0.155
	(0.062)	(0.064)	(0.084)	(0.089)
Enrollment in quartile 2 school	-0.013	0.066	-0.109	-0.058
	(0.065)	(0.064)	(0.076)	(0.084)
Enrollment in quartile 3 school	0.130	0.065	0.099	0.024
	(0.059)	(0.054)	(0.073)	(0.080)
Enrollment in quartile 4 school	0.236	0.155	0.295	0.183
	(0.062)	(0.067)	(0.087)	(0.085)
Over-id p-value	0.353	0.121	0.198	0.003
N	8,129	8,144	7,493	8,144

Notes: This table reports 2SLS estimates of school enrollment effects for 2004-2016 exam applicants with non-degenerate offer risk at one of the exam schools. The endogenous variable is enrollment in 7^{th} grade at each specific exam school or at one of the group of schools. Enrollment at O'Bryant is omitted from the model so each coefficient corresponds to the treatment effect with respect to O'Bryant. All models control for school-specific offer risk, school-by-year linear running variables and the set of variables listed in Table 1, except MCAS 6^{th} grade scores. Standard errors clustered at the school of origin and application year are reported in parentheses.

Table 6: Consequences of Alternative Admission Schemes

Panel A: Accuracy of model predicting applicant non-exam school alternative

	Non-exam alternative for non-offered applicants			
	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Actual distribution	25.7%	26.0%	26.8%	21.5%
Predicted distribution	26.2%	25.6%	26.7%	21.5%

Panel B: Comparison of counterfactual admission schemes to actual admission scheme

		Characteristics of offered applicants					Gains in MCAS Math	
		Non-exam alternative for offered applicants					Net % of	Average
		Quartile 1	Quartile 2	Quartile 3	Quartile 4	% minority	app. affected	gain
Actual admission scheme		26%	26%	27%	21%	42%		
Optimal admission scheme: priority depending on non-exam alternative		37%	35%	18%	10%	53%	20%	0.13
Automatic admission for top middle school students	top 5%	26%	26%	28%	21%	44%	0%	-0.01
	top 10%	26%	26%	28%	21%	48%	0%	0.02
Different composite score used to ranked applicants	MCAS instead of ISEE	25%	26%	27%	21%	44%	1%	-0.04
Share of seats reserved to students whose closest school has less than median VA	25% reserve	27%	27%	26%	20%	42%	3%	0.13
	50% reserve	31%	30%	22%	18%	43%	10%	0.12
	75% reserve	33%	33%	18%	16%	48%	15%	0.12

Notes: This table reports the distribution of non-exam school alternatives under different admission schemes for 2004-2016 exam applicants from a BPS school with baseline test scores. Panel A explores the accuracy of the model predicting applicants' non-exam school alternative by comparing the distribution of alternative schools predicted by the model to the actual distribution for non-offered applicants. Panel B compares the distribution of non-exam school alternatives for applicants offered an exam school under four different admission schemes to the distribution of alternative schools for applicants offered an exam school under the actual admission scheme. The table also displays the average gain in MCAS Math 8$^{\text{th}}$ grade test scores and the net share of applicants gaining under each counterfactual admission scheme.

A Data

Boston Public Schools (BPS) is the source of the application files for exam schools. The State of Massachusetts (DESE) provided the enrollment files and MCAS data. Application, enrollment and test scores files are merged using the State unique identifier (SAS-ID). In this appendix section, I describe these data sets, the construction of the distance variable and the additional sample restrictions.

Application data

The exam school application file contains a record for each exam school applicant consisting of an application ID number, state ID (SAS-ID) number, name, gender, race, date of birth, application year, grade of application, preferences over the three exam schools, and the composite score for admission. Each record also includes the school offered to the applicant (if any). This dataset covers students who applied to exam schools between 1995 and 2017 for entrance in 7^{th}, 9^{th} and 10^{th} grades. Offered applicants enroll in the fall of the application year. The analysis sample only includes 7^{th} grade applicants from 2004-2016 for which both baseline and outcome test scores are available. I also exclude from the analysis duplicate observations and applicants with missing application ID number.

Admission rank cutoffs for each year and each school correspond to the rank of the last offered applicant, after excluding admitted applicants with incorrect ranks. Applicants are deemed incorrectly ranked if they are offered an exam school seat although their rank is much higher than the rank of the last but one admitted applicant at the same school. Based on these empirical admission rank cutoffs, I construct for each school a simulated offer variable which is preferred to the actual offer variable throughout the analysis. Despite the identified irregularities, the match replicates 99% on average.

Enrollment data

The Massachusetts enrollment file spans school years 2001-2002 through 2017-2018. For each student enrolled in Boston Public Schools, each file contains snapshots at the start of the school-year (October) and at the end of the school-year (June) of the student's grade, school and demographic information. These records are identified by a unique student identifier (the SAS-ID). The variables of interest in the enrollment files are grade, year, sex, race, low-income status, special education status (SPED), and native speaker status. The school each student was enrolled prior to application is obtained from the end of the school-year enrollment files while the October enrollment files are used to determine the school each student enrolls after application. Enrollment data is only available for students enrolled in Boston Public Schools, which excludes students who enrolled in private schools before or after applying to an exam school.

Test score data

The MCAS test score file spans school years from 2002 to 2018. Each record includes scores for two subjects (English and Math), as well as the grade (4^{th}, 6^{th}, 7^{th} and 8^{th} grade) and the year in which the test was taken. I standardize scores among Boston test-takers by year and grade. For multiple time test takers, the last test score for each grade is considered. 4^{th} grade Math and English MCAS test scores constitute the baseline scores. 7^{th} and 8^{th} grade English and 7^{th} and 8^{th} grade Math MCAS test scores are the main outcomes of interest. 6^{th} grade Math and English scores are only available after 2004 and 2006 respectively, while the 7^{th} grade Math MCAS test was only introduced in 2006.

Distance to schools

Only non-exam schools where at least one student enrolled after submitting an exam school application during the 2004-2016 period are included in the set of potential schools of enrollment. The distance between each sending middle school of the dataset and each potential school of enrollment is computed using the shortest road distance between the two points. It does not take into account differences in traffic or in speed limit across roads.

Sample restrictions

The analysis sample is restricted to students enrolled in a non-charter BPS school prior to application. This excludes the possibility for students to enroll in charter after application since admissions to Massachusetts charter schools occur either in 5$^{\text{th}}$ or 6$^{\text{th}}$ grade through lotteries. Hence, the analysis only considers students that may either enroll in an exam school or in a non-charter BPS school. Students with no baseline test scores, i.e. who were not enrolled in BPS during 4$^{\text{th}}$ grade, are also excluded from the analysis.

Description of value-added sample and model

The value-added models for math and English are estimated on the sample of all non-charter BPS schools. Schools with fewer than 25 students enrolled in 7^{th} grade are excluded from the sample. The math value-added model uses Math MCAS test scores from 8^{th} grade but considers the school where students were enrolled in 7^{th} grade. The English value-added model uses 7^{th} grade MCAS English test scores. Both models include indicators for sex, race, subsidized lunch eligibility, special education status, English-language learner status, school year, along with cubic functions of all the baseline Math and ELA test scores available (3$^{\text{rd}}$, 4$^{\text{th}}$, 5$^{\text{th}}$ and 6$^{\text{th}}$ grades). For each year, the value-added of each school is computed using the data from two years prior.

B Computation of the propensity scores

Following the methodology in Abdulkadiroglu et al. (2022), I exploit the rank order list of applicants to compute admission propensity scores at each exam school. Specifically, an applicant whose score is close to a school's admission cutoff has a local risk of admission at this school of one half. Based on the applicant's rank order list, the overall probability of admission at each school can be computed by considering the admission probability at more preferred schools.

Formally, let $s = 1, 2, 3$ index exam schools in set S. $\theta_i = (\succ_i)$ denotes applicant i's type, where $\succ_i$ is applicant i's ranking of schools. The school specific ranking of applicant i used for admission is denoted by R_{is}; this is the school specific RD running variable. R_i is the vector of rankings at each school for applicant i. Each applicant gets an offer at school s if and only if her ranking is below the school specific cutoff τ_s, i.e. iff $R_{is} \leq \tau_s$.

Considering applicant rank order lists, let B_{θ_s} be the set of schools type θ prefers to s

$$B_{\theta_s} = \{s' \in S | s' \succ_\theta s\}$$

Given a bandwidth δ, an applicant type θ and rankings vector R_i, define the risk of being seated at school s as

$$\Psi_s(\theta, R, \delta) = \begin{cases} 0 & \text{if } R_s < \tau_s - \delta \text{ or } R_b > \tau_b + \delta \text{ for some } b \in B_{\theta_s} \\ 0.5^{m_s(\theta,R)} & \text{if } R_s > \tau_s + \delta \text{ and } R_b \leq \tau_b + \delta \text{ for all } b \in B_{\theta_s} \\ 0.5^{1+m_s(\theta,R)} & \text{if } \tau_s - \delta \leq R_s \leq \tau_s + \delta \text{ and } R_b \leq \tau_b + \delta \text{ for all } b \in B_{\theta_s} \end{cases}$$

where $m_s(\theta, R) = |\{b : b \in B_{\theta_s} \quad \text{and} \quad \tau_b - \delta \leq R_b \leq \tau_b + \delta\}|$

Under the Assumption that the distribution of each running variable is continuous at the admission cutoff for each applicant type, Theorem 1 of Abdulkadiroglu et al. (2022) shows that

$$\lim_{\delta \to 0} E[D_i(s) | \theta_i = \theta, R_i = R, W_i = W] = \Psi_s(\theta, R, \delta)$$

Where $D_i(s)$ is an indicator for receiving an offer from school s and W_i is a vector of observed and unobserved characteristics of student i. Controlling for the propensity score, offers from school s are locally as good as randomly assigned and can thus be used as instruments for enrollment.

Empirically, the propensity score can be computed as the sample equivalent of the theoretical local propensity score described above using the information contained in the applicant rank order lists and the observed admission cutoffs. Theorem 2 of Abdulkadiroglu et al. (2022) establishes uniform convergence of the empirical propensity score in an asymptotic sequence that increases market size with a shrinking bandwidth. This justifies conditioning on the empirical propensity score to eliminate OVB in school effect estimates.

When computing the scores, I separately estimate bandwidths for each school and cohort according to Imbens and Kalyanaraman (2012). As an intermediate step, I also estimate bandwidths for each outcome variable; I keep the smallest bandwidth for each school and cohort.

C What does 2SLS identify?

As shown in Section 2.1, questions regarding counterfactual assignments cannot be answered without knowledge of both substitution and match effects. In this section, I show that these effects are hard to identify separately, and that 2SLS identifies a weighted average of both effects, even when estimated on a specific subgroup.

I am interested in the treatment effect of enrolling in exam schools, which can indexed as school 0. Let D_i be a dummy equal to 1 when student i enrolls in school 0. School 0 treatment effect is given by β in the regression of Y_i on D_i,

$$Y_i = \alpha + \beta D_i + u_i. \tag{4}$$

The decision to enroll in a specific school is typically endogenous and correlated with student characteristics. Hence, the large literature interested in school treatment effects has leveraged instrumental variables based on specific admission rules (lotteries, admission tests, etc.) and student characteristics (distance). Suppose there exists a valid binary instrument Z for enrollment in school 0, and let D_{i1} and D_{i0} denote the potential values of D_i when $Z_i = 1$ and $Z_i = 0$ respectively. Since Z is a valid instrument, it satisfies the following assumptions.

Assumption 2

1. ***Instrument relevance**: $E[D_{i0}] \neq E[D_{i1}]$*

2. ***Random assignment and exclusion**: Z_i is independent of $(D_{i1}, D_{i0}, Y_i(D_i, Z_i))$ and $Y_i(D_i, 0) = Y_i(D_i, 1) = Y_i(D_i)$*

3. ***Monotonicity**: $D_{i1} \geq D_{i0}$ $\forall i$ and $D_{i1} > D_{i0}$ for some i*

Proposition 2 establishes that the LATE obtained from the 2SLS regression which instruments D with Z is a weighted average of pairwise comparisons between exam schools and non-exam school alternatives indexed by $j = 1, ..., J$, with weights given by the distribution of outside options for compliers. The ω_j weight captures the share of compliers that have outside option j, and the $\omega_{x|j}$ weight captures the share of compliers with outside option j that have characteristics x.

Proposition 2 (2SLS identification)
Suppose there exists an instrument Z_i for D_i that satisfies Assumption 2. The 2SLS regression using Z_i as an instrument identifies

$$\beta_{LATE} = \underbrace{E[Y_{i0} - \sum_j \omega_j Y_{ij} \mid D_{i1} > D_{i0}]}_{\textit{substitution effects}} + \underbrace{\sum_x E[\omega_{x|0}\varepsilon_{i0} - \sum_j \omega_j \omega_{x|j}\varepsilon_{ij} \mid X_i = x, D_{i1} > D_{i0}]}_{\textit{match effects}}. \tag{5}$$

with weights defined as

$$\omega_j = Pr[S_i = j \mid D_{i1} > D_{i0}], \text{ and}$$
$$\omega_{x|j} = Pr[X_i = x \mid D_{i1} > D_{i0}, S_i = j].$$

Proof. Z satisfies Assumption 2 thus β_{LATE} identifies the average treatment effect for compliers:

$$\begin{aligned}\beta_{LATE} &= E[Y_i(D_i=1) - Y_i(D_i=0)|D_{i1} > D_{i0}] \\ &= E[Y_i(D_i=1)|D_{i1} > D_{i0}] - E[Y_i(D_i=0)|D_{i1} > D_{i0}]\end{aligned}$$

Denoting by $\bar{Y}_i$ student $4i$'s average achievement across schools and using the framework notation:

$$\begin{aligned}E[Y_i(D_i=1)|D_{i1} > D_{i0}] &= E[Y_{i0} + \bar{Y}_i + E[\varepsilon_{i0}|X_i = x] \mid D_{i1} > D_{i0}, D_i = 1] \\ &= E[Y_{i0}|D_{i1} > D_{i0}] + E[\bar{Y}_i|D_{i1} > D_{i0}] + \sum_x \omega_{x|0} E[\varepsilon_{i0}|X_i = x, D_{i1} > D_{i0}] \\ E[Y_i(D_i=0)|D_{i1} > D_{i0}] &= \sum_{j=1}^{J} \omega_j E[Y_{ij} + \bar{Y}_i + E[\varepsilon_{ij}|X_i = x]|D_{i1} > D_{i0}, D_i = 0] \\ &= \sum_{j=1}^{J} \omega_j \{E[Y_{ij}|D_{i1} > D_{i0}] + E[\bar{Y}_i|D_{i1} > D_{i0}] \\ &\quad + \sum_x \omega_{x|j} E[\varepsilon_{ij}|X_i = x, D_{i1} > D_{i0}]\} \\ &= \sum_{j=1}^{J} \omega_j E[Y_{ij}|D_{i1} > D_{i0}] + E[\bar{Y}_i|D_{i1} > D_{i0}] \\ &\quad + \sum_x \sum_{j=1}^{J} \omega_j \omega_{x|j} E[\varepsilon_{ij}|X_i = x, D_{i1} > D_{i0}]\}\end{aligned}$$

Replacing and rearranging these expressions in the formula for β_{LATE}, one obtains the desired decomposition:

$$\beta_{LATE} = \underbrace{E[Y_{i0} - \sum_j \omega_j Y_{ij} \mid D_{i1} > D_{i0}]}_{\text{substitution effects}} + \underbrace{\sum_x E[\omega_{x|0}\varepsilon_{i0} - \sum_j \omega_j \omega_{x|j} \varepsilon_{ij} \mid X_i = x, D_{i1} > D_{i0}]}_{\text{match effects}}.$$

□

Corollary 1 points out that 2SLS estimators for different subgroups do not capture pure match effects if compliers with different covariate values substitute differently from non-exam school alternatives. It is thus necessary to compute match effects controlling for non-exam school alternatives. In other words, one need fist to decompose treatment effect by non-exam school alternatives before attempting to capture match effects.

Corollary 1 (Subgroup 2SLS identification)
Suppose there exists an instrument Z_i for D_i that satisfies Assumption 2. Using Z_i as an instrument on the subgroup of observations with $X_i = x$ identifies

$$\beta_{LATE|X_i=x} = \underbrace{E[Y_{i0} - \sum_j \omega_{j|x} Y_{ij} \mid D_{i1} > D_{i0}]}_{\textit{substitution effects}} + \underbrace{E[\varepsilon_{i0} - \sum_j \omega_{j|x} \varepsilon_{ij} \mid X_i = x, D_{i1} > D_{i0}]}_{\textit{match effects}}, \quad (6)$$

where

$$\omega_{j|x} = Pr[S_i = j \mid D_{i1} > D_{i0}, X_i = x].$$

Proof. This follows directly from Proposition 1 by replacing ω_j by $\omega_j|x$ and dropping the $\sum_x$ since X takes only one value in the subsample.

□

D Proofs

Proposition 1 (Identification of treatment effect by outside option)
Suppose there exists a valid instrument Z_i for enrollment in school 0*, and a vector of $J-1$ covariates $\{W_{ik}\}_{k=2}^J$ that satisfies Assumption 1.*

1. *Conditioning on $\{W_{ik}\}_{k=2}^J$ and Z_i, the interaction of Z_i and $(1, \{W_{ik}\}_{k=2}^J)$ identifies $E[Y_{i0} - Y_{ij} \mid D_{i1} > D_{i0}]$, $\forall j = 1, ..., J$.*

2. *For any covariate X_i, conditioning on $\{W_{ik}\}_{k=2}^J$ and X_i, the interaction of Z_i, $(1, \{W_{ik}\}_{k=2}^J)$ and X_i identifies $E[Y_{i0} - Y_{ij} \mid X_i, D_{i1} > D_{i0}], \forall j = 1, ..., J$.*

Proof. To identify the treatment effect of attending an exam school (indexed by 0) by non-exam school alternative (indexed by $j = 1, ..., J$), one would like to estimate

$$Y_i = \beta_0 + \beta S_{i1} + \sum_{k=2}^{J} \beta_j S_{ij} + \epsilon_i$$

The exam school is excluded from the estimation equation. It thus constitutes the school of reference to which school 1 and the other schools are compared, i.e. β corresponds to the treatment gain from attending school 1 instead of the exam school.

For simplicity, let's first consider a case with only two non-exam school alternatives. The reasoning is similar with more than two outside options but the number of terms is multiplied.

Define $Z_{i2} = W_{i2} \times Z_i \; \forall i$ and assume w.l.o.g. that W_2 is a continuous variable. Denote by $\tilde{Z}_2$, Z_2 partialled out from W_2. Note that since Z is randomly assigned by Assumption 1 $\tilde{Z}_i = Z_i \; \forall i$.

The first stage equations give

$$E[S_{i1}|Z_i, \tilde{Z}_{i2}] = \alpha_1 + \alpha_1^1 Z_i + \alpha_1^2 \tilde{Z}_{i2}$$
$$E[S_{i2}|Z_i, \tilde{Z}_{i2}] = \alpha_2 + \alpha_2^1 Z_i + \alpha_2^2 \tilde{Z}_{i2}$$

Plugging these into the second stage, we obtain the following reduced form

$$\begin{aligned} E[Y_i|Z_i, \tilde{Z}_{i2}] &= \alpha + \beta(\alpha_1 + \alpha_1^1 Z_i + \alpha_1^2 \tilde{Z}_{i2}) + \beta_2(\alpha_2 + \alpha_2^1 Z_i + \alpha_2^2 \tilde{Z}_{i2}) \\ &= \alpha + \beta\alpha_1 + \beta_2\alpha_2 + \underbrace{(\beta\alpha_1^1 + \beta_2\alpha_2^1)}_{\alpha_y^1} Z_i + \underbrace{(\beta\alpha_1^2 + \beta_2\alpha_2^2)}_{\alpha_y^2} \tilde{Z}_{i2} \end{aligned}$$

with $E[u_i|Z_i\tilde{Z}_{i2}] = 0$ by Assumption 1.

By definition, the 2sls estimator equals the reduced form estimates times the inverse of the first stage estimates:

$$\begin{pmatrix} \beta \\ \beta_2 \end{pmatrix} = \begin{pmatrix} \alpha_1^1 & \alpha_2^1 \\ \alpha_1^2 & \alpha_2^2 \end{pmatrix}^{-1} \begin{pmatrix} \alpha_y^1 \\ \alpha_y^2 \end{pmatrix}$$

Solving the system for β and β_2:

$$\beta = \frac{\alpha_2^2 \alpha_y^1 - \alpha_2^1 \alpha_y^2}{\alpha_1^1 \alpha_2^2 - \alpha_2^1 \alpha_1^2}$$
$$\beta_2 = \frac{\alpha_1^1 \alpha_y^2 - \alpha_1^2 \alpha_y^1}{\alpha_1^1 \alpha_2^2 - \alpha_2^1 \alpha_1^2}$$

Consider a local evaluation point w_2, using the fact that $D_i + S_{i1} + S_{i2} = 1$ and the monotonicity conditions from Assumptions 1 and 2:

$$\begin{aligned}
\alpha_y^1 &= E[Y_i|Z_i = 1, \tilde{Z}_{i2}] - E[Y_i|Z_i = 0, \tilde{Z}_{i2}] \\
&= E[Y_{i0} + (Y_{i1} - Y_{i0})S_{i1} + (Y_{i2} - Y_{i0})S_{i2}|Z = 1, \tilde{Z}_{i2}] \\
&- E[Y_{i0} + (Y_{i1} - Y_{i0})S_{i1} + (Y_{i2} - Y_{i0})S_{i2}|Z = 0, \tilde{Z}_{i2}] \\
&= E[Y_{i1} - Y_{i0}|0 \leftarrow 1](E[S_{i1}|Z_i = 1, \tilde{Z}_{i2}] - E[S_{i1}|Z_i = 0, \tilde{Z}_{i2}]) \\
&+ E[Y_{i2} - Y_{i0}|0 \leftarrow 2](E[S_{i2}|Z_i = 1, \tilde{Z}_{i2}] - E[S_{i2}|Z_i = 0, \tilde{Z}_{i2}]) \\
&= E[Y_{i1} - Y_{i0}|0 \leftarrow 1]\alpha_1^1 + E[Y_{i2} - Y_{i0}|0 \leftarrow 2]\alpha_2^1
\end{aligned}$$

where $0 \leftarrow 1$ denotes instrument Z compliers moving from treatment 1 to treatment 0 at point (w_2), i.e. from school 1 to the exam school at point (w_2). Similarly, where $0 \leftarrow 2$ denotes instrument Z compliers moving from school 2 to the exam school at point (w_2).

One can derive a similar expression for α_y^2:

$$\begin{aligned}\alpha_y^2 &= \frac{\partial E[Y_i|Z_i, \tilde{Z}_{i2}]}{\partial \tilde{Z}_{i2}} \\ &= E[Y_{i1} - Y_{i0}|0 \leftarrow 1(w_2')]\alpha_1^2 + E[Y_{i2} - Y_{i0}|0 \leftarrow 2(w_2')]\alpha_2^2\end{aligned}$$

where $0 \leftarrow 2(w_2')$ denotes instrument Z compliers moving from school 2 to the exam school at point $w_2' \downarrow w_2$ but not at point w_2, i.e.

$$E[Y_{i2} - Y_{i0}|0 \leftarrow 2(w_2')] = \lim_{w_2' \downarrow w_2} E[Y_{i2} - Y_{i0}|S_{2i}(Z_i = 1, W_{2i} = w_2') = 0, S_{2i}(Z_i = 1, W_{2i} = w_2) = 1]$$

These expressions indicate that the multi-treatment estimate aggregates treatment effect from different groups of compliers. Nonetheless, the constant treatment effect within covariate condition from Assumption 2 implies that for any (w_2, w_2')

$$\begin{aligned}E[Y_{i1} - Y_{i0}|0 \leftarrow 1(w_2)] &= E[Y_{i1} - Y_{i0}|0 \leftarrow 1(w_2')] \\ E[Y_{i2} - Y_{i0}|0 \leftarrow 2(w_2)] &= E[Y_{i2} - Y_{i0}|0 \leftarrow 2(w_2')]\end{aligned}$$

Thus, plugging in α_y^0 and α_y^2 on the expressions for β :

$$\begin{aligned}\beta &= \frac{\alpha_2^2\alpha_1^1 E[Y_{i1} - Y_{i0}|0 \leftarrow 1] + \alpha_2^2\alpha_2^1 E[Y_{i2} - Y_{i0}|0 \leftarrow 2]}{\alpha_1^1\alpha_2^2 - \alpha_2^1\alpha_1^2} \\ &- \frac{\alpha_2^1\alpha_1^2 E[Y_{i1} - Y_{i0}|0 \leftarrow 1] + \alpha_2^1\alpha_2^2 E[Y_{i2} - Y_{i0}|0 \leftarrow 2]}{\alpha_1^1\alpha_2^2 - \alpha_2^1\alpha_1^2} \\ \beta &= \frac{(\alpha_2^2\alpha_1^1 - \alpha_2^1\alpha_1^2)E[Y_{i1} - Y_{i0}|0 \leftarrow 1]}{\alpha_1^1\alpha_2^2 - \alpha_2^1\alpha_1^2} \\ \beta &= E[Y_{i1} - Y_{i0}|0 \leftarrow 1]\end{aligned}$$

and similarly for β_2

$$
\begin{aligned}
\beta_2 &= -\frac{\alpha_1^2\alpha_1^1 E[Y_{i1} - Y_{i0}|0 \leftarrow 1] + \alpha_1^2\alpha_2^1 E[Y_{i2} - Y_{i0}|0 \leftarrow 2]}{\alpha_1^1\alpha_2^2 - \alpha_2^1\alpha_1^2} \\
&\quad + \frac{\alpha_1^1\alpha_1^2 E[Y_{i1} - Y_{i0}|0 \leftarrow 1] + \alpha_1^1\alpha_2^2 E[Y_{i2} - Y_{i0}|0 \leftarrow 2]}{\alpha_1^1\alpha_2^2 - \alpha_2^1\alpha_1^2} \\
\beta_2 &= \frac{(\alpha_1^1\alpha_2^2 - \alpha_2^1\alpha_1^2) E[Y_{i2} - Y_{i0}|0 \leftarrow 2]}{\alpha_1^1\alpha_2^2 - \alpha_1^2\alpha_2^1} \\
\beta_2 &= E[Y_{i2} - Y_{i0}|0 \leftarrow 2]
\end{aligned}
$$

$\square$

E Additional Tables

Table A1: Characteristics of BPS Enrolled Students and Exam School Applicants

	Enrolled students				Exam school applicants		
		Exam schools				With any-exam risk	With school-specific risk
	BPS	BLS	BLA	OBR	All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
% Female	0.49	0.55	0.55	0.54	0.52	0.54	0.54
% Hispanic	0.32	0.11	0.20	0.28	0.24	0.26	0.23
% Black	0.34	0.14	0.24	0.30	0.28	0.30	0.27
% White	0.10	0.36	0.22	0.13	0.19	0.14	0.18
% Asian	0.16	0.34	0.28	0.24	0.23	0.23	0.25
% FRPL eligible	0.82	0.42	0.66	0.79	0.70	0.76	0.70
% Native speaker	0.59	0.64	0.55	0.52	0.58	0.55	0.56
% SPED	0.24	0.01	0.02	0.02	0.06	0.04	0.03
% English learners	0.14	0.00	0.01	0.03	0.06	0.04	0.03
MCAS English 4^{th}	0.00	1.67	1.05	0.79	0.70	0.63	0.84
MCAS English 6^{th}	0.07	1.57	1.08	0.86	0.72	0.72	0.90
MCAS Math 4^{th}	0.02	1.75	1.09	0.88	0.72	0.66	0.88
MCAS Math 6^{th}	0.08	1.79	1.30	1.06	0.82	0.83	1.03
N students	35,979	2,754	2,259	1,288	13,937	6,907	9,297

Notes: This table reports descriptive statistics for students enrolled in BPS schools and exam school applicants. Statistics are computed on the sample of students enrolled in a BPS school in 6^{th} grade between 2003-04 and 2015-16. Columns 1-4 report statistics for the sample of 7^{th} grade students with demographic information who enrolled in any BPS school and each of the three exam schools respectively. Columns 5-7 report statistics for the sample of exam school applicants with demographic information. Column 6 restricts the sample to applicants with non-degenerate risk of being offered a seat at any of the three exam schools. Column 7 restricts the sample to applicants with non-degenerate assignment risk for at least one of the three exam schools, including applicants who are offered an exam seat with probability one. Column 6 sample is a subsample of Column 7 sample. FRPL eligible students are students eligible for free or reduced-price lunch. SPED students are special education students.

Table A2: 2SLS Estimates of Exam School Effects by Non-Exam Alternative for Different Group Splitting

	MCAS 7th grade			MCAS 8th grade		
	3 groups	4 groups	5 groups	3 groups	4 groups	5 groups
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Second stage for English MCAS						
Enrollment in group 1 school	0.015	-0.026	-0.043	-0.003	-0.026	-0.022
	(0.059)	(0.065)	(0.070)	(0.061)	(0.065)	(0.074)
Enrollment in group 2 school	0.065	0.031	0.046	0.046	0.031	0.038
	(0.058)	(0.066)	(0.074)	(0.054)	(0.066)	(0.069)
Enrollment in group 3 school	0.259	0.196	0.068	0.143	0.096	0.075
	(0.059)	(0.062)	(0.066)	(0.061)	(0.078)	(0.063)
Enrollment in group 4 school		0.276	0.243		0.132	0.093
		(0.063)	(0.065)		(0.090)	(0.065)
Enrollment in group 5 school			0.204			0.088
			(0.075)			(0.074)
over-id p-value	0.335	0.575	0.330	0.987	0.840	0.660
N	5,953	5,953	5,953	5,978	5,978	5,978
Panel B: Second stage for Math MCAS						
Enrollment in group 1 school	-0.071	-0.046	-0.075	-0.191	-0.175	-0.181
	(0.078)	(0.085)	(0.096)	(0.084)	(0.091)	(0.102)
Enrollment in group 2 school	0.010	-0.140	-0.143	-0.042	-0.131	-0.212
	(0.070)	(0.081)	(0.091)	(0.077)	(0.087)	(0.090)
Enrollment in group 3 school	0.281	0.126	0.055	0.182	0.038	0.042
	(0.082)	(0.086)	(0.080)	(0.084)	(0.082)	(0.090)
Enrollment in group 4 school		0.326	0.181		0.205	0.106
		(0.090)	(0.088)		(0.097)	(0.097)
Enrollment in group 5 school			0.313			0.139
			(0.107)			(0.101)
over-id p-value	0.067	0.062	0.132	0.742	0.309	0.605
N	5,429	5,429	5,429	5,978	5,978	5,978

Notes: This table reports 2SLS estimates by non-exam school alternative for different group splitting of schools. The 2SLS estimates for MCAS English are reported in Panel A; the 2SLS estimates for MCAS math are displayed in Panel B. All models control for any-exam offer risk, school-by-year linear running variables and the set of variables listed in Table 1, except MCAS 6^{th} grade scores. Standard errors clustered at the school of origin and application year are reported in parentheses.

Table A3: Differential Attrition by Distance to Non-Exam Alternatives

	MCAS English		MCAS Math	
	7^{th} grade	8^{th} grade	7^{th} grade	8^{th} grade
	(1)	(2)	(3)	(4)
Exam offer	0.023	0.038	0.010	0.038
	(0.033)	(0.035)	(0.032)	(0.035)
Exam offer X distance to quartile 1	0.005	0.004	0.005	0.004
	(0.005)	(0.005)	(0.005)	(0.005)
Exam offer X distance to quartile 2	0.005	0.004	0.005	0.004
	(0.004)	(0.004)	(0.004)	(0.004)
Exam offer X distance to quartile 3	0.006	0.006	0.006	0.006
	(0.005)	(0.005)	(0.004)	(0.005)
Exam offer X distance to quartile 4	0.016	0.012	0.015	0.012
	(0.004)	(0.004)	(0.004)	(0.004)
N	6,907	6,907	6,907	6,907

Notes: This table reports the effects of receiving an offer by distance to each group's closest school on follow-up data availability for 2004-16 applicants. The model controls for any-exam offer risk, school-by-year linear running variables and the set of variables listed in Table 1, except MCAS 6^{th} grade scores. Standard errors clustered at the school of origin and application year are reported in parentheses.

Table A4: OLS Estimates of Exam School Effects by Non-Exam Alternative

	MCAS English		MCAS Math	
	7th grade	8th grade	7th grade	8th grade
	(1)	(2)	(3)	(4)
Panel A: OLS for any exam school enrollment				
Enrollment in quartile 1 school	-0.270	-0.198	-0.406	-0.363
	(0.036)	(0.032)	(0.037)	(0.047)
Enrollment in quartile 2 school	-0.218	-0.137	-0.369	-0.162
	(0.032)	(0.035)	(0.041)	(0.048)
Enrollment in quartile 3 school	-0.196	-0.097	-0.227	-0.136
	(0.029)	(0.030)	(0.038)	(0.039)
Enrollment in quartile 4 school	0.004	-0.022	-0.101	0.039
	(0.029)	(0.032)	(0.058)	(0.052)
N	5,953	5,978	5,429	5,978
Panel B: OLS for school specific enrollment				
Enrollment at BLS	0.203	0.160	0.290	0.085
	(0.023)	(0.023)	(0.031)	(0.027)
Enrollment at BLA	0.229	0.063	0.040	-0.210
	(0.023)	(0.024)	(0.027)	(0.026)
Enrollment in quartile 1 school	-0.164	-0.173	-0.407	-0.493
	(0.037)	(0.035)	(0.039)	(0.047)
Enrollment in quartile 2 school	-0.114	-0.103	-0.361	-0.296
	(0.032)	(0.036)	(0.043)	(0.047)
Enrollment in quartile 3 school	-0.102	-0.087	-0.244	-0.291
	(0.031)	(0.031)	(0.040)	(0.040)
Enrollment in quartile 4 school	0.110	0.009	-0.101	-0.107
	(0.032)	(0.033)	(0.058)	(0.051)
N	8,130	8,146	7,495	8,146

This table reports OLS estimates of the effect of enrollment at different schools for 2004-16 exam school applicants. The OLS estimates for enrollment in any of the exam school are reported in Panel A; the school-specific OLS estimates are displayed in Panel B. All models control for the set of variables listed in Table 1, except MCAS 6th grade scores. Standard errors clustered at the school of origin and application year are reported in parentheses.

Table A5: Exam School Effects for Applicants Below and Above the Median Baseline Math Score

	MCAS English		MCAS Math	
	7^{th} grade	8^{th} grade	7^{th} grade	8^{th} grade
	(1)	(2)	(3)	(4)
Panel A: Second stage for any exam school enrollment				
Enrollment exam school	-0.086	-0.027	-0.018	0.056
	(0.052)	(0.051)	(0.068)	(0.072)
Enrollment exam school X above median	-0.045	-0.082	-0.107	-0.077
	(0.035)	(0.035)	(0.047)	(0.046)
First Stage F-statistic	1,402.9	1,411.9	1,300.3	1,411.9
Panel B: Second stage for exam school enrollment by non-exam school alternative				
Enrollment in quartile 1	-0.057	-0.063	-0.115	-0.187
	(0.071)	(0.073)	(0.091)	(0.096)
Enrollment in quartile 1 X above median	0.087	0.091	0.155	-0.018
	(0.075)	(0.085)	(0.105)	(0.105)
Enrollment in quartile 2	-0.055	0.009	-0.168	-0.152
	(0.079)	(0.068)	(0.092)	(0.093)
Enrollment in quartile 2 X above median	0.139	0.121	0.127	0.043
	(0.105)	(0.084)	(0.097)	(0.103)
Enrollment in quartile 3	0.191	0.056	0.096	0.010
	(0.070)	(0.062)	(0.090)	(0.101)
Enrollment in quartile 3 X above median	0.009	0.100	0.055	0.062
	(0.064)	(0.067)	(0.085)	(0.095)
Enrollment in quartile 4	0.256	0.093	0.288	0.117
	(0.074)	(0.071)	(0.098)	(0.094)
Enrollment in quartile 4 X above median	-0.012	0.048	0.158	0.342
	(0.091)	(0.106)	(0.145)	(0.133)
First Stage F-statistic	167.1	166.6	158.3	166.6
over-id p-value	0.829	0.447	0.098	0.261
N	5,953	5,978	5,429	5,978

This table reports 2SLS estimates of the effect of enrollment at different schools for above and below median applicants. The sample is restricted to 2004-2016 applicants with non-degenerate any-exam offer risk. Above median applicants correspond to applicants with 4^{th} grade Math MCAS test score above the median score of students with non-degenerate any-exam school risk. Instruments and distance controls are interacted with above the median status. The set of instruments and controls is otherwise as described in Tables 2 and 5. Standard errors clustered at the school of origin and application year are reported in parentheses.

Table A6: Exam School Effects for Minority and Non-Minority Applicants

	MCAS English		MCAS Math	
	7^{th} grade	8^{th} grade	7^{th} grade	8^{th} grade
	(1)	(2)	(3)	(4)
Panel A: Second stage for any exam school enrollment				
Enrollment in exam school	-0.065	-0.016	-0.022	0.036
	(0.055)	(0.057)	(0.071)	(0.080)
Enrollment in exam school X minority	-0.070	-0.075	-0.063	-0.018
	(0.043)	(0.039)	(0.050)	(0.061)
First Stage F-statistic	1,194.0	1,192.9	1,046.4	1,192.9
Panel B: Second stage for exam school enrollment by non-exam school alternative				
Enrollment in quartile 1	-0.181	-0.074	-0.197	-0.241
	(0.075)	(0.083)	(0.084)	(0.111)
Enrollment in quartile 1 X minority	0.276	0.067	0.254	0.114
	(0.079)	(0.080)	(0.096)	(0.116)
Enrollment in quartile 2	0.013	-0.012	-0.041	-0.077
	(0.095)	(0.091)	(0.110)	(0.116)
Enrollment in quartile 2 X minority	-0.082	0.058	-0.176	-0.104
	(0.087)	(0.091)	(0.117)	(0.121)
Enrollment in quartile 3	0.193	0.035	0.036	-0.029
	(0.082)	(0.069)	(0.094)	(0.100)
Enrollment in quartile 3 X minority	0.002	0.118	0.131	0.114
	(0.078)	(0.065)	(0.082)	(0.097)
Enrollment in quartile 4	0.277	0.111	0.385	0.270
	(0.080)	(0.082)	(0.107)	(0.110)
Enrollment in quartile 4 X minority	0.029	0.038	-0.073	-0.121
	(0.094)	(0.098)	(0.115)	(0.120)
First Stage F-statistic	130.9	129.1	131.2	129.1
over-id p-value	0.479	0.049	0.063	0.579
N	5,953	5,978	5,429	5,978

This table reports 2SLS estimates of the effect of enrollment at different schools for minority and non-minority applicants. The sample is restricted to 2004-2016 applicants with non degenerate any-exam offer risk. Minority applicants refer to applicants who are either Black or Hispanic. Instruments and distance controls are interacted with minority status. The set of instruments and controls is otherwise as described in Tables 2 and 5. Standard errors clustered at the school of origin and application year are reported in parentheses.

Table A7: Heterogeneity in Each Exam School Effects by Non-exam School Alternative

	MCAS English		MCAS Math	
	7th grade	8th grade	7th grade	8th grade
	(1)	(2)	(3)	(4)
Panel A: Second stage for applicants below and above the median baseline math score				
Enrollment in BLS	-0.095	-0.020	-0.135	-0.304
	(0.067)	(0.072)	(0.079)	(0.078)
Enrollment in BLS X above median	0.060	0.074	0.011	0.010
	(0.045)	(0.051)	(0.054)	(0.051)
Enrollment in BLA	0.100	-0.080	-0.135	-0.370
	(0.054)	(0.052)	(0.065)	(0.065)
Enrollment in BLA X above median	0.074	0.076	0.019	0.043
	(0.043)	(0.040)	(0.053)	(0.052)
Enrollment in Non-Exam	0.048	0.027	0.018	-0.042
	(0.053)	(0.051)	(0.069)	(0.072)
Enrollment in Non-Exam X above median	0.100	0.124	0.124	0.129
	(0.047)	(0.044)	(0.061)	(0.057)
Panel B: second stage for minority and non-minority applicants				
Enrollment in BLS	-0.076	-0.011	-0.138	-0.303
	(0.065)	(0.066)	(0.077)	(0.075)
Enrollment in BLS X minority	0.021	0.040	0.035	0.011
	(0.041)	(0.044)	(0.047)	(0.048)
Enrollment in BLA	0.132	-0.082	-0.106	-0.323
	(0.055)	(0.052)	(0.064)	(0.064)
Enrollment in BLA X minority	0.009	0.072	-0.042	-0.051
	(0.041)	(0.043)	(0.046)	(0.050)
Enrollment in Non-Exam	0.052	0.014	0.034	-0.004
	(0.055)	(0.058)	(0.073)	(0.082)
Enrollment in Non-Exam X minority	0.064	0.109	0.056	0.023
	(0.047)	(0.047)	(0.057)	(0.069)
N	8,129	8,144	7,493	8,144

Notes: This table reports 2SLS estimates of the effect enrollment at different schools for minority and non-minority applicants and below and above the median math baseline Math score applicants. The sample is restricted to 2004-2016 applicants with non-degenerate offer risk at one of the exam schools. Minority applicants refer to applicants who are either Black or Hispanic. Above median applicants correspond to applicants with 4th grade Math MCAS test score above the median score of students within any bandwidth. Instruments and distance controls are interacted with minority and above the median status respectively. The set of instruments and controls is otherwise as described in Tables 2 and 5. Standard errors clustered at the school of origin and application year are reported in parentheses.