STILL WORTH THE TRIP?

SCHOOL BUSING EFFECTS IN BOSTON AND NEW YORK*

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Abstract

School assignment in Boston and New York City came to national attention in the 1970s as courts across the country tried to integrate schools. Today, district-wide choice allows Boston and New York students to enroll far from home. Although 1970s desegregation efforts likely benefited minority students, urban school transportation is increasingly costly and may not generate the gains in learning and educational attainment seen decades ago. We estimate contemporary causal effects of non-neighborhood school attendance and school travel on racial integration, achievement, and college enrollment using an identification strategy that exploits partly-random assignment generated by the Boston and New York school matching algorithms. Instrumental variables estimates suggest distance and travel boost integration for those who choose to travel but have little or no effect on test scores. Travel reduces post-secondary attainment and on-time high school graduation in New York. IV estimates show no human capital gains from travel even for students who indicate a strong preference for non-neighborhood schools. These findings are explained in part by the fact that the schools travelers travel to differ little in value-added terms from schools nearby. Negative effects on college enrollment in New York appear to arise from travel itself rather than diminished college value-added. *JEL Codes: I20, C26, I28, C21, C52.* Word Count: 9770.

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I Introduction

> Bus transportation has been an integral part of the public education system for years, and was perhaps the single most important factor in the transition from the one-room schoolhouse to the consolidated school ... we find no basis for holding that the local school authorities may not be required to employ bus transportation as one tool of school desegregation. Desegregation cannot be limited to the walk-in school.
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> *Swann v. Charlotte-Mecklenburg Board of Education, 402 U.S. 1, 1971*

The question of who goes to school where is as contentious today as it was in 1954, when the Supreme Court set out to integrate American schools. Neighborhood-based school assignment, once common in cities and still the norm in suburban school districts, necessarily reflect patterns of residential segregation. Non-neighborhood assignment schemes may mitigate the consequences of residential segregation, perhaps facilitating an equitable allocation of high-quality seats. But travel to non-neighborhood schools is costly. This is documented in Figure I, which plots average annual per-pupil transportation expenditure in the 100 largest US school districts (by enrollment) for 1991-2020. The Boston and New York City school districts are among the highest transportation spenders, with recent annual costs of $1,500-$2,200 per student (in 2017 dollars). Boston and New York transportation spending is also growing: the cost of getting kids to school in these bellwether cities roughly doubled in the quarter century covered by the figure.

[insert figure I here]

High transportation costs reflect the fact that many large urban districts allow families to choose schools district-wide, lengthening school commutes for many. District-wide choice is a feature of school assignment in Boston, Chicago, Denver, Indianapolis, Newark, New Orleans, Tulsa, and Washington, DC, to name a few. In choice districts, seats at over-subscribed schools are typically allocated by algorithms that reflect family preferences and a limited set of school priorities. In the 1970s and 1980s, by contrast, non-neighborhood schooling in urban districts arose largely through court action (or the threat of court action) meant to integrate segregated schools. Today's voluntary choice schemes evolved as courts withdrew from the school assignment arena. District-wide choice is meant to afford all students a shot at more integrated and higher-quality schools, without imposing an allocation of seats.[1]

This paper asks whether school travel in the modern choice paradigm boosts integration and educational outcomes as hoped, especially for minority students. Our investigation focuses on Boston and New York, cities of special interest because of their high transportation costs and because these cities' schools are among the most segregated in the country. The latter fact is documented in Figure II, Panel (a) of which plots minority exposure for minority students enrolled in the 100 largest districts. Minority exposure, defined as the proportion of a student's schoolmates who are Black or Hispanic, is plotted separately for Black and Hispanic students. Figure II shows minority exposure to be especially high for both minority groups in Boston and New York. Moreover, as can be seen in Panel (b) of the figure, Boston and New York minority exposure is among the highest relative to cities with comparable levels of overall minority enrollment.

[insert figure II here]

Our analysis estimates causal effects of non-neighborhood school attendance and school travel time on integration and education outcomes in the population of public school students for whom travel is facilitated by district-wide school choice. As a descriptive matter, Boston and New York students who opt for non-neighborhood schools have higher test scores and are more likely to go to college than those who travel less. These differences, which come from ordinary least squares (OLS) estimates with a few demographic controls, may reflect selection bias as much or more than causal effects. Specifically, students from more motivated or better-off families may be more likely to travel.

The problem of selection bias is solved here using the conditional random assignment generated by school matching algorithms. Students who list both neighborhood and non-neighborhood schooling options, as many do, may be seated at a nearby school or at a school farther away. Conditional on an applicant's preferences and school priorities, modern choice algorithms randomize seat assignment. This conditional random assignment manipulates distance and travel independently of potential outcomes. Our instrumental variables (IV) estimation strategy exploits conditional random assignment using an econometric framework that builds on the propensity-score-based methods developed in Abdulkadiroğlu et al. (2017) and Abdulkadiroğlu et al. (2022). In a method-

[1] An extensive literature examines the design and impact of modern school choice systems. Empirical analyses include Chubb and Moe (1990), Hoxby (2003), and Kahlenberg (2003); theoretical models of school choice are developed in Avery and Pathak (2021), Barseghyan, Clark and Coate (2019), and Grigoryan (2021).

ological contribution, these methods are extended here to IV models where the instrument depends on a multinomial propensity score (similar to Imbens (2000) and Hirano and Imbens (2004)) and on a vector of observed covariates. In addition to allowing for an ordered treatment, this extension addresses the fact that non-neighborhood school assignment depends on an applicants' residential address as well as on their assigned school.

IV estimates using conditionally randomized school offers as instruments for school travel show that minority applicants who travel farther indeed enroll in schools with fewer minority peers as a result. Over a third of students in the samples analyzed here attend schools where the student body is over 90% Black or Hispanic peers, a segregation measure termed *minority isolation*.[2] Non-neighborhood school enrollment reduces minority isolation markedly. For Black Boston students, for instance, non-neighborhood attendance reduces the probability of attending a minority isolated school by 22 percentage points.

The integrating effects of non-neighborhood enrollment notwithstanding, travel to more distant schools does not appear to increase student achievement, high school graduation rates, or post-secondary attainment. IV estimates of non-neighborhood and travel effects on achievement are close to zero and estimated precisely enough to rule out modest positive effects, while IV estimates of effects on high school graduation and college attendance by minority students are mostly negative. Although race-specific estimates of effects on graduation and college enrollment for Boston students are noisy, pooled Boston graduation estimates are negative and marginally significantly different from zero. The corresponding estimates for Hispanic New Yorkers show statistically significant reductions in on-time graduation on the order of 2-3 percentage points. Estimated college enrollment reductions for all New York applicants, also statistically significant, are roughly 2 points. As with high school graduation effects, estimated college enrollment reductions are largest for Hispanic New Yorkers. This constellation of findings is reproduced in IV estimates that allow for differential impacts according to preferences for distance and travel.

[2]Such measures have a long history in public discussion of segregated schools. The Morgan v. Hennigan 379 F. Supp. 410 (D. Mass. 1974) decision, for example, discusses "racially identifiable schools," noting that 84% of white students attended schools that were more than 80% white, while 62% of Black students attended schools that were more than 70% Black. Cohen (2021) likewise defines intensely segregated schools to be those with 90% students of color, defined as all nonwhites. Similarly, Potter (2022) defines segregated schools as those where 90% of students are of the same race. Descriptive evidence suggesting racial isolation is harmful to minorities has motivated integration policy since at least United States Commission on Civil Rights (1967), a companion to the influential Coleman Report (Coleman, 1966).

Because busing is motivated by the belief that more distant schooling options are better for minority applicants, school quality is a natural mediator of school distance and travel effects. To make this mediation hypothesis concrete, we gauge school quality with value-added, a measure meant to approximate the causal effect of attending a particular school. We then ask whether negative effects of travel on graduation and college attendance can be explained by negative effects on value-added. This analysis shows that travelers attend schools with value-added similar to that of the neighborhood schools they would have attended had they not traveled. Moreover, an econometric framework allowing for separate effects of travel and value-added, instrumented by offered travel and offered value-added, suggests travel has direct negative effects. The upshot, therefore, is that, while travel boosts integration, the schools travelers travel to are no better than those nearby. At the same time, travel comes at the cost of reduced graduation and college enrollment rates for many students, perhaps because important school resources are harder to access when schools are farther from home.

Our study builds on a wide range of previous work. Recently, Cordes, Rick and Schwartz (2022) concludes that long bus rides leave test scores unchanged while reducing attendance and increasing chronic absenteeism among New York elementary school students (this study uses idiosyncratic variation in bus routing to identify causal effects). Chingos and Monarrez (2020) surveys mostly descriptive research on the link between school choice and segregation, while Monarrez (2020) considers the extent to which race determines school attendance boundaries. Our work likewise connects with extensive academic research considering the effects of school choice on students, including Hastings and Weinstein (2008), Deming (2011), Deming et al. (2014), and Campos and Kearns (2024). Other related research examines the consequences of attendance at various types of schools or sectors, such as charter and pilot schools, exam schools, magnet schools, and schools with high value added.[3] These investigations largely ignore questions related to distance and travel.

We also build on research that considers integration effects directly, including Welch and Light (1987), Hoxby (2000*b*), Rossell and Armor (1996), Rivkin and Welch (2006), and Hanushek, Kain and Rivkin (2009). The end of *de jure* segregation appears to have yielded important economic gains

[3] A non-exhaustive list of relevant studies includes Cullen, Jacob and Levitt (2006); Abdulkadiroğlu et al. (2011); Abdulkadiroğlu et al. (2017); Angrist et al. (2016); Lucas and Mbiti (2014); Ajayi (2014); Hoxby, Murarka and Kang (2009); Dobbie and Fryer (2011, 2014); Abdulkadiroğlu et al. (2016). Chubb and Moe (1990) suggests that choice engenders competition that may promote quality; research exploring these considerations includes Hoxby (2000*a*), Hastings, Kane and Staiger (2009), and Campos and Kearns (2024).

for Blacks (e.g., Smith and Welch (1989) and Card and Krueger (1992)). Guryan (2004), Johnson (2019), and Anstreicher, Fletcher and Thompson (2022) likewise report estimates showing early integration-induced education gains for Black students outside the South. But initial integration efforts typically coincided with major changes in education inputs, especially school spending. Evidence on integration effects from more recent periods is more mixed (see, e.g., Hoxby (2000*b*) and Card and Rothstein (2007)). These divergent findings may reflect the fact that average per-pupil spending today often increases with higher minority attendance (reflecting, for instance, extra allocations for special needs and limited English proficiency students). It's noteworthy, therefore, that our econometric framework uses school assignment lotteries to isolate distance and travel effects while implicitly holding district-level variables related to school spending fixed.

The next section sketches the history of desegregation efforts and school assignment schemes in Boston and New York. Section III describes our data and presents descriptive statistics. Section IV reports OLS estimates and details the econometric framework used to estimate causal effects of school distance and travel. Section V reports the IV estimates this framework generates. Following a discussion of effects on achievement and college attendance, this section presents a mediation analysis based on school value-added. The paper concludes with a simulation characterizing the integration consequences of a neighborhood-focused cost-saving centralized assignment plan. This simulation highlights the trade-offs between lower transportation spending and reduced integration. At the same time, while integration may be of intrinsic value, our estimates suggest that in urban districts today, a return to neighborhood schools–such as is now being discussed in some large urban districts–is unlikely to reduce human capital and may even generate some gains.

II Background

A A Tale of Two Cities: Court-Ordered and Voluntary Integration

A seemingly quotidian matter, transportation policy in many school districts is the legacy of decades of racial strife. The debate over busing in the Boston Public Schools (BPS) came to national attention in April 1976, when the front page of the Boston Herald American featured a photo captioned "The Soiling of Old Glory" (Masur, 2008). Snapped on Boston's City Hall Plaza, this picture showed an angry white teen using the American flag to attack African American attorney

Ted Landsmark. The attacker was a participant in an unruly and sometimes violent anti-school-busing protest, while the victim was a bystander destined to play an important role in Boston school policy debates.

Massachusetts' Racial Imbalance Act of 1965 laid the legal groundwork for school busing in Boston. The Act defined racial imbalance in statistical terms and required that schools deemed racially imbalanced desegregate or lose state funding. This legislation notwithstanding, until 1974, Boston students attended schools in catchment areas designed to segregate by race. The elected Boston School Committee of the 1960s failed to cooperate with state efforts to desegregate schools. School committee defiance ultimately led to a 1974 Federal District Court ruling imposing the state's busing plan on the city. United States District Judge Arthur Garrity, the presiding judge in the case, effectively managed Boston school assignment until 1983, with the state taking over through 1988. Garrity oversaw a mandatory busing plan that divided Boston into 867 residential geocodes (shown in Figure A1). Each geocode was paired with a particular school in an effort to engineer racially-balanced enrollment. Only in 1989 did responsibility for school assignment revert to the district.

Boston's "controlled choice" assignment plan of the early 1990s, described in Willie and Alves (1996) and Willie, Edwards and Alves (2002), initially targeted racial balance. In 1997, however, the Boston desegregation case was officially closed. Two years later, the Boston School Committee voted to eliminate the use of race and ethnicity for purposes of school assignment. Since the 2000-2001 school year, Boston school assignment has ignored race. From 1999 to 2004, the nascent Boston school match used the widely-criticized immediate acceptance algorithm (Abdulkadiroğlu and Sönmez, 2003; Pathak and Sönmez, 2008). The Boston school match has since employed the student-proposing deferred acceptance algorithm (DA) to assign seats at public schools other than charter and exam schools. DA in Boston uses a random lottery number to distinguish otherwise identical applicants. The Boston match relies on choice rather than court-ordered busing to facilitate school access across neighborhoods.[4]

[insert figure III here]

Following the end of court-ordered busing in Boston, some measures of segregation of Black and

[4] The match includes traditional and pilot schools. Boston pilot schools, run by the district, are meant to be a model halfway between the broad autonomy of state-authorized charter schools and traditional public schools. Abdulkadiroğlu et al. (2011) estimates charter and pilot school effects on test scores.

Hispanic students initially increased. Figure III details the evolution of school-level racial exposure from 1988 to 2022, plotting the proportion of a student's schoolmates who are Black or Hispanic, as well as the proportion attending racially isolated schools, defined here as schools that are at least 80% or at least 90% Black or Hispanic. Panel (a) of the figure shows that, in 1988, fewer than 15 percent of Boston's Black students were enrolled in the most racially isolated schools. In 2003, minority isolation peaked at around 50 percent.

Among Black students in Boston, exposure to other Black students has fallen since 2003, while exposure to Hispanics has increased. The combination of falling Black exposure and rising Hispanic exposure has generated relatively stable combined minority exposure over time. A measure of minority isolation based on an 80% threshold likewise stabilized around 2003. As can be seen on the right side of Panel (a) in the figure, the evolution of Hispanic exposure to minority peers in Boston mostly mirrors that seen for Blacks. But the higher level and more steeply-sloping increase in Boston Hispanic students' peer share Hispanic is a noteworthy difference.[5]

Desegregation efforts in New York have been voluntary rather than a consequence of court action. In the 1950s and 1960s, New York City school assignment was mostly neighborhood-based. Unsurprisingly, segregated neighborhoods led to similarly segregated neighborhood schools. In the 1960s, critics of the city's *de facto* segregation argued that schools attended by Black children were overcrowded, run-down, and staffed by inexperienced teachers. Attempts both to mandate (and to proscribe) cross-neighborhood busing nevertheless foundered (Delmont, 2016). Dissatisfaction with educational opportunities for New York's minority children came to a head in February 1964, with a boycott in which nearly half a million mostly nonwhite students stayed home, one of the largest protests in US history. The anti-segregation boycott was followed that year by a white-led counter-boycott. In 2004, decentralized community control of schools gave way to city-wide administration through the NYC Department of Education (Abdulkadiroğlu et al., 2005; Ravitch, 2011). Since then, New Yorkers have debated the role of neighborhoods and geography in the city's assignment system. A 2021 reform proposal, for instance, aimed to remove neighborhood-determined priorities from the centralized match (Veiga, 2021).

Contemporary discussions of New York school segregation often focus on the fact that white

[5]Data for Figures II and III (further detailed in Online Appendix B) are from the Common Core survey, documented in `https://nces.ed.gov/ccd/pubschuniv.asp`. Caetano and Maheshri (2023) notes the growing importance of Hispanic enrollment for segregation trends nationwide.

and upper-income families have many options that effectively bypass mostly-minority traditional public schools.[6] Alternatives include private schools, screened public schools that select applicants according to a variety of criteria, and highly coveted seats at the city's exam or specialized high schools, including the renowned Stuyvesant, Brooklyn Tech, and Bronx Science. New York's many other selective "screened schools" came to prominence in the 1970s, when the city expanded the use of selective admissions in the hope of encouraging mostly white and Asian middle-class families to remain in city schools.

Measures of segregation in New York public schools have declined since the late 1990s, falling from levels much above those initially seen in Boston. New York segregation trends are documented in Panel (b) of Figure III. In 1988, over 70% of Black New York students attended the most racially isolated schools (again, defined as those with over 90% minority enrollment). By 2022, this proportion had fallen below 50%. Trends in minority isolation based on an 80% cutoff, as well as overall minority exposure, slope more gently downward over this period than does the trend in isolation based on a 90% cutoff. Still, minority isolation and minority exposure in New York show a marked drop over the three decades spanned by Figure III. Like Boston, New York has seen steady growth in the Hispanic enrollment share, a fact reflected in increasing exposure to Hispanic peers and decreasing exposure to Black peers among both Black and Hispanic students.

Boston and New York segregation patterns have partly converged since the 1980s. While New York segregation has trended lower for longer than in Boston, in both cities, minority exposure is recently around 75%, while minority isolation has fallen since the turn of the century. Both cities have also seen a remarkable reduction in exposure to Black peers for both Black and Hispanic students, with a corresponding increase in Hispanic exposure. Motivated by these evolving patterns of racial diversity, our investigation considers school distance and travel effects on Black and Hispanic students separately as well as jointly.

B Busing and Choice

Since the early 2000s, Boston has assigned seats centrally in a match that takes as inputs school priorities over students and student preferences over schools, which students submit in the form of a rank-order list. From 2001-05, Boston assigned students using the immediate acceptance

[6] This viewpoint is reflected in the New York Times' widely-heard 2020 podcast, Nice White Parents.

algorithm. Since 2006, the Boston match has employed a version of DA, with priority given to siblings of enrolled students and to students residing in a school's designated walk zone. Appendix Figure A1 maps Boston geocodes, originally defined in the Garrity era and used during our study period. School-specific walk zones are determined by one-mile radius circles centered at each school; residents of any geocode intersected by a school's circle are said to reside in the school's walk zone (in what follows, we call these "Garrity walk zones").[7]

The Boston match, which covers traditional and pilot schools, breaks ties using a single random lottery number assigned to each student. Boston students may also attend publicly-funded charter schools and one of three public selective-enrollment exam schools. Boston charter schools run single-school lotteries, while Boston exam schools run two separate DA matches for 7th grade and 9th grade admissions, using weighted averages of elementary school GPA, middle school GPA and an admissions exam score to rank applicants. Although Boston charter and exam school students are eligible for transportation services, we focus on schools in the traditional sector since effects of travel to schools outside the traditional sector are harder to interpret.[8]

New York's centralized assignment scheme is also based on student preferences, school-specific priorities, and a DA match. New York priorities depend on many factors, including geography and attendance at an open house prior to the match. Within New York priority groups, tie-breaking relies either on a random lottery number or on school-specific non-lottery criteria like test scores, interviews, and auditions. Schools using non-lottery tie-breakers are known as "screened schools," while those using lottery tie-breaking are said to be "unscreened." The New York high school match excludes charter schools and a few highly selective exam schools such as Stuyvesant and Bronx Science (these are called "specialized high schools" in New York vernacular). As in Boston, New York exam schools run a separate match.

Sixth graders in Boston currently qualify for yellow school bus service if their home-school walking distance exceeds 1.5 miles. All Boston students in grade 7 and higher qualify for passes

[7]See Dur et al. (2018) for more on Boston's walk-zone policy. Motivated by high transportation costs, Boston adopted a "Home-Based plan" in 2014 limiting the set of schools each applicant might rank, while still including at least some with good outcomes (Shi, 2015; Pathak and Shi, 2021).

[8]Over our sample period from 2002 to 2017, Boston's three exam schools admitted students solely based on their composite score and a random tie-breaker. Abdulkadiroğlu, Angrist and Pathak (2014) uses the exam school match to estimate causal effects of exam school attendance on educational outcomes in Boston and New York. Abdulkadiroğlu et al. (2011) and Cohodes, Setren and Walters (2021) use single-school charter lotteries to estimate Boston charter effects.

granting free use of public city transport from September through June (BPS, 2021).[9] In New York, all high school students who live farther than 0.5 miles from school are eligible for MetroCards granting free use of city subways and buses (NYC, 2021).[10]

III Data and Samples

We obtained BPS data on all applicants for 6th- and 9th-grade seats in the centralized middle and high school matches for the school years beginning fall 2002-17. Match files include information on applicants' preferences over schools, school priorities, and lottery tie-breakers. Data on school enrollment come from the Massachusetts Department of Elementary and Secondary Education (DESE). DESE files contain school enrollment data, as well as demographic information including race, subsidized lunch status, sex, special education status, and language proficiency status. We also obtained DESE data from the Massachusetts Comprehensive Assessment System (MCAS), a standardized assessment taken by all Massachusetts public school students. MCAS tests are taken in Grades 3-10. MCAS outcomes examined here are Grade 6 Math scores and Grade 7 ELA scores for Grade 6 applicants and Grade 10 scores for Grade 9 applicants. Baseline scores are from Grade 4 for middle school applicants and from Grades 7-8 for high school applicants.[11] Test scores are standardized by test-grade-year to have mean zero and unit variance within a subject-grade-year among enrolled students in Boston in our sample who are tested in a given year. For concision, we construct a single MCAS outcome from the mean of Math and ELA scores.

College outcomes for Boston high school students are measured using data from the National Student Clearinghouse (NSC) database. NSC data were obtained by DESE, which aims to match Massachusetts public school graduates to NSC every year and matches non-graduates every other year. The NSC records supplied by DESE are used to code dummies for any college attendance and for four-year college attendance.

The New York Department of Education (DOE) provided data on applicants to 9th grade

[9] Boston 7th and 8th graders were bused until 2014-15 (BPS, 2014).

[10] Until 2019, students in grades 7-12 living between 0.5 and 1.5 qualified for half-fare bus-only MetroCards (Corcoran, 2018).

[11] The ELA baseline changes because MCAS testing expanded during our sample period. Grade 7 ELA scores are used for applicants enrolled in Grade 9 in school years 2002-03 through 2005-06 and Grade 8 ELA scores are used for applicants enrolled in Grade 9 in school years 2006-07 through 2013-2014.

public high school programs from fall 2012 to fall 2016.[12] New York application files include the information used in the high school match. The DOE also provided information on student assignment and enrollment, residential location, and demographic characteristics.

New York test score data come from two sorts of assessments. SAT scores, from tests taken mostly in 11th grade, provide achievement outcomes. We construct a single SAT outcome score from the mean of Math and reading standardized scores. Baseline test score data are from New York State standardized Math and ELA assessments taken in 6th grade. For purposes of our analysis, all scores are standardized to have mean zero and unit variance in the population of New York charter, traditional public school, and exam school students, separately by subject, grade, and year. Data on New York graduates' college enrollment data come from the DOE's annual match of its graduates to the NSC and were also provided by the DOE.

Our analysis examines two busing-related treatments, the first determined by attendance at a non-neighborhood school, and the second a measure of travel time. For Boston students, non-neighborhood assignment and attendance are defined according to whether students live in a school's Garrity walk zone. For New York students, non-neighborhood schooling is defined according to whether students are assigned or enroll at a school outside their district of residence (The DOE partitions New York City into 32 districts). In both cities, travel time to school is given by public transit travel time between a student's residence and school, setting an arrival time of 8:00 am on January 31st, 2022. Travel time is the shortest combination of walking, local and express bus, and subway modes, estimated using the HERE Public Transit API. Residential addresses are approximate (for Boston, this is the centroid of the geocode of residence; for New York, this is the centroid of the census tract of residence). A set of online appendices further detail our data, samples, and variable definitions.

Finally, as noted in the discussion of segregation trends, we focus on impacts on Black and Hispanic students as well as on students overall. This focus reflects public interest in school quality for disadvantaged minorities and decades of scholarship documenting important changes in the quality of the schools minority students attend (see, e.g., Welch and Light (1987); Card and Krueger (1992); Rivkin and Welch (2006)). Moreover, as in many large urban districts that pay for transportation,

[12] Exam and charter schools do not participate in the centralized high school match. See Abdulkadiroğlu, Pathak and Roth (2005) for a detailed description of New York's exam-school match.

the Boston and New York public school population is predominantly Black or Hispanic. Because most busing is within-district, we leave study of relatively rare inter-district busing programs (such as the Massachusetts Metco program) for future work (see Bruhn (2023) and Setren (2024)).

A Sample Characteristics

Table I describes the students in our analysis samples. Roughly three-quarters of Boston match applicants rank a non-neighborhood school first, while a similar fraction enrolls outside their neighborhood. The demand for non-neighborhood enrollment is almost as high in New York, where roughly two-thirds of applicants rank a non-neighborhood option first with a similar proportion enrolling out of neighborhood. Students in both districts travel an average of 33-38 minutes to school each way; distance traveled averages around 4 miles.[13] Seventy percent of Boston students are eligible for busing; almost all New York students are busing-eligible.

[insert table I here]

Like many large urban school districts, New York and Boston public schools have high minority and low-income enrollment. Boston enrollment is 77% Black or Hispanic, with 70% qualifying for a subsidized lunch. The corresponding figures for New York are 69% and 75%, respectively. Both districts enroll substantial numbers of limited English proficiency and special education students.

The IV strategy used to estimate causal effects looks at match participants only, a group referred to here as *applicants*. Applicant characteristics appear in columns 2 and 6 of Table I.[14] Non-applicants in Boston are mostly continuing 6th graders enrolled in K-8 schools or those applying to charter and exam schools only. The New York applicant sample excludes high-needs special education students, assigned outside the match, as well as students who enroll in the city's public schools after the match.[15]

With a few exceptions, applicants have demographic characteristics broadly similar to those of the enrolled sample. In particular, Boston applicants are a little more likely to be low income: column 2 shows that 77% of applicants qualify for a subsidized lunch, while 70% of enrolled students

[13] Elementary school students travel less. Focusing on Grades 3-6 in New York and using data from the NYC Office of Pupil Transportation, Cordes, Rick and Schwartz (2022) reports that the average home-to-school travel time is 21.1 minutes.

[14] Appendix Table A1 details the sample selection rules used to define each sample.

[15] Students whose individualized education program (IEP) places them in a designated special needs category are assigned outside the match (NYC Match, 2021). Student who arrive over the summer are seated administratively.

do so. The most noteworthy difference between the enrolled and applicant Boston samples is lower baseline scores in the latter. This partly reflects attendance in charter and exam schools, which are not part of the match, by students who applied exclusively to these schools. Exam schools in particular tend to enroll higher achievers (Abdulkadiroğlu, Angrist and Pathak, 2014).

The New York applicant and enrolled student samples likewise appear demographically similar. At the same time, mean baseline scores for New York applicants exceed mean baseline scores in the New York enrolled sample. This reflects the exclusion of many special education students from the former. Note also that while our applicant samples exclude those who apply to charter and exam schools only, they include match participants ultimately seated in a charter or exam school. It's noteworthy, therefore, that the instrumental variables used to identify causal effects of distance and travel are uncorrelated with charter and exam-school attendance in the sample used for causal analysis (this is shown in Appendix Table A2).

The samples used for causal inference, described in columns 3 and 4 for Boston and 7 and 8 for New York, consist of the set of applicants for whom non-neighborhood enrollment or travel is randomized. These *experimental samples* consist of applicants whose assignments can be changed by redrawing tie-breakers. Just over one-quarter of New York applicants and roughly 37% of Boston applicants are subject to a tie-breaking experiment. In data from both cities, minority and low income (defined by free or reduced-price lunch eligibility) applicants are disproportionately likely to be subject to experimental variation in assignment. Baseline test scores for the experimental samples are also lower than among all applicants, especially in New York. Lower baseline scores in the New York experimental sample reflect New York's many screened schools: as we explain below, screened school tie-breaking generates experimental variation local to screened school admissions cutoffs. Students with high baseline scores are therefore more likely to be sure of obtaining a screened-school seat.

IV Econometric Framework

A OLS Estimates

We are interested in the causal effects of school distance and travel on the school environment and academic outcomes. Ordinary least squares (OLS) estimates of the relationship between non-neighborhood attendance and academic achievement provide a natural benchmark for the IV estimates that follow. OLS estimates are generated by fitting a model that can be written:

$$Y_i = \alpha G_i + X_i'\Gamma + \eta_i, \tag{1}$$

where G_i indicates non-neighborhood school attendance, X_i is a vector of controls, and η_i is a regression residual. Coefficient α is the parameter of interest. In Boston data, G_i indicates attendance at a school outside a student's Garrity walk zone. In New York data, G_i indicates out-of-district attendance.

Equation (1) is estimated on the sample of enrolled students (a subset of the enrolled sample described in the first and fifth columns of Table I, limited to students with outcome data). Covariate vector X_i includes dummies for race, gender, special needs status, free or reduced-price lunch eligibility, and English proficiency status; along with grade and year dummies. To control for differences across neighborhoods, equation (1) includes fixed effects for each walk zone in Boston and for each district in New York (these are determined by students' residential address). Given our focus on traditional public schools, OLS estimates come from models that include dummies for exam and charter sector attendance, and a dummy for match participation. Models for New York add dummies for attendance in District 75 or 79, district codes allocated to high-needs special education students and students with other unique needs (e.g., incarcerated youth or those pursuing a GED). The dependent variable, Y_i, is a test score or a measure of high school graduation or college attendance. Boston test scores are from the MCAS (sum of 6th grade Math and 7th grade ELA for middle school, sum of 10th grade Math and ELA for high school). New York test scores are from the SAT, taken by approximately 70% of students. We measure high school graduation with a dummy indicating whether students 'graduate on-time', meaning within four years of enrolling in 9th grade.[16]

Boston students who enroll out-of-neighborhood tend to have higher average test scores and are more likely to go to college than students who attend schools closer to home. This is documented in the first three columns in Panel A of Table II, which reports estimates of α in equation (1) separately for all, Black, and Hispanic students. Specifically, Table II shows that Boston students

[16] Graduation for students who transfer to a different district during high school is coded as missing.

who enroll beyond their neighborhood score about 0.03σ higher on MCAS tests. Boston students who enroll in non-neighborhood schools are also 1.7 percentage points more likely to enroll in college. Estimates for subsamples of Black and Hispanic students are similar. In contrast, non-neighborhood attendance does not significantly affect four-year college enrollment rates and has a small, but statistically significant, negative effect on the rate of on-time high school graduation.

[insert table II]

OLS estimates for New York also show a strong association between achievement and non-neighborhood attendance. New York students attending non-neighborhood schools score roughly 0.08σ higher on the SAT, results reported in column 1 of Panel B in Table II. The corresponding estimated achievement gains for minority New Yorkers at non-neighborhood schools are a little smaller, though still substantial. Non-neighborhood attendance in New York is also associated with higher rates of college-going, mostly driven by 4-year college attendance which is increased by 1.5 points.[17] Similar to Boston, non-neighborhood attendance has a small negative effect on the likelihood of graduating high school on time.

The non-neighborhood schooling treatment is defined by arbitrary neighborhood boundaries. We therefore explore causal effects of travel time as well. Travel effects are scaled in twenty-minute increments, a scaling motivated by Figure IV, which plots the distribution of travel time for non-neighborhood attendance compliers. Among applicants induced to enroll out-of-neighborhood by virtue of being offered a non-neighborhood seat in the match, travel times average 16-19 minutes longer than they would have been in the absence of such an offer. This figure also highlights the skewness of non-neighborhood commute times relative to the compressed distribution for those enrolling close to home. The model is estimated by replacing the dummy for non-neighborhood attendance by the continuous measure of travel time to the enrolled school. Estimated travel-time effects reflect outcomes for students who ride an hour or more to school as well as outcomes for students whose commute is far shorter.

[insert figure IV]

OLS estimates for travel time effects, reported in columns 4 to 6 of Table II, show positive effects of travel on test scores and college enrollment, comparable to those estimated for non-neighborhood

[17] Blagg, Rosenboom and Chingos (2018) documents a similar association between travel time and test scores in Washington, DC.

attendance. Notably, travel time effects are more precisely estimated than non-neighborhood attendance effects, with standard errors twice smaller. In Boston, an additional 20 minutes of travel is associated with 0.02σ higher MCAS scores and 1.3 points higher college attendance rate. In NYC, travel time effects are somewhat smaller than non-neighborhood attendance effects: attending a school 20 minutes further increases SAT scores by 0.05σ and 4-year college enrollment rates by 1.1 points. Travel time in New York is also associated with higher graduation rates, though the gains are small. It remains to be seen, however, whether the association between school distance and travel and educational outcomes documented in Table II reflects causal effects or selection bias.

B Identification and Estimation of Causal Effects

Tie-breaking in the Boston and New York school assignment algorithms generates a research design that identifies causal effects. In both cities, applicants submit rank-order lists of preferences for school programs and are granted priorities by each program (many New York schools run multiple programs, each admitting separately). We refer to an applicant's preferences and priorities their type, denoted θ_i for applicant i. School assignment differences for students with the same value of θ are due solely to the tie-breaking embedded in the match.

Boston uses a single randomly drawn lottery number as tie-breaker for all schools. To see how lottery tie-breaking can be used to identify causal effects of school distance and travel, consider a constant-effects model of the effects of non-neighborhood attendance, indicated by dummy G_i as before. Potential outcomes $\{Y_{0i}, Y_{1i}\}$ are indexed against this. The constant causal effect of interest, $\beta = Y_{1i} - Y_{0i}$, is identified by an IV estimand that uses non-neighborhood *assignment*, Z_i, as an instrument for G_i in a two-stage least squares (2SLS) procedure incorporating a control function derived from our understanding of the Boston and New York matches.

The details behind this argument are fleshed out as follows. Let $D_i(s)$ indicate whether applicant i is offered a seat at school $s \in S$, where S denotes the set of schools in the match. Although Z_i is not randomly assigned, it's a function of the set of conditionally randomized offers, $\{D_i(s)\}$, and a vector of covariates, g_i. Specifically,

$$Z_i = \sum_s D_i(s) g_i(s), \tag{2}$$

where $g_i(s)$ indicates whether s is a non-neighborhood school for i. Collect the set of $g_i(s)$ for applicant i in vector g_i. With lottery tie-breaking, identification is a consequence of the following conditional independence property:

$$E[Y_{0i}|\theta_i, g_i, Z_i] = E(E[Y_{0i}|\theta_i, g_i, \{D_i(s)\}]|\theta_i, g_i, Z_i) = E[Y_{0i}|\theta_i, g_i]. \tag{3}$$

The first equals sign uses (2); the second uses lottery tie-breaking, which implies that, conditional on type, offers of a seat at s are determined by lottery and therefore ignorable in the sense of being independent of potential outcomes. Conditioning on g_i is irrelevant for the ignorability of school-specific offers, $D_i(s)$, but necessary for ignorability of Z_i.

This conditional independence property leads to the following identification result:

Proposition 1. *Suppose the effect of Bernoulli treatment G_i is constant and given by $\beta = Y_{1i} - Y_{0i}$. Given instrumental variable Z_i, defined in (2) and satisfying (3), we have that:*

$$\beta = \frac{E[(Z_i - \mu_i)Y_i]}{E[(Z_i - \mu_i)G_i]}, \tag{4}$$

where $\mu_i \equiv E[Z_i|\theta_i, g_i]$ and the denominator is presumed to be non-zero. Moreover,

$$\mu_i = \sum_s \psi_s(\theta_i) g_i(s), \tag{5}$$

where

$$\psi_s(\theta_i) = E[D_i(s)|\theta_i] = P[D_i(s) = 1|\theta_i]$$

is the DA propensity score derived in Abdulkadiroğlu et al. (2017).

Proposition 1 is a consequence of the fact that, by virtue of the conditional independence characterized by (3), we can write

$$Y_i = \beta_{IV} G_i + h(\theta_i, g_i) + \varepsilon_i, \tag{6}$$

where

$$h(\theta_i, g_i) \equiv E[Y_{0i}|\theta_i, g_i], \tag{7}$$

$$\varepsilon_i \equiv Y_{0i} - h(\theta_i, g_i), \tag{8}$$

and these two terms are mean-independent of the centered instrument $Z_i - \mu_i$. Mean-independence of $Z_i - \mu_i$ and $h(\theta_i, g_i) + \varepsilon_i$ is the orthogonality condition yielding (4).

As in the original Rosenbaum and Rubin (1983) propensity score framework (and extensions to ordered and continuous treatments as in Imbens (2000); Hirano and Imbens (2004)), the dimension reduction implied by (5) is also useful. Control function μ_i depends on θ_i solely via the the profile of assignment risk, $\{\psi_s(\theta_i)\}$. Although θ_i has many points of support (there are almost as many types of applicants as there are applicants), DA propensity scores depend on only a few characteristics of an applicant's rank-order list and the associated school-specific (but not applicant-specific) cutoffs determined by the match. Abdulkadiroğlu et al. (2017) uses a large-market approximation to derive this result, giving a formula for $\psi_s(\theta_i)$ that's employed here to estimate μ_i for each applicant.[18]

Let $\hat{\mu}_i$ denote consistent estimates of μ_i computed from large-market estimates of the profile of assignment risk. Plugging these into the sample analog of (4) gives an estimator,

$$\hat{\beta}_{IV} = \frac{\sum_s (Z_i - \hat{\mu}_i) Y_i}{\sum_s (Z_i - \hat{\mu}_i) G_i},$$

that converges to β by the continuous mapping theorem. β is also estimated consistently (and conveniently) via 2SLS with first and second stages:

$$G_i = \gamma Z_i + \kappa_1 \mu_i + \xi_{1i}, \tag{9}$$

$$Y_i = \beta G_i + \kappa_2 \mu_i + \xi_{2i}. \tag{10}$$

To see why, suppose first that μ_i is known and recall that a just-identified 2SLS estimand with covariate μ_i can be written as IV using instrument $\tilde{Z}_i^*$, defined as the residual from a regression of Z_i on μ_i (see, e.g., Angrist and Pischke (2009)). Here, $\mu_i = E[Z_i|\theta_i, g_i]$, so $E[Z_i|\mu_i] = \mu_i$, a linear

[18] Since we focus on students who are assigned seats in the match, the relevant DA propensity score is normalized by match participants' probability of being assigned any school in the match.

function of μ_i. The population regression of Z_i on μ_i therefore yields the CEF residual,

$$\tilde{Z}_i^* = Z_i - E[Z_i|\theta_i] = Z_i - \mu_i.$$

In practice, μ_i must be estimated, but 2SLS estimates controlling for $\hat{\mu}_i$ (denoted $\hat{\beta}_{2SLS}$), are likewise consistent for β as long as $\hat{\mu}_i$ converges to μ_i.[19]

Our 2SLS estimates incorporate two extensions to this framework. The first, relevant for both Boston and New York, covers ordered treatments like travel time, T_i, rather than Bernoulli G_i. Swapping T_i for G_i in (9) and (10), the instrument for T_i is an applicant's travel time to the school they're offered in the match. Formally, let $t_i(s)$ denote the time it takes applicant i to travel to school s and collect school-specific travel times for applicant i in vector t_i. The offered travel instrument can then be written:

$$Z_i^T = \sum_s D_i(s)t_i(s), \tag{11}$$

with control function $\mu_i^T = E[Z_i^T|\theta_i, t_i]$ where $D_i(s)$ is a school-specific offer dummy as before. The extension of Proposition 1 to this case solves the problem of causal identification with an ordered treatment tackled previously by Imbens (2000). In particular, inclusion of the control function for an ordered treatment obviates the need to condition on multiple conditional probabilities as in earlier work.

Second, because New York's high school match employs a mix of lottery and non-lottery tie-breaking, the control function for New York uses the more elaborate characterization of assignment risk derived in Abdulkadiroğlu et al. (2022). This *local DA propensity score* relies on the fact that, in a shrinking bandwidth around DA admissions cutoffs, non-lottery tie-breakers behave like lottery numbers. The local DA propensity score, written $\psi_s(\theta_i, \tau_i(\delta_N))$, depends on a collection of indicators for cutoff proximity, denoted $\tau_i(\delta_N)$ and determined in part by a data-driven bandwidth, δ_N. The conditioning variables that define non-neighborhood control function μ_i for New York

[19] Appendix B derives the limiting distribution of $\hat{\beta}_{2SLS}$ assuming match applicants constitute a random sample from the population of interest. The estimation error in empirical propensity scores originates in the randomness of lottery draws rather than sampling variance. Even so, sampling experiments discussed in Appendix A.7 of Abdulkadiroğlu et al. (2017) suggest that conventional robust-standard-error-based p-values for score-controlled reduced-form estimates match the corresponding randomization-based p-values closely. Angrist et al. (2024) uses a similar 2SLS estimator based on centralized assignment to estimate individual school value-added. Borusyak and Hull (2023) develops an IV strategy that uses simulation methods to compute terms like μ_i.

applicants include $\tau_i(\delta_N)$ as well as applicant type and the vector of non-neighborhood indicators, g_i. The control function for non-neighborhood offers in New York can therefore be written:

$$\mu_i = E[Z_i|\theta_i, g_i, \tau_i(\delta_N)] \approx \sum_s \psi_s(\theta_i, \tau_i(\delta_N))g_i(s),$$

where the assignment risk profile, $\{\psi_s(\theta_i, \tau_i(\delta_N))\}$, allows for both lottery and non-lottery tie-breakers (this risk profile is approximate, characterizing offer rates as $\delta_N \to 0$). 2SLS estimates of non-neighborhood attendance effects for New York are computed using versions of (9) and (10) that add design controls in the form of local-linear functions of screened-school tie-breakers; these functions employ the bandwidth used to define $\tau_i(\delta_N)$.[20]

The 2SLS estimator characterized by (9) and (10) is derived here under constant effects. In practice, treatment effects may be heterogeneous. Extending results in Angrist and Imbens (1995) and Angrist, Graddy and Imbens (2000), Borusyak and Hull (2023) shows that a centered IV estimand of the form described by (4) can be written as a weighted average of covariate-specific causal effects. With a dummy treatment and a dummy instrument, as in the non-neighborhood schooling model, the IV-estimand is a weighted average of conditional-on-covariates treatment effects for covariate-specific compliant subpopulations defined by the response of G_i to Z_i. Characteristics of the set of non-neighborhood compliers, defined as applicants who enroll in a non-neighborhood seat when offered but not otherwise, match those of applicants with non-neighborhood assignment risk (the latter reported in columns 3 and 7 in Table I).

Appendix Table A2 reports a set of results meant to validate our research design. Even when instruments are randomly assigned, differential attrition may lead to selection bias. Roughly 80% of Boston match applicants have an MCAS Math or ELA outcome. Table A2 shows that the likelihood of observing these outcomes is unrelated to both non-neighborhood-offer and offered-travel instruments. Roughly 70% of New York students take the SAT. New York students who travel are slightly less likely to have SAT scores. College outcomes – which come from administrative data from the National Student Clearinghouse – are unlikely to be compromised by instrument-related

[20]The bandwidths used here are estimated as suggested by Calonico, Cattaneo and Titiunik (2014). Bandwidths are computed separately for each test score variable; we use the smallest of these for each program. We set $\delta_N = 0$ for screened programs with fewer than 5 applicants in the bandwidth who are either below or above the tie-breaker cutoff. Design controls are as specified in equation (12) of Abdulkadiroğlu et al. (2022). These include dummies indicating applicants who applied to each program and dummies indicating applicants in each bandwidth.

differences in follow-up.

A second set of diagnostics evaluates covariate balance. Appendix Table A2 also reports coefficients from regressions of baseline covariates on instruments, controlling for estimated μ_i. Balance regressions for Boston show no statistically significant relationship between instruments and baseline covariates. This highlights the balancing property that motivates our μ_i-controlled 2SLS strategy. Balance estimates for New York applicants show a few small, marginally significant differences, but the magnitudes of these seem unlikely to lead to substantial omitted variables bias. In any case, the 2SLS estimates discussed below are from models that include the baseline covariates as controls. Control for covariates changes the 2SLS estimates little while improving precision.

Beyond the usual concerns with differential attrition and covariate balance, research designs exploiting centralized school assignment may be compromised by spillover effects that lead to violations of the IV exclusion restriction supporting a causal interpretation of 2SLS estimates. When one applicant is offered a non-neighborhood seat, another may be offered the neighborhood seat not taken. This in turn may change neighborhood peer composition even for those who don't travel. Spillovers of this sort can be seen as a violation of the non-interference or stable unit treatment values (SUTVA) assumption that typically underpins causal inference (see, e.g., Imbens and Rubin (2015)). In the large-market framework used to construct μ_i, however, an individual applicant's school assignment is determined solely by their own tie-breakers and type. Offers are therefore theoretically uncorrelated across applicants. Our empirical exploration of possible SUTVA violations (not reported) suggests spillover effects are indeed negligible.

V IV Estimates

A Integration Consequences of School Distance and Travel

Minority students who enroll in non-neighborhood schools have fewer minority classmates as a result. This is documented in Table III, which reports 2SLS estimates of non-neighborhood attendance effects on peer composition, separately by city. The table shows estimates for all applicants with experimental variation in neighborhood attendance or travel time and for two subgroups defined by race. The associated first-stage estimates for all applicants (not shown in the table) imply that a non-neighborhood offer increases rates of non-neighborhood attendance by 0.38 in Boston and 0.56 in New York. Estimated first stages for Black and Hispanic applicants are similar.

[insert table III here]

In all-applicant samples, the impact of both non-neighborhood schooling and an additional twenty minutes of travel on the proportion of a student's classmates who are Black or Hispanic is modest. Disaggregating by race, however, effects on Black applicants' minority exposure (defined as the proportion of a student's schoolmates who are Black or Hispanic in 6th and 9th grades in Boston and 9th grade in New York) are substantial. Non-neighborhood attendance in Boston causes Black students to attend schools with 7.8 percentage points fewer Black peers and 5.8 percentage points more Hispanic peers, resulting in a decrease in overall minority exposure of 2 percentage points. Non-neighborhood school attendance also reduces minority isolation for Black Boston applicants sharply, a fall of 22 percentage points compared to a mean of about 48 points. We focus on minority isolation defined by a 90% rather than an 80% cutoff since the higher threshold features in contemporary discussions of segregation (e.g., Cohen (2021) and Potter (2022)).

Among Black New York applicants, non-neighborhood school attendance results in a roughly 8.5 percentage point reduction in Black peers and a 4.8-point increase in Hispanic peers. Overall minority (Black or Hispanic) exposure falls by 3.6 percentage points for Black New Yorkers who attend non-neighborhood schools. Non-neighborhood attendance also reduces minority isolation by around 3.4 points.

Non-neighborhood school attendance places Boston's Hispanic applicants in a more integrated environment, though to a far lesser degree than for Black applicants. Specifically, non-neighborhood attendance reduces Hispanic applicants' peer share Hispanic and peer share Black, but neither of these effects (on the order of 1 point) are significantly different from zero. Non-neighborhood school attendance reduces Hispanic minority isolation by over 10 points. Estimated integration effects for New York's Hispanic students are larger than for those in Boston. These estimates suggest that non-neighborhood attendance decreases peer share Hispanic by 5.9 percentage points while boosting peer share Black by 1 percentage point. Non-neighborhood attendance reduces Hispanic New York applicants' minority isolation by almost 9 percent, larger than the impact for Black applicants.

The pattern of estimated effects of school travel time on peer race and minority isolation mostly parallels that seen in estimates of effects of non-neighborhood attendance. For Black students, for example, twenty minutes of travel reduces minority exposure by about 3.8 points in Boston and by

4.7 points in New York. Travel effects are also consistently negative across racial groups.

On balance, the estimates in Table III suggest that non-neighborhood attendance has substantial integrating effects, especially for Black applicants. Non-neighborhood attendance reduces same-race exposure more for Hispanic New Yorkers than for Hispanics in Boston. Effects on peer share Black and Hispanic students tend to be offsetting, however, so that changes in overall minority exposure due to non-neighborhood schooling are well below the corresponding changes in same-race exposure. Estimates of the effects of twenty minutes of travel on integration are broadly similar to, but more precise than, estimated effects of non-neighborhood attendance.

B Effects on Achievement and College Attendance

Non-neighborhood attendance and lengthier school travel times reduce minority applicants' same-race exposure and cut minority isolation sharply. In addition to these integration effects, we might expect non-neighborhood schooling and travel to increase learning and college attendance as well. The 2SLS estimates reported in Tables IV, however, show little evidence of positive non-neighborhood schooling or travel effects on achievement, high school graduation rates, and college enrollment.

[insert table IV here]

As can be seen in the first three columns of Table IV, Boston students who enroll out-of-neighborhood have test scores and college attendance rates comparable to those of students who stay in-neighborhood. Among Black Boston applicants, for example, non-neighborhood attendance generates an estimated 0.03σ (se=0.06) improvement in MCAS scores. In the Boston sample, non-neighborhood school attendance is estimated to reduce the likelihood of on-time high school graduation by 10.3 percentage points, though this significant impact is imprecisely estimated (se=5.0).

2SLS estimates of the effects of twenty minutes of additional travel are more precise than the corresponding estimates of non-neighborhood effects. For example, the estimated effect of travel on MCAS scores for Black applicants in Boston is 0.019σ with an estimated standard error of 0.032. By contrast, the standard error of the corresponding non-neighborhood effect is about twice as big. It's therefore noteworthy that most of the relatively precise estimated effects of twenty-minute travel time on test scores, on-time high school graduation, and college enrollment are not significantly different from zero.[21] In this context, the only (marginally) statistically significant estimated travel effect for Boston students is a 5.5 percentage point *decrease* in the likelihood of any college attendance for Blacks.

Most of the 2SLS estimates of the impact of non-neighborhood schooling on Boston students' test scores, high school graduation rates, and college attendance rates are smaller than the corresponding OLS estimates. At the same time, few of the 2SLS estimates for non-neighborhood attendance in Boston are estimated precisely enough to be statistically distinct from the corresponding OLS estimates. But 2SLS estimates for New York are considerably more precise than those for Boston. With estimated standard errors of around $0.02 - 0.03$, 2SLS estimates for New York provide sharp evidence of null effects of non-neighborhood enrollment and travel time on academic achievement.

Statistically significant negative estimates for college enrollment and high school graduation in Panel B of Table IV suggest extra travel may be harmful in New York, at least for some. These negative effects are largest among Hispanic students. Estimates for this group, reported in column 6, suggest twenty minutes of additional travel reduces on-time high school graduation rates by 2.5 points, college-going by 5.0 points, and four-year college attendance by 3.5 points. Estimated travel effects on Black college-going are also negative, but not statistically distinguishable from zero.[22]

Perhaps gains from travel are diluted by the fact that some travelers are reluctant. This possibility is examined by estimating a model that interacts travel time with an indicator for applicants whose first choice is a neighborhood school. Roughly a quarter of Boston and a third of New York match participants place a neighborhood school at the top of their rank-order list. The interacted model is estimated using offered travel and an interaction between offered travel and an indicator for in-neighborhood first choice as instruments, controlling for the offered travel control function and the interaction of this function with the in-neighborhood first-choice indicator.

[insert table V here]

Results from this interacted model, reported in Table V, show no evidence of travel benefits

[21] Estimates allowing for nonlinear travel time effects, reported in Appendix Table A3, suggest nonlinearity here is unimportant.

[22] Distance and travel have little effect on absences, suspensions, or a composite disciplinary index in Boston. Estimates for New York suggest twenty minutes of extra travel increases days absent by 0.8, but the corresponding estimated non-neighborhood effect is not significantly different from zero. Estimates of travel effects on behavioral outcomes are reported in Appendix Table A4.

for students who prefer non-neighborhood enrollment. Effects on students who did not rank a neighborhood school first are given by the main travel effect in this model. These estimates, reported in the first row of each panel in Table V show a pattern of zeros for test scores and negative effects on high school graduation and college similar to that seen in Table IV. Estimated interactions with preferences are noisy for Boston but reasonably precise for New York. Across both cities and demographic groups, 2SLS estimates of coefficients on terms interacting travel time with neighborhood first choice are not statistically different from zero. On balance, these results suggest that even among students who wish to travel, the gains from travel are illusive.

C Mediation Analysis

The integration gains afforded by school travel fail to boost achievement, high school graduation rates, or college enrollment. Contrasting findings for integration and education outcomes are reconciled here using a model of travel and other mediators of school effectiveness. Following a preliminary analysis of offered travel on school quality, we focus on New York since the New York sample is large enough for our random assignment research design to distinguish multiple causal forces. The resulting estimates suggest travel improves school quality little. At the same time, holding school quality fixed, travel decreases on-time high school graduation rates and college enrollment. This may be explained by the fact that longer commute times come at the expense of study and sleep time (a hypothesis examined in Carrell, Maghakian and West (2011) and Cordes, Rick and Schwartz (2022)). Alternately, teachers and school guidance counselors may better serve students who reside close to school.[23]

School value-added provides a parsimonious summary of school effectiveness that should reflect the impact of education inputs like class size, teacher skills, and peers on student achievement.[24] Let $\nu(s)$ denote the value added by school s, a quantity defined separately for achievement, graduation, and college enrollment, but assumed to be the same for all students. The casual value added

[23]The US Department of Transportation's *Safe Routes to School Program* cites a number of potential benefits of short school commutes (USDOT, 2021).

[24]Appendix Table A5 reports estimates of the effect of distance and travel on student-teacher ratios and two measures of teacher qualifications. Although non-neighborhood schooling and school travel appear to increase average class size, these changes are likely too small to have measurable downstream consequences. (Krueger (1999) estimates a 0.2σ increase in achievement from a 10-student reduction in class size, while the class size consequences estimated here amount to a change of less than one student.) Effects on teacher qualifications are even smaller.

experienced by student i is

$$V_i = \sum_i A_i(s)\nu(s),$$

where $A_i(s)$ is a dummy indicating whether i attends school s.

We're interested in a model of achievement in which achievement depends both on causal value added and travel time. This is captured by assuming

$$\begin{aligned} Y_i &= \sum_s A_i(s)\{\lambda\nu(s) + \beta t_i(s)\} + h(\theta_i) + \varepsilon_i \\ &= \lambda V_i + \beta T_i + h(\theta_i) + \varepsilon_i, \end{aligned} \tag{12}$$

where T_i is travel time to the school i attends, $E[Y_{0i}|\theta_i] = h(\theta_i)$ is the conditional mean reference-level potential outcome Y_{0i} for an applicant of type θ_i, and ε_i is the random part of Y_{0i}. Parameters λ and β in this two-endogenous-variables model are identified by using a full set of school offer dummies, $\{D_i(s)\}$, as well as offered travel, Z_i^T, as instruments for V_i and T_i. Were $\nu(s)$ perfectly measured, we expect IV estimates of λ to be close to 1, with discrepancies due to the fact that value-added estimates are model- and sample-dependent.

In practice, causal value added is unobserved. Suppose, however, that $\nu(s)$ is predicted by an observed value-added estimate denoted by M_s according to

$$\nu(s) = M_s'\varphi + \eta_s, \tag{13}$$

where η_s is a regression residual. We can then write

$$V_i = M_{s(i)}'\varphi + \eta_{s(i)}, \tag{14}$$

where $s(i)$ is the school that applicant i attends. As in Angrist et al. (2024), we use risk-controlled (RC) VAM to predict $\nu(s)$ in equation (14). RC VAM is constructed by regressing outcomes on attendance dummies and school assignment propensity scores, $\psi_s(\theta_i)$. Assuming school choice is independent of potential outcomes conditional on risk controls, RC VAM identifies causal school effects for all schools, including those that are undersubscribed (for which lotteries yield no first stage). Angrist et al. (2024) shows the resulting estimates are a good guide to math value-added for New York schools.

Using (14) to substitute for V_i in (12) yields an equation with observed treatments on the right-hand side. We have:

$$Y_i = \pi' M_{s(i)} + \beta T_i + h(\theta_i) + [\varepsilon_i + \varphi \eta_{s(i)}], \tag{15}$$

where $\pi = \varphi\lambda$. This equation is identified using the set of school-offer dummies plus offered-travel as instruments for $M_{s(i)}$ and T_i, provided these instruments are uncorrelated with the compound error term in brackets.

Causal value-added $\nu(s)$ is a measure of school quality unrelated to travel. But RC VAM may reflect the average travel of students that enroll in school s. This is a consequence of the fact that RC VAM is constructed by regressing outcomes on the set of attendance dummies, $\{A_i(s)\}$. Ignoring control functions (which differ for RC VAM and travel), equation (14) implies that this regression estimates $\lambda\nu(s) + \beta E[t_i(s)|A_i(s) = 1]$ plus omitted variables bias arising from $Cov(A_i(s), \varepsilon_i)$. Predictive residual $\eta_{s(i)}$ is therefore likely correlated with offered travel, effectively a violation of the IV exclusion restriction used to identify (15). If, for instance, travel reduces achievement, schools attended by students with longer average commutes have lower RC VAM for a given $\nu(s)$. This exclusion problem is addressed by adding average travel time for students enrolled at s as an additional mediator in M_s. Specifically, M_s includes $E[t_i(s)|A_i(s) = 1]$ as well as RC VAM for $s(i)$. Equation (15) then has three variables to be instrumented: two school-level mediators in $M_{s(i)}$ plus individual travel, T_i. This is identified by using school-specific offer dummies plus offered travel as instruments. As when estimating (9) and (10), 2SLS estimates here include control functions for each instrument.[25]

Our mediation analysis begins with effects of offered travel, Z_i^T, on school quality as measured by RC VAM. These estimates, reported for both Boston and New York, reveal the extent to which students travel to schools with higher or lower RC VAM. As can be seen in Table VI, estimates of the effect of 20 minutes of offered travel on test score value-added show small but statistically significant increases in test scores around 0.01σ in both Boston and New York (effects on tests scores for Boston Hispanics are not significantly different from zero. Although positive, this increase in school quality

[25] For offered travel, this is μ_i^T based on equation (11), while for school offer-dummy instruments these are school offer propensity scores, $\psi_s(\theta_i)$.

is likely too small for travel to generate detectable travel effects on individual students.

[insert table VI here]

The value-added measures used to compute the estimates in each row of Table VI are outcome-specific. Offered travel effects on high school graduation and college value-added, reported in the second and third rows of the table, are small and not significantly different from zero. For all applicants, for instance, offered travel changes realized college RC VAM by only about 0.0025 and -0.001 standard deviations in Boston and NYC, respectively. On balance, the weak effects of offered travel on RC VAM suggests that changing school quality is unlikely to be an important mediator of travel effects. But travel may have direct effects, a possibility model (15) allows for.

As can be seen in Table VII, 2SLS estimates of equation (15) suggest travel has little direct impact on achievement beyond effects mediated by changing school quality. Importantly, however, in the all-applicant sample, an additional 20-minutes of travel time is estimated to reduce on-time graduation by 1.6 percentage points and to reduce college enrollment by 2.3 percentage points. This losses are driven mostly by by Hispanic students, for whom travel lowers graduation rates by 2.7 points and college attendance by 4.3 points. Estimates for Black students are also negative but smaller and not significantly different from zero.

[insert table VII here]

The estimates in Table VII also show RC VAM to be a good predictor of education outcomes, while average travel time does not seem to be very important. 2SLS estimates for Boston, analogous to those for New York, appear in Appendix Table A6. These estimates are relatively imprecise but, like the corresponding estimates for New York, show negative direct effects of travel on high-school graduation and college attendance. The overall picture here is easily summarized: travel changes school quality little, while having negative effects on graduation rates and college enrollment that are not mediated by changes in school quality.

VI Conclusion: Busing Trade-Offs

> We can no longer afford to spend millions a year to bus children across Boston to schools that are not demonstrably better than schools near their homes.
>
> *Theodore Landsmark, The Boston Globe, January 2009*

The estimates reported here align with Boston attorney Ted Landsmark's contention that busing today contributes little to the education of children who are bused. Our estimates suggest that, for some students, longer school commutes may even reduce graduation rates and college enrollment. At the same time, a shift to proximity-based "neighborhood" assignment may increase segregation. By how much? We gauge this by simulating a match in which both students and schools rank one another by distance. This imagined neighborhood assignment scenario uses information on student addresses (geocode in Boston, census tract in New York), school addresses, and school capacities. Because neighborhood schools are typically expected to accommodate all neighboring families, the simulation sets each school's capacity to the maximum enrolled there in the years for which we have data.

The simulation compares the enrollment distribution observed in our data with enrollment under binding neighborhood assignment for all students enrolled in match-participating schools (not just students participating in the match) in 2006-2013 for Boston and 2012-2016 for New York. The first four columns of Table VIII show empirical student-weighted average travel time, the share of students eligible for publicly funded school transportation ("busing eligibility"), and measures of segregation, while remaining columns show predicted changes under neighborhood assignment. Neighborhood assignment reduces travel time by about 13 minutes for Boston middle and high school students and by as much as 17 minutes for Black New York high school students. The share eligible for school transportation falls sharply in Boston, a decline of about 50 percentage points, with more modest though still substantial declines in New York.[26]

[insert table VIII here]

Black New York 9th graders are estimated to see the largest increase in same-race exposure from the shift to neighborhood schools, a change of 5.4 points. And minority isolation (that is, attendance in a school with a student body 90 or more percent minority) is predicted to jump sharply for Black students in Boston. For others, neighborhood assignment affects integration much less. Remarkably, the simulation predicts little change in segregation for Hispanics. Consequently, under neighborhood assignment, minority students as a group enroll in schools with shares of same-race peers close to those experienced today. These patterns reflect the fact that our reassignment scenario puts every student in play: when students who used to travel are pulled back to neighborhood

[26]For simulation purposes, Boston busing eligibility criteria are those used for 6th graders.

schools, some now attending these schools are displaced.

Sharp drops in busing eligibility should reduce school transportation costs, though precise savings for specific grades are hard to pin down. We can get a rough idea of possible savings, however, by using average yellow-bus transportation costs for Boston 6th graders and the value of public transportation passes for high school students in Boston and New York. Boston public school student M7 MBTA cost $30 per month (in 2022) and are issued for 12 months. Averaging yellow-bus costs for 6th graders with this amount (estimated at around $3,158 per rider as of February 2020) results in an average annual savings of around $3268 (in 2022 dollars) per formerly-transported-student in Boston (BPS, 2020; MBTA, 2024).[27] New York MetroCards cost $127 a month in 2022 and are valid only on days when school is in session, implying an annual savings of roughly $1,300 per formerly-transported student in New York (NYCDOE, 2022).[28]

Transportation cost savings might be used to improve school quality. The Jackson and Mackevicius (2024) meta-analysis of research on school spending suggests that, over the course of four years, $1,000 of additional annual spending can be expected to boost test scores by about 0.0316σ and increase college attendance rates by approximately 2.8 percentage points. In practice, we can't say how transportation savings might be used. But the possibility of considerable savings highlights the value of a fresh look at busing-resource trade-offs, especially as we've recently marked the 50th anniversary of the Garrity decision. Our findings are also noteworthy in light of an ongoing discussion of substantial travel-reducing revisions to the Chicago choice plan (see Harrington (2023) and Jackson (2023)).

A complete analysis of busing trade-offs should include the effect of neighborhood school assignment on overall district attendance. Some families may be attracted by neighborhood schools, but others may leave in response to limits on choice (Epple et al. (2014) explores these issues). Neighborhood assignment might also dilute incentives for school effectiveness, a possibility considered in, e.g., Card, Dooley and Payne (2010) and Campos and Kearns (2024). Investigations of these issues are natural directions for further work.

[27] Costs are adjusted to May 2022 dollars using the Consumer Price Index (Series ID = CUUR0000SA0). Student M7 MBTA pass' costs have been steady at $30 per month since 2019, according to the Internet Archive.

[28] These calculations differ from the sums reported in Figure 1, which include fixed costs and costs for elementary school students.

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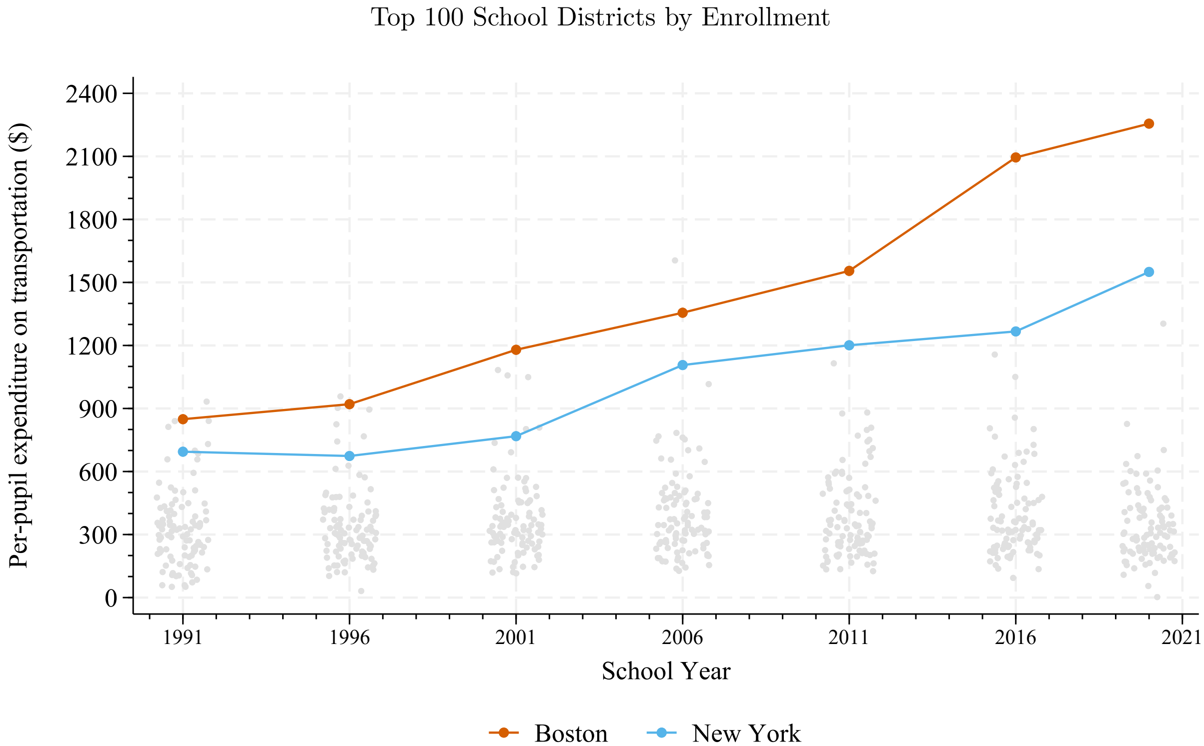
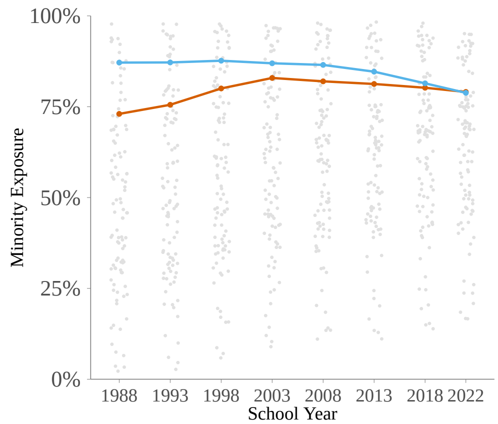
Figure I:
Per-Pupil Annual Expenditures on Student Transportation



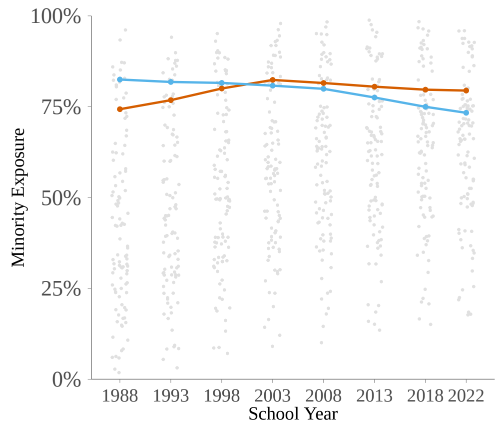
Notes: Transportation expenditure corresponds to the amount spent on the transportation of public school students, including vehicle operation, monitoring riders, and vehicle servicing and maintenance. Per-capita amounts are computed by dividing total costs by total district enrollment. Enrollment data used for the denominator are from the NCES and count the number of students for which the district is financially responsible. Fiscal year t is defined as the school year ending in t. Expenditure data is adjusted to June 2017 dollars using the Consumer Price Index (Series ID = CUUR0000SA0). The sample used here consists of schools in the 100 largest districts by enrollment. The set of districts meeting these criteria varies by year.
Source: National Center for Education Statistics (NCES) Common Core of Data.

Figure II:
Minority Exposure

(a) Top 100 School Districts by Enrollment

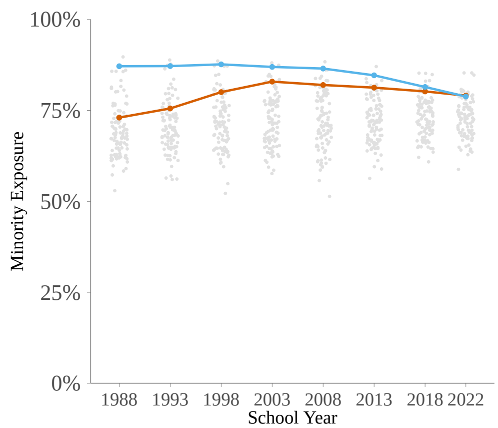


Black students

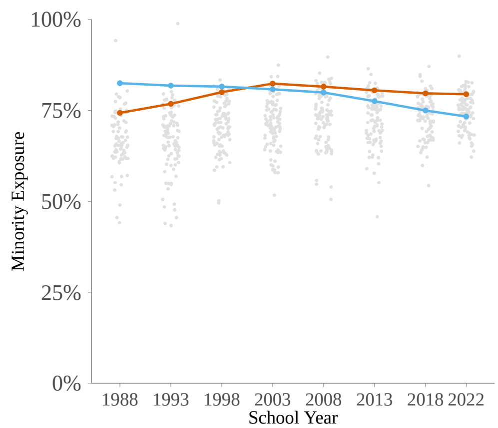


Hispanic students

(b) Top 100 School Districts by Enrollment with 60-80% Minority, School Year 1988-2022



Black students

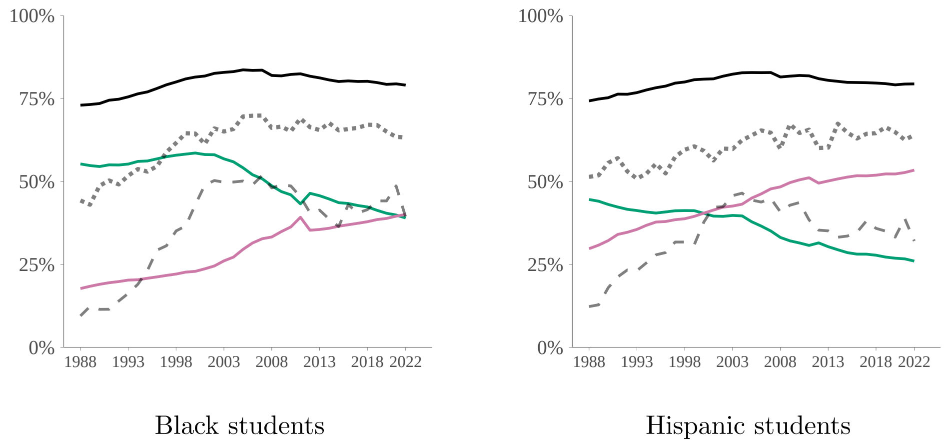


Hispanic students

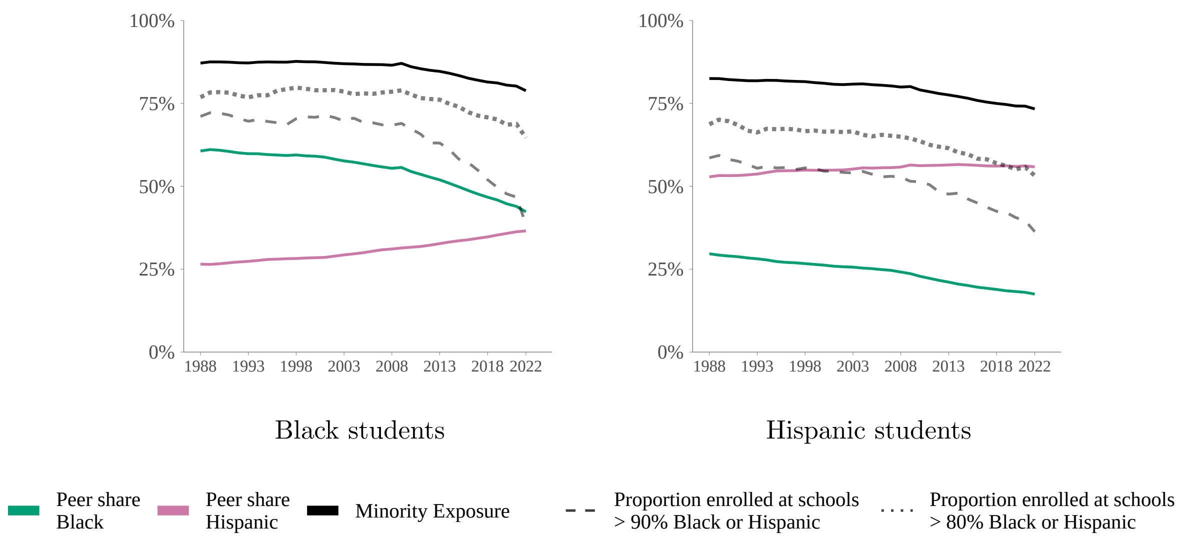
Notes: The sample of top 100 districts are as described in Figure I. Minority exposure is defined as the proportion of a student's classmates that are Black or Hispanic. Year t on the x-axis marks the school year starting in t. Schools with missing race data are omitted. New York Public Schools consist of schools in the aggregated New York City district code and the New York City community districts. Virtual, future, closed, or inactive schools are not included in the sample. The sample includes all grades, but does not include students in adult education. Black peers those categorized as "non-Hispanic, Black" in the Common Core.
Source: Common Core Public Elementary and Secondary School Universe Survey, documented in `https://nces.ed.gov/ccd/pubschuniv.asp`.

Figure III:
Racial Exposure in Boston and New York Public Schools, 1988-2022

(a) Boston Public Schools



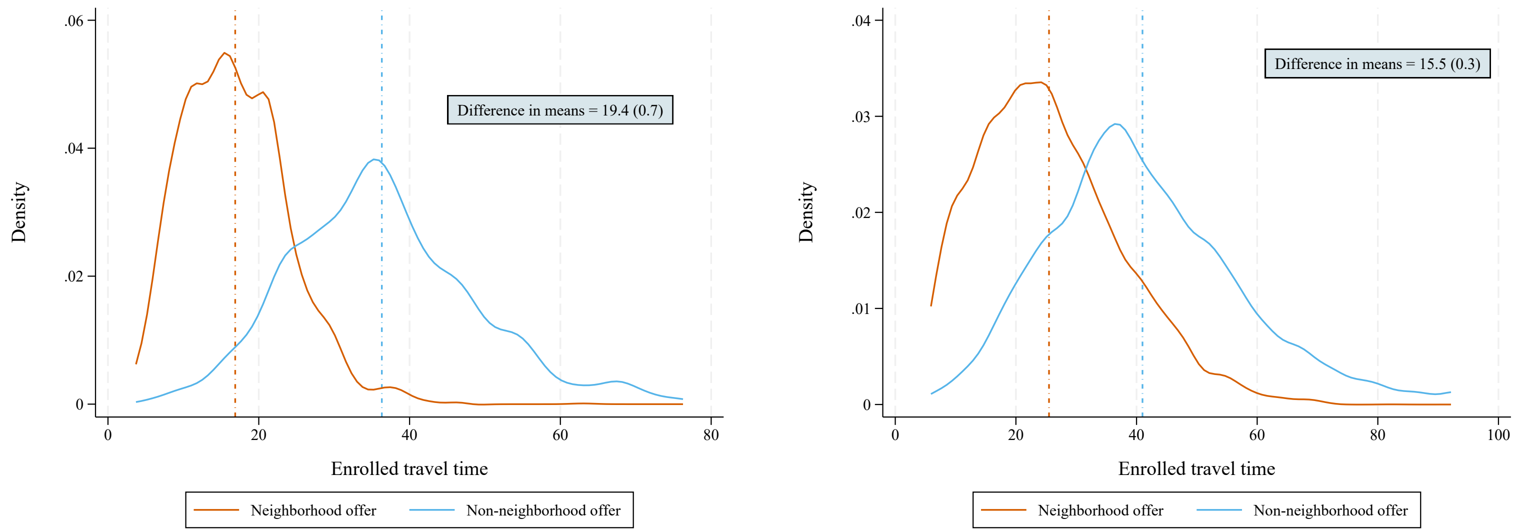
(b) New York Public Schools



Notes: Solid lines plot the proportion of a student's peers falling into various race groups. Minority exposure is as defined in Figure II. Dotted and dashed lines plot the proportion of students enrolled at schools with a student body that's either over 80% or over 90% Black or Hispanic. Variable definitions and sample restrictions are as for Figure II.

Source: Common Core Public Elementary and Secondary School Universe Survey, documented in `https://nces.ed.gov/ccd/pubschuniv.asp`.

Figure IV:
Travel Time Distributions for Non-Neighborhood Compliers



(i) Boston

(ii) New York

Notes: This figure plots the distributions of travel time (in minutes) for Boston and New York non-neighborhood compliers. Densities for non-neighborhood compliers are estimated using 2SLS, controlling for student demographics, baseline achievement, and offer risk. Densities for non-offered compliers are estimated by replacing attendance with one minus attendance in this 2SLS procedure. The model uses a Gaussian kernel and the Silverman (1986) rule of thumb bandwidth. Vertical dashed lines indicate mean potential outcomes.

Table I:
Boston and New York Analysis Samples

	Boston (6th and 9th grade)				New York (9th grade)			
	Enrolled sample	Applicant sample	Non-nbhd experimental sample	Travel experimental sample	Enrolled sample	Applicant sample	Non-nbhd experimental sample	Travel experimental sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Travel								
Ranked a non-nbhd school first	0.73	0.73	0.58	0.79	0.67	0.66	0.62	0.73
Enrolled in a non-nbhd school	0.74	0.70	0.55	0.77	0.64	0.65	0.54	0.70
Enrolled travel time	32.83	33.15	28.41	35.53	36.41	37.46	33.87	38.49
Enrolled distance	3.62	3.66	2.91	4.05	4.48	4.70	3.82	4.80
Eligible for busing	0.71	0.71	0.57	0.77	0.96	0.96	0.96	0.97
Race								
Black	0.41	0.42	0.41	0.46	0.29	0.27	0.26	0.30
Hispanic	0.36	0.39	0.42	0.39	0.40	0.39	0.46	0.44
White or Asian	0.21	0.18	0.16	0.13	0.29	0.33	0.28	0.25
Other covariates								
Free and reduced-price lunch	0.70	0.77	0.79	0.79	0.75	0.73	0.77	0.78
Female	0.49	0.50	0.49	0.50	0.49	0.51	0.50	0.52
Special education	0.19	0.17	0.19	0.18	0.18	0.07	0.08	0.08
Limited English proficiency	0.18	0.15	0.14	0.13	0.14	0.09	0.05	0.05
Baseline math	0.01	-0.10	-0.10	-0.15	-0.01	0.18	0.07	0.05
Baseline English	0.01	-0.08	-0.11	-0.12	-0.02	0.18	0.04	0.04
School sectors								
Enrolled in charter	0.17	0.10	0.10	0.11	0.05	0.04	0.04	0.04
Enrolled in exam	0.10	0.04	0.03	0.04	0.06	0.07	0.05	0.05
N	132,607	79,659	11,053	28,791	389,284	297,734	25,463	72,146

Notes: Statistics for Boston use data on middle school students enrolled in 6th grade and high school students enrolled in 9th grade in 2002-03 to 2017-18. Statistics for New York use data on high school students enrolled in 9th grade in 2012-13 to 2016-17. Columns 1 and 5 report descriptive statistics for the sample of enrolled students who have demographic information. Columns 2 and 6 report statistics for the sample of match applicants who have demographic information. The experimental samples in columns 3, 4, 7, and 8 restrict the applicant sample to offered students who have (i) non-degenerate risk of school assignment, (ii) non-missing baseline test scores, and (iii) non-missing geographic information (residential geocodes in Boston and census tracts or districts in New York). Columns 3 and 7 further subset to students with non-neighborhood school assignment risk, while columns 4 and 9 subset to students with travel risk. Boston baseline test scores are from the MCAS (4th grade Math and ELA for middle school, 8th grade Math and 7th/8th grade ELA for high school); New York baseline scores are 6th grade scores from the NY state standardized assessments. Charter and exam schools are considered to lie outside of a student's neighborhood. Travel time and distance are by public transit; units are in minutes and miles, respectively. Busing eligibility is defined as being enrolled at a school that has a driving distance of more than 1.5 miles in Boston and more than 0.5 miles in New York.

Table II:
OLS Estimates of Travel Effects

		Non-neighborhood Attendance			Travel Time (20 minutes)		
		All applicants	Black applicants	Hispanic applicants	All applicants	Black applicants	Hispanic applicants
		(1)	(2)	(3)	(4)	(5)	(6)
					A. Boston		
MCAS (Math & ELA)		0.0275	0.0463	0.0365	0.0249	0.0529	0.0148
		(0.0059)	(0.0094)	(0.0094)	(0.0030)	(0.0046)	(0.0050)
	N	107,468	42,938	37,989	106,924	42,609	37,813
Graduate on time		-0.0148	-0.0193	-0.0124	0.0007	0.0088	-0.0060
		(0.0061)	(0.0100)	(0.0094)	(0.0023)	(0.0037)	(0.0038)
	N	55,979	22,583	20,201	55,654	22,370	20,110
Any college		0.0169	0.0151	0.0210	0.0132	0.0200	0.0117
		(0.0057)	(0.0093)	(0.0087)	(0.0023)	(0.0035)	(0.0036)
Four-year college		0.0091	0.0004	0.0093	0.0105	0.0159	0.0087
		(0.0051)	(0.0081)	(0.0076)	(0.0020)	(0.0031)	(0.0032)
	N	55,745	22,441	20,125	55,421	22,228	20,035
					B. New York		
SAT (Math & Verbal)		0.0837	0.0585	0.0656	0.0506	0.0486	0.0610
		(0.0032)	(0.0062)	(0.0047)	(0.0016)	(0.0028)	(0.0027)
	N	259,218	67,277	94,741	256,817	66,557	93,885
Graduate on time		-0.0029	-0.0060	-0.0091	0.0026	0.0069	-0.0024
		(0.0016)	(0.0033)	(0.0026)	(0.0008)	(0.0015)	(0.0014)
	N	337,019	92,547	132,918	326,345	88,646	128,529
Any college		0.0078	0.0073	0.0023	0.0072	0.0124	0.0033
		(0.0018)	(0.0037)	(0.0029)	(0.0009)	(0.0016)	(0.0015)
Four-year college		0.0149	0.0099	0.0108	0.0114	0.0092	0.0117
		(0.0018)	(0.0035)	(0.0026)	(0.0009)	(0.0016)	(0.0015)
	N	353,706	100,576	140,397	342,922	96,631	135,987

Notes: This table reports OLS estimates of the relationship between non-neighborhood attendance or travel time and achievement, on time high school graduation, and college-going. The Boston sample includes students enrolled in the 2002-17 school years; the New York sample cover the population enrolled in the 2012-16 school years. All models control for student demographic characteristics, match participation, charter school attendance, and exam school attendance. Boston test scores are from the MCAS (6th grade Math and 7th grade ELA for middle school, 10th grade Math and ELA for high school); New York test scores are from the SAT. Approximately 70% of New York students take the SAT.

Table III:
2SLS Estimates of School Travel Effects on Peer Race

	Non-neighborhood Attendance			Travel Time (20 minutes)		
	All applicants	Black applicants	Hispanic applicants	All applicants	Black applicants	Hispanic applicants
	(1)	(2)	(3)	(4)	(5)	(6)
			A. Boston			
Peer share Black	-0.0283	-0.0778	-0.0078	-0.0266	-0.0612	0.0051
	(0.0098)	(0.0156)	(0.0153)	(0.0056)	(0.0075)	(0.0089)
Peer share Hispanic	0.0205	0.0578	-0.0085	0.0016	0.0232	-0.0223
	(0.0092)	(0.0134)	(0.0146)	(0.0050)	(0.0064)	(0.0085)
Peer share Black or Hispanic	-0.0078	-0.0200	-0.0163	-0.0250	-0.0380	-0.0173
	(0.0081)	(0.0115)	(0.0114)	(0.0043)	(0.0059)	(0.0062)
Minority isolation	-0.1426	-0.2221	-0.1086	-0.1371	-0.1950	-0.1010
	(0.0276)	(0.0475)	(0.0431)	(0.0161)	(0.0235)	(0.0262)
N	11,053	4,479	4,587	28,682	13,219	11,062
			B. New York			
Peer share Black	-0.0089	-0.0845	0.0105	-0.0132	-0.0683	0.0162
	(0.0040)	(0.0134)	(0.0051)	(0.0028)	(0.0064)	(0.0038)
Peer share Hispanic	-0.0235	0.0482	-0.0585	-0.0170	0.0212	-0.0486
	(0.0040)	(0.0105)	(0.0058)	(0.0026)	(0.0050)	(0.0040)
Peer share Black or Hispanic	-0.0324	-0.0363	-0.0480	-0.0301	-0.0471	-0.0324
	(0.0043)	(0.0098)	(0.0058)	(0.0028)	(0.0048)	(0.0041)
Minority isolation	-0.0508	-0.0341	-0.0883	-0.0597	-0.0950	-0.0769
	(0.0090)	(0.0272)	(0.0141)	(0.0060)	(0.0127)	(0.0100)
N	25,463	6,512	11,670	71,423	21,747	31,327

Notes: This table reports 2SLS estimates of non-neighborhood attendance and travel time effects on peer racial composition, computed in samples of Boston and New York experimental samples. In columns 1-3, the instrument is non-neighborhood assignment. The endogenous variable is 6th or 9th grade non-neighborhood attendance. The non-neighborhood first stages are approximately 0.44 for Boston and 0.66 for New York; race-specific first stages are similar. In columns 4-6, the instrument is travel time offered. The endogenous variable is 6th or 9th grade enrolled travel time. Travel time effects are per 20 minutes of travel. Travel time first stages are around 0.38 for Boston and 0.56 for New York. Models include control function μ_i, defined in Equation 5, as well as student demographic variables and baseline achievement. School peer shares are computed using samples of all enrolled students in 6th or 9th grade. Minority isolation is defined as enrolled at a school where the proportion of Black or Hispanic exceeds 90%.

Table IV:
2SLS Estimates of School Travel Effects on Achievement

		Non-neighborhood Attendance			Travel Time (20 minutes)		
		All applicants	Black applicants	Hispanic applicants	All applicants	Black applicants	Hispanic applicants
		(1)	(2)	(3)	(4)	(5)	(6)
A. Boston							
MCAS (Math & ELA)		-0.0076	0.0312	-0.0680	0.0094	0.0185	-0.0259
		(0.0363)	(0.0581)	(0.0554)	(0.0217)	(0.0319)	(0.0339)
	N	9,353	3,847	3,851	23,798	11,089	9,046
Graduate on time		-0.1025	-0.0906	-0.1289	-0.0373	-0.0501	-0.0466
		(0.0494)	(0.0886)	(0.0666)	(0.0215)	(0.0308)	(0.0336)
	N	5,049	1,757	2,409	17,518	7,948	7,122
Any college		0.0169	-0.0977	0.0155	-0.0142	-0.0556	-0.0096
		(0.0458)	(0.0836)	(0.0604)	(0.0207)	(0.0296)	(0.0318)
Four-year college		0.0334	-0.0331	0.0298	-0.0048	-0.0286	0.0000
		(0.0407)	(0.0719)	(0.0523)	(0.0182)	(0.0258)	(0.0279)
	N	5,018	1,740	2,400	17,410	7,887	7,089
B. New York							
SAT (Math & Verbal)		0.0017	0.0360	0.0239	0.0053	-0.0003	0.0155
		(0.0145)	(0.0351)	(0.0205)	(0.0089)	(0.0156)	(0.0137)
	N	19,079	4,496	8,315	52,506	14,709	21,920
Graduate on time		-0.0169	-0.0164	-0.0338	-0.0131	-0.0055	-0.0245
		(0.0089)	(0.0242)	(0.0147)	(0.0056)	(0.0114)	(0.0097)
	N	22,696	5,562	10,299	63,476	18,562	27,637
Any college		-0.0221	-0.0473	-0.0443	-0.0267	-0.0198	-0.0493
		(0.0115)	(0.0294)	(0.0180)	(0.0072)	(0.0136)	(0.0120)
Four-year college		-0.0145	-0.0205	-0.0299	-0.0204	-0.0100	-0.0347
		(0.0114)	(0.0287)	(0.0170)	(0.0071)	(0.0132)	(0.0112)
	N	23,583	5,942	10,771	66,171	19,813	28,956

Notes: This table reports 2SLS estimates of non-neighborhood attendance and travel time effects on student achievement, on time high school graduation, and college attendance, computed in Boston and New York experimental samples. The sample, instrument, endogenous variable, and controls are as described in Table III.

Table V:
2SLS Estimates of School Travel Effects by Desired Travel

	MCAS (Math & ELA) / SAT (Math & Verbal)		Graduate on time		Any college		Four-year college	
	All applicants	Black or Hispanic	All applicants	Black or Hispanic	All applicants	Black or Hispanic	All applicants	Black or Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Boston								
Travel time	0.0106	0.0091	-0.0322	-0.0408	-0.0094	-0.0291	-0.0006	-0.0099
	(0.0251)	(0.0268)	(0.0231)	(0.0245)	(0.0225)	(0.0236)	(0.0200)	(0.0208)
First choice in neighborhood	-0.0537	-0.0340	-0.0135	-0.0136	-0.0073	-0.0075	-0.0164	-0.0133
	(0.0297)	(0.0327)	(0.0376)	(0.0399)	(0.0349)	(0.0365)	(0.0300)	(0.0305)
Travel time* First choice in neighborhood	-0.0100	-0.0390	-0.0480	-0.0543	-0.0459	-0.0499	-0.0419	-0.0484
	(0.0518)	(0.0553)	(0.0662)	(0.0683)	(0.0585)	(0.0586)	(0.0485)	(0.0465)
N	23,798	20,135	17,518	15,070	17,410	14,976	17,410	14,976
B. New York								
Travel time	0.0098	0.0165	-0.0092	-0.0097	-0.0263	-0.0352	-0.0159	-0.0221
	(0.0103)	(0.0113)	(0.0066)	(0.0082)	(0.0084)	(0.0099)	(0.0082)	(0.0094)
First choice in neighborhood	0.0332	0.0550	0.0159	0.0190	0.0091	0.0087	0.0126	0.0122
	(0.0156)	(0.0176)	(0.0099)	(0.0127)	(0.0125)	(0.0152)	(0.0122)	(0.0144)
Travel time* First choice in neighborhood	-0.0141	-0.0326	-0.0128	-0.0202	0.0001	0.0046	-0.0142	-0.0034
	(0.0203)	(0.0244)	(0.0124)	(0.0168)	(0.0162)	(0.0207)	(0.0162)	(0.0201)
N	52,506	36,629	63,476	46,199	66,171	48,769	66,171	48,769

Notes: This table reports 2SLS estimates of travel time effects on student achievement, on time high school graduation, and college attendance separately for students whose first choice is in neighborhood and those whose first choice is out of neighborhood. The sample, instrument, endogenous variable, and controls are as described in Table III.

Table VI:
Effects of Offered Travel on RC VAM for Education Outcomes

		Boston			New York		
		All applicants	Black applicants	Hispanic applicants	All applicants	Black applicants	Hispanic applicants
		(1)	(2)	(3)	(4)	(5)	(6)
MCAS (Math and ELA) / SAT (Math and Verbal)		0.0075	0.0112	0.0051	0.0107	0.0122	0.0079
		(0.0025)	(0.0036)	(0.0039)	(0.0016)	(0.0025)	(0.0021)
	N	24,016	6,535	5,650	52,815	14,792	22,034
Graduation on time		-0.0017	-0.0046	0.0008	-0.0014	0.0016	-0.0028
		(0.0010)	(0.0015)	(0.0016)	(0.0007)	(0.0015)	(0.0012)
	N	17,711	8,063	7,187	63,900	18,695	27,791
Any college		0.0025	0.0016	0.0030	-0.0007	0.0030	-0.0029
		(0.0011)	(0.0016)	(0.0018)	(0.0007)	(0.0014)	(0.0012)
	N	17,601	8,001	7,153	66,625	19,935	29,144

Notes: This table reports first-stage effects of offered travel time on enrolled value-added for Boston middle and high school applicants and New York high school applicants. The sample is as described in Table III. Models include control functions μ_i, as defined in Equation 5, as well as student demographic variables and baseline achievement. The risk-controlled value-added computation follows that in Angrist et al. (2024).

Table VII:
School Value-Added and Travel Effects in New York

		All applicants	Black applicants	Hispanic applicants
		(1)	(2)	(3)
			A. SAT (Math & Verbal)	
Travel time		-0.0097	-0.0309	0.0068
		(0.0091)	(0.0156)	(0.0143)
Value-added		0.7673	0.9810	0.7444
		(0.0511)	(0.1003)	(0.0720)
Average travel time		0.0235	0.0666	0.0065
		(0.0178)	(0.0352)	(0.0253)
	N	52,443	14,669	21,890
			B. On-time Graduation	
Travel time		-0.0155	-0.0088	-0.0265
		(0.0059)	(0.0114)	(0.0102)
Value-added		0.5821	0.6504	0.6171
		(0.0502)	(0.0983)	(0.0737)
Average travel time		0.0039	-0.0027	0.0019
		(0.0115)	(0.0244)	(0.0179)
	N	63,351	18,478	27,588
			C. Any College	
Travel time		-0.0233	-0.0204	-0.0433
		(0.0076)	(0.0137)	(0.0126)
Value-added		0.8278	0.8627	0.7464
		(0.0548)	(0.1029)	(0.0792)
Average travel time		-0.0143	-0.0001	-0.0066
		(0.0150)	(0.0300)	(0.0226)
	N	65,955	19,679	28,865

Notes: This table reports 2SLS estimates of the effects of travel time and value-added on student achievement, on time high school graduation, and college attendance for New York high school applicants. The estimates come from an over-identified model that instruments enrolled travel time, enrolled value-added, and average travel time to enrolled school with individual school offer dummies and offered travel time, scaled in 20 minute increments. The sample is as described in Table III. Models include control functions μ_i, as defined in Equation 5, as well as school-level propensity scores, running variable controls, student demographic variables, and baseline achievement. The risk-controlled value-added computation follows that in Angrist et al. (2024).

Table VIII:
Neighborhood Reassignment

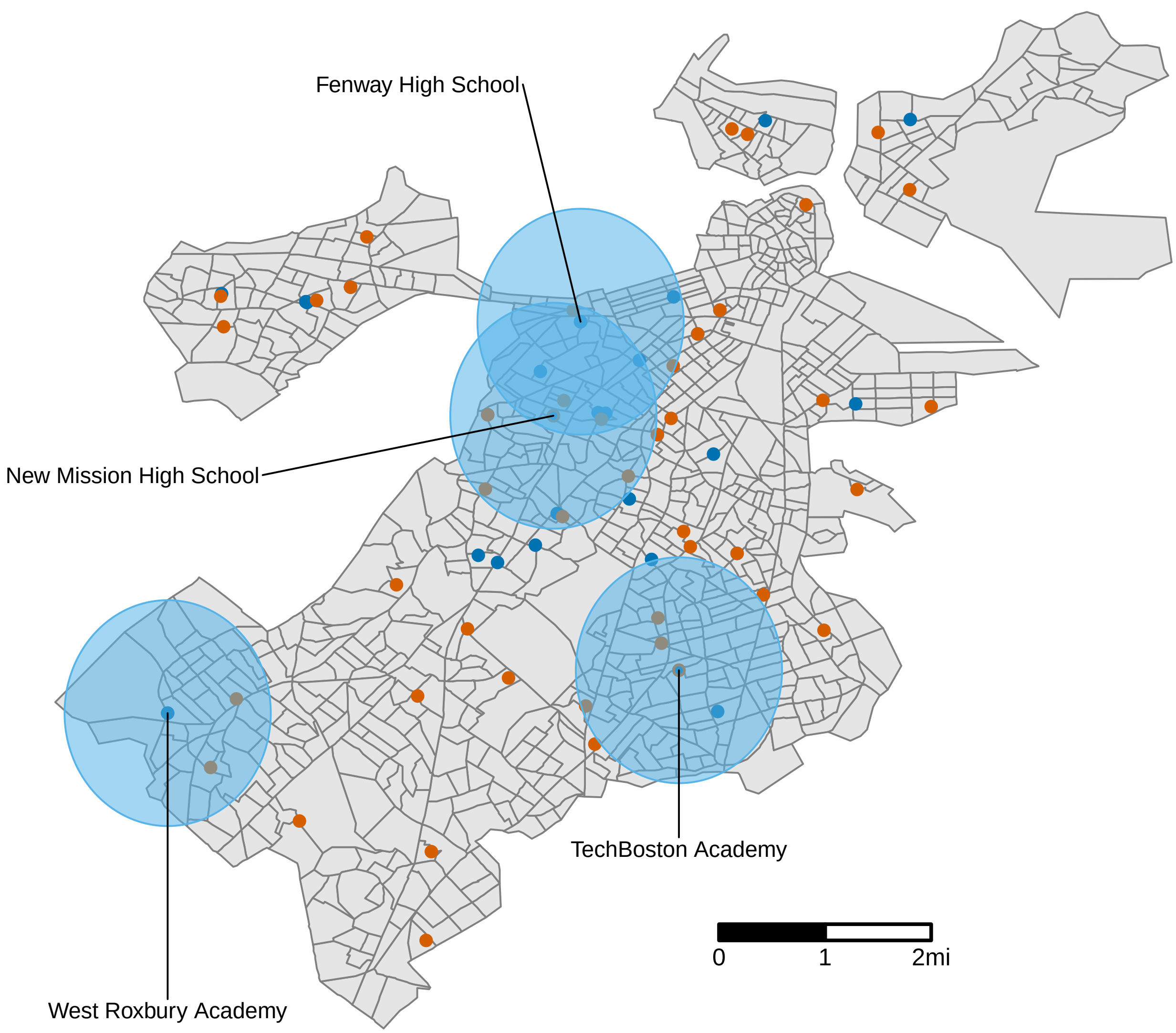
	Observed Enrollment				Simulated Changes			
	Time	Busing eligibility	Same-race exposure	Minority isolation	Time	Busing eligibility	Same-race exposure	Minority isolation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					A: Boston (6th and 9th grade)			
Black students	34.1	0.74	0.494	0.444	-12.9	-0.48	0.015	0.061
Hispanic students	31.2	0.66	0.480	0.371	-13.2	-0.47	-0.016	-0.022
Black or Hispanic students	32.6	0.70	0.837	0.408	-13.1	-0.47	0.001	0.019
					B: New York (9th grade)			
Black students	39.8	0.97	0.468	0.493	-17.0	-0.23	0.054	0.040
Hispanic students	33.7	0.95	0.532	0.406	-13.6	-0.23	0.012	0.000
Black or Hispanic students	36.2	0.96	0.790	0.442	-15.0	-0.23	0.006	0.016

Notes: The Boston sample includes students enrolled in the 2006-13 school years; the New York sample includes students enrolled in the 2012-16 school years. The baseline scenario reflects student-weighted average enrolled school characteristics among the sample of students who: (i) participate in the match and enroll in a match school, or (ii) do not participate in the match but enroll in a match school and have non-missing geographic information. Columns 1-4 report these baseline statistics. Columns 5-8 characterize simulated alternative assignments generated by a match in which students and schools rank each other by proximity (where proximity is defined in terms of driving distance). School capacities in this simulation are set to equal maximum observed enrollment (in 2001-2016 for Boston and in 2009-2019 for New York). The statistics in columns 5-8 are changes relative to columns 1-4. Travel time is by public transit. Same-race exposure is defined as the proportion same-race in the assigned school (in the same grade). Minority isolation is defined as in Table III. Students are deemed busing-eligible when their driving distance is at least 1.5 miles in Boston and at least 0.5 miles in New York.

A Appendix

A Additional Figures and Tables

Figure A1: Boston Geocodes and Walk Zones



Notes: Lines mark geocode boundaries. Blue shading marks a few school walk zones. Blue dots mark Boston high schools in 2013. Red dots mark Boston middle schools in 2013.

Table A1: OLS and 2SLS Sample Construction

	Boston		New York	Sample
	6th grade	9th grade	9th grade	
	(1)	(2)	(3)	(4)
A: OLS				
All enrolled students with demographic information	76,543	84,073	394,777	
With geographic information	63,475	69,132	389,284	Enrolled sample
B: 2SLS				
Applicants in the match with demographic information	36,500	43,198	298,840	
With geographic information	36,490	43,169	297,734	Applicant sample
Ranked at least two programs, the first over-subscribed	30,519	38,426	269,734	
Doesn't clear marginal priority at first choice	16,971	26,927	172,035	
Has non-degenerate risk of school assignment	14,284	22,354	101,939	
Who are offered a seat	12,926	20,672	90,012	
Enroll in a school	12,501	19,800	83,952	
With baseline scores	11,326	18,087	75,672	
Have non-neighborhood assignment risk	6,004	5,049	25,463	Non-nbhd experimental sample
Have travel assignment risk	11,168	17,623	72,146	Travel experimental sample

Notes: This table illustrates the construction of the OLS and 2SLS samples. The OLS sample starts from the sample of all enrolled students with demographic information and excludes students with missing geographic information (geocodes in Boston and residential districts in New York). The 2SLS sample starts with the subset of all match applicants with demographic information and after following the sample restrictions described in the rows of the table, splits into two experimental samples.

Table A2: Attrition and Covariate Balance

		Boston			New York		
		Mean	Non-nbhd assignment	Travel time	Mean	Non-nbhd assignment	Travel time
		(1)	(2)	(3)	(4)	(5)	(6)
				A: Attrition and selection into other sectors			
Has outcome MCAS (Math & ELA) / SAT (Math & Verbal)		0.828	0.0020	0.0002	0.695	-0.0143	-0.0084
			(0.0092)	(0.0051)		(0.0067)	(0.0037)
	N		12,635	32,967		30,579	85,185
Charter enrolled		0.095	-0.0002	0.0056	0.04	0.0014	0.0030
			(0.0071)	(0.0038)		(0.0029)	(0.0017)
	N		12,171	31,618		28,477	79,512
Exam enrolled		0.074	0.0019	0.0025	0.048	-0.0020	-0.0021
			(0.0096)	(0.0039)		(0.0032)	(0.0017)
	N		5,556	19,290		28,477	79,512
				B: Baseline covariates			
Black		0.405	0.0044	0.0048	0.256	0.0010	0.0020
			(0.0128)	(0.0067)		(0.0064)	(0.0037)
Hispanic		0.415	0.0042	-0.0035	0.458	0.0059	0.0048
			(0.0127)	(0.0066)		(0.0075)	(0.0040)
Female		0.494	0.0012	-0.0039	0.497	0.0005	-0.0038
			(0.0131)	(0.0068)		(0.0077)	(0.0040)
Special education		0.188	-0.0071	-0.0043	0.077	0.0028	-0.0001
			(0.0102)	(0.0052)		(0.0042)	(0.0023)
Limited English proficiency		0.143	-0.0029	-0.0012	0.055	0.0020	-0.0037
			(0.0086)	(0.0042)		(0.0035)	(0.0016)
Free and reduced-price lunch		0.793	0.0022	0.0010	0.772	-0.0018	-0.0070
			(0.0103)	(0.0055)		(0.0068)	(0.0037)
Baseline math		-0.097	0.0191	0.0011	0.07	-0.0109	0.0023
			(0.0236)	(0.0120)		(0.0113)	(0.0060)
Baseline English		-0.114	-0.0131	-0.0089	0.04	-0.0101	0.0034
			(0.0245)	(0.0126)		(0.0124)	(0.0064)
	N	11,053	11,053	28,682	25,463	25,463	71,423

Notes: This table reports coefficients from regressions of the variables listed in each row on distance and travel instruments. Column 1 and 4 report sample means for each dependent variable. The independent variable in columns 2 and 5 is a non-neighborhood school assignment. The independent variable in columns 3 and 6 is offered travel time. Estimates in columns 2, 3, 5, and 6 are computed in the Boston and New York risk samples corresponding to the independent variable. The instruments and controls are as in Table III. Travel time effects are per 20 minutes of travel. Exam school enrollment is computed in the sample of 9th grade applicants in Boston.

Table A3: 2SLS Estimates of Quadratic School Travel Effects on Test Scores and College Attendance

		All applicants		Black applicants		Hispanic applicants	
		Travel Time	Travel Time Squared	Travel Time	Travel Time Squared	Travel Time	Travel Time Squared
		(1)	(2)	(3)	(4)	(5)	(6)
				A. Boston			
MCAS (Math & ELA)		-0.0191	0.0004	-0.0159	0.0004	-0.0351	0.0001
		(0.0706)	(0.0009)	(0.1026)	(0.0013)	(0.1151)	(0.0015)
	N	23,798	23,798	11,089	11,089	9,046	9,046
On time graduation		-0.0269	-0.0001	-0.0561	0.0001	-0.0802	0.0004
		(0.0821)	(0.0009)	(0.1286)	(0.0014)	(0.1283)	(0.0015)
	N	17,518	17,518	7,948	7,948	7,122	7,122
Any college		0.0863	-0.0012	0.0116	-0.0008	0.1143	-0.0015
		(0.0785)	(0.0009)	(0.1206)	(0.0013)	(0.1213)	(0.0015)
Four-year college		0.0839	-0.0011	0.0761	-0.0012	0.1597	-0.0020
		(0.0710)	(0.0008)	(0.1073)	(0.0012)	(0.1089)	(0.0013)
	N	17,410	17,410	7,887	7,887	7,089	7,089
				B. New York			
SAT (Math & Verbal)		0.0218	-0.0002	0.1086	-0.0012	-0.0036	0.0002
		(0.0273)	(0.0003)	(0.0553)	(0.0006)	(0.0428)	(0.0006)
	N	52,506	52,506	14,709	14,709	21,920	21,920
On time graduation		-0.0308	0.0002	0.0008	-0.0001	-0.0567	0.0004
		(0.0172)	(0.0002)	(0.0389)	(0.0004)	(0.0307)	(0.0004)
	N	63,476	63,476	18,562	18,562	27,637	27,637
Any college		-0.0454	0.0002	-0.0780	0.0006	-0.0472	-0.0000
		(0.0219)	(0.0003)	(0.0464)	(0.0005)	(0.0372)	(0.0005)
Four-year college		-0.0279	0.0001	-0.0266	0.0002	-0.0660	0.0004
		(0.0219)	(0.0003)	(0.0453)	(0.0005)	(0.0356)	(0.0005)
	N	66,171	66,171	19,813	19,813	28,956	28,956

Notes: This table reports 2SLS estimates of quadratic travel time effects on student achievement and college attendance, computed in samples of Boston middle and high school applicants and New York high school applicants. The instruments are offered travel time and travel time squared, and the endogenous are enrolled travel time and travel time squared. The sample and controls are as described in Table III. Standardized test outcomes are as described in Table II.

Table A4: 2SLS Estimates of School Travel Effects on Behavioral Outcomes

		Non-neighborhood Attendance			Travel Time (20 minutes)		
		All applicants	Black applicants	Hispanic applicants	All applicants	Black applicants	Hispanic applicants
		(1)	(2)	(3)	(4)	(5)	(6)
					A. Boston		
Days absent		1.9263	-1.8561	4.9002	1.4376	1.0948	1.8466
		(2.3267)	(3.5897)	(3.4104)	(1.4483)	(2.0642)	(2.2367)
	Mean	24.162	23.161	24.625	25.850	25.248	26.878
	SD	41.21	40.50	39.23	42.00	41.72	41.13
	N	10,920	4,426	4,536	28,291	13,054	10,905
Number of suspensions		0.0133	-0.0130	0.0709	0.0090	0.0441	0.0048
		(0.0675)	(0.1329)	(0.0880)	(0.0427)	(0.0649)	(0.0687)
	Mean	0.272	0.381	0.228	0.306	0.393	0.265
	SD	1.195	1.474	1.011	1.849	1.945	1.957
Disciplinary index		0.0677	0.0236	0.1251	0.0356	0.0376	0.0443
		(0.0491)	(0.0814)	(0.0734)	(0.0294)	(0.0439)	(0.0453)
	Mean	-0.086	-0.112	-0.012	-0.054	-0.076	0.022
	SD	0.948	0.944	0.985	0.992	0.984	1.025
	N	10,276	4,093	4,342	26,600	12,104	10,410
					B. New York		
Days absent		0.1726	-0.1678	0.7345	0.8116	0.5618	1.4364
		(0.4441)	(1.2389)	(0.7326)	(0.2917)	(0.6031)	(0.5012)
	Mean	12.190	13.925	14.112	12.961	14.759	14.896
	SD	19.06	20.97	20.29	19.90	21.54	21.02
	N	25,428	6,507	11,660	71,422	21,747	31,327
Number of suspensions		-0.0058	-0.0177	-0.0041	-0.0047	0.0064	-0.0132
		(0.0082)	(0.0300)	(0.0117)	(0.0059)	(0.0148)	(0.0082)
	Mean	0.069	0.133	0.063	0.080	0.147	0.067
	SD	0.365	0.527	0.336	0.408	0.564	0.358
	N	25,463	6,512	11,670	71,423	21,747	31,327

Notes: This table reports 2SLS estimates of distance and travel effects on behavioral outcomes, computed in the samples of Boston middle and high school applicants and New York high school applicants. The samples, instruments, and controls are as in Table III. The corresponding endogenous variables are 6th or 9th grade non-neighborhood attendance and enrolled travel time. Travel time effects are per 20 minutes of travel. Outcomes are measured in either 6th or 9th grade. Following Jackson (2018), the disciplinary index equals the first principal component of the following outcomes: ever being suspended, number of suspensions, ever being truant, number of days truant, ever attending a DYS school, and number of days absent. The index is standardized to have mean zero and standard deviation one among all enrolled students. When constructing the index, outcomes are coded so that a positive estimate reflects an increase in discipline.

Table A5: 2SLS Estimates of School Travel Effects on School Characteristics

		Non-neighborhood Attendance			Travel Time (20 minutes)		
		All applicants	Black applicants	Hispanic applicants	All applicants	Black applicants	Hispanic applicants
		(1)	(2)	(3)	(4)	(5)	(6)
					A. Boston		
Student-teacher ratio		-0.0017	-0.0089	0.2120	0.4317	0.6983	0.3809
		(0.1509)	(0.2570)	(0.2142)	(0.1050)	(0.1561)	(0.1556)
	Mean	12.808	12.586	12.759	12.960	12.872	12.816
	SD	2.871	2.971	2.624	3.062	3.167	2.869
Percent of teachers licensed in teaching assignment		0.0066	-0.0044	0.0106	0.0020	-0.0055	0.0088
		(0.0082)	(0.0142)	(0.0118)	(0.0049)	(0.0074)	(0.0072)
	Mean	0.901	0.881	0.913	0.902	0.888	0.911
	SD	0.140	0.160	0.125	0.134	0.147	0.121
Percent of core academic classes taught by highly-qualified teachers		0.0140	0.0086	0.0178	0.0094	0.0142	-0.0001
		(0.0052)	(0.0093)	(0.0074)	(0.0033)	(0.0050)	(0.0048)
	Mean	0.876	0.872	0.877	0.880	0.878	0.879
	SD	0.118	0.126	0.114	0.114	0.118	0.112
	N	9,073	3,789	3,647	23,506	11,157	8,766
					B. New York		
Student-teacher ratio		0.1918	0.0581	0.2794	0.2112	0.1230	0.3268
		(0.0323)	(0.0915)	(0.0487)	(0.0217)	(0.0487)	(0.0327)
	Mean	4.422	4.327	4.252	4.451	4.347	4.319
	SD	1.378	1.572	1.359	1.377	1.523	1.370
Percent of teachers licensed in teaching assignment		0.0140	0.0272	0.0182	0.0148	0.0148	0.0202
		(0.0035)	(0.0096)	(0.0055)	(0.0022)	(0.0043)	(0.0038)
	Mean	0.789	0.767	0.771	0.787	0.766	0.773
	SD	0.157	0.164	0.164	0.160	0.168	0.167
Percent of core academic classes taught by highly-qualified teachers		0.0075	0.0024	0.0194	0.0097	0.0046	0.0172
		(0.0027)	(0.0072)	(0.0043)	(0.0016)	(0.0032)	(0.0029)
	Mean	0.856	0.845	0.842	0.857	0.847	0.845
	SD	0.106	0.110	0.112	0.106	0.109	0.112
	N	19,026	4,855	8,744	53,857	16,367	23,682

Notes: This table reports 2SLS estimates of distance and travel effects on school characteristics, computed in the samples of Boston middle and high school applicants and New York high school applicants. The sample, instruments, and controls are as in Table III. The corresponding endogenous variables are 6th or 9th grade non-neighborhood attendance and travel time. Travel time effects are per 20 minutes of travel. The sample is limited to offered applicants with travel time and non-neighborhood risk respectively.

Table A6: School Value-Added and Travel Effects in Boston

		All Applicants	Black Applicants	Hispanic Applicants
		(1)	(2)	(3)
			A. MCAS (Math & ELA)	
Travel time		-0.0202	-0.0063	-0.1137
		(0.0250)	(0.0337)	(0.0423)
Value-added		0.8545	0.7029	0.8952
		(0.1364)	(0.2006)	(0.1960)
Average travel time		0.0312	-0.0081	0.1555
		(0.0429)	(0.0530)	(0.0795)
	N	23,556	10,931	8,992
			B. On-time Graduation	
Travel time		-0.0219	-0.0208	-0.0287
		(0.0268)	(0.0379)	(0.0427)
Value-added		0.4715	0.6152	0.0545
		(0.3269)	(0.4484)	(0.5333)
Average travel time		-0.0064	-0.0140	-0.0203
		(0.0397)	(0.0516)	(0.0696)
	N	17,328	7,841	7,068
			C. Any College	
Travel time		0.0036	-0.0324	-0.0310
		(0.0258)	(0.0357)	(0.0408)
Value-added		0.4919	0.2527	0.5979
		(0.2767)	(0.3554)	(0.4764)
Average travel time		-0.0201	-0.0095	0.0487
		(0.0382)	(0.0491)	(0.0666)
	N	17,220	7,780	7,035

Notes: This table reports 2SLS estimates of the effects of travel time and value-added on student achievement, on time high school graduation, and college attendance for Boston high school applicants. The estimates come from an over-identified model that instruments enrolled travel time, enrolled value-added, and average travel time to enrolled school with individual school offer dummies and offered travel time, scaled in 20 minute increments. The sample is as described in Table III. Models include control functions μ_i, as defined in Equation 5, as well as school-level propensity scores, running variable controls, student demographic variables, and baseline achievement. The risk-controlled value-added computation follows that in Angrist et al. (2024).

B 2SLS Inference with an Estimated Control Function

Section B discusses 2SLS estimates of distance and travel effects with first and second stages that can be written:

$$G_i = \gamma Z_i + \kappa_1 \mu_i + \xi_{1i},$$
$$Y_i = \beta G_i + \kappa_2 \mu_i + \xi_{2i}.$$

This appendix derives the limiting distribution of a 2SLS estimator, denoted $\hat{\beta}_{2SLS}$, computed by replacing the control function, μ_i, with an estimate, $\hat{\mu}_i$.

Replacing μ_i with $\hat{\mu}_i$ in the second stage equation, we have:

$$Y_i = \beta G_i + \kappa_2 \hat{\mu}_i + (\nu_{2i} + \kappa_2(\mu_i - \hat{\mu}_i)).$$

Define

$$\tilde{Z}_i = (I - P_{\hat{\mu}})Z_i,$$

where $P_{\hat{\mu}}$ is the matrix that projects onto $\hat{\mu}_i$, so that $\sum_i \tilde{Z}_i \hat{\mu}_i = 0$ by construction. Then, $\hat{\beta}_{2SLS}$ can be written:

$$\begin{aligned}
\hat{\beta}_{2SLS} &= \frac{\sum_i \tilde{Z}_i Y_i}{\sum_i \tilde{Z}_i G_i} \\
&= \beta + \frac{\sum_i \tilde{Z}_i(\xi_{2i} + \kappa_2(\mu_i - \hat{\mu}_i))}{\sum_i \tilde{Z}_i G_i} \\
&= \beta + \frac{\sum_i \tilde{Z}_i(\xi_{2i} + \kappa_2 \mu_i)}{\sum_i \tilde{Z}_i G_i} \\
&= \beta + \frac{\sum_i \tilde{Z}_i u_i}{\sum_i \tilde{Z}_i G_i},
\end{aligned}$$

where $u_i = \xi_{2i} + \kappa_2 \mu_i$. Given random sampling, the limiting distribution of our 2SLS estimator has sampling variance proportional to $E[\tilde{Z}_i^2 u_i^2]$.

Note that the residual needed for this formula is consistently estimated by $\hat{u}_i = Y_i - \hat{\beta}_{2SLS}$. Our calculation uses the residual generated by 2SLS, however, that is, $\hat{\nu}_{2i} = Y_i - \hat{\beta}_{2SLS} - \kappa_2 \hat{\mu}_i$. In practice, the distinction between the two residuals matters little for estimated standard errors. This is apparent from a comparison of the standard deviation of the two residuals. These estimated standard deviations are less than 1% apart for both test scores and college enrollment outcomes in Boston and New York.