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Still Worth the Trip? School Busing Effects in Boston and New York

Joshua Angrist
Guthrie Gray-Lobe
Clemence M. Idoux
Parag A. Pathak

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MIT Department of Economics
77 Massachusetts Avenue, Bldg. E53-390
Cambridge, MA 02139

National Bureau of Economic Research
1050 Massachusetts Avenue, 3rd Floor
Cambridge, MA 02138

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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, perhaps enhancing integration. Urban school transportation is increasingly costly, however, and has unclear integration and education consequences. We estimate the causal effects of non-neighborhood school enrollment and school travel on integration, achievement, and college enrollment using an identification strategy that exploits partly-random assignment in the Boston and New York school matches. Instrumental variables estimates suggest distance and travel boost integration for those who choose to travel, but have little or no effect on test scores and college attendance. We argue that small effects on educational outcomes reflect modest effects of distance and travel on school quality as measured by value-added.

Joshua Angrist
Department of Economics, E52-436
MIT
77 Massachusetts Avenue
Cambridge, MA 02139
and NBER
angrist@mit.edu

Guthrie Gray-Lobe
University of Chicago
graylobe@uchicago.edu

Clemence M. Idoux
MIT
cidoux@mit.edu

Parag A. Pathak
Department of Economics, E52-426
MIT
77 Massachusetts Avenue
Cambridge, MA 02139
and NBER
ppathak@mit.edu

A data appendix is available at http://www.nber.org/data-appendix/w30308

1 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.
> *Swann v. Charlotte-Mecklenburg Board of Education, 402 U.S. 1, 1971*

One of the most polarizing policy debates in American cities concerns the impact of school assignment on racial integration in public schools. Neighborhood-based assignment, once common in cities and still the norm in suburban school districts, necessarily reflects patterns of residential segregation. Non-neighborhood school assignment may mitigate the segregating effects of zoned neighborhood assignment. At the same time, travel to non-neighborhood schools can be costly. This is documented in Figure 1, which plots average annual per-pupil transportation expenditure in the 100 largest US school districts (by enrollment) for 1997-2017. The Boston and New York City school districts are at or near the top of per-pupil transportation costs, spending \$1,200-\$2,100 per student (in 2017 dollars) annually. Boston and New York transportation spending is also growing: the cost of getting kids to school in these bellwether cities roughly doubled in the two decades covered by the figure.

School transportation expenditures today are driven in part by the fact that many large urban school districts allow families to choose schools district-wide, lengthening school commutes for some. 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 in the form of a rank-order list 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. Choice in large urban districts is appealing because choice systems potentially decouple school assignment from underlying residential segregation. Moreover, where school quality is unevenly distributed over neighborhoods, district-wide choice affords all students a shot at schools viewed as high-quality.[1]

This paper asks whether school travel in the modern choice paradigm is working as hoped, boosting integration and learning, especially for minority students. Our investigation focuses on Boston and New York, two cities of special interest because of their high transportation costs and because they've long been battlegrounds in the fight over school integration. We estimate the effects of non-neighborhood school enrollment for students for whom school travel is facilitated by school choice. In both cities, students who opt for non-neighborhood schooling have higher test scores and

[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).

are more likely to go to college than those who travel less. But these estimates may reflect selection bias arising from the fact that more motivated or better-off families are more likely to travel.

We solve the problem of selection bias using the conditional random assignment to schools embedded in Boston and New York's school matching algorithms. A given student may be offered a seat at a school in his or neighborhood, or a seat farther away. Conditional on an applicant's preferences and school priorities, modern choice algorithms randomize seat assignment, thereby manipulating distance and travel independently of potential outcomes. The estimation strategy that exploits this variation builds on the propensity score and instrumental variables methods developed in Abdulkadiroğlu et al. (2017) and Abdulkadiroğlu et al. (2022). These methods are extended here to causal models where the control function needed to eliminate omitted variables bias depends on a multinomial propensity score as well as a vector of observed covariates.[2] This extension addresses the fact that non-neighborhood assignment depends on an applicant's residential location as well as on their assigned school.

Instrumental variables (IV) estimates using conditionally randomized school offers as instruments for school travel show that the modern choice paradigm helps integrate schools. Minority applicants who travel enroll in schools with fewer minority peers as a result. About forty percent of our sample of students attend schools that have more than 90% Black or Hispanic peers, a measure of minority isolation.[3] Non-neighborhood school enrollment decreases minority isolation markedly. For Black Boston students, in particular, non-neighborhood attendance reduces the probability of attending an isolated school by 17 percentage points. The integrating effects of non-neighborhood assignment, however, do not appear to increase student achievement or college-going. IV estimates of non-neighborhood school effects on achievement are close to zero and mostly precise enough to rule out modest positive effects. Non-neighborhood school attendance likewise appears to leave college attendance unchanged. Models that parameterize non-neighborhood enrollment by travel time generate similar results.

Non-neighborhood schools may change the school environment by altering class size or characteristics of instructional staff, as well as through integration. We summarize school-based effects of travel with school value-added, a measure of a school's causal effect on achievement. We then ask whether modest effects of travel on achievement and college attendance can be explained by modest effects on value-added. Travel indeed results in only small increases in value-added. Moreover, models that use both offered travel and offered value-added as instruments for value-added of the school attended pass an over-identification test. This result supports an exclusion restriction

[2] Heckman and Robb (1985) introduce the term *control function* to refer to functions that, when added to a regression model for causal effects, eliminate selection bias.

[3] 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 define 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 that 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).

claiming that travel has little or no effect on education outcomes except through value-added. A parsimonious explanation for our findings, therefore, is that travel facilitates integration but does not translate into large enough changes in value-added to change education outcomes much.

Our analysis builds on a wide range of previous work. A recent study closely related to ours by Cordes, Rick and Schwartz (2022) concludes that long bus rides reduce attendance and increase chronic absenteeism among New York elementary school students, with little effect on test scores (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. The analysis reported here 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 (2022). 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.[4] This work largely ignores 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 has been shown to have yielded important economic gains 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 integration-induced education gains for Black students outside the South. But the changes studied in this earlier work typically coincided with major changes in per-pupil expenditure. Evidence from recent periods is more mixed (see, e.g., Hoxby (2000*b*) and Card and Rothstein (2007)). This may reflect the fact that average spending per-pupil today often increases with higher minority enrollment, reflecting the higher costs of educating 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 spending fixed.

The next section sketches the history of desegregation efforts and assignment regimes in the Boston and New York school districts. Section 3 discusses the data used in this study and presents descriptive statistics. Section 4 reports OLS estimates and details the econometric framework used to estimate causal effects of school distance and travel. Section 5 reports the estimates coming out of this framework. Following a discussion of effects on achievement and college attendance, this section considers causal effects on school value-added. The paper concludes with a simulation characterizing the integration consequences of a neighborhood-focused cost-reducing centralized

[4] 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*a*); Lucas and Mbiti (2014); Ajayi (2014); Hoxby, Murarka and Kang (2009); Dobbie and Fryer (2011, 2014); Abdulkadiroğlu et al. (2016). Chubb and Moe (1990) suggest 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 (2022).

assignment scheme. The 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, reduced integration is unlikely to reduce human capital.

2 Background

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

A seemingly quotidian matter, school transportation in many 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 soon to play a prominent 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 A2). 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 immediate acceptance algorithm, a widely-criticized assignment mechanism (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 neighborhood.[5]

[5]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.

Following the end of court-ordered busing in Boston, intense segregation of Black and Hispanic students initially increased. Figure 2 describes the evolution of school-level racial exposure from 1988 to 2018, 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, less than 15 percent of Boston's Black students were enrolled in intensely segregated schools, defined as those with at least 90 percent minority enrollment. By 2003, this figure peaked at 50 percent. While Black students' exposure to other Black students has fallen since 2003, Black exposure to Hispanics has increased. The combination of falling Black exposure and rising Hispanic exposure 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), the evolution of Hispanic exposure to minority peers in Boston mostly mirrors that seen for Blacks. A higher level and more steeply-sloping increase in Boston Hispanic's peer share Hispanic is a noteworthy difference.[6]

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, though not necessarily by design. 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. Decentralized community control of schools gave way in 2004 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 other geographical considerations in the city's assignment system. A 2021 reform proposal, for instance, aimed to remove school priorities based on neighborhoods in the centralized match (Veiga, 2021).

Contemporary discussions of New York school segregation often focus on the fact that white and upper income families have many options that effectively bypass mostly-minority traditional public schools.[7] Alternative options 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 enrollment "screened schools" came to prominence in the 1970s, when the city

Abdulkadiroğlu et al. (2011) estimates charter and pilot school effects on test scores.

[6]Data for Figure 2 (further detailed in Online Appendix B) are from the Common Core survey, documented in `https://nces.ed.gov/ccd/pubschuniv.asp`. Caetano and Maheshri (2022) note the growing importance of Hispanic enrollment for segregation trends nationwide.

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

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 2. In 1988, over 70% of Black New York students attended intensely segregated schools (again, defined as those with over 90% minority enrollment). By 2018, this proportion had fallen to around 50%. The trend in minority isolation based on an 80% cutoff, as well as overall minority exposure, slopes more gently downward over this period than does the trend in intense segregation. Still, minority isolation and minority exposure in New York show a marked drop over the three decades spanned by Figure 2. 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. As in Boston, the level of peer share Hispanic is markedly higher for Hispanic New Yorkers than for Blacks.[8]

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, peer share minority is recently around 75%, while intense segregation 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.

2.2 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 rank. From 2001-05, Boston assigned students using the immediate acceptance 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 A2 maps Boston geocodes, originally defined in the Garrity era and used during our study period. Walk zones for each school are determined by drawing a one-mile radius circle around the school; residents of any geocode intersected by this circle live in the school's walk zone (in what follows, we call these "Garrity walk zones").[9]

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-

[8]Rising Hispanic exposure and declining exposure to Black peers are typical of America's large urban districts, especially from the point of view of Black students. This is documented in Appendix Figure A1, which reports segregation trends in the largest 100 districts with minority enrollment shares comparable to those of Boston and New York.

[9]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).

school lotteries, while Boston exam schools run a separate DA match using a weighted average of middle school GPA and an admissions exam score as tie-breaker period. While 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.[10]

New York centralized assignment 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 is based 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 granting free use of public city transport from September through June (BPS, 2021).[11] 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).[12]

3 Data and Samples

We obtained BPS data on all applicants for 6th and 9th grade seats in the centralized middle and high school matches covering the school years beginning fall 2002-13. Match files include information on applicants' preferences over schools, school priorities, and lottery tie-breakers. Data on school enrollment comes 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 4th grade for middle school applicants and from Grades 7-8 for high school applicants.[13]

[10]Boston's three exam schools admit students mainly in 7th and 9th grade. Abdulkadiroğlu, Angrist and Pathak (2014) use 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.

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

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

[13]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.

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.

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 enrollment and for four-year college enrollment.

The New York Department of Education (DOE) provided data on applicants to 9th grade public high school programs from fall 2012 to fall 2016.[14] New York application files include all information used in the high school match. The DOE also provided information on student 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. 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 related to distance in the form of non-neighborhood schools, the second a measure of travel time. For Boston students, non-neighborhood assignment and enrollment 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 the data, sample, and variable construction.

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 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, the Boston and New York public schools population is predominantly Black or Hispanic. Because most busing is within-district, we leave study of relatively rare inter-district busing programs (such

[14] 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.

as the Massachusetts Metco program) for future work.

3.1 Sample Characteristics

Table 1 describes the students in our analysis samples. Over 70% of the Boston and New York students bodies are Black or Hispanic, and three-fourths have household incomes low enough to qualify for a free or reduced-price lunch. Students in both districts travel on average between 33 and 36 minutes to school each way.[15] Roughly three-quarters of Boston match applicants rank a non-neighborhood school first, and a similar fraction enroll 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 their neighborhood.

As can be seen by comparing the first two columns in the table, Boston students who enroll in non-neighborhood schools have demographic characteristics much like those in the overall sample. The most noteworthy difference between the full and neighborhood-enrolled Boston samples is higher baseline scores in the latter. This partly reflects enrollment in charter and exam schools, which are defined as non-neighborhood for all applicants for purposes of this table. Exam schools in particular tend to enroll higher achievers (Abdulkadiroğlu et al., 2011).

The IV strategy used to estimate causal effects looks at match participants only. 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, who obtain assignments outside the match, as well as students who decide to enroll in the city's public schools after the match.[16] Appendix Table A1 details the sample selection rules used to define our analysis samples.

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 3 shows that 79% of applicants qualify for a subsidized lunch vs 75% of those enrolled in Boston. Boston applicants also have lower baseline test scores. Again, this difference can be explained, at least in part, by the fact that the applicant sample excludes students who applied only to exam or charter schools.

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 important, therefore, that the instrumental variables used to identify causal effects of distance

[15] 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) report that the average home-to-school travel time is 21.1 minutes.

[16] 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 placed administratively.

and travel are uncorrelated with charter and exam-school enrollment in the sample used for causal analysis (this is shown in Appendix Table A2).

The experimental samples used for causal inference, described in columns 4 and 8 of Table 1, consist of the set of applicants for whom school assignment is not deterministic given their preferences and priorities. In other words, this sample contains applicants whose assignments can be changed by redrawing tie-breakers. Just over one-quarter of New York applicants and roughly 38% 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 scores for the experimental samples are also lower than among all applicants, especially in New York. The 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.

4 Econometric Framework

4.1 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 enrollment 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 enrollment at a school outside a student's Garrity walk zone. In New York data, G_i indicates out-of-district enrollment.

Equation (1) is estimated on the sample of enrolled students (a subset of the enrolled sample described in the first column of Table 1, limited to students with data on residential location and outcomes). 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 enrollment, and a dummy for match participation. Models for New York add dummies for enrollment 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 college enrollment. 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, taken by approximately 70% of students.

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 of Table 2, which reports estimates of α in equation (1) separately for all, Black, and Hispanic students. Specifically, Table 2 shows that Boston students who enroll beyond their neighborhood score about 0.06σ higher on MCAS Math tests and roughly 0.05σ higher on MCAS ELA tests. Boston students who enroll in non-neighborhood schools are also 4.3 percentage points more likely to enroll in college and 3.6 percentage points more likely to enroll in a four-year college. Estimates for subsamples of Black and Hispanic students are similar.

OLS estimates for New York also show a strong association between achievement and non-neighborhood enrollment. New York students attending non-neighborhood schools score roughly 0.07σ higher on SAT math and 0.08σ higher on SAT reading, results reported in column 4 of Table 2. The corresponding estimated achievement gains for minority New Yorkers at non-neighborhood schools are a little smaller, though still substantial. And non-neighborhood enrollment in New York is associated with higher rates of college attendance and four-year college enrollment, though not as much as in Boston.[17] It remains to be seen, however, whether the association between non-neighborhood schooling and educational outcomes documented in Table 2 reflects causal effects or selection bias.

4.2 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 numbers tie-breaker. To see how lottery tie-breaking can be used to identify causal effects of school distance a travel, consider a constant-effects model of the effects of non-neighborhood enrollment, 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

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

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 says 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 offers, 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 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).

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) use 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 in to 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:

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

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

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 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 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

[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.

[19] Appendix A.2 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, simulation evidence in Appendix A.7 of Abdulkadiroğlu et al. (2017) suggests 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. (forthcoming) uses a similar 2SLS estimator based on centralized assignment to estimate individual school value-added.

school s. The offered travel instrument can then be written:

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

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). Derivation 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 control function μ_i for New York applicants include $\tau_i(\delta_N)$ as well as applicant type and the distance vector, t_i. The control function for offered travel time in New York is:

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

where school-specific travel times for applicant i are collected in vector t_i and 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 $\delta_N \to 0$). Finally, 2SLS estimates for New York are computed using a version of (9) and (10) that adds design controls in the form of local-linear functions of screened-school tie-breakers; these functions use the same bandwidth used in $\tau_i(\delta_N)$.[20]

The 2SLS estimator characterized by (9) and (10) is derived here under constant effects. In reality, treatment effects may be heterogeneous. Extending results in Angrist and Imbens (1995) and Angrist, Graddy and Imbens (2000), Borusyak and Hull (2021) show that a centered IV estimand of the form (4) can be written as a weighted average of covariate-specific causal effects. For example, when using a Bernoulli-distributed treatment and 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.

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. Columns 2 and 3 of Table A2 show

[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 which have 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 that applied to each program and dummies indicating applicants in each bandwidth.

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. 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-induced differences in follow-up.

A second set of diagnostics evaluates covariate balance. Appendix Table A2 also reports coefficients on offer instruments from regressions of covariates on these 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 listed in these tables 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.

5 IV Estimates

5.1 Integration Consequences of Non-neighborhood Enrollment

Minority students who enroll in non-neighborhood schools have fewer same-race classmates as a result. This is documented in Table 3, which reports 2SLS estimates of non-neighborhood enrollment effects on peer composition, separately by city. The table shows estimates for all applicants with experimental variation in neighborhood enrollment and for two applicant groups defined by race. The corresponding first-stage estimates for all applicants imply that a non-neighborhood offer increases rates of non-neighborhood enrollment by 0.43 in Boston and 0.66 in New York (estimated first stages for Black and Hispanic only are similar).

In samples including all applicants, the impact of non-neighborhood schooling 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 are substantial. Specifically, non-neighborhood enrollment in Boston causes Black students to attend schools with 9.4 percentage points fewer Black peers and 5.8 percentage points more Hispanic peers, resulting in a total decrease in Black or Hispanic peer share of about 3.6 percentage points. Non-neighborhood school enrollment also sharply reduces minority isolation for Black Boston applicants, a fall of almost 17 percentage points compared to a mean of 43 points. We focus here and in the rest of the paper on minority isolation defined by a 90% rather than an 80% cutoff since the higher threshold features in contemporary discussions of segregation such as Cohen (2021) and Potter (2022).

Among Black New York applicants, non-neighborhood school enrollment results in a roughly 9 percentage point reduction in Black peers and a 5 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 enrollment also reduces minority isolation more sharply for Hispanic New York students than for Blacks. The former effect is almost 8 percentage points, while the latter is around 3 points, and not significantly different from zero.

Non-neighborhood school enrollment also integrates the peer environment of Boston's Hispanic applicants, though to a far lesser degree than for Black applicants. Specifically, non-neighborhood enrollment reduces Hispanic applicant's peer share Hispanic and increases Hispanics' peer share Black, but neither of these effects (on the order of 2 points) are significantly different from zero. Non-neighborhood school enrollment also likewise no significant effect on Hispanic minority isolation in Boston. Estimated integration effects for New York's many Hispanic students are larger than for Hispanics in Boston. These estimates suggest that non-neighborhood enrollment decreases peer share Hispanic by 5.9 percentage points, while boosting peer share Black by 1.2 percentage points.

On balance, the estimates in Table 3 indicate that non-neighborhood enrollment has substantial integrating effects, especially for Black applicants. Non-neighborhood enrollment reduces same-race exposure more for Hispanic New Yorkers than for Hispanics in Boston. At the same time, effects on peer share Black and Hispanic tend to be offsetting, so that changes in overall minority exposure due to non-neighborhood schooling are well below the corresponding changes in same-race exposure.

5.2 Non-Neighborhood Effects on Achievement and College Attendance

Non-neighborhood enrollment boosts integration in the sense of reducing minority applicants' same-race exposure and, for Blacks in Boston and Hispanics in New York, by reducing minority isolation. We might therefore expect non-neighborhood schooling to increase learning and college enrollment as well. The 2SLS estimates reported in Table 4, however, show little evidence of non-neighborhood schooling effects on achievement and college attendance to match the integration gains documented in Table 3.

As can be seen in the first three columns of Table 4, Boston students who enroll out-of-neighborhood have test scores and college enrollment rates close to those of students who stay in-neighborhood. Among Black Boston applicants, for example, non-neighborhood enrollment generates only an estimated 0.024σ (se=0.06) improvement in Math scores. This estimate contrasts with the much larger (and more precise) OLS estimate of roughly 0.09σ (se=0.01), suggesting that the latter reflects selection bias.

Most of the 2SLS estimates of the impact of non-neighborhood schooling on Boston students' achievement and college attendance rates are smaller than the corresponding OLS estimates. At the same time, few of the 2SLS estimates for Boston are estimated precisely enough to be statistically distinct from the corresponding OLS estimates. 2SLS estimates for New York applicants are considerably more precise than those for Boston. With estimated standard errors of around $0.02 - 0.03$, most of the 2SLS estimates for New York amount to reasonably precise null effects of neighborhood schooling on test scores and college-going.

5.3 Travel Time Effects

The non-neighborhood schooling treatment is defined in part by arbitrary neighborhood boundaries. We therefore explore causal effects of a school-travel treatment that various continuously above zero. Estimated effects on integration and education outcomes are similar when minutes of travel time to school, T_i, replaces neighborhood schooling as a mediator of school distance and travel effects.

Table 5 presents 2SLS estimates of school travel effects. These are reported in terms of twenty minute increments, a scaling that facilitates comparison to the non-neighborhood effects in the previous table. This scaling is also motivated by Figure 3, which plots the distribution of travel time for non-neighborhood enrollment compliers. Among applicants induced to enroll out-of-neighborhood by virtue of being offered a non-neighborhood seat in the match, travel times are roughly 16-21 minutes longer than they would have been in the absence of such an offer. This figure also highlights the relative skewness of non-neighborhood commute times relative to the compressed distribution for those enrolling close to home. 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.

As with non-neighborhood enrollment, twenty minutes of additional travel integrates the school environment, especially for Black applicants. The pattern of estimated effects of school travel on peer race and minority isolation, presented in Panel A of Table 5, mostly parallels that seen in estimates of effects of non-neighborhood enrollment. For Black students, for example, 20 minutes of travel reduces same-race exposure by about 6 points in Boston and by about 7 points in New York. Travel effects on minority isolation are larger in magnitude than the effects of non-neighborhood enrollment on minority isolation; the former are also consistently negative across all racial groups.

Large integration effects of travel notwithstanding, Panel B of Table 5 again shows little evidence of a travel effect on achievement or college attendance. Most of the estimated travel effects on education outcomes in Panel B are small and not significantly different from zero. Also noteworthy is the fact that standard errors for estimated travel effects are markedly below the standard errors of estimated non-neighborhood schooling effects in Table 4.

A few statistically significant estimates in Panel B of Table 5 hint at deleterious effects of travel on Hispanic students in New York. Estimates for this group, reported in column 6, suggest twenty-minutes of additional travel reduces college-going by 4 points and four-year college attendance by 3 points. Estimated travel effects on Black college-going are also negative, though only one (for any college attendance in Boston) is (marginally) significantly different from zero.[21]

5.4 Value-Added and Other Mediators

Minority families who opt for a longer ride to school benefit from a more integrated school environment as a result. Yet, the absence of travel effects on educational outcomes suggests the schools travelers travel to are no better than those they might attend nearby. Are neighborhood schools really no worse than schools farther away? We explore this hypothesis using a measure of school quality, rather than racial integration, as the principal mediator of distance and travel effects.

Schools differ in many ways, but a school's causal value added offers a parsimonious summary of school quality that should reflect the cumulative impact of education inputs like class size, teacher skills, and peers on student achievement. This observation motivates an investigation of the extent to which effects of travel on school value added–or lack thereof–can account for the mostly absent effects of travel on education-related outcomes.[22]

For purposes of this investigation, value added is estimated using a risk-controlled value-added model (RC VAM). Introduced in Angrist et al. (forthcoming), RC VAM measures causal effects of individual schools on achievement by controlling for student characteristics, including lagged test scores, and for the probability a student is seated at each of the schools in their rank-order list. Angrist et al. (forthcoming) validate RC VAM using the subset of students and schools where seats are randomly assigned. As in Angrist et al. (forthcoming), we focus on test-score value added rather than college value added because lagged test-score controls enhance the predictive validity of test-score VAM estimates.[23]

Our investigation of value added as a mediator of travel effects is motivated by a version of (10) in which the RC VAM-derived value added of the school student i attends, denoted by V_i, replaces non-neighborhood enrollment. The VAM second stage can be written:

$$Y_i = \beta_v V_i + \kappa_2 \mu_{ji} + \nu_{2i}, \tag{12}$$

where β_v is the causal effect of attendance at a higher-value-added school and μ_{ji} is an instrument-specific control function defined below. In the VAM literature, causal parameter β_v is a *forecast coefficient* gauging the extent to which students randomized to attend higher-value-added schools learn more as a result (see, e.g., Angrist et al. (2016*b*)).

[21]Distance and travel have little effect on absences, suspensions, or a composite disciplinary index in Boston. In New York, twenty-minutes travel increases days absent by 0.8, but the corresponding estimated non-neighborhood effect is not significantly different from zero. These and other estimates of travel effects on behavioral outcomes are reported in Appendix Table A3.

[22]Appendix Table A4 summarizes direct estimates of distance and travel effects on school inputs in the form of student-teacher ratios and two measures of teacher qualifications. These rely on the same causal framework used to estimate travel effects on peer race and minority isolation. 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 effects here are at most a 0.7 increase in size). Effects on teacher qualifications are even smaller.

[23]Chetty, Friedman and Rockoff (2014) argue that VAM assumptions are more plausible for test scores than for longer-run outcomes where lagged measures of the dependent variable are unavailable.

The first of two instruments used to estimate (12) is offered value added, defined by:

$$Z_{1i} = \sum_s D_i(s) v_i(s),$$

where $v_i(s)$ is value added by school s in applicant i's cohort. The second instrument is offered travel, defined as before by:

$$Z_{2i} = \sum_s D_i(s) t_i(s),$$

though now written Z_{2i} to distinguish this instrument from the first. Analogous to (9) and (10), control function μ_{ji} is defined by

$$\mu_{ji} \equiv E[Z_{ji}|\theta_i, t_i, v_i]; j = 1, 2; \tag{13}$$

where v_i is the vector of school value-added and t_i is again the vector of travel times. The first stage equation in this setup regresses V_i on one or both instruments and the corresponding μ_{ji}.

As a benchmark for first-stage estimates of travel effects on value added, Columns 1, 4, and 7 in Panel A of Table 6a (for Boston) and Table 6b (for New York) show that offered value added (instrument Z_{1i}) is a strong predictor of value added at the school an applicant attends. Moreover, as can be seen in Panel B of the table, 2SLS using offered value added to instrument value added at the school attended generates precise estimates of β_v of around 0.8 or higher. These high estimates validate the predictive value of RC VAM estimates of value added in our samples.

In contrast with offered value added, offered school travel (instrument Z_{2i}) changes value added little. This is documented in columns 2, 5, and 8 in Panel A of Tables 6a and 6b. An additional 20 minutes of offered travel time leads Boston travellers to enroll at schools with only slightly higher value-added, a gain ranging from just under 0.01σ for Hispanic applicants to 0.016σ for Blacks. The corresponding estimates for New York applicants, shown in the same columns in Panel A of Table 6b, are even smaller: the largest quality increase due to school travel in New York is 0.009σ for Blacks.[24] Unsurprisingly, weak first-stage effects of travel on value added lead to imprecise and essentially uninformative second stage estimates (these appear in Panel B of the two tables in the columns containing the first-stage estimates for offered travel).

The results in Tables 6a and 6b suggest that weak effects of travel on education outcomes can be explained by small effects of travel on school quality. But perhaps travel affects outcomes through channels other than value added. For instance, a long commute requires earlier mornings, potentially reducing sleep (Carrell, Maghakian and West, 2011). Commuting time might also eat into study time.[25] These considerations raise the possibility that negative aspects of travel offset any gains due to integration.

The precisely-estimated forecast coefficients reported in Panel B of these tables imply that

[24] Effects of non-neighborhood enrollment on value added (not reported in the tables) are even smaller.

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

instrument Z_{1i} is sufficient to identify and reliably estimate β_v. Identification of β_v by Z_{1i} alone allows us to test whether value-added satisfies the exclusion restriction implied by our explanation of null travel effects on achievement and college-going. The argument here can be boiled down to two statements: (A) Value added mediates travel effects; (B) Travel changes value added little. This restriction is tested by asking whether whether the offered-travel instrument, Z_{2i}, predicts outcomes net of the value-added effects captured by $\beta_v V_i$. In other words, in a regression of 2SLS residual $Y_i - \beta_v V_i$ on both instruments, does Z_{2i} matter? This test is implemented as the test of over-identifying restrictions obtained when using both offered travel and offered value added to instrument V_i in equation (12).[26]

The results of this test, along with the associated first- and second-stage estimates, appear in columns 3, 6, and 9 in Tables 6a and 6b. Over-identified estimates of the value-added forecast coefficient, β_v, are similar to the just-identified estimates obtained using Z_{1i} only. Unsurprisingly, therefore, statistics testing over-identifying restrictions, shown at the bottom of Panel B, offer no evidence against an exclusion restriction attributing all travel effects to changes in school quality as measured by school value added. These findings therefore support our claim that that small effects of travel on education outcomes are explained by small travel effects on school quality.[27]

6 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 longtime Boston schools observer Ted Landsmark's contention that busing today is of little educational consequence. At the same time, while reduced travel seems likely to leave education outcomes unchanged, a shift to proximity-based assignment, effectively, "neighborhood schools," may increase segregation. By how much? We gauge this by simulating a match in which both students and schools rank one another in order of proximity. This imagined neighborhood assignment scenario uses information on the residential location of each student (geocode in Boston, census tract in New York), the location of each school, and the school capacities. Because neighborhood schools are typically expected to accommodate all neighboring families, the simulation raises each school's capacity to the maximum enrolled there during the years for which we have data.

Our simulation results compare status quo enrollment patterns with patterns under binding neighborhood assignment for all students enrolled in match-participating schools (not just those

[26] Over-identified models control for both μ_{1i} and μ_{2i}. Hausman (1983) shows that the Sargan over-identification test statistic can be computed as sample size times the R^2 from a regression of 2SLS residuals on excluded instruments, partialing out any controls.

[27] Cordes, Rick and Schwartz (2022) argue that long bus rides increase absenteeism, while also noting that controlling for student distance from school, within-route effects on absenteeism largely disappear. These results therefore seem consistent with our estimated null effects of travel, which likewise control for student-school distance.

participating in the match). Table 7 reports simulated neighborhood assignment impacts on average travel time, the share of students eligible for publicly funded school transportation ("busing"), and measures of segregation. Simulated neighborhood assignment reduces travel time by about 13 minutes for Boston middle and high-schoolers and by as much as 17 minutes for 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.[28]

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, enrollment 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, for Hispanics, our simulation predicts no change in minority isolation and only small changes in same-race exposure (for Boston Hispanics, this falls slightly). Consequently, under neighborhood assignment, minority students as a group enroll in schools with shares of same-race peers matching those experienced today. These patterns reflect the fact that the reassignment scenario puts every student in play: when students who used to travel are pulled back to neighborhood schools, some now attending these schools are displaced.

Sharp drops in busing eligibility can be expected to reduce school transportation costs markedly. As with changes in transportation usage, precise savings from reduced busing 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 CharlieCards cost \$90 / month (in 2022) and are issued for 10 months; averaging yellow-bus costs for 6th graders with this amount (estimated at around \$1,732 in 2014) results in an average annual savings of around \$1020 (in 2022 dollars) per formerly-transported-student in Boston (BPS, 2014*a*).[29] 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).[30]

These savings could be used to improve school quality. A recent meta-analysis of school expenditure effects by Jackson and Mackevicius (2021) suggests that, over the course of four years, \$1,000 of additional annual spending might boost test scores by about 0.044σ and increase college attendance rates by approximately 3.9 percentage points. In practice, we can't say how transportation savings might be allocated over students. It seems reasonable to imagine, however, that additional spending would likely target high-need students. In any case, the possibility of such gains highlights the value of a fresh look at busing-resource trade-offs.

A complete analysis of busing trade-offs should include the effect of neighborhood school assignment on overall district enrollment. 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).

[28] Boston eligibility criteria are those for 6th grade.

[29] Costs are adjusted to May 2022 dollars using Consumer Price Index (Series ID = CUUR0000SA0).

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

Neighborhood assignment might also dilute incentives for school effectiveness, an issue considered in, e.g., Card, Dooley and Payne (2010) and Campos and Kearns (2022). Shi (2015) and Pathak and Shi (2021) examine recent efforts to reduce transportation costs in Boston by limiting choice without a full return to neighborhood schools. The Covid pandemic has also spurred efforts to reduce school transportation services across the country (see, for example, Washington (2021) and Russell (2021)). We expect to leverage quasi-experimental student assignment to generate further evidence on the causal connections between school choice, racial integration, and human capital in the near future.

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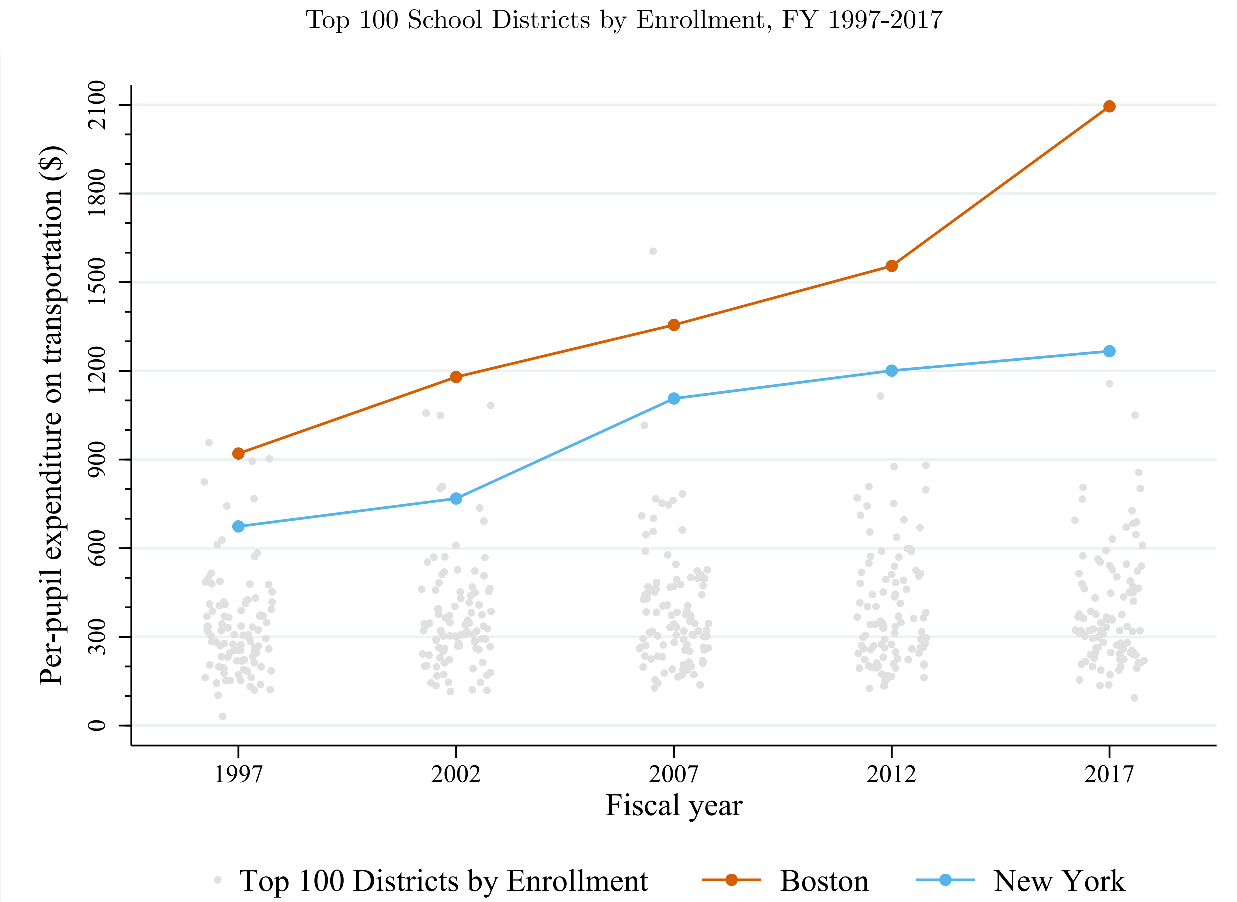
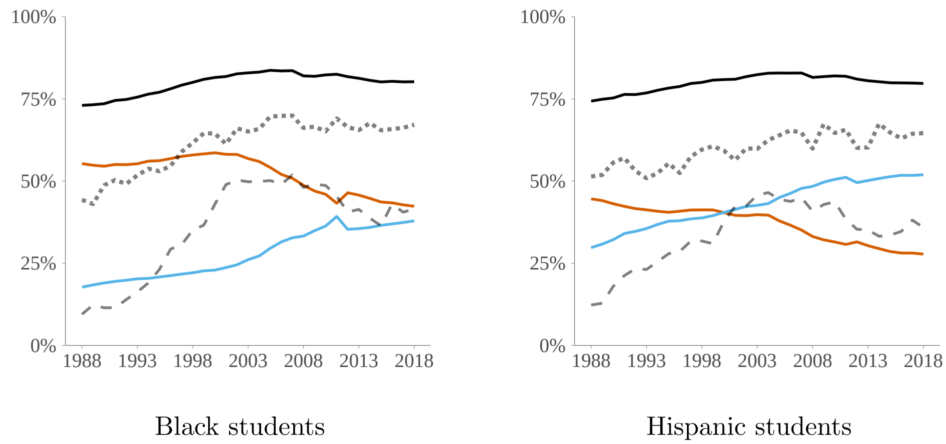
Figure 1: Per-Pupil Annual Expenditures on Student Transportation



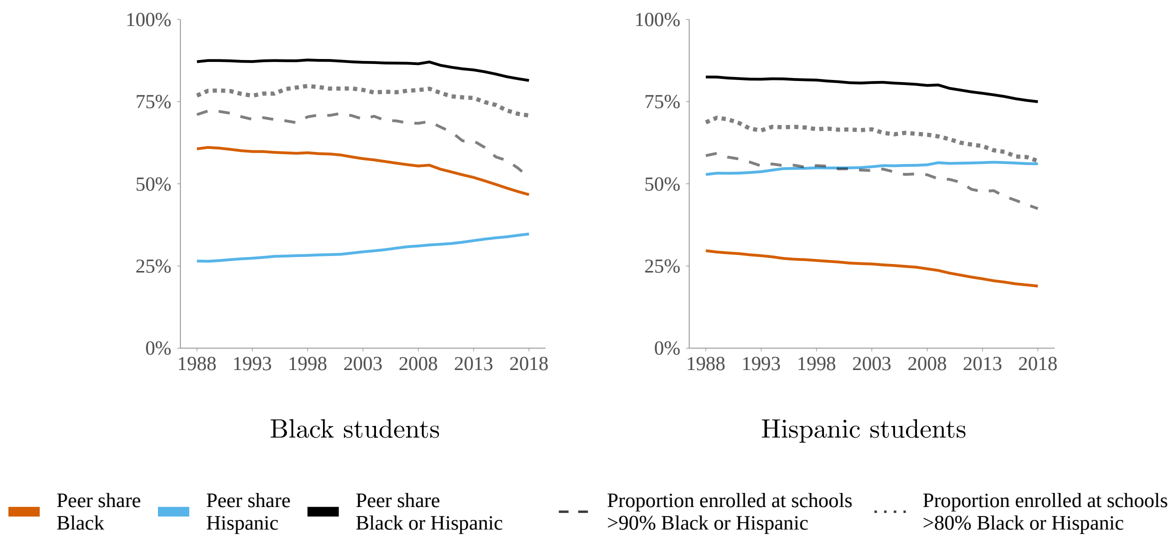
Source: National Center for Education Statistics (NCES) Common Core of Data.
Notes: Transportation expenditure corresponds to the amount spent on the transportation of public school students, including vehicle operation, monitoring riders, and vehicle servicing and maintenance. Per-capita amounts are computed by dividing total costs by total district enrollment. Enrollment data 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). Top 100 districts by enrollment is a within-year designation.

Figure 2: Racial Exposure in Boston and New York Public Schools, 1988-2018

(a) Boston Public Schools



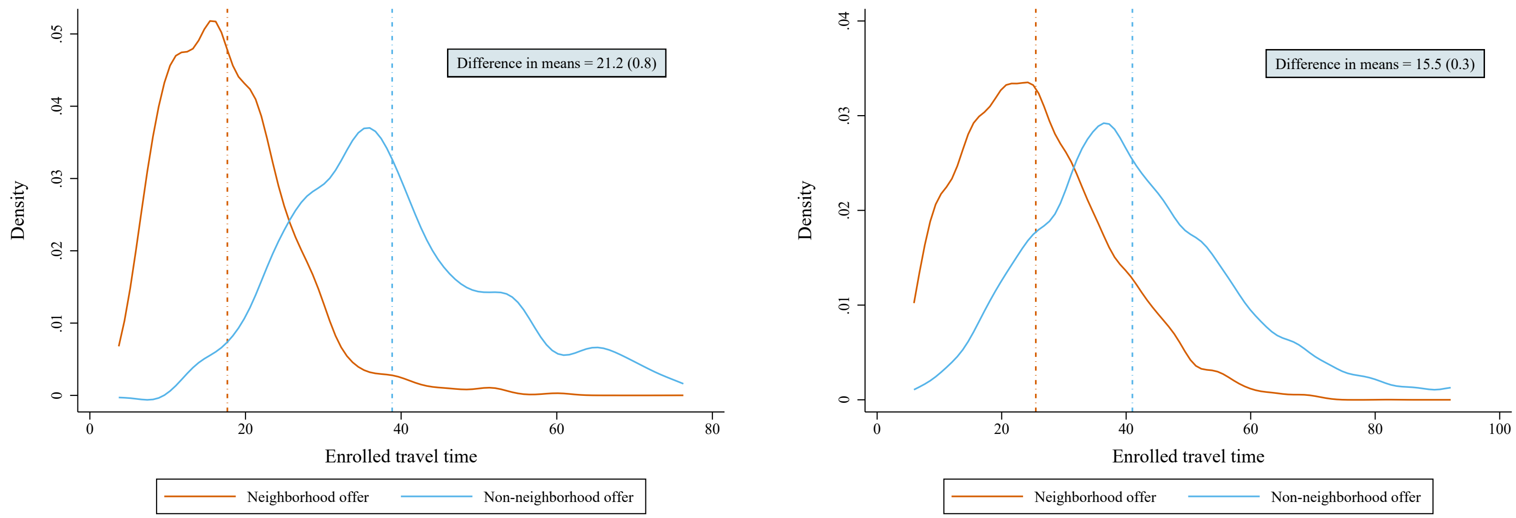
(b) New York Public Schools



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

Notes: Solid lines plot the proportion of a student's peers falling into various race groups. 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. Year t on the x-axis marks the school year ending 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 is defined as "non-Hispanic, Black."

Figure 3: 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 neighborhood compliers. Densities for non-neighborhood compliers are estimated using 2SLS regressions of the interaction of a kernel density function and a non-neighborhood enrollment indicator on the enrollment indicator, instrumented by a non-neighborhood assignment indicator, controlling for student demographics, baseline achievement, and offer risk. Densities for non-offered compliers are estimated by replacing enrollment with one minus enrollment 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 1: Boston and New York Analysis Samples

	Boston (6th and 9th grade)				New York (9th grade)			
	Enrolled sample	Enrolled (non-nbhd) sample	Applicant sample	Experimental sample	Enrolled sample	Enrolled (non-nbhd) sample	Applicant sample	Experimental sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Race								
Black	0.43	0.45	0.44	0.48	0.29	0.33	0.27	0.30
Hispanic	0.34	0.31	0.37	0.37	0.40	0.40	0.39	0.44
White or Asian	0.22	0.22	0.18	0.13	0.29	0.25	0.33	0.24
N	119,915	82,031	66,801	24,879	394,777	250,254	298,840	75,686
B: Other covariates								
Free and reduced-price lunch	0.73	0.73	0.79	0.80	0.75	0.76	0.73	0.78
Female	0.49	0.50	0.50	0.51	0.49	0.50	0.51	0.52
Special education	0.18	0.19	0.17	0.18	0.18	0.19	0.07	0.08
Limited English proficiency	0.16	0.12	0.14	0.11	0.14	0.12	0.09	0.05
Baseline math	0.01	0.07	-0.10	-0.14	-0.01	0.03	0.18	0.04
Baseline English	0.01	0.06	-0.07	-0.11	-0.02	0.02	0.18	0.04
C: School sectors								
Enrolled in charter	0.13	0.19	0.08	0.09	0.05	0.08	0.04	0.04
Enrolled in exam	0.10	0.15	0.03	0.04	0.06	0.09	0.07	0.05
Enrolled in offered school			0.69	0.62			0.77	0.74
D: Travel								
Ranked a non-nbhd school first			0.72	0.79			0.66	0.73
Enrolled in a non-nbhd school	0.77	1.00	0.74	0.79	0.64	1.00	0.65	0.69
Enrolled travel time	32.9	38.3	32.9	35.3	36.4	43.2	37.5	38.3
Enrolled distance	3.63	4.43	3.61	4.01	4.48	5.85	4.70	4.76
Eligible for busing	0.71	0.88	0.70	0.76	0.96	0.99	0.96	0.97
N	98,379	73,803	66,281	24,879	389,284	250,135	297,734	75,686

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 2013-14. 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 restrict the sample to students who enroll in a non-neighborhood school. Columns 3 and 7 report statistics for the sample of match applicants who have demographic information. The experimental samples in columns 4 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). 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. The sample sizes after Panel A count students with demographic information; the sample sizes after Panel D count students who also have geographic information.

Table 2: OLS Estimates of Non-Neighborhood Schooling Effects

		Non-neighborhood enrollment					
		Boston			New York		
		All students	Black students	Hispanic students	All students	Black students	Hispanic students
		(1)	(2)	(3)	(4)	(5)	(6)
MCAS Math / SAT Math		0.0646	0.0870	0.0694	0.0737	0.0507	0.0542
		(0.0071)	(0.0107)	(0.0117)	(0.0033)	(0.0062)	(0.0048)
	Mean	-0.000	-0.258	-0.151	0.002	-0.416	-0.334
MCAS ELA / SAT Reading		0.0458	0.0719	0.0360	0.0846	0.0596	0.0700
		(0.0070)	(0.0109)	(0.0114)	(0.0034)	(0.0067)	(0.0050)
	Mean	0.015	-0.155	-0.132	0.002	-0.306	-0.295
	N	83,221	34,936	27,965	259,224	67,278	94,742
Any college		0.0432	0.0453	0.0399	0.0065	0.0061	-0.0052
		(0.0063)	(0.0097)	(0.0098)	(0.0020)	(0.0039)	(0.0031)
	Mean	0.427	0.388	0.365	0.520	0.430	0.446
Four-year college		0.0364	0.0353	0.0360	0.0220	0.0127	0.0135
		(0.0055)	(0.0084)	(0.0084)	(0.0018)	(0.0034)	(0.0026)
	Mean	0.324	0.277	0.253	0.335	0.246	0.232
	N	40,918	17,416	14,154	303,975	89,375	121,046

Notes: This table reports OLS estimates of the relationship between non-neighborhood school enrollment and achievement or college-going. The Boston sample includes students enrolled in the 2002-13 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 enrollment, and exam school enrollment. The models also include fixed effects for school walk zone in Boston and residential school district in New York. The New York specification also includes an indicator for district 75 schools (serving only special education students) and district 79 schools (serving specialized student populations such as incarcerated youth or adults pursuing a GED). 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 3: 2SLS Estimates of Non-Neighborhood Schooling Effects on Peer Race

		Non-neighborhood enrollment					
		Boston			New York		
		All applicants	Black applicants	Hispanic applicants	All applicants	Black applicants	Hispanic applicants
		(1)	(2)	(3)	(4)	(5)	(6)
Peer share Black		-0.0259	-0.0938	0.0230	-0.0073	-0.0874	0.0116
		(0.0094)	(0.0143)	(0.0155)	(0.0039)	(0.0122)	(0.0049)
	Mean	0.452	0.505	0.412	0.297	0.458	0.260
	SD	0.171	0.160	0.166	0.226	0.255	0.170
Peer share Hispanic		0.0251	0.0583	-0.0204	-0.0246	0.0514	-0.0590
		(0.0086)	(0.0123)	(0.0140)	(0.0039)	(0.0094)	(0.0056)
	Mean	0.378	0.347	0.423	0.426	0.372	0.532
	SD	0.150	0.136	0.151	0.222	0.218	0.192
Peer share Black		-0.0008	-0.0355	0.0026	-0.0320	-0.0360	-0.0474
or Hispanic		(0.0078)	(0.0109)	(0.0113)	(0.0041)	(0.0090)	(0.0055)
	Mean	0.830	0.852	0.835	0.724	0.830	0.793
	SD	0.128	0.114	0.114	0.260	0.197	0.210
Minority isolation		-0.0618	-0.1650	0.0222	-0.0436	-0.0325	-0.0780
		(0.0289)	(0.0449)	(0.0488)	(0.0086)	(0.0236)	(0.0131)
	Mean	0.367	0.433	0.366	0.394	0.572	0.459
	N	24,879	11,984	9,103	75,686	23,077	33,436

Notes: This table reports 2SLS estimates of non-neighborhood enrollment effects on peer racial composition, computed in samples of Boston middle and high school applicants and New York high school applicants. The sample is restricted to offered applicants with non-degenerate risk of school assignment. The instrument is non-neighborhood assignment. The endogenous variable is 6th or 9th grade non-neighborhood enrollment. The non-neighborhood first stages are approximately 0.43 for Boston and 0.66 for New York; race-specific first stages are similar. 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 4: 2SLS Estimates of Non-Neighborhood Schooling on Test Scores and College Attendance

		Non-neighborhood enrollment					
		Boston			New York		
		All applicants	Black applicants	Hispanic applicants	All applicants	Black applicants	Hispanic applicants
		(1)	(2)	(3)	(4)	(5)	(6)
MCAS Math / SAT Math		0.0368	0.0244	-0.0322	-0.0094	0.0050	0.0045
		(0.0424)	(0.0600)	(0.0719)	(0.0147)	(0.0314)	(0.0202)
	Mean	-0.115	-0.276	-0.126	-0.022	-0.375	-0.259
MCAS ELA / SAT Verbal		0.0301	0.0565	0.0314	0.0146	0.0416	0.0384
		(0.0419)	(0.0603)	(0.0702)	(0.0153)	(0.0347)	(0.0211)
	Mean	-0.078	-0.160	-0.100	-0.000	-0.255	-0.208
	N	21,240	10,348	7,637	55,430	15,548	23,345
Any college		0.0194	-0.0129	0.0015	-0.0168	-0.0405	-0.0221
		(0.0419)	(0.0649)	(0.0603)	(0.0127)	(0.0300)	(0.0192)
	Mean	0.391	0.380	0.372	0.570	0.488	0.522
Four-year college		0.0099	0.0477	-0.0300	-0.0094	-0.0093	-0.0040
		(0.0369)	(0.0563)	(0.0520)	(0.0119)	(0.0267)	(0.0167)
	Mean	0.271	0.261	0.245	0.358	0.281	0.276
	N	14,492	6,872	5,572	58,597	18,199	26,173

Notes: This table reports 2SLS estimates of non-neighborhood enrollment effects on student achievement and college enrollment, computed in samples of Boston middle and high school applicants and New York high school applicants. The sample, instrument, endogenous variable, and controls are as described in Table 3. Standardized test outcomes are as described in Table 2.

Table 5: 2SLS Estimates of School Travel Effects on Peer Race and Education Outcomes

		Travel time					
		Boston			New York		
		All applicants	Black applicants	Hispanic applicants	All applicants	Black applicants	Hispanic applicants
		(1)	(2)	(3)	(4)	(5)	(6)
		A: Peer race and minority isolation					
Peer share Black		-0.0272	-0.0602	-0.0006	-0.0133	-0.0698	0.0165
		(0.0057)	(0.0076)	(0.0096)	(0.0029)	(0.0064)	(0.0039)
Peer share Hispanic		0.0026	0.0221	-0.0199	-0.0161	0.0242	-0.0478
		(0.0050)	(0.0065)	(0.0084)	(0.0026)	(0.0050)	(0.0040)
Minority isolation		-0.1551	-0.2084	-0.1352	-0.0555	-0.1012	-0.0581
		(0.0169)	(0.0239)	(0.0287)	(0.0059)	(0.0123)	(0.0099)
	N	24,833	11,953	9,089	74,936	22,829	33,124
		B: Test scores and college attendance					
MCAS Math / SAT Math		0.0246	0.0229	-0.0107	-0.0011	-0.0007	0.0063
		(0.0254)	(0.0357)	(0.0421)	(0.0095)	(0.0165)	(0.0145)
	N	21,200	10,320	7,626	55,049	15,445	23,173
Any college		-0.0146	-0.0612	0.0048	-0.0252	-0.0187	-0.0421
		(0.0217)	(0.0303)	(0.0344)	(0.0082)	(0.0155)	(0.0135)
Four-year college		-0.0060	-0.0262	-0.0012	-0.0209	-0.0037	-0.0314
		(0.0190)	(0.0262)	(0.0298)	(0.0075)	(0.0137)	(0.0116)
	N	14,450	6,843	5,559	57,979	17,996	25,907

Notes: This table reports 2SLS estimates of travel time effects on the variables at left, computed in the samples of Boston middle and high school applicants and New York high school applicants. The sample is as described in Table 3. Travel time effects are per 20 minutes of travel. The instrument is travel time offered. The endogenous variable is 6th or 9th grade enrolled travel time. Travel time first stages are around 0.40 for Boston and 0.57 for New York. The sample is as described in Table 3. Models include a control function – analogous to μ_i, defined in Equation 5 – that controls for the expected offered travel time across possible lottery draws, as well as student demographic variables and baseline achievement. Minority isolation is defined as in Table 3. Standardized test outcomes are as described in Table 2.

Table 6a: Estimates of School Value-Added Effects on MCAS Math in Boston

	All applicants			**Black applicants**			**Hispanic applicants**		
	Just-ID		Over-ID	Just-ID		Over-ID	Just-ID		Over-ID
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				A: First-stage effects on value-added					
Instrumental variable:									
Offered school MCAS Math value-added	0.434		0.431	0.455		0.450	0.400		0.397
	(0.018)		(0.018)	(0.024)		(0.024)	(0.031)		(0.031)
Offered travel time		0.014	0.007		0.016	0.009		0.009	0.007
		(0.003)	(0.003)		(0.004)	(0.004)		(0.005)	(0.005)
				B: 2SLS estimates of value-added effects					
Endogenous variable:									
MCAS Math value-added	0.814	0.589	0.810	0.738	0.518	0.747	0.825	-0.329	0.803
	(0.100)	(0.579)	(0.099)	(0.136)	(0.788)	(0.134)	(0.173)	(1.429)	(0.172)
Over-ID test statistic			0.015			0.006			0.277
p-value			0.90			0.94			0.60
N	20,222	21,216	20,114	9,798	10,331	9,742	7,315	7,633	7,276

Notes: This table shows 2SLS estimates of the effects of enrolled school Math value-added on 6th and 10th grade MCAS test scores (for 6th and 9th grade applicants, respectively). Columns 1, 4, and 7 report estimates from a just-identified model that instruments enrolled value-added with offered travel time, scaled in 20 minute increments. Columns 2, 5, and 8 report the results of instrumenting enrolled value-added with offered value-added. Remaining columns report the results of instrumenting enrolled value-added with both offered travel time and offered value-added. Estimates are computed in the sample of offered applicants for Boston middle and high school, with non-degenerate risk of school assignment. The sample is as described in Table 3. Models include control functions μ_1 and μ_2, as defined in Equation 13, as well as student demographic variables and baseline achievement. Value-added is computed using 6th and 10th grade MCAS math test scores (for 6th and 9th grade applicants, respectively). The risk-controlled value-added computation follows that in Angrist et al. (forthcoming).

Table 6b: Estimates of School Value-Added Effects on SAT Math in New York

	All applicants			**Black applicants**			**Hispanic applicants**		
	Just-ID		Over-ID	Just-ID		Over-ID	Just-ID		Over-ID
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				A: First-stage effects on value-added					
Instrumental variable:									
Offered school SAT Math value-added	0.607		0.608	0.589		0.590	0.649		0.649
	(0.018)		(0.018)	(0.025)		(0.026)	(0.033)		(0.033)
Offered travel time		0.006	-0.002		0.009	0.000		0.003	-0.001
		(0.002)	(0.002)		(0.003)	(0.003)		(0.003)	(0.002)
				B: 2SLS estimates of value-added effects					
Endogenous variable:									
SAT Math value-added	0.818	-0.100	0.819	0.948	-0.106	0.954	0.792	0.693	0.793
	(0.042)	(0.578)	(0.042)	(0.082)	(0.855)	(0.082)	(0.059)	(1.715)	(0.059)
Over-ID test statistic			2.923			2.108			0.005
p-value			0.09			0.15			0.94
N	55,428	55,130	55,128	15,548	15,478	15,478	23,345	23,199	23,199

Notes: This table shows 2SLS estimates of the effects of enrolled school Math value-added on SAT math scores. The specifications follow those in Table 6a. Estimates are computed in the sample of offered applicants for New York high school, with non-degenerate risk of school assignment. The sample is as described in Table 3. Models include control functions μ_1 and μ_2, as defined in Equation 13, as well as student demographic variables and baseline achievement. Math value-added corresponds to contemporaneous-year risk-controlled school value-added model estimates. Value-added is computed using SAT math test scores. The risk-controlled value-added computation follows that in Angrist et al. (forthcoming).

Table 7: Neighborhood Reassignment with Increased School Capacities

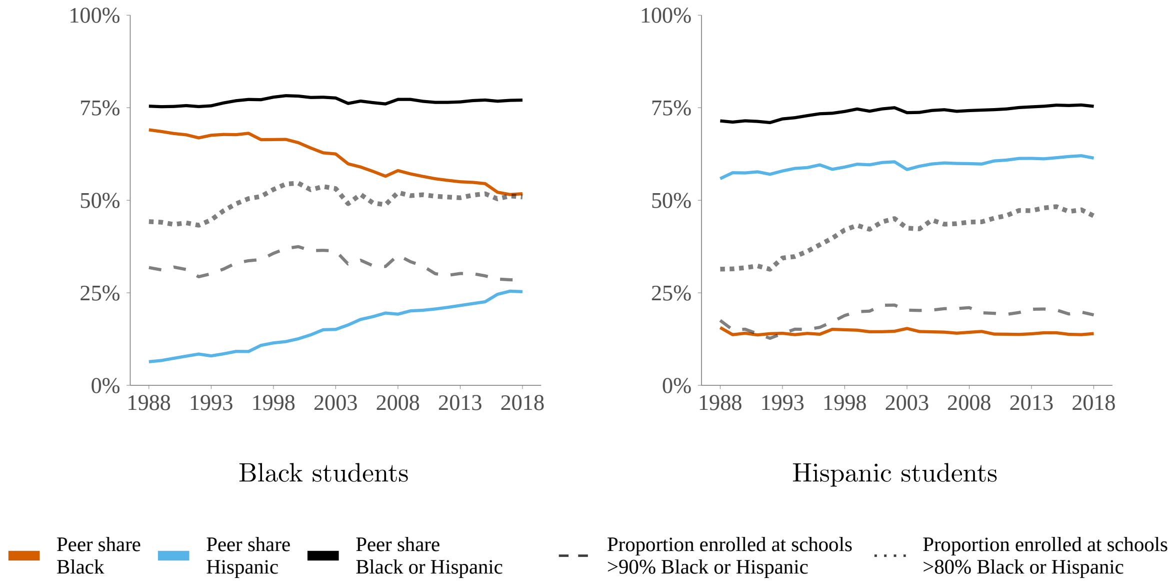
	Baseline scenario				Change relative to baseline			
	Time	Busing eligibility	Same-race exposure	Minority isolation	Time	Busing eligibility	Same-race exposure	Minority isolation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Boston (6th and 9th grade)								
Black students	34.0	0.73	0.489	0.430	-13.1	-0.48	0.019	0.105
Hispanic students	31.1	0.66	0.478	0.365	-13.4	-0.47	-0.015	0.003
Black or Hispanic students	32.6	0.70	0.834	0.398	-13.2	-0.47	0.002	0.055
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. Baseline statistics in columns 1-4 show student-weighted average enrolled school characteristics. Statistics are for 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 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 3. 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.1 Additional Figures and Tables

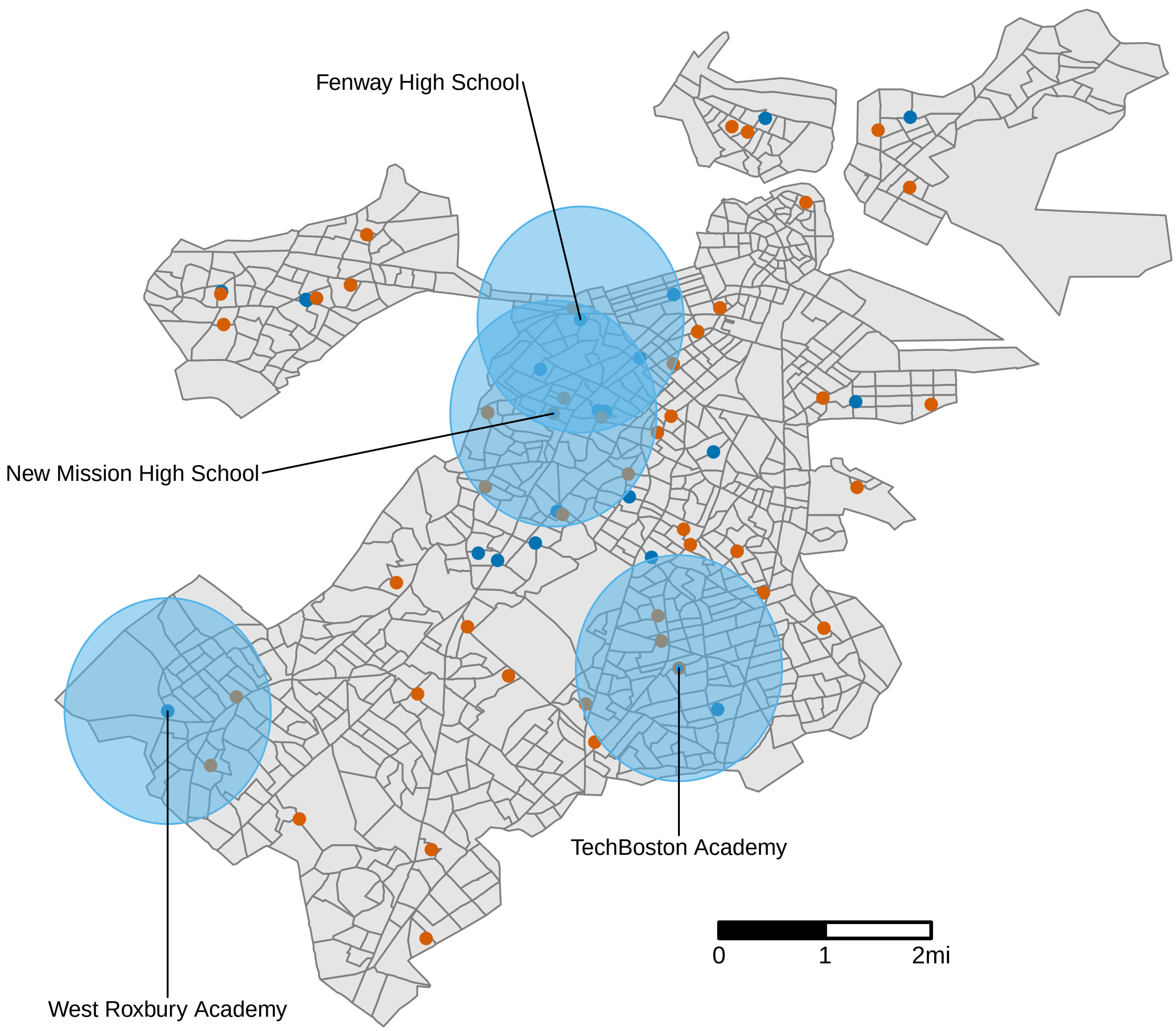
Figure A1: Racial Exposure in 100 High-Minority-Share Districts, 1988-2018



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

Notes: The sample used here consists of schools in the 100 largest districts by enrollment, drawn from the set of districts with 60-80% minority enrollment. The set of districts meeting these criteria varies by year. School districts with less than 10 years of data are excluded. Other variable definitions and sample restrictions are as described in Figure 2.

Figure A2: 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
	6th grade	9th grade	9th grade
	(1)	(2)	(3)
A: OLS			
All enrolled students with demographic information	56,827	63,088	394,777
With geographic information	48,143	50,236	389,284
B: 2SLS			
Applicants in the match with demographic information	32,800	34,001	298,840
With geographic information	32,661	33,620	297,734
Ranked at least two programs, the first over-subscribed	27,224	29,766	269,734
Is not guaranteed assignment at first choice	15,159	20,475	172,040
Has non-degenerate risk of school assignment	12,958	17,922	101,960
Who are offered a seat	11,746	16,556	90,030
Enroll in a school	11,358	15,818	83,970
With baseline scores	10,350	14,529	75,686

Notes: This table illustrates the construction of the OLS and 2SLS samples. The OLS sample start 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 ends with the final row after implementing the sample restrictions described in the rows of the table.

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)	(5)
			A: Attrition and selection into other sectors			
Has outcome MCAS Math / SAT Math	0.837	0.0059	0.0033	0.68	-0.0120	-0.0083
		(0.0087)	(0.0052)		(0.0065)	(0.0036)
Has outcome MCAS ELA / SAT Reading	0.820	0.0011	0.0026	0.68	-0.0120	-0.0083
		(0.0092)	(0.0054)		(0.0065)	(0.0036)
N		28,344	28,341		90,063	89,451
Charter enrolled	0.091	0.0140	0.0070	0.043	0.0011	0.0030
		(0.0069)	(0.0039)		(0.0028)	(0.0017)
N		27,176	27,173		83,970	83,502
Exam enrolled	0.065	0.0047	0.0059	0.043	-0.0016	-0.0019
		(0.0081)	(0.0042)		(0.0032)	(0.0017)
N		15,818	15,818		83,970	83,502
			B: Baseline covariates			
Black	0.482	0.0019	0.0040	0.305	0.0014	0.0016
		(0.0131)	(0.0073)		(0.0062)	(0.0037)
Hispanic	0.366	0.0013	-0.0064	0.442	0.0047	0.0050
		(0.0126)	(0.0070)		(0.0072)	(0.0040)
Female	0.505	0.0207	-0.0008	0.515	-0.0034	-0.0038
		(0.0132)	(0.0073)		(0.0074)	(0.0040)
Special education	0.182	-0.0044	-0.0075	0.079	0.0023	-0.0001
		(0.0104)	(0.0057)		(0.0040)	(0.0023)
Limited English proficiency	0.114	0.0035	-0.0014	0.049	0.0006	-0.0035
		(0.0081)	(0.0043)		(0.0033)	(0.0016)
Free and reduced-price lunch	0.805	-0.0035	0.0011	0.779	-0.0028	-0.0072
		(0.0103)	(0.0058)		(0.0066)	(0.0037)
Baseline math	-0.145	0.0033	0.0043	0.045	-0.0082	0.0024
		(0.0232)	(0.0127)		(0.0109)	(0.0060)
Baseline English	-0.112	-0.0144	-0.0084	0.036	-0.0087	0.0030
		(0.0243)	(0.0136)		(0.0118)	(0.0064)
N	24,879	24,879	24,833	75,686	75,686	74,936

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 are computed in the samples of Boston middle and high school applicants and New York high school applicants. For columns 2 and 4, the sample, instrument (non-neighborhood assignment), and controls are as in Table 3. For columns 3 and 5, the sample instrument (travel-time offered), and controls are as in Table 5. Travel time effects are per 20 minutes of travel. Exam school enrollment is restricted to the sample of 9th grade applicants in Boston.

Table A3: 2SLS Estimates of School Travel Effects on Behavioral Outcomes

		Boston			New York		
		All applicants	Black applicants	Hispanic applicants	All applicants	Black applicants	Hispanic applicants
		(1)	(2)	(3)	(4)	(5)	(6)
				A: Non-neighborhood enrollment			
Days absent		-2.1073	-2.5351	-4.2450	0.2248	0.1439	0.6216
		(2.4686)	(3.5440)	(3.9591)	(0.4278)	(1.1200)	(0.6878)
	Mean	26.118	25.405	27.348	13.086	14.875	14.985
	SD	42.20	41.91	41.35	20.00	21.64	21.11
	N	24,629	11,882	9,006	75,570	23,053	33,403
Number of suspensions		0.0732	0.0060	0.1460			
		(0.0675)	(0.1105)	(0.1070)			
	Mean	0.303	0.375	0.269			
	SD	1.608	1.374	1.991			
Disciplinary index		-0.0087	-0.0413	0.0391			
		(0.0526)	(0.0778)	(0.0895)			
	Mean	0.007	-0.002	0.069			
	SD	1.011	0.983	1.069			
	N	22,909	10,921	8,498			
				B: Travel time			
Days absent		0.5319	1.3816	-0.8008	0.7848	0.4354	1.4132
		(1.5096)	(2.0779)	(2.4337)	(0.2894)	(0.5961)	(0.4975)
	Mean	26.102	25.403	27.303	13.011	14.767	14.930
	SD	42.16	41.91	41.26	19.92	21.54	21.04
	N	24,583	11,851	8,992	74,935	22,829	33,124
Number of suspensions		0.0444	0.0536	0.0354			
		(0.0425)	(0.0618)	(0.0740)			
	Mean	0.300	0.370	0.269			
	SD	1.602	1.358	1.992			
Disciplinary index		0.0295	0.0372	0.0321			
		(0.0314)	(0.0454)	(0.0510)			
	Mean	0.005	-0.005	0.068			
	SD	1.009	0.979	1.070			
	N	22,863	10,890	8,484			

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. For Panel A, the sample, instrument (non-neighborhood assignment), and controls are as in Table 3. In Panel B, the sample instrument (travel-time offered), and controls are as in Table 5. The corresponding endogenous variables are 6th or 9th grade non-neighborhood enrollment 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 A4: 2SLS Estimates of School Travel Effects on School Characteristics

		Boston			New York		
		All applicants	Black applicants	Hispanic applicants	All applicants	Black applicants	Hispanic applicants
		(1)	(2)	(3)	(4)	(5)	(6)
		A: Non-neighborhood enrollment					
Student-teacher ratio		0.176	0.514	0.295	0.183	0.022	0.264
		(0.155)	(0.246)	(0.233)	(0.031)	(0.082)	(0.045)
	Mean	13.11	13.00	13.02	4.43	4.34	4.30
	SD	3.10	3.20	2.92	1.37	1.53	1.36
Percent of teachers licensed in teaching assignment		0.009	0.012	0.004	0.012	0.022	0.016
		(0.008)	(0.012)	(0.012)	(0.003)	(0.008)	(0.005)
	Mean	0.90	0.89	0.91	0.79	0.77	0.77
	SD	0.14	0.15	0.12	0.16	0.17	0.17
Percent of core academic classes taught by highly-qualified teachers		0.027	0.031	0.026	0.006	-0.001	0.017
		(0.005)	(0.008)	(0.008)	(0.003)	(0.006)	(0.004)
	Mean	0.88	0.88	0.88	0.86	0.85	0.84
	SD	0.11	0.12	0.11	0.11	0.11	0.11
	N	22,709	10,888	8,353	56,871	17,300	25,194
		B: Travel time					
Student-teacher ratio		0.500	0.726	0.561	0.208	0.113	0.326
		(0.110)	(0.159)	(0.168)	(0.022)	(0.048)	(0.032)
	Mean	13.11	13.00	13.02	4.44	4.34	4.30
	SD	3.10	3.20	2.92	1.37	1.52	1.36
Percent of teachers licensed in teaching assignment		0.001	-0.004	0.006	0.015	0.015	0.020
		(0.005)	(0.007)	(0.007)	(0.002)	(0.004)	(0.004)
	Mean	0.90	0.89	0.91	0.79	0.77	0.77
	SD	0.14	0.15	0.12	0.16	0.17	0.17
Percent of core academic classes taught by highly-qualified teachers		0.010	0.016	0.002	0.010	0.004	0.017
		(0.003)	(0.005)	(0.005)	(0.002)	(0.003)	(0.003)
	Mean	0.88	0.88	0.88	0.86	0.85	0.84
	SD	0.11	0.12	0.11	0.11	0.11	0.11
	N	22,706	10,886	8,353	56,492	17,199	25,011

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. For Panel A, the sample, instrument (non-neighborhood assignment), and controls are as in Table 3. In Panel B, the sample instrument (travel-time offered), and controls are as in Table 5. The corresponding endogenous variables are 6th or 9th grade non-neighborhood enrollment and enrolled travel time. Travel time effects are per 20 minutes of travel. The sample is limited to offered applicants with non-degenerate risk of school assignment.

A.2 2SLS Inference with an Estimated Control Function

Section 4.2 discusses 2SLS estimates of distance and travel effects with first and second stages that can be written:

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

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:

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

where $u_i = \nu_{2i} + \kappa_2 \mu_i$. 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.