Unequal Assignments to Public School and the Limits of School Choice

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Abstract

This paper studies the limits of school choice policies in the presence of residential sorting. Using data from the Boston Public Schools choice system, I show that white prekindergarteners are assigned to higher-achieving schools than minority students, and that cross-race school achievement gaps under choice are no lower than would be generated by a neighborhood assignment rule. To understand why choice-based assignments do not reduce gaps in school achievement, I use data on applicants' rank-order choices to estimate preferences over schools, and consider a series of counterfactual assignments. I find that half of the gap in school achievement between white and Black or Hispanic students is explained by minorities' longer travel distance to high-performing schools. Differences in demand parameters explain a smaller fraction of the gap, while algorithm rules have no effect.

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1 Introduction

Since the late 1980s, many cities across the United States have adopted centralized school choice systems.[1] These systems allow families a choice among public schools, as opposed to neighborhood assignments where school districts assign students to schools based on proximity to residences. Neighborhood assignments replicate residential segregation and can sustain educational inequality across racial and income groups. By decoupling residences and schools, choice systems have the potential to reduce school segregation and equalize access to educational quality. As Boston Public Schools' superintendent wrote in the proposal for the 1988 choice plan: "My overall goal is to create a student assignment plan that provides all Boston students with high-quality desegregated education" (Boston Desegregation Project 1988).[2]

This paper asks how effectively choice systems reduce cross-racial gaps in access to high-achieving schools relative to a geographic assignment, and why. Using assignment data from Boston Public Schools (BPS), I begin by showing that under Boston's choice system, white prekindergartners are assigned to higher-achieving schools than Black and Hispanic students. Moreover, I document that cross-race school achievement gaps under choice are no lower than would be generated by an assignment based on proximity between residences and schools.[3] This suggests that there are limits to the effectiveness of school choice systems in equalizing access to high-performing schools.

[1] According to the non-profit *Education Commission of the States*, 47 states plus the District of Columbia have passed laws to allow or mandate a version of school choice. School districts that have implemented open enrollment include New York, Boston, Cambridge, Charlotte, and New Haven.

[2] Other school districts across the country share the view that a guiding principle of a student assignment plan includes creating equitable access to high-quality schools. This includes the Charlotte-Mecklenburg School District. See https://www.cms.k12.nc.us/boe/Pages/2010%20GuidingPrinciplesforStudentAssignment.aspx

[3] I generate a neighborhood assignment matching students to schools in order of proximity while taking into account school capacities. Specifically, I run a DA algorithm where preferences and priorities are fully determined by distance.

An effective policy response to the above depends on an understanding of why the effects of choice are limited. I argue that cross-race differences in choice-based assignments stem from either differences in demand for high-achieving schools, or from assignment rules that generate different probabilities of assignment conditional on parents' preferences. Thinking about demand, a key component is the distance between a school and the family's residence. If parents value proximity, as most papers in the literature find,[4] the benefit of attending a high-achieving school may be upset by the larger distances to these schools. For instance, parents might have less flexibility to adjust their routines to get their children to schools that are farther, or might worry about longer commutes in school buses. Then, differences in distance to high-achieving schools may translate into inequities in effective access to these schools. Also, differences in demand for location-independent school attributes can explain part of the gap.

Turning to rules, those that link assignments to a student's residential location may contribute to the cross-race gap. Two assignment rules in Boston do this. First, students are prioritized for assignment based on proximity to schools. This means, that students who live closer to high-achieving schools are more likely to get assigned to these schools.[5] If white families live closer to high-achieving schools they have higher priority at these schools more often. Giving priority to students based on proximity to schools is common across school choice systems. Examples include the cities of New York and Barcelona (Abdulkadiroğlu et al. 2005, Calsamiglia and Güell 2018). The second rule limits the menu of schools a student can apply to based on her residential location.[6] If the menus of Black, Hispanic and

[4]See Agarwal and Somaini 2019 for a summary

[5]Although this priority mechanically increases the probability that a student in the walk-zone of a school—defined as a one mile radius—has a higher probability of being assigned to it relative to students who lives farther, Dur et al. (2018) show that under the design of Boston's algorithm this rule did not importantly increase the fraction of walk-zone students relative to an assignment where the proximity priority is abolished. This is explained, as the paper discusses, by the precedence order between seats with a proximity priority and seats without it.

[6]BPS has had this type of restrictions since the early 1990s, and modified these menus in 2014, after the end of my study period.

white students have a different share of high-achieving seats, this restriction can mechanically contribute to differences in access to high-performing schools.

To disentangle the contribution of differences in distance related costs, preferences for location-independent school attributes, and assignment rules, I first estimate a model of school demand using data on the rankings submitted by all first-round applicants to prekindergarten between 2010 and 2013. Under some identification assumptions, the demand model allows me to separately identify parental preferences for proximity and the average valuation for each school net of travel costs. In a second step, I use the preference parameters estimated in step one to generate counterfactual assignments that help me quantify the contribution of each mechanism. Under these counterfactual assignments, I vary the distance to schools, parents' demand parameters, and the assignment rules, to quantify the change in the gap under each.

To estimate the contribution of travel costs I study a counterfactual change in residential location. Here I ask, how would the ranking and subsequent assignment of a single Black or Hispanic student change if he faced the menu of distances that a typical white student faces? To answer this question I randomly assign a counterfactual residential location from the distribution of white students' locations, and generate counterfactual assignments in the new location using the parameters of the demand model and the tractable assignment algorithm. By changing the location of a single student, I am able to evaluate the effects of a location change under the assumption that there are no changes to each school's demographic composition, that might affect parental preferences. This counterfactual parallels the Moving to Opportunity (MTO) experiment that relocated families from high-poverty neighborhoods to low-poverty communities in the late 1990's.[7] While the papers that study the MTO experiment study medium and long-term consequences of the relocation on health, income and labor outcomes; results from the counterfactual I propose capture the immediate effect of reducing the cost of accessing high-performing public education.

[7]Papers that study the impacts of this experiment include Ludwig et al. (2013), Chetty et al. (2016), Katz et al. (2001), Kling et al. (2007), Clampet-Lundquist and Massey (2008)

In this context, changing the residential location of a student doesn't only change the distance menu. Students who are relocated may select schools from a different choice menu, and have a proximity priority at a different set of schools. To disentangle the effect of travel costs and assignment rules, in a second counterfactual I generate assignments assuming that there are no restrictions on choice menus, and later consider the case where proximity priorities are eliminated. The results from these counterfactuals pin down the effect of algorithm rules, and in combination with the results from the location change counterfactual, pin down the effect of travel costs.

To estimate the effect of heterogeneity in the demand for location-independent school attributes, I simulate assignments assuming a change in parental demand parameters. I generate assignments where Black and Hispanic students take white students' demand parameters, while the original residential location of each student is unchanged. Results from this counterfactual highlight how differences in demand for any location-independent school attribute impact the observed gap. Differences across races in these parameters may capture any dimension of heterogeneity in parental preferences, including the racial composition of students, teachers and staff, the languages taught at each school, or the schools' teaching and discipline methods and curriculum choices.

I find that after a change in residential location, the gap in school achievement between minority students and white students was reduced by around a half, and a change in demand parameters explains 17% for Hispanic families and 32% for Black families. Eliminating proximity priorities and choice menu restrictions does not have any impact on the distribution of school achievement by race. This suggests that the effect of the residential location change is fully explained by changes in travel costs to high-achieving schools and not by location-specific assignment rules.

The salience of travel costs on the resulting school choice assignments has important policy implications. It suggests that school choice alone may not be able to upset the undesirable effects of residential segregation on public schools. Although the effects of distance may be weaker for high-schoolers and other older students who plausibly face lower transportation costs, barriers to their access to high-achieving schools in the earlier years may be critical for subsequent outcomes (Cunha and Heckman 2007). Moreover, since under school choice students are typically grandfathered into subsequent grades in the same school, students are likely to attend the same school from prekindergarten through the end of elementary school, amplifying the potential inequities.

Related Literature. This paper relates to several literatures. The first strand examines the effectiveness of school choice in generating system-wide improvements in school productivity. One side of the debate argues that by fostering school competition, choice systems boost school effectiveness (Friedman 1982, Chubb and Moe 1990, Hoxby 2003). Now, choice systems may not generate system-wide effects on school effectiveness if parents rank schools not by their effect on student outcomes but other school characteristics (Abdulkadiroğlu et al. 2020, Hastings et al. 2009, Barseghyan et al. 2014, Borghans et al. 2015). Abdulkadiroğlu et al. (2020) finds that conditional on peer quality, parental choices in New York City (NYC) are unrelated to school effectiveness, and Hastings et al. (2009) shows that minority families in Charlotte trade high-performing schools for schools with a low fraction of same-race peers. Related to this, this paper shows that given the salience of travel costs, parents might rank nearby schools of lower quality weakening competitive pressures for improvement.[8]

Moreover, this paper is related to the literature that studies the impact of school choice policies on student sorting. Most of the papers in this strand of literature focus on studying the effects of voucher policies on the composition of the student body by achievement and income, in both the public and private sectors (Epple and Romano 1998, Epple et al. 2004, Hsieh and Urquiola 2006, Altonji et al. 2015). I study an open enrollment plan, and the mechanisms that explain the observed sorting by race into schools by achievement levels.

This paper also contributes to the literature on neighborhood effects. I show that choice

[8] Allende 2019, studies the education market in Peru and finds that horizontal differentiation across schools explained by distance contributes to reduced competition in this market.

systems alone may not be sufficient to equalize opportunity for residents of impoverished neighborhoods. Growing up in low-opportunity areas has been found to be related to adult earnings and educational achievement (Chetty et al. 2014, Chetty et al. 2016, Chetty and Hendren 2018, Chetty et al. 2018), and some of these effects may be explained by the provision of public education in these areas (Biasi 2019, Laliberte 2018). This paper shows a first-order effect of location in the access to high-performing public education. I show that for many families the travel costs offset the benefits of attending high-achieving schools. The salience of families' perceived cost of attending a distant school and its effect on school demand is consistent with results that show substantial spatial variation of place-based effects for geographies as small as census tracts.

Finally, my analysis adds to a recent series of studies that leverage ranking data from centralized school assignments to study school demand and the properties of these assignments (Kapor et al. 2020, Luflade 2018, Agarwal and Somaini 2018, Fack et al. 2019, Calsamiglia et al. 2020, Hastings et al. 2009, Borghans et al. 2015, Abdulkadiroğlu et al. 2020, Abdulkadiroğlu et al. 2017, Oosterbeek et al. 2019, Son 2020). Some of these papers study parental demand for schools under mechanisms that provide incentives to misrepresent preferences, and evaluate the welfare implications of such mechanisms. Others use rankings to study the determinants of parental demand and its implications for choice systems. In the closest paper to mine, Son (2020) quantifies the contribution of students' residential location, parental preferences, admission policies and optimization frictions on racial integration and the proportion of students assigned to their top five schools using data from the NYC high-school match. I concentrate on access to school achievement as opposed to school segregation, and focus on prekindergarteners for whom schooling investments are likely to have lasting effects.

My analysis focuses on studying differences in average school achievement at the schools assigned to white, Black, and Hispanic students. Average achievement is a bundled measure of the academic ability of the students a school enrolls and the capacity of a school to generate improvements in student outcomes. In this paper, I am not able to speak of differences in effectiveness as opposed to peer composition, and how gaps in achievement map onto these.

Nevertheless, schools that enroll high-achieving peers have been found to be more effective (Abdulkadiroğlu et al. 2020). This suggests that inequities in the access to high-achieving schools imply some inequities in the access to effective schools as well.

The rest of the paper is organized as follows. Section 2 discusses the institutional context and the data. Section 3 summarizes the main observed differences in application behavior, and discusses and presents evidence on the mechanisms. Section 4 presents the model used to recover demand parameters, discusses the assumptions, the estimation, and analyzes the results. Section 5 describes the methodology and assumptions made to run the counterfactual exercises and the results. I conclude in Section 6.

2 Elementary School Choice in Boston

2.1 The Assignment Mechanism

Parents who wish to apply for a prekindergarten seat in a school within BPS are required to submit to the school district a ranking of programs and schools ordered by preference. A school typically offers a couple of general education programs, as well as programs for language learners.[9] Students can rank any number of programs with the condition that they are housed in a school the student is eligible for. Eligibility is determined by the student's residential location. During the study period, Boston was divided into three zones: the north, east and west zones (Figure 2a). Students were eligible for any general education program in their residence zone, plus any within a mile of their home. Geographic restrictions that determine eligibility for language programs are similar to those of general education programs, nevertheless these restrictions are not always binding (Pathak and Shi 2013a). I assume, as Pathak and Shi (2013a) do, that English language learner (ELL) students can

[9]At the prekindergarten level general education programs are typically referred to as inclusion programs. I exclude from my analysis students applying to substantially separate programs since assignments for these students don't always follow the assignment rules and allow for exceptions when needed.

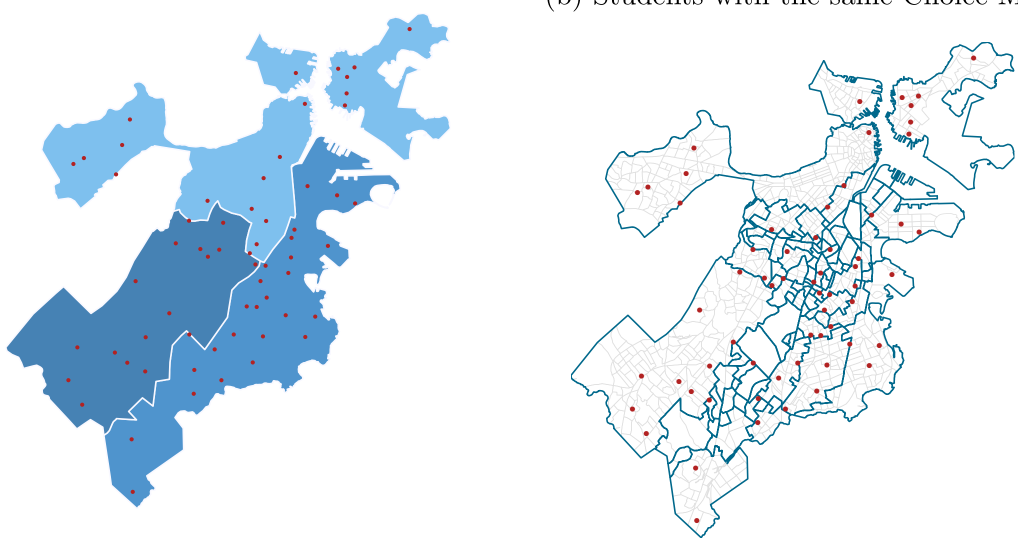
apply to any program across the city. There are also a handful of city-wide schools that accept applications from students all over the city. I refer to the set of schools a parent can apply to as the parents' choice-menu. Figure 2b shows a partition of the city that groups families with the same choice-menu.

Although parents in Boston apply to programs within schools, I make the simplifying assumption that parents rank schools. I transform school-program rankings into school rankings eliminating instances where different programs in the same school are ranked, and keeping the first time a school appears in the ranking (a similar assumption is made in Abdulkadiroğlu et al. 2020). Going forward I refer to parental preferences for schools.

Figure 1: Zoning and Choice Menus

(a) Zones

(b) Students with the same Choice Menu



Note: Red points are schools with a prekindergarten program in 2010. Choice menus are built using data on school and geocode coordinates.

Students are assigned to schools following a priority structure defined by the school district that is common across schools. Under this priority structure, students who have a sibling at a school have a higher priority at that school than students who do not have a sibling at the school. Also, students who live within a mile of a school—called the walk-zone of a school—have priority at that school over students that live farther away. Overall, students who both have a sibling and live in the walk-zone have the highest priority. These are followed by students who have a sibling, and then those who live in the walk-zone. The remaining students have the lowest priority. Ties within each group are broken with a random number assigned to each applicant. This guarantees that priorities generate a strict ordering of students.[10] School districts also determine school capacities, that is the number of seats available at each program. Preferences, priorities, and capacities feed the assignment algorithm that is a version of Gale and Shapley (1962) student-proposing DA algorithm (Balinski and Sönmez 1999; Abdulkadiroğlu and Sönmez 2003).

The DA algorithm guarantees that parents do not have incentives to misrepresent their true preferences when submitting rankings (Dubins and Freedman 1981, Roth 1982). This holds under the assumption that students are allowed to rank all desirable schools. Instances where school authorities restrict the length of submitted rankings may not generate truthful reports, even under the DA algorithm (Haeringer and Klijn 2009, Calsamiglia et al. 2010). BPS is one of a few districts that does not restrict the length of the submitted rankings. These properties make Boston a good setting for studying parental school demand.

Students assigned to a school farther than a mile from their homes are eligible for free bus transportation to and from school. The pick-up and drop-off location is set by the district to a location within the mile of a student's home. BPS estimates that the majority of riders are in elementary school, and attend a school with high populations of low income families. Among prekindergarten students, around half opted in for school transportation.

[10] This priority structure is typically used in half of the seats in each school, while the remaining seats ignore walk-zone priorities all together. A more detailed description of the algorithm is given in the Appendix D. Dur et al. (2018) and Sönmez et al. (2019) discuss this design and it's properties.

2.2 Data

I use two main data sources. First, data from BPS that covers the universe of first-round applicants to prekindergarten between the years 2010 and 2013. For each applicant I observe the rank-ordered list submitted, the school assigned or an indicator for whether the student was unassigned, and the priority that generated the assignment.[11] I also observe the residential location[12] and demographic information of the student including their race.[13] First-round applicants represent over 80% of admitted students (Pathak and Shi 2017); the rest apply in the second round and are assigned after first-round applicants.

Second, I use yearly data on school characteristics from the Massachusetts Department of Education (DOE). From this source, I measure school achievement using the fraction of third-grade students at each school scoring advanced or proficient in the Massachusetts Comprehensive Assessment System (MCAS) math test. Most of the schools that offer a prekindergarten program also offer a third-grade program, and only a few offer up to first grade. For these I do not have measures of school achievement.[14]

Using the location of each school and the geocode of residence of each student, I measure the distance between students and schools in one of two ways: first, as the walking distance between the geocode's centroid and the school, and alternatively as the linear distance between the two points. The former is obtained using Google maps travel estimates. Using these locations, I also generate the walk-zone priority status for each student-school pair and

[11] A student will be unassigned if he is rejected from every school on his submitted rank list. Students who are unassigned in the first round can reapply in the second round or search for options outside the school district

[12] Residential locations are coded by the school district at the geocode level. Geocodes partition the city in 868 polygons of average area of 0.1 sq. miles. The assignment algorithm is built using such geocodes, hence that level of aggregation does not represent any loss of information for purposes of the assignment algorithm.

[13] I remove from my sample students with an invalid geocode that represent around 2% of the sample

[14] 5 schools in each year offer up to first grade

the choice-menu of each student, recreating the procedure used by BPS.[15]

Ideally, I would have the sibling priority status of every student at every school. Nevertheless, I only observe the sibling priority status of student i at school j, if i was assigned to j with this priority. Throughout the analysis, I assume that all students that are not assigned with a sibling priority do not have a sibling at any school, and that students assigned with a sibling priority at j do not have a sibling at other schools. Using data on the priorities that generated each assignment, I find evidence in support of this assumption. I find that in most schools every student who applied with a sibling priority was admitted. This means that for the set of schools each student ranks, I am able to observe a sibling status when it indeed exists, with the exception of students who have a sibling priority at multiple schools or those who rank the sibling school sufficiently low and are assigned to a school ranked higher. In the first case, I'm only able to account for the sibling status at the sibling's school ranked higher.[16]

Students. The sample has $8,869$ applicants to prekindergarten between 2010 and 2013. Close to half of the applicants to prekindergarten in Boston are Hispanic, while Black and white students are around one-fifth of the sample each. Asian and other minority families make-up around 10% of the applicant pool. This composition is in contrast to Boston's resident makeup, where white residents account for about half of the population.

Families can choose from a set of 25 schools on average. This contrasts with other school choice settings, such as NYC's high-school system, where families choose from about 700

[15]Student i is in the walk-zone of school j if a one-mile radius from school j intersects the geocode of residence of i. Similarly, I define the choice-menu of each student using data on the zone in which each school and geocode lies.

[16]If the following conditions are satisfied a school did not reject a student with a sibling priority: First, if there are fewer assigned students than available seats then no student was rejected. Second, if a school accepted a student with either the walk-zone priority or with no priority then that school did not reject anyone with a sibling priority. Otherwise the resulting match would not be stable. The number of schools that do not satisfy either of these in 2010 is 3, in 2011 is 2 and in 2012 is 6. For these schools I cannot rule out that they rejected a student with a sibling priority.

Table 1: Student Descriptive Statistics

	All	Black	Hispanic	White	Asian	Other
Applicants	8,869	22.9	42.8	22.8	7.8	3.6
Tract Income	55,551	43,705	49,873	76,753	55,166	63,660
	(25,429)	(19,205)	(21,711)	(24,850)	(22,875)	(27,363)
Applications						
Size of Choice Menu	24.8	26.0	24.8	23.5	25.0	24.4
	(2.4)	(2.2)	(2.4)	(1.9)	(1.9)	(2.3)
Distance in Choice Menu	2.6	2.4	2.7	2.7	2.6	2.5
	(0.8)	(0.7)	(0.9)	(0.8)	(0.8)	(0.8)
Maximum distance in Choice Menu	5.6	5.5	5.8	5.3	5.9	5.3
	(1.3)	(1.1)	(1.3)	(1.5)	(1.2)	(1.4)
Length of Submitted List	5.0	5.5	5.0	4.8	4.1	5.7
	(3.1)	(3.4)	(3.0)	(2.8)	(2.7)	(3.6)
Share English Language Learners	37.5	19.4	58.2	11.4	64.7	11.7
Assignments						
Assigned Rank	1.8	1.9	1.7	1.7	1.6	2.3
	(2.2)	(2.1)	(1.8)	(2.7)	(1.6)	(3.3)
Distance to Assigned School	1.2	1.3	1.3	1.0	1.1	1.2
	(1.3)	(1.3)	(1.3)	(1.0)	(1.1)	(1.2)
Share Assigned with Sibling Priority	36.0	31.3	34.4	43.8	40.0	33.9
Share Assigned with Walk-Zone Priority	48.4	47.4	46.6	53.5	48.1	49.1
Share Unassigned	26.1	23.0	24.2	33.2	22.7	30.8

Note: The first row of the table shows the total number of applicants and the share in each group. The second row shows the average tract-level household income taken from the 5 year 2010 ACS (I match the geocode of each applicant to a census tract by overlaying both geographies and keeping the tract with the largest share of each geocode's area). For the rest of the statistics, I show the mean and below the standard deviations in parenthesis with the exception of variables marked as shares, in which case I show a fraction. The average size of the choice menu and length of submitted list are measured in number of schools. I show linear distances measured in miles. The distance between a student and a school is the linear distance between the coordinates of each school and the centroid of the geocode of residence of each student. The length of the submitted list and the rank of the assigned school are computed under rankings transformed from school-program based to school based, the numbers under these transformations are smaller than the ones obtained under the school-program rankings. The share of students assigned with a sibling or walk-zone priorities are expressed as a fraction of all assigned students. If a students is assigned with a sibling and a walk-zone priority then it is included in both categories.

options (Son 2020). Out of these options, families typically rank five options. Black students submit longer lists while white students submit shorter lists, potentially reflecting that outside options of white families are ranked higher among public schools.[17] Students who are unassigned after running the assignment algorithm may apply in a subsequent round. Since prekindergarten attendance is not mandatory, there are applicants who are not assigned to any school and who need to search for options outside of the public school district. About a quarter of the students that apply in the first round are unassigned, and out of all unassigned students near 75% do not enroll in any public school.

Figure A.2 shows the spatial distribution of students by race. Although there are clear sorting patterns, students of all races can be found across the city. One way to quantify this is to zoom close to each school and see the distribution of residents in a close buffer by race. If I consider a 1.2 mile radius around every school, I find that on average there are several hundred students of each race who can apply to each school; and for all schools there are students of all races. Similarly, looking at applications I find that the average school has a couple hundred applicants from each race, and every school has applicants of all races (Table A.2).[18]

Schools. Between 2010 and 2013, there were a total of 67 public schools that offered a prekindergarten program and not all schools had prekindergarten seats in all years. There is substantial variation in students' demographic characteristics and school achievement among these schools. While on average the share of third-grade students scoring advanced or proficient in math is 46%, the school with the lowest achievement had 2% of students scoring advanced or proficient, while for the highest-performing school the fraction was 90%. On average, schools have 32% Black students and 15% white students. Since both white and Black students represent about 20% of all applicants, this reflects the fact that schools with few white students ($< 10\%$) are about four times more likely than those with few Black stu-

[17] Son (2020) documents something similar in the case of NYC's high-school choice system.

[18] I chose a 1.2 miles buffer because this is the average linear distance students travel to their assigned school. Choosing instead a one-mile buffer gives similar results.

Table 2: Descriptive Statistics: Schools

	Mean	StDev	Min	Max
Capacity	30.9	15.7	6.0	108.0
Achievement				
% Scoring Advanced-Proficient Math	46.1	19.2	2.0	90.0
% Scoring Advanced-Proficient English	37.8	16.0	10.0	86.0
Demographics				
% Black Students	32.0	19.3	2.1	79.7
% Hispanic Students	44.2	19.3	14.3	91.1
% White Students	14.6	14.7	0.0	65.8
% Low Income K Students	67.5	19.8	7.7	100.0
Observations	258 (67 distinct schools)			

Note: I do not observe achievement data for all schools in all years. There are a total of 17 missing observations (school-year pairs) of schools that do not offer third grade or for which data is restricted due to a small set of test takers.

dents. Each school has on average 70% low-income students, and the school with the lowest fraction of low-income students has 8%.

3 The Gap in School Achievement and the Possible Explanations

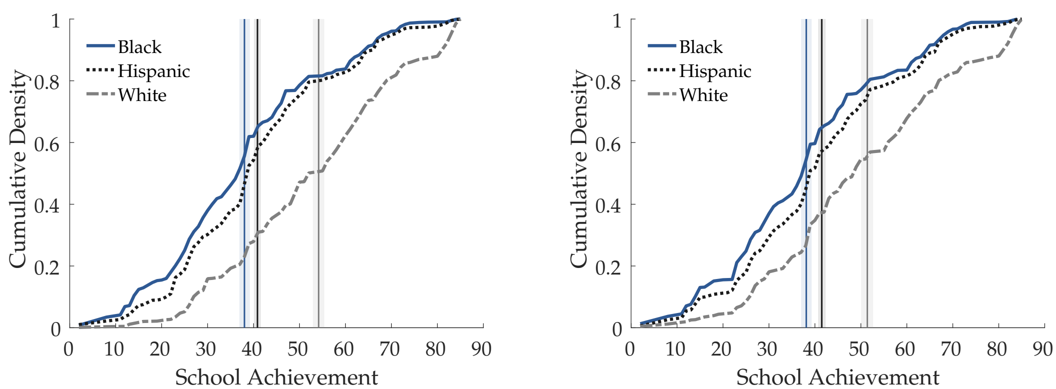
In this section I describe the two main facts that motivate the paper. Then, I discuss the mechanisms that can explain why in a choice setting we do not see a more equitable access to high-achieving schools. Finally I provide some evidence on the relevance of each mechanism.

3.1 The Racial Gap in School Achievement

Between 2010 and 2013, white prekindergarteners in Boston were assigned to schools that had higher average achievement and a smaller fraction of low-income students and minority students than their Black and Hispanic peers. While the average white student was assigned to a school where more than half of students scored advanced or proficient, these measures were close to 40% for Black and Hispanic students (Figure 3a). In terms of demographics, the average white student was assigned to a school with nearly 50% low-income kindergarten students, and for Hispanic and Black students the percentage is closer to 70%.[19]

Figure 3: Distribution of School Achievement under School Choice and Neighborhood Assignments

(a) School Choice DA Assignment (b) Neighborhood Assignment



Note: On the left, I plot the distribution of school achievement for the students assigned to prekindergarten between 2010 and 2013 by BPS. The measure of school achievement is the fraction of third-grade students in each school who scored advanced or proficient in the MCAS math test. On the right, I plot the distribution of school achievement under a counterfactual assignment where the same set of students are assigned to the school closest to their home, respecting school capacities.

Moreover, cross-race differences in school achievement under the choice system are not lower

[19]If instead I considered the achievement and demographic characteristics of each school one year prior to the assignments these numbers don't change much. The gap in school achievement is 0.4 pp larger for Black students and 1.5 pp larger for Hispanic students. Data from a year before assignments are measures of the characteristics of schools that are observable to parents when they apply for admission at prekindergarten.

than those generated under a neighborhood assignment rule. Comparing the distribution of school characteristics generated by an assignment rule that uses parents' stated preferences with a neighborhood assignment serves as a good benchmark. The latter shows how these gaps would look if a neighborhood assignment were implemented under the current residential choices in Boston. I generate this alternative assignment running the DA algorithm with the set of all students assigned via the choice system, and redefining their preferences and priorities to be determined exclusively by proximity: students prefer schools closer to home, and schools prioritize students that live closer to schools. Under the proposed neighborhood assignment, the distribution of school achievement is similar to that obtained under the choice rule (Figure 3b). Furthermore, I cannot reject the null hypothesis that the mean achievement is equal across assignments for Black and Hispanic students.[20]

Under this hypothetical experiment, I find that the choice and neighborhood assignments are different for around 80% of students. Out of these, white students sort into higher achieving schools, and although the distance travelled increases for all groups, white students experience the least increase. On average, minority students are not assigned to schools with higher achievement, although the average demographic composition of the schools assigned via the choice system suggests parents of minority students have preferences for desegregation: average shares of white and Black students at these schools are closer to the overall proportion of these groups in the student population (Table 3). This raises the questions: Why does giving parents the option to choose not translate into a more equitable access to high-achieving schools? How is this related with the trade-offs parents make across different school characteristics, the distribution of schools in space, and the assignment rules?

3.2 The Mechanisms

Studying how location effectively matters in choice-based settings is a first order concern to evaluate the equity consequences of choice-based policies. Even in a choice-based system

[20]Two tail p-values are 0.4 and 0.2 for Black and Hispanic students, respectively.

Table 3: Descriptive Statistics: Neighborhood Assignments and DA Assignments

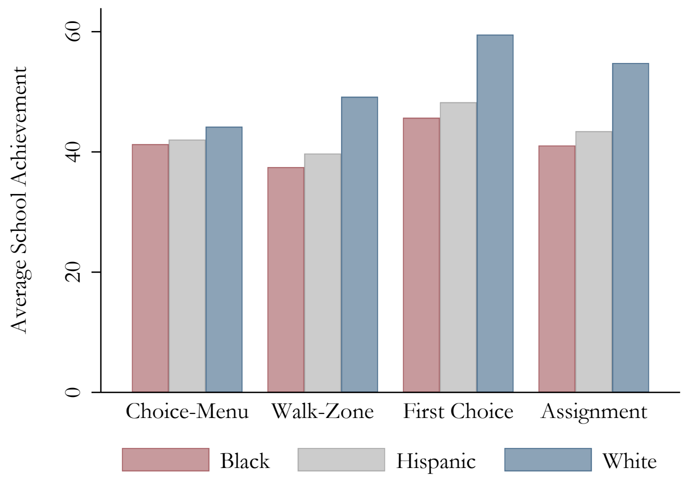
	All	Black	Hispanic	White
% Students assigned to the same school	21.1	17.2	19.1	28.9
Students assigned to a different school				
Achievement - DA	44.5	39.4	42.6	53.8
Achievement - Neighborhood	43.9	40.0	43.1	50.6
Distance - DA	1.5	1.6	1.5	1.3
Distance - Neighborhood	0.4	0.3	0.3	0.4
% Low income in Kindergarten - DA	68.0	71.5	70.8	56.5
% Low Income in Kindergarten - Neighborhood	68.6	71.7	70.7	58.7
% Black students - DA	30.9	43.0	27.3	23.5
% Black students - Neighborhood	33.0	42.7	32.1	23.7
% White students - DA	13.8	8.6	11.2	26.4
% White students - Neighborhood	12.8	7.2	11.8	22.7

Note: The first line in the table shows the fraction of students in each group who are assigned to the same school under BPS's assignment and the hypothetical neighborhood assignment described. Below, I show average school characteristics of the assigned school restricting the sample to students who are assigned to a different school under both rules.

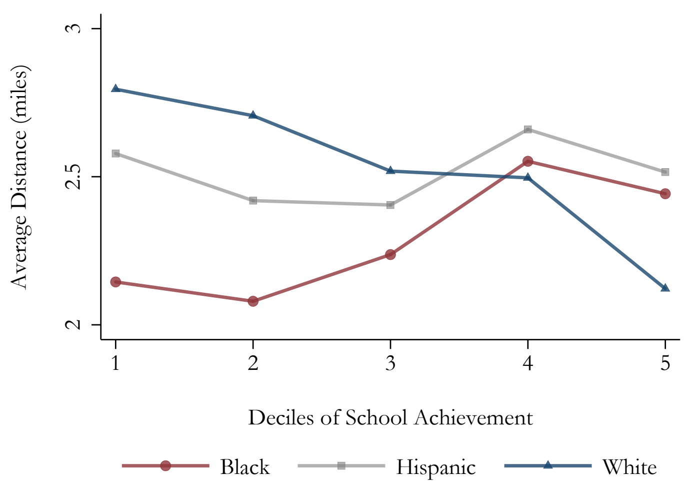
where the link between residences and schools is weakened, the residential location of families may play a crucial role in their school assignment. If parents value proximity, the benefit of attending a high-achieving school may be upset by high travel costs. Also, assignment rules that constrain geographically the choices of families, or that prioritize students based on proximity to schools, can generate geographic inequities even in the absence of travel costs.

Figure 4: Reduced form Evidence

(a) Average School Achievement



(b) Distance to Schools by Race and Achievement



Note: Panel (a) shows the average school achievement weighted by capacity at the schools in the choice-menu and walk-zone of applicants by race. Also, the average school achievement for the schools ranked first and the schools assigned to students by race. In panel (b) I plot the average linear distance between schools and students of each race, by school achievement deciles. The positive slope for Black and Hispanic students show that, conditional on their location these families trade-off proximity and achievement. Also, white students live on average closer to schools in the top deciles of achievement.

Differences in school assignments may also be responding to differences in parental demand for location-independent school attributes. The average valuation parents place on schools, net of travel costs, aggregate the relative valuation of each school's amenities and disamenities. This includes school characteristics that are likely valued by all parents, like appropriate infrastructure, student safety, a low student-teacher ratio, and high school achievement. It also includes school characteristics that can be amenities for some families and disamenities for others, like the racial composition of students and teachers, the languages taught at each school, or the schools' teaching methods and curriculum choices. Heterogeneity in school demand is generated if parents disagree in the valuation of any single school characteristic.[21]

An analysis of the characteristics of the schools in the choice-menu of students reveals that choice-menu restrictions are unlikely to be an important contributor to the school achievement gap. The average school achievement at the schools in the choice-menu of Black, white and Hispanic students is similar (Figure 4a). On the contrary, Figure 4b shows that high-achieving schools tend to be closer to white families and this may impact assignments via walk-zone priorities or a higher school demand explained by lower commuting costs.[22]. Moreover, the figure shows that while Black and Hispanic families make trade-offs between proximity and school achievement white families do not; the schools closest to them are on average higher-achieving than those farther.[23] Having Black and Hispanic families bear larger commuting distances to high-achieving schools may impact their demand for these schools. Consistent with this, Figure 4a shows that the schools ranked first by white families are higher-achieving than those of Black and Hispanic families, and this gap resembles that of assignments. Off course, cross-race differences in school demand can also be stemming from heterogeneity in parental demand for location-independent school attributes. A reduced form analysis of rankings is insufficient to disentangle the contribution of preference

[21] Allers 2019 in The Washington Post describes how issues of representation and discrimination can be at the heart of Black families' choices, and sometimes conflict with academic attributes of schools.

[22] Related to this Walters (2018) finds suggestive evidence that in Boston charter middle schools tends to locate in lower-achieving areas of the city.

[23] See Table A.1 for regression results on these relationship

heterogeneity and travel costs. In the next section I discuss the model and identification assumptions used to estimate the contribution of each.

4 Estimating Parent Preferences

In this section, I present the model and assumptions used to recover parental preferences for schools. At the end of the section I discuss the estimated parameters and the fit of the model.

4.1 Model and Identification

I model preferences using a random utility model where $i \in \mathcal{I}$ index students and $j \in \mathcal{J}$ index schools. To capture rich heterogeneity in preferences I estimate separate models for 20 subgroups of students defined by the intersection of students' covariates. This strategy follows that of Abdulkadiroğlu et al. 2020 in a school choice setting and Hastings et al. 2017 and Langer 2016 in other settings. The covariate cells are defined as the intersection of the students' race, whether the student is an English language learner, and the quartile of the census tract income. For each covariate cell c we use data on individual choices to estimate the model

$$u_{ij} = \beta_c d_{ij} + \delta_{cj} + \epsilon_{ij} \tag{1}$$

where student i is in cell c. The variable d_{ij} denotes the walking distance from i's residence to school j and β_c summarizes preferences for proximity for parents in cell c. The parameter δ_{cj} summarize the location-independent attractiveness of school j. This includes parents' assessment of school characteristics that are observable and unobservable to the econometrician. Finally, ϵ_{ij} represents i's idiosyncratic taste for school j. I assume the ϵ_{ij} are independent and distributed type-1 extreme value with scale parameter λ_c.

Truth-telling. I assume that submitted rankings are truthful. This means that parents rank all acceptable schools in true preference order. A school is acceptable if it is preferred to the outside option, which is the best option parents can find if unassigned in the first round. This assumption is motivated by the algorithm's incentive compatibility and the property that there are no restrictions on the number of schools parents can rank. Having restrictions over the length of submitted lists, even under the DA mechanism, can generate reports that are not truthful (Haeringer and Klijn 2009, Calsamiglia et al. 2010, Luflade 2018). Boston's choice system satisfies both properties.

Truth-telling can be violated if admission outcomes are largely predictable. In this case, parents may misrepresent preferences by not ranking schools that are desirable but where parents perceive a low probability of admission. This is more likely to occur in settings where an applicant knows her own priority and the distribution of priorities before applying. For example, a college choice setting where priorities are determined by a test score and historical cutoffs are observable to applicants. In the case of Boston, although parents can observe the category where they lie in the priority ladder, meaning, they know their sibling and walk-zone status, they do not observe the random number that determines their actual priority ranking, nor do they observe historical cutoffs to predict the fraction of applicants with a sibling and walk-zone priority. Moreover, even if parents were able to predict these probabilities with some level of accuracy, analysis of the admissions data reveals that there are only two programs (school-program combinations) that did not admit any students without a priority during the period 2010 to 2012, meaning that the probability of being accepted without a sibling and walk-zone status was not zero for the overwhelming majority of programs.

Consideration Set. I assume that students consider all schools in their choice set. This means, families can process information about all the schools they are eligible for and can rank all those options. The assumption is motivated by the relative small size of choice sets in this setting, where families have an average of 25 schools to choose from. This is in contrast with assumptions made in Son 2020, where families are asked to choose from around 430 high school programs in New York City. The author estimates that in this context families are aware of about 65 programs.

Consistent with the assumptions on consideration and truth-telling, if $R_i = (R_{i1}, \cdots, R_{il_i})$ is the rank-ordered list submitted by i and $\mathcal{J}_i$ is the choice-menu of i then

$$R_{i1} = \arg\max_{j \in \mathcal{J}_i} u_{ij} \tag{2}$$

$$R_{ik} = \arg\max_{j \in \mathcal{J}_i \setminus \{R_{im}: m<k\}} u_{ij} \tag{3}$$

Moreover, if u_{i0} is the utility of the outside option then,

$$u_{ij} > u_{i0} \quad \forall \quad j \in R_i \tag{4}$$

$$u_{i0} > u_{ij} \quad \forall \quad j \in \mathcal{J}_i \setminus R_i \tag{5}$$

The utility u_{i0} represents the expected utility at the time of the application of the best accessible alternative if unassigned in the first round. In practice, this includes options outside of the school district and undersubscribed schools within the district. Recall that out of all students unassigned in the first round, about 75% do not end up enrolling into any program within the school district. This means that of all the students for whom we get to observe their outside option, a majority have outside options that lie outside the school district. The remaining 25% do enroll in a school within the district after applying in a second round. Their choices in a second round may be additionally based on information acquired between rounds one and two about the availability of outside options. Concretely, parochial and other private options typically announce admission decisions simultaneously to BPS. If parents overestimated the probability of admission into their outside options, they will need to reconsider their choices within BPS. Second round applicants may also include parents who overestimated their probabilities of assignment into schools within the district in the first round. I do not model these dynamic considerations; instead, I interpret the parameters of the model as a summary of parents' preferences and expectations in the first round of applications.[24]

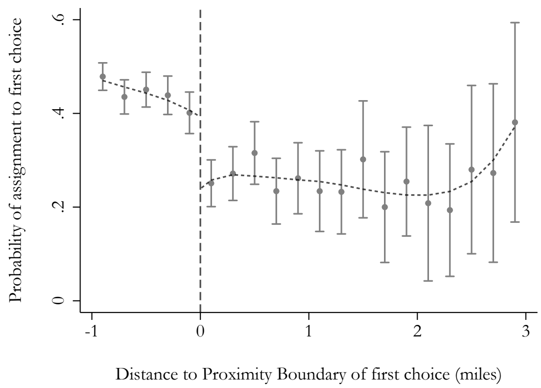
[24] Kapor et al. (2020) estimate interim beliefs in a similar setting

Identification. The parameters of the model $\{\beta_c, (\delta_{cj})_j\}_c$ are only identified modulo the scale parameters λ_c. This means that, unless we are willing to assume λ_c is common across races, the cell-specific parameters of the model can't be compared. To guarantee identification of the school-specific mean utilities, I normalize the utility of the outside option to zero. This means, the δ_{cj} are estimated as deviations from that of the outside good (Train 2009).

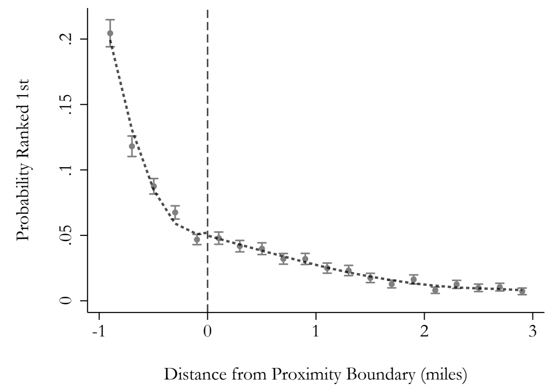
Two distinct sources of variation identify school mean utilities and preferences for proximity. Rankings of students who are equidistant from any pair of schools generate the variation used to identify school mean utilities, while parents who rank schools farther away on top of schools closer to their residence help identify parents' preferences for proximity. Identification of the distance parameter relies on the assumption that ϵ_{ij} is independent of d_{ij} conditional on the schools' fixed effects. I assume families in cell c sort into neighborhoods following desirable observable and unobservable school characteristics captured by δ_{cj}. The identification assumption is violated if families systematically choose their residence according to ϵ_{ij}. In that case, the distance parameter will be biased away from zero driving the conclusion that students care about distance more than they really do.

The geographic discontinuities generated by the assignment algorithm provide good variation to study how predominant sorting is in this context. School walk-zone boundaries generate a sharp discontinuity in the probability of assignment since students that are in the walk-zone have a higher priority than students who live outside. Figure 5a shows the discontinuity in the probability of getting assigned to the first ranked school at the proximity boundary. This means that families who choose their residence near a school they find desirable benefit from choosing their residence 0.9 miles from the school relative to 1.1 miles from it. If families are sorting on these boundaries, we may see parents who live less than a mile from the desired school rank it in the first position more often than parents who live slightly more than a mile from it. In Figure 5, I plot the probability of ranking a school in the first position as the distance to the proximity boundary of that school changes. The zero in the x-axis represents the one-mile proximity threshold. That is, at zero a student lives at exactly one mile from the school in question. To the left of zero, students live within the walk-zone. The downward

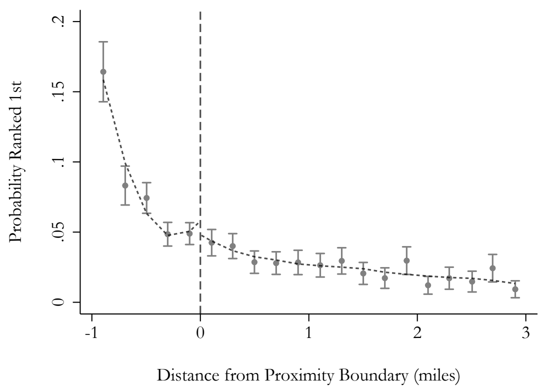
Figure 5: Proximity Priority and Ranking Behaviour



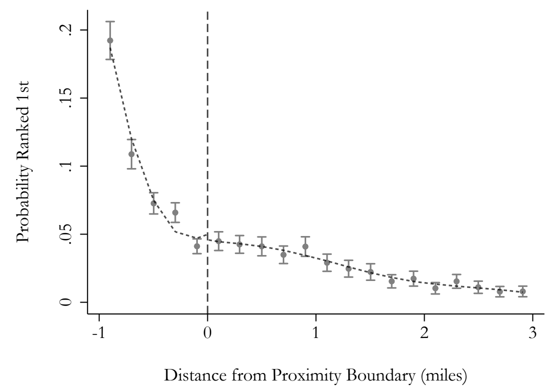
(a) Assignment to First Choice



(b) All Students - First Rank



(c) Black students - First Rank

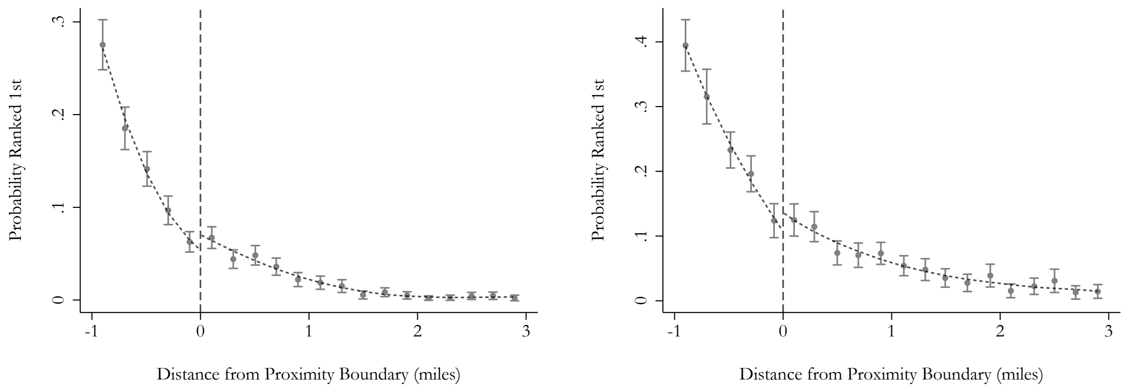


(d) Hispanic students - First Rank

Note: The graphs show bin-scatter plots with equally sized distance bins in each side of the boundary. For every student-school pair, I construct the linear distance between that student and the proximity boundary of the school. Negative values indicate that a student lives within the proximity priority zone. In panel (a) I restrict to every student's first ranked school and in the vertical axis I plot the probability of getting assigned to that first choice. In panels (b) - (f), the vertical axis is the probability that a family ranks a school as their first choice. Range plots show 95% confidence intervals, while the dashed line represents a local linear fit estimated on each side of the boundary. Competitive schools in panel (f) are the five schools that are ranked in the first position more often. Results are similar if I instead consider the schools who accepted the least number of students without any priority.

trend shows that parents value proximity while a discontinuity at zero is evidence of sorting. The plots show no evidence of sorting on these boundaries for students in any group, and

Figure 5: Proximity Priority and Ranking Behaviour (Continued)



(e) White students - First Rank

(f) Competitive Schools - First Rank

for a sample restricted to competitive schools.

The parameters estimated correspond to the estimates of preferences conditioned on the information parents have about the schools in their choice menu. Although it may be possible that parents don't have perfect information on all schools when they submit rankings, as other papers have studied in similar settings (Hastings and Weinstein 2008, Allende et al. 2019, Ajayi et al. 2020, Bergman et al. 2020), results are conditioned on the knowledge that parents realistically have and the assumption that the information structure is unchanged across counterfactuals.

Estimation and Inference. I estimate preferences for the sub-sample of Black, Hispanic and white students. I do not estimate preference parameters for Asian students and other racial minorities due to a small sample size.[25] The subgroups of English language learners that are Black or white are small, then I combine them in a single category.[26] A concern that arises from this is whether some of these language learners are of Hispanic origin who self-identify as Black or white. I find little evidence in support of that; less tan 5% of white

[25] For these groups I use the submitted rankings instead of simulated rankings in the counterfactuals.

[26] There are a total of 362 English learners that identify as Black and 263 that identify as white (Figure A.3).

English learners choose Spanish as their first language and about 0.3% of Black English learners do. Besides English, which is a choice by many English learners, those that self identify as white select Arabic as their first language while those that self-identify as Black select Cape Verdean more often (Figure A.4). In consequence there are a total of 20 covariate cells, with students spread across the city.

I estimate utility parameters for each cell by maximum likelihood. Details about the likelihood function are shown in Appendix C. Bootstrapped standard errors are obtained by sampling the data by student with replacement, keeping the application profile submitted by each student and re-estimating the model in each of 100 samples.

4.2 Parameter Estimates

Tables A.4, A.5 and A.6 show the estimated parameters for all cells. Negative signs for the distance parameters (β_c) show that parents value proximity, as many papers have found (see a summary in Agarwal and Somaini (2019)). School mean utilities δ_{cj}, summarize the cell-specific average attractiveness of a school after discounting the effect of distance. School mean utilities have a positive correlation with school achievement and the share of white students, and a negative correlation with the fraction of Black students and low income students (Table A.7). Nevertheless, the effect of the racial composition of a school on school mean utilities is stronger than that of achievement, suggesting demographics is a big component of parental preferences for schools and a plausible source of preference heterogeneity (Table A.8).

To assess how preferences for proximity compare across groups, I simulate rankings after varying the distance between schools and applicants. Figure A.5 shows the average number of positions in the ranking a school would lose, and the share of applicants lost if the distance between a school and an applicant was increased by 0.1, 0.5, 1 and 2 miles. I find that in both the intensive and extensive margins, the rankings of white students are more sensitive to increases in distance. These results are influenced not only by parents' preference for proximity, but also by the availability of substitutes near families' residences, and the length of the lists submitted. Shorter lists submitted by white students explain in part the larger responses in both the extensive and intensive margins.

Fit. To evaluate the fit of the model I compare the characteristics of an assignment carried out using the rankings submitted by parents to BPS with assignments based on rankings simulated using the demand model.

To simulate rankings using the parameters of the model I assume that families rank every school that is preferred to the outside option in preference order. Then, the position of the outside option determines the length of the simulated rankings. I will make this assumption through-out the counterfactuals as well. The parameters of the model closely predict the distribution of school achievement, share of low-income students and, share of Black students at the schools assigned to students in each group, as well as the distribution of distance (Figure A.6). In some sense this is not surprising since the estimation procedure ensured we would approximate the distribution of distance, and this alone heavily influences the school to which a student ends up being matched. The model also approximates fairly well the fraction of students that are assigned to their first, second, and n-th choice, as well as the fraction of unassigned students in each group. These statistics, and specially the fraction of unassigned students, depend importantly on the length of the submitted rankings since there are remaining seats after the first round concludes (both in our simulated version of market and in the market we observe). This suggests that the estimated values of the parameters—relative to the value of the outside option—captures well the trade-offs involved in choosing the number of schools a parent ranks.

5 Counterfactual Assignments

In this section, I describe how and under what assumptions the counterfactual assignments are generated, and then discuss the results. I simulate counterfactual assignments taking all the students that applied for a seat in 2011, and all the schools open for admission in that year. The counterfactuals will be used to estimate the contribution of the mechanisms described in Section 3.

5.1 Changing the location of a student

To estimate how much of the cross-race gap in school achievement can be attributed to the location of students, I evaluate how the submitted rankings and subsequent assignments of minority students would change if their residential location were randomly drawn from the set of white students' locations. After drawing a new residential location for a single minority student, I use the demand model to generate the ranking that the student would have submitted at that new location. Demand parameters do not change; nevertheless, the change in distance to all schools will shift travel costs. Also, choice-menu restrictions may limit and/or expand parents' available choices. I further assume that the length of the simulated rankings is determined by the position of the outside option—in other words, parents rank every school preferred to the outside option.

I consider the relocation of one student at a time. Changing the residential location of a single student guarantees that schools are unchanged across counterfactuals, and in consequence preference parameters are the same. If the locations of all students changed simultaneously we would expect, for instance, the demographic composition of schools to change. This means that I estimate the average impact of relocating a single minority student as opposed to the effect of relocating every minority student at the same time.

To build counterfactual locations, I randomly pair minority students and white students. In each counterfactual, the minority student will take the white student's residential location, choice-menu and, walk-zone priority. I consider three distinct assumptions to handle sibling priorities after a relocation. First, I assume students with a sibling lose any sibling priority they previously held. This is the case for a family that relocates and searches for a new school for both siblings. Second, I assume that families keep the sibling priority they had, meaning that the older sibling holds her seat and the youngest searches for one in the new location. Third, I use the parameters of the model to predict the school where the older sibling would have been assigned had they lived in the new location when the older sibling was applying for schools. Finally, I assume every student with a sibling has a sibling priority at their first-ranked school in the new location. This represents the extreme case where the older sibling is always assigned to the first ranked school, and hence the younger sibling is in a high priority group at that school. For each assumption, I generate assignments for all minority students at both their original location and their counterfactual locations and the corresponding distributions of school achievement. I simulate 60 counterfactual assignments for each minority student[27].

Assuming a student loses any sibling priority gives the same results to assuming they keep their original sibling priority. This is because in most cases the school where they held the priority is sufficiently far from their new residence. Having no sibling priority or having it in a school far enough, puts minority students in the counterfactual at a disadvantage relative to white students, for whom at least a fraction are in a high priority group at a school near their residence.

Despite being at a disadvantage, such a relocation translated into increased access to high achieving schools that reduced the gap by 46% for Black students and 43% for Hispanic students. Under the scenario where families lose any sibling priority, average school achievement increased 8 pp for Black students and 6 pp for Hispanic students.[28]

A counterfactual where parents hold a sibling priority at a school near their residence contributes to a larger reduction in the gap. To predict the place where an older sibling would have been placed in the new residential location, I start by noting that running the assignment algorithm from the hypothetical situation where no student has a sibling priority predicts well the distribution of school achievement of the sibling's school, for those students with a sibling. This is because the residential location of parents strongly predicts the school

[27]for each I draw three possibly distinct residential locations and at each I consider 20 draws of ϵ_{ij}

[28]If instead we assumed students held a sibling priority in the school they did before the relocation, the gap reduction would be the same.

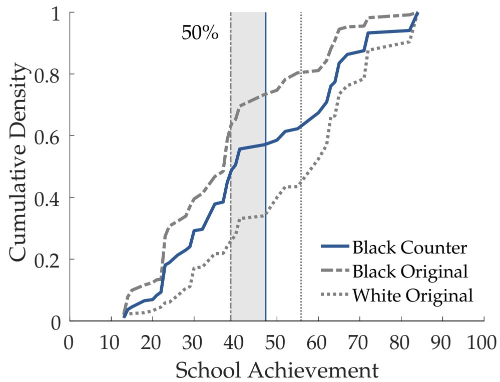
assignments. Figure B.9 shows the distribution of school achievement of the sibling's school for the students in my sample, and overlayed is the distribution of school achievement obtained for these same families after running the assignment algorithm assuming no one has a sibling. These graphs show that if we re-started in a world where no one has a sibling priority, we would rapidly converge to the assignments we see today, taking as input only families' locations. I use this observation and generate a counterfactual sibling priority at the school a student would have been assigned after a relocation if no one had a sibling priority.[29] Figure 6 shows the distribution of school achievement for the schools assigned to white, Black and Hispanic students in their original residential locations, and for Black and Hispanic students in their counterfactual locations. In this case, the gap between Black and white students reduced 50% and the gap between Hispanic and white students reduced 46%. Finally, if I assumed that students with a sibling have the priority at their first ranked school after a location change, I find the gap shrinks by 55% for Black students and 51% for Hispanic students.

In summary, changes in location contribute to increased access into high-achieving schools for minority students even when relocation comes at the cost of losing any sibling priority. If families do not lose this priority, the relocation has a larger effect on the gap. Concretely, for Black students the reduction in the gap after a location change is between 46% and 55%, while for Hispanic students it's between 43% and 51%. Recall that BPS provides eligible students bus transportation. In the absence of this service, the original gap in access to quality education is expected to be larger (Trajkovski et al. 2021), as well as the effect of a location change in that case.

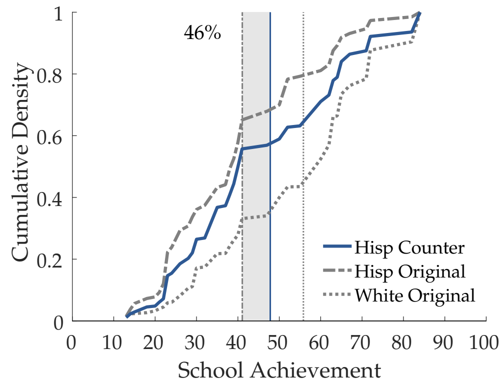
[29] After running the assignments under the no-siblings assumptions I get that around 15% of students with a sibling end up unassigned. To predict the school where these students would have had a sibling, I choose the school in their ranking where they were closer to be assigned—that is, the school where the applicant was closest to the last assigned student in the priority ranking. The distance is measured by counting the number of students between both.

Figure 6: Change location of a student: Achievement at the Assigned School

(a) Black students



(b) Hispanic students



Note: Distribution of achievement in schools assigned to Black and Hispanic students under a counterfactual assignment where they are randomly assigned to a new residence drawn from the distribution of whites' residences, and the sibling's school in the new location is predicted using the parameters of the model for the students with a sibling. This is compared to the distribution for Black, Hispanic and white students in their original location.

5.2 Changing demand parameters

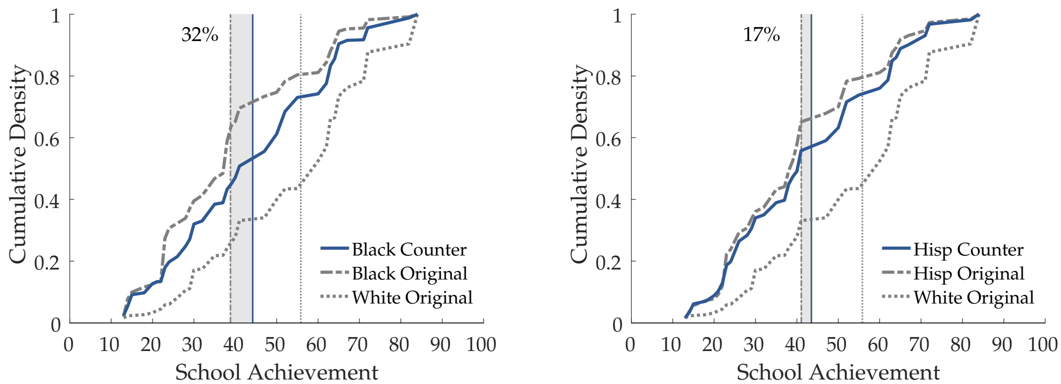
To study the contribution of demand heterogeneity on the gap in school achievement, I evaluate how the submitted rankings and subsequent assignments of minority students would change if the demand parameters of minority families where those of white parents. In this counterfactual the residential location, walk-zone, and sibling priorities of every student are unchanged. For consistency with the previous counterfactual, I change the demand parameters of one student at a time. The counterfactual ranking obtained describes how parents of a minority student would rank schools in their original residential location if their demand parameters were those of white parents.

Under the proposed change, Black and Hispanic students are assigned to schools with higher average achievement. Figure 7 shows the distributions of school achievement for white and minority students under the original setting and for minority students in the counterfactual. Mean school achievement increased 6 pp for Black families and 2 pp for Hispanic families. The gap reduced by 32% and 17% for Black and Hispanic students, respectively.

Figure 7: Change in demand parameters of a student: Achievement at the Assigned School

(a) Black students

(b) Hispanic students



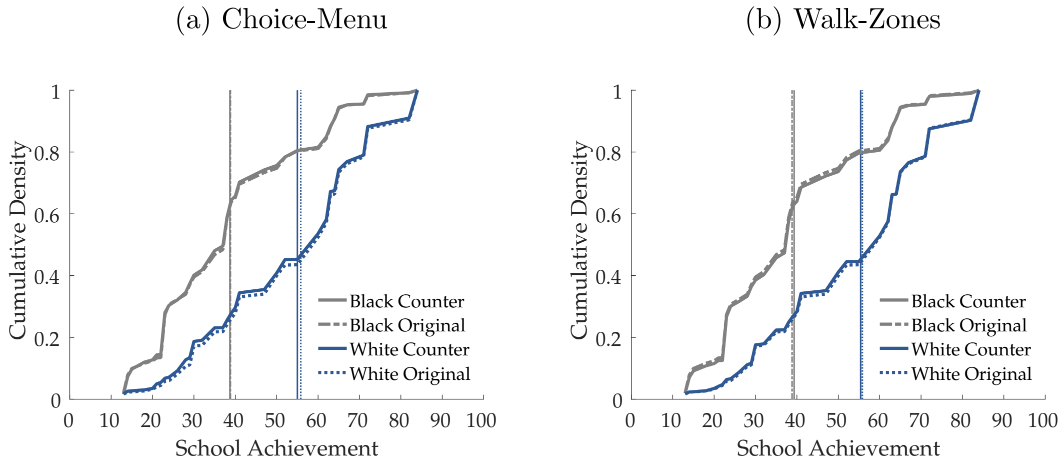
Note: Distribution of achievement in schools assigned to Black and Hispanic students under a counterfactual assignment where these students have the demand parameters of white students. This is compared to the original distribution of school achievement for Black, Hispanic and white students.

5.3 Eliminate Choice Menus and Walk-zone Priorities

The effect of location on school assignments is sizeable. When a student changes locations not only do her travel costs change, but also, her choice menu and the set of schools where she has a walk-zone priority change. To disentangle the effect of the last two from that of travel costs, I run two additional counterfactual assignments. In the first, I eliminate choice-menu restrictions and allow parents to rank schools from across the city. Under this setting, parents of a minority student can rank the same schools they would have ranked under the location change counterfactual. Then, the only reason why these rankings wouldn't coincide is differences in travel costs to these schools from both locations. In the second counterfactual, I eliminate walk-zone priorities and run the assignment algorithm assuming no one has this priority. Eliminating priorities doesn't change parental rankings, but it does change assignments via priorities. This counterfactual captures the effect of a change in the probability of assignment into a school, that is explained solely by the walk-zone priority. Notice that since these counterfactuals are about algorithm rules, I do not isolate the effect of the counterfactual into a single student; instead, I change assignment rules for all students simultaneously, including white students.

When limits to choice-menus and walk-zone priorities are eliminated, expected school achievement does not change. Figure 8 shows the distribution and average school achievement for Black students after eliminating choice-menu restrictions on the left, and the walk-zone priority on the right. Results for Hispanic students coincide (Figure B.10).

Figure 8: Eliminate location-specific rules - Black Students



Note: Distribution of achievement in schools assigned to Black and white students under a counterfactual assignment where choice menu restrictions are eliminated (on the left), and walk-zone priorities are abolished (on the right).

These results imply that these assignment rules do not contribute to the cross-race gap in school achievement. In consequence, the results suggest that the effect of location is entirely explained by changes in the distance to high-achieving schools and not by assignment rules that are location-specific. Moreover, these results suggest that changing these rules is not an effective policy response to increase access to high achieving schools for all students.

5.4 Change in the School Match After a Location Change

So far we have established that if a minority family faced the menu of distances that a typical white family does, they would access high-achieving schools at a higher rate. But, these higher-achieving schools may not be preferred by minority families to the schools assigned to them in their original locations, for a variety of reasons. Using the parameters of the model I can assess whether Black and Hispanic students are assigned to schools with higher mean value after a location change. To do this, I compare the location-independent value of the school assigned under the original setting and the counterfactual. Let $\mu(i) \in \mathcal{J}$ be the school assigned to i under the original setting and $\tilde{\mu}(i) \in \mathcal{J}$ be the school assigned to i under the counterfactual. If G_r is the set of students in racial group $r = \{B, H\}$, then the average change in school mean value for the students in G_r expressed in miles is

$$\sum_{i \in G_r} \frac{\delta_{c\tilde{\mu}(i)} - \delta_{c\mu(i)}}{|\beta_c| \cdot |G_r|}$$

After a location change, Black and Hispanic students are matched to schools that have on average higher mean-value. The average change in school value for Black and Hispanic students is equivalent to reducing students' travel distance by 0.2 to 0.5 miles. These results imply that the costs generated by distance not only restrict access to quality but result in matches that are on average of lower mean value to these families.

6 Conclusion

Among other objectives, choice-based systems are intended to increase equity and to foster diversity by offering students the option to sort into their preferred schools, while weakening the link between residence location and school assignments. I document that in Boston, cross-racial gaps in access to high-achieving schools are no lower under choice relative to a neighborhood assignment rule. I show that both cross-race differences in distance to high-achieving schools, and heterogeneity in demand for non-location school attributes contribute to this gap, while assignment rules that are location specific don't.

The salience of travel costs shows a first-order channel for why neighborhoods matter, highlighting how the effective provision of public goods can be affected by geography at very granular levels. In some way these results are not surprising. Most, if not all, of the papers that study parental school demand agree that distance is a key factor that determines parental choices. This paper takes this observation one step further and quantifies how much this cost limits the effectiveness of school choice policies in equalizing access to high-achieving schools. The results show that even in a generous choice environment where parents face minimal restrictions to their choices, distance can contribute greatly to inequity in access to high-achieving schools. This finding is not only relevant for the pre-kindergarten population. Not only we know that early investments can have lasting impacts on adult outcomes, but also, choice systems are typically designed to grandfather students into subsequent grades within a school. Then, even if travel costs are lower for older children, early assignments are held for several years after. In consequence, inequities in pre-kindergarten extend well beyond that period. Some limitations of my analysis are worth highlighting. I study the differential access to high-achieving schools as opposed to other measures of quality that are not mediated by peer composition, like school value-added. Although gaps in access to effective school may look different from those studied here, the fact that effective schools tend to enroll high achieving peers, and that parents don't value effectiveness over peer composition (Abdulkadiroğlu et al. 2020), suggest both that gaps in access to effective schools exist and that choice systems aren't likely to reduce them.

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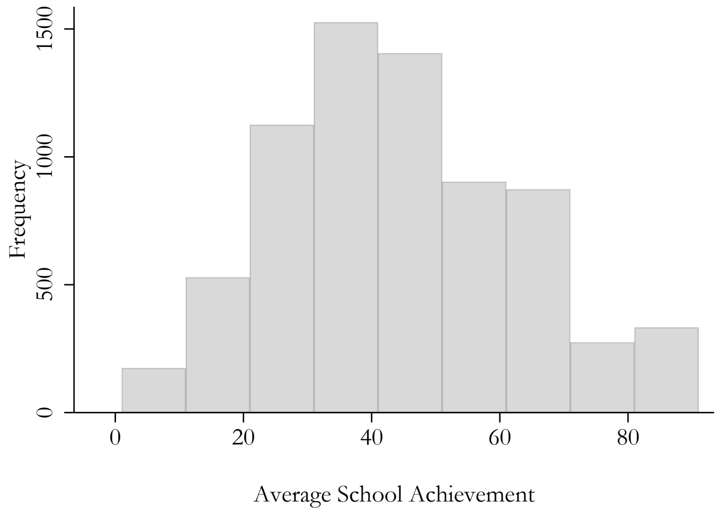
Appendix

A Supplementary Tables and Figures

Figure A.1: Histogram of School Achievement



Note: Histogram of school achievement weighted by school capacity for the years 2010 to 2012. School Achievement is measured as the fraction of third-grade students scoring advanced of proficient in the MCAS math tests.

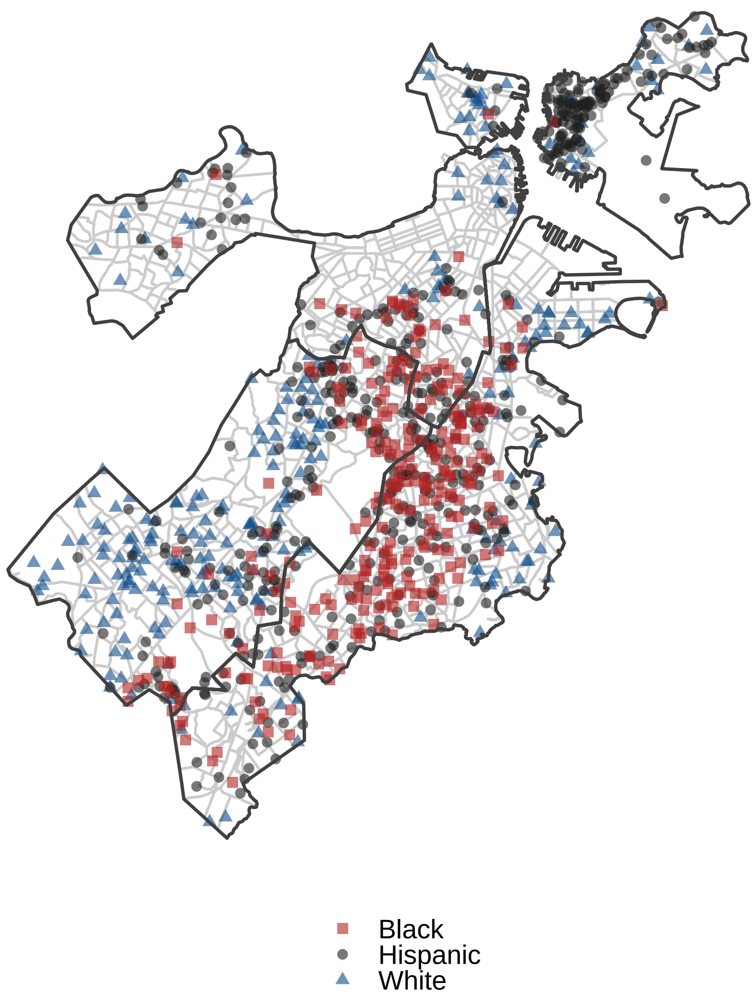
Table A.1: Relation Between Distance to Schools and School Achievement

	School Achievement		
	Black	Hispanic	White
Distance	1.08	0.06	−1.66
	(0.05)	(0.04)	(0.05)
Observations	46,647	82,853	41,894

Standard errors in parentheses

Note: Each column shows a regression between school achievement and distance. Each observation is a student-school pair, for schools in the choice-menu of every student. Standard errors in parenthesis.

Figure A.2: Spatial Distribution of Applicants by Race



Note: Each point represents 10 students from the 2010-2012 pooled data, located randomly at the census tract level.

Table A.2: Applicants per school and near each school

	Mean	StDev	Min	Max
Applicants per school				
Black	163.2	110.6	24	522
Hispanic	278.7	173.5	21	1,104
White	141.5	172.4	3	721
Potential Applicants within 1.2 miles of each school				
Black	924.8	819.0	28	2,748
Hispanic	1,512.6	881.0	212	3,280
White	564.7	443.5	36	2,152

Note: The first block shows statistics by race on the number of applicants per school. In the second block I show statistics by race on the number of students that live within 1.2 miles of each school, measured using linear distance.

Table A.3: Characteristics of schools with missing school achievement

	Not Missing Achievement	Missing Achievement
% Black	32.3	28.8
	(19.4)	(18.3)
% Hispanic	43.7	48.8
	(19.5)	(16.9)
% White	14.6	14.5
	(15.0)	(12.2)
% Low Income in K	67.4	68.7
	(19.9)	(19.4)
Observations	235	23

Note: Statistics of school year observations where I do not observe school achievement.

Figure A.3: Spatial Distribution of Applicants by Race

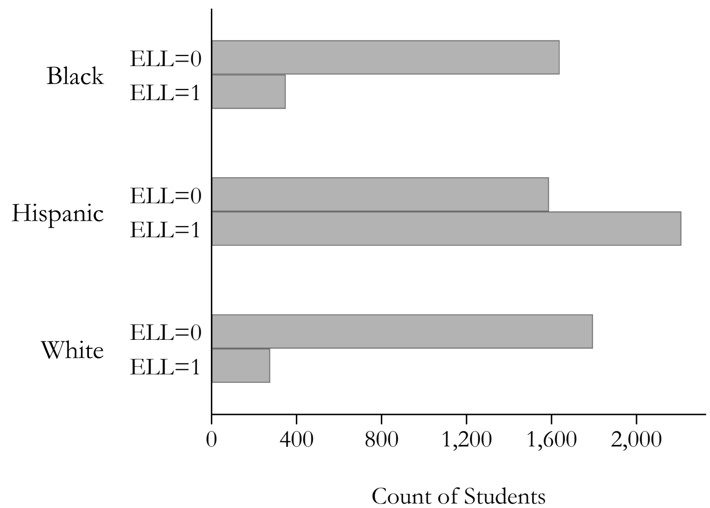


Figure A.4: Spatial Distribution of Applicants by Race

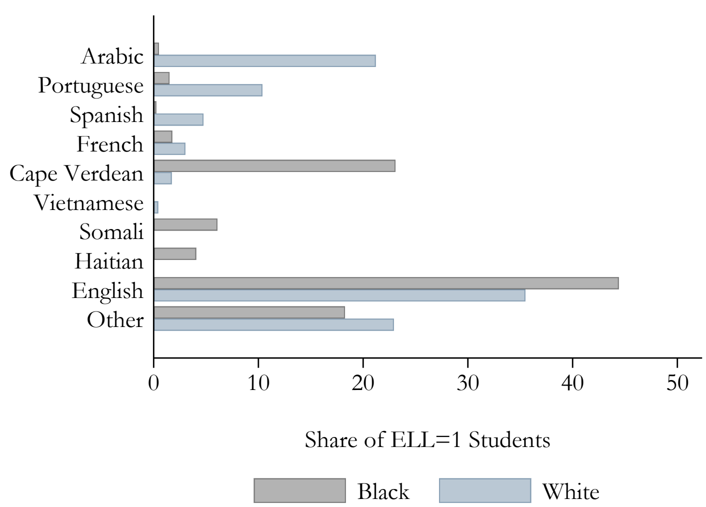


Table A.4: School Mean Utilities - Part 1

	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6	Group 7	Group 8	Group 9	Group 10	Group 11	Group 12	Group 13	Group 14	Group 15	Group 16	Group 17	Group 18	Group 19	Group 20
Distance	-0.35	-0.44	-0.36	-0.34	-0.34	-0.46	-0.46	-0.67	-0.70	-0.68	-0.73	-1.01	-0.79	-0.77	-0.76	-1.02	-1.13	-0.76	-0.81	-0.94
	(0.03)	(0.03)	(0.03)	(0.09)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.02)	(0.04)	(0.06)	(0.06)	(0.05)	(0.08)	(0.07)	(0.18)	(0.07)	(0.03)	(0.03)
sch 1	0.58	1.33	1.11	0.79	0.54	0.83	1.43	2.11	1.22	0.05	0.93	2.96	1.51	0.42	1.11	2.73	3.30	3.60	2.43	2.87
	(0.17)	(0.41)	(0.22)	(0.34)	(0.28)	(0.78)	(0.16)	(0.19)	(0.22)	(0.40)	(0.18)	(0.31)	(0.44)	(0.62)	(0.32)	(0.37)	(0.51)	(2.18)	(0.18)	(0.11)
sch 2	0.47	0.91	0.99	0.60	0.20	1.29	1.42	2.56	0.87	0.81	0.83	2.99	1.17	0.49	1.28	3.69	4.06	5.00	2.76	3.41
	(0.18)	(0.41)	(0.25)	(0.31)	(0.27)	(0.63)	(0.18)	(0.23)	(0.23)	(0.37)	(0.22)	(0.34)	(0.57)	(3.36)	(0.34)	(0.37)	(0.57)	(1.21)	(0.17)	(0.10)
sch 3	-1.27	-1.23	-1.74	-2.30	-1.58	-1.19	-0.16	-1.38	-1.82	-1.06	-1.35	-1.55	-0.77	-0.76	-0.84	-16.56	-22.39	-19.33	-14.31	-2.49
	(0.15)	(0.29)	(2.63)	(3.48)	(0.21)	(0.29)	(0.33)	(0.54)	(0.21)	(0.22)	(0.36)	(3.89)	(0.24)	(0.36)	(0.48)	(0.00)	(0.00)	(0.00)	(0.00)	(5.70)
sch 4	-1.29	-0.93	-0.94	-0.83	-0.16	-0.16	0.40	0.63	1.31	1.38	1.77	1.85	-0.43	-0.43	0.29	0.92	0.30	-0.55	0.20	0.03
	(0.12)	(0.16)	(0.19)	(0.39)	(0.11)	(0.14)	(0.13)	(0.17)	(0.09)	(0.12)	(0.12)	(0.19)	(0.28)	(0.35)	(0.36)	(0.29)	(0.47)	(2.72)	(0.19)	(0.13)
sch 5	0.19	0.27	0.69	0.43	0.30	0.10	0.03	-0.80	-1.45	-0.22	-0.43	-11.96	-0.03	0.35	1.75	-12.90	-23.65	-20.91	-0.45	-0.63
	(0.18)	(0.24)	(0.27)	(6.56)	(0.23)	(0.33)	(0.29)	(4.92)	(1.90)	(0.39)	(3.77)	(0.00)	(2.62)	(0.56)	(2.90)	(0.00)	(0.00)	(0.00)	(4.54)	(4.40)
sch 6	-1.12	-1.06	-0.83	-0.88	-1.65	-1.36	-1.16	-1.53	-1.59	-1.78	-1.39	-1.67	-1.06	-0.38	-1.10	-0.98	-0.94	-1.48	-1.01	-2.65
	(0.14)	(0.19)	(0.32)	(0.60)	(0.22)	(0.32)	(0.31)	(0.42)	(0.21)	(0.33)	(0.41)	(5.10)	(0.29)	(0.31)	(3.39)	(7.25)	(10.22)	(7.09)	(0.55)	(4.32)
sch 7	0.32	1.00	1.19	-0.50	-0.07	0.44	0.90	1.31	0.82	0.52	0.24	1.44	1.89	1.32	1.61	3.26	5.21	1.99	2.55	2.60
	(0.34)	(0.59)	(0.68)	(3.16)	(0.34)	(0.23)	(0.44)	(0.65)	(0.22)	(0.10)	(0.30)	(0.52)	(0.53)	(0.32)	(0.63)	(0.57)	(14.81)	(0.44)	(0.45)	(0.37)
sch 8	-0.10	-0.47	-0.47	-0.30	-0.79	-0.87	-0.95	-0.46	-1.13	-1.02	-0.67	-1.47	-0.79	-0.01	0.43	-15.18	-19.91	-20.33	-1.65	-0.37
	(0.18)	(0.15)	(0.12)	(4.67)	(0.26)	(0.24)	(0.18)	(5.22)	(0.28)	(0.28)	(0.23)	(6.58)	(0.52)	(0.37)	(0.27)	(0.00)	(0.00)	(0.00)	(0.50)	(5.13)
sch 9	-0.81	-0.73	-0.17	-0.28	-0.61	0.14	0.29	-0.15	-0.12	-0.92	-0.49	1.14	0.53	-0.62	-0.01	1.42	1.57	0.77	0.55	0.53
	(0.19)	(0.37)	(0.21)	(0.35)	(0.26)	(0.35)	(0.15)	(0.18)	(0.23)	(0.34)	(0.19)	(0.23)	(0.26)	(0.42)	(0.33)	(0.26)	(0.49)	(0.63)	(0.16)	(0.10)
sch 10	-0.21	0.14	-0.04	0.07	-0.44	0.64	0.07	0.17	0.25	-1.42	-0.27	0.75	0.52	-1.11	0.22	1.45	0.47	1.20	0.60	0.38
	(0.16)	(0.43)	(0.23)	(0.34)	(0.25)	(0.52)	(0.18)	(0.18)	(0.19)	(0.52)	(0.15)	(0.29)	(0.38)	(4.57)	(0.34)	(0.22)	(0.57)	(4.95)	(0.17)	(0.10)
sch 11	-0.26	-0.12	0.03	0.06	-0.03	0.65	0.38	0.96	0.36	-1.05	-0.16	1.48	0.80	-0.55	0.23	1.50	1.27	2.39	1.20	1.39
	(0.18)	(0.33)	(0.26)	(0.28)	(0.24)	(0.56)	(0.18)	(0.14)	(0.24)	(0.48)	(0.17)	(0.24)	(0.42)	(2.22)	(0.28)	(0.27)	(0.48)	(1.43)	(0.14)	(0.08)
sch 12	-1.40	-1.00	-1.31	-2.18	-1.49	-1.43	-0.89	-2.42	-1.94	-1.19	-0.98	-1.11	-0.91	-1.06	-1.17	-15.51	-21.61	-1.25	-0.93	-1.93
	(0.17)	(0.17)	(0.17)	(6.56)	(0.24)	(0.29)	(0.18)	(6.26)	(0.26)	(0.22)	(0.27)	(4.65)	(0.31)	(0.36)	(3.46)	(0.00)	(0.00)	(4.25)	(0.37)	(4.92)
sch 13	-0.25	-0.36	-0.42	-0.34	-0.18	-0.16	0.40	-0.04	-0.47	0.53	0.93	0.54	-0.66	0.54	0.79	0.02	-26.23	-0.50	-0.14	-0.59
	(0.18)	(0.18)	(0.19)	(0.63)	(0.23)	(0.25)	(0.21)	(0.31)	(0.16)	(0.16)	(0.19)	(0.26)	(0.42)	(0.24)	(0.47)	(0.68)	(0.00)	(5.32)	(0.35)	(0.27)
sch 14	-0.90	-0.30	-0.24	-0.18	-1.57	-0.86	-0.85	-1.02	-1.06	-1.67	-1.47	-1.32	-0.76	-1.43	-0.57	-0.34	-0.79	-17.75	-0.44	-0.65
	(0.13)	(0.33)	(0.31)	(0.35)	(0.18)	(2.30)	(1.39)	(0.34)	(0.15)	(0.36)	(0.43)	(0.39)	(0.26)	(3.25)	(2.81)	(2.67)	(4.70)	(0.00)	(0.37)	(0.22)
sch 15	-0.36	-0.47	-0.15	-0.80	-0.63	-0.08	-0.91	-0.37	-1.07	-0.80	-0.41	-0.48	-0.57	-0.56	-0.35	-0.33	-0.21	-0.24	-0.21	-0.05
	(0.12)	(0.27)	(0.39)	(0.41)	(0.16)	(0.27)	(1.89)	(0.33)	(0.13)	(0.19)	(0.24)	(0.29)	(0.27)	(0.29)	(0.37)	(0.52)	(6.32)	(6.39)	(0.42)	(0.31)
sch 16	-0.57	-0.64	-0.79	-0.58	-0.63	-0.91	-0.28	-0.98	-0.37	-0.01	0.21	-0.74	-0.03	-0.26	0.13	-1.34	-21.72	-1.10	-0.75	-1.43
	(0.12)	(0.13)	(0.15)	(2.11)	(0.17)	(0.19)	(0.15)	(2.28)	(0.13)	(0.15)	(0.16)	(3.08)	(0.23)	(0.26)	(0.43)	(6.88)	(0.00)	(4.15)	(0.38)	(3.82)
sch 17	-0.95	-0.63	-0.76	-0.67	-1.11	-0.35	0.19	-0.25	-1.27	-0.25	0.36	-0.43	0.02	0.37	0.90	-0.22	-25.00	0.58	0.75	0.08
	(0.16)	(0.14)	(0.21)	(3.95)	(0.23)	(0.19)	(0.18)	(0.36)	(0.19)	(0.14)	(0.16)	(0.35)	(0.22)	(0.23)	(0.35)	(2.63)	(0.00)	(0.33)	(0.21)	(0.24)
sch 18	-0.72	-1.02	-1.13	-0.60	-0.96	-1.52	-1.28	-1.54	-1.79	-1.50	-0.98	-0.59	0.07	-0.33	0.37	0.63	1.59	-0.88	-2.67	-2.08
	(0.14)	(0.17)	(0.16)	(4.03)	(0.24)	(0.26)	(0.23)	(4.92)	(0.31)	(0.32)	(0.25)	(5.77)	(0.27)	(0.34)	(0.38)	(3.26)	(15.74)	(2.33)	(4.38)	(3.47)
sch 19	0.03	0.46	1.07	0.02	-0.47	0.73	0.76	0.90	0.47	1.47	0.90	0.73	1.35	1.08	0.87	1.00	1.95	1.97	1.30	0.48
	(0.24)	(0.44)	(0.55)	(0.54)	(0.30)	(0.22)	(0.55)	(2.33)	(0.28)	(0.18)	(0.29)	(2.96)	(0.36)	(0.34)	(0.58)	(6.77)	(4.06)	(0.21)	(0.29)	(0.47)
sch 20	-0.53	0.67	-0.06	-1.08	-1.16	0.51	1.19	0.45	-0.67	0.93	0.62	0.70	0.20	0.87	1.30	0.40	1.46	1.44	2.03	1.25
	(0.32)	(0.41)	(4.26)	(1.77)	(0.42)	(0.31)	(0.55)	(3.06)	(0.41)	(0.16)	(0.36)	(4.94)	(0.52)	(0.36)	(0.41)	(6.86)	(6.41)	(0.43)	(0.33)	(0.55)
sch 21	-0.10	0.63	0.46	-0.75	-1.05	0.18	0.95	0.28	0.42	1.40	0.71	0.91	0.75	1.66	0.07	2.13	2.50	0.68	0.95	1.58
	(0.25)	(0.47)	(2.69)	(0.77)	(0.45)	(0.34)	(0.46)	(4.64)	(0.27)	(0.16)	(0.30)	(3.55)	(0.55)	(0.35)	(0.45)	(0.64)	(3.00)	(0.41)	(0.28)	(0.45)
sch 22	0.62	1.10	0.75	0.65	0.82	1.14	1.32	2.11	0.36	1.67	1.11	1.29	0.58	1.93	1.32	-10.82	4.55	2.25	2.86	3.32
	(0.20)	(0.20)	(0.17)	(4.20)	(0.27)	(0.19)	(0.17)	(2.55)	(0.29)	(0.24)	(0.20)	(3.17)	(0.49)	(0.38)	(0.31)	(0.00)	(8.32)	(0.85)	(0.32)	(0.50)
sch 23	-0.17	0.14	0.03	-0.87	-0.29	0.26	0.08	1.07	-0.49	0.50	0.20	-0.94	0.27	0.81	0.54	0.59	-14.51	-1.07	-0.17	0.50
	(0.17)	(0.18)	(0.14)	(6.86)	(0.24)	(0.20)	(0.13)	(3.41)	(0.29)	(0.29)	(0.18)	(6.18)	(0.43)	(0.41)	(0.32)	(4.84)	(0.00)	(5.41)	(0.22)	(5.44)
Race	Black	Black	Black	Black	Hisp	Hisp	Hisp	Hisp	Hisp	Hisp	Hisp	Hisp	B/w	B/w	B/w	B/w	white	white	white	white
ELL	N	N	N	N	N	N	N	N	Y	Y	Y	Y	Y	Y	Y	Y	N	N	N	N
Income	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
# student school pairs	18,873	10,631	9,794	3,152	11,832	9,128	11,436	7,063	16,913	21,308	11,062	5,455	4,757	4,980	3,417	2,660	1,392	3,041	12,098	25,562

Note: School mean utilities, for each school and group. Bootstrapped standard errors in parenthesis

Table A.5: School Mean Utilities - Part 2

	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6	Group 7	Group 8	Group 9	Group 10	Group 11	Group 12	Group 13	Group 14	Group 15	Group 16	Group 17	Group 18	Group 19	Group 20
sch 24	-0.43	0.07	0.25	0.64	-0.55	-0.16	0.41	1.13	-0.12	-1.13	-0.33	0.91	0.16	-1.18	-0.23	1.59	2.59	0.68	1.84	1.81
	(0.12)	(0.26)	(0.20)	(0.33)	(0.15)	(0.38)	(0.18)	(0.19)	(0.15)	(0.30)	(0.19)	(0.27)	(0.24)	(0.49)	(0.37)	(0.31)	(0.52)	(4.82)	(0.17)	(0.10)
sch 25	-0.30	-0.30	-0.63	0.04	-0.19	0.52	-0.23	-0.08	-0.29	-1.24	0.50	-0.07	0.19	-0.74	-0.30	0.09	-0.01	1.52	0.07	-0.01
	(0.13)	(0.43)	(0.39)	(0.31)	(0.18)	(0.46)	(0.31)	(0.22)	(0.11)	(0.30)	(0.24)	(0.28)	(0.22)	(0.49)	(0.55)	(0.44)	(1.17)	(3.73)	(0.22)	(0.17)
sch 26	-0.90	-0.76	-0.62	-1.72	-1.16	-0.18	-0.95	-1.91	-0.29	-1.38	-0.29	-0.28	-0.88	-2.36	-0.84	-0.30	-1.64	-15.42	-0.92	-1.80
	(0.15)	(0.34)	(0.32)	(4.92)	(0.18)	(0.46)	(0.33)	(0.56)	(0.10)	(0.26)	(0.22)	(0.27)	(0.29)	(6.28)	(2.43)	(2.63)	(10.10)	(0.00)	(0.35)	(0.30)
sch 27	-1.25	-2.68	-2.39	-1.35	-1.38	-2.51	-2.05	-1.25	-1.18	-2.55	-2.25	-1.90	-0.69	-2.26	-1.92	-0.30	-0.18	-19.81	-1.84	-1.92
	(0.13)	(0.53)	(0.44)	(3.56)	(0.21)	(2.05)	(0.42)	(0.45)	(0.18)	(1.85)	(1.44)	(6.41)	(0.31)	(3.81)	(4.84)	(6.24)	(10.31)	(0.00)	(2.34)	(1.64)
sch 28	-1.30	-0.60	-1.21	-2.96	-1.49	-1.37	-1.26	-2.51	-0.86	-0.26	-0.27	-0.24	-0.78	0.33	0.16	-1.40	-21.28	-2.42	-2.01	-3.05
	(0.12)	(0.13)	(0.18)	(5.90)	(0.17)	(0.25)	(0.22)	(3.44)	(0.12)	(0.16)	(0.20)	(0.40)	(0.29)	(0.22)	(0.47)	(7.18)	(0.00)	(7.12)	(1.89)	(4.29)
sch 29	-1.07	-1.16	-0.30	-0.52	-0.47	0.09	0.13	0.76	-0.30	0.35	0.04	0.59	-1.26	-0.65	-1.17	0.85	1.26	0.89	1.81	2.32
	(0.21)	(3.35)	(1.52)	(0.31)	(0.22)	(0.24)	(0.56)	(0.24)	(0.12)	(0.14)	(0.20)	(0.22)	(0.46)	(1.97)	(5.46)	(0.30)	(0.57)	(0.44)	(0.29)	(0.12)
sch 30	-0.86	-1.55	-1.50	-1.53	-1.02	-0.74	-0.84	-1.40	0.04	-0.95	-0.15	0.44	-0.03	-1.28	0.03	-0.14	-26.09	-16.55	0.14	-0.24
	(0.21)	(4.87)	(2.73)	(3.08)	(0.28)	(1.31)	(2.38)	(4.49)	(0.14)	(0.39)	(0.31)	(0.28)	(0.28)	(2.41)	(0.55)	(1.96)	(0.00)	(0.00)	(0.27)	(0.17)
sch 31	-0.57	-0.40	-0.09	0.47	-0.48	0.49	0.87	1.35	0.49	-0.76	0.84	1.23	-0.02	-2.49	0.08	1.36	2.11	3.43	2.14	2.00
	(0.15)	(0.41)	(0.27)	(0.29)	(0.20)	(0.44)	(0.23)	(0.15)	(0.10)	(0.25)	(0.16)	(0.16)	(0.31)	(6.56)	(0.41)	(0.28)	(0.54)	(1.70)	(0.17)	(0.11)
sch 32	-0.97	0.16	1.03	-0.97	-1.05	-0.54	-0.23	0.15	0.14	0.30	-0.30	-0.04	0.83	0.36	-1.47	-0.07	1.56	0.61	1.15	1.33
	(0.39)	(2.60)	(0.56)	(0.53)	(0.35)	(0.36)	(2.03)	(0.25)	(0.29)	(0.16)	(0.53)	(0.41)	(0.46)	(1.56)	(7.13)	(0.74)	(24.65)	(3.20)	(0.29)	(0.13)
sch 33	0.46	1.30	0.66	0.14	0.23	0.87	0.78	1.33	0.36	0.09	0.07	0.96	0.44	-0.41	0.37	1.39	3.07	1.08	2.30	2.29
	(0.14)	(0.35)	(0.44)	(0.44)	(0.22)	(0.27)	(0.33)	(0.24)	(0.13)	(0.26)	(0.19)	(0.27)	(0.30)	(0.47)	(0.34)	(0.31)	(2.35)	(1.98)	(0.22)	(0.14)
sch 34	-1.63	-1.18	-1.26	-1.42	-1.64	-2.09	-0.95	-12.98	-2.02	-1.64	-1.55	-13.95	-1.08	-1.52	-15.95	-0.44	-11.99	-21.56	-1.25	-11.57
	(0.34)	(0.28)	(0.30)	(7.39)	(2.71)	(6.31)	(0.50)	(0.00)	(0.48)	(2.69)	(2.52)	(0.00)	(3.94)	(3.09)	(0.00)	(7.02)	(0.00)	(0.00)	(5.21)	(0.00)
sch 35	0.53	0.28	0.20	-0.01	0.12	0.08	0.21	-0.09	-0.52	0.02	0.25	0.53	0.39	0.89	0.72	0.69	2.06	-0.19	-0.63	-0.83
	(0.13)	(0.11)	(0.12)	(0.57)	(0.18)	(0.16)	(0.11)	(0.41)	(0.14)	(0.16)	(0.15)	(0.40)	(0.27)	(0.17)	(0.42)	(3.98)	(22.41)	(0.41)	(0.26)	(0.43)
sch 36	-1.05	-1.12	-1.03	-0.24	-1.04	-0.87	0.01	0.86	-0.76	-1.40	-0.52	0.60	-0.05	-1.23	0.41	1.66	2.49	1.44	1.76	1.73
	(0.16)	(3.52)	(0.37)	(0.33)	(0.22)	(6.81)	(0.25)	(0.18)	(0.23)	(0.58)	(0.25)	(0.25)	(0.44)	(5.09)	(0.44)	(0.31)	(0.69)	(5.10)	(0.18)	(0.10)
sch 37	-1.05	0.55	0.61	-1.95	-0.18	0.85	0.07	0.21	0.37	1.16	0.81	1.17	1.37	0.88	0.31	1.18	3.08	0.82	0.58	-0.84
	(0.37)	(0.54)	(0.52)	(6.78)	(0.29)	(0.17)	(0.41)	(2.26)	(0.24)	(0.07)	(0.22)	(0.27)	(0.49)	(0.26)	(0.65)	(0.70)	(17.79)	(0.33)	(0.36)	(4.72)
sch 38	0.10	0.78	0.67	0.09	-0.10	1.08	1.16	2.12	1.14	0.65	0.68	2.65	1.23	-0.55	1.05	3.05	3.70	5.10	2.22	2.74
	(0.15)	(0.39)	(0.25)	(0.32)	(0.28)	(0.57)	(0.20)	(0.19)	(0.21)	(0.26)	(0.20)	(0.28)	(0.50)	(5.28)	(0.32)	(0.35)	(0.66)	(1.39)	(0.18)	(0.10)
sch 39	-1.05	-0.86	-1.14	-1.42	-1.59	-1.35	-1.47	-17.59	-0.84	-0.43	-0.24	-1.93	-0.41	-0.41	-0.24	-0.34	1.18	-1.83	-15.76	-12.23
	(0.18)	(0.18)	(0.23)	(7.14)	(0.26)	(0.26)	(0.30)	(0.00)	(0.18)	(0.16)	(0.23)	(6.76)	(0.27)	(0.29)	(2.49)	(6.94)	(17.00)	(6.97)	(0.00)	(0.00)
sch 40	-0.69	-0.38	-0.68	0.01	-1.10	-0.17	-0.17	-0.50	-1.34	-0.11	0.34	-0.25	-0.24	0.27	0.91	-0.39	-24.50	0.76	0.64	-0.82
	(0.12)	(0.12)	(0.14)	(0.65)	(0.22)	(0.20)	(0.16)	(0.33)	(0.19)	(0.14)	(0.16)	(0.33)	(0.24)	(0.24)	(0.40)	(3.22)	(0.00)	(0.34)	(0.18)	(0.40)
sch 41	-1.39	0.35	-0.10	-14.40	-0.72	0.21	-0.04	-0.61	0.33	0.48	1.20	1.21	0.91	0.69	1.19	1.02	2.39	1.24	0.88	-0.32
	(0.47)	(0.51)	(1.96)	(0.00)	(0.35)	(0.19)	(0.39)	(4.34)	(0.24)	(0.09)	(0.23)	(0.37)	(0.49)	(0.29)	(0.55)	(0.67)	(17.62)	(0.33)	(0.34)	(1.82)
sch 42	-1.19	-1.74	-1.10	-0.28	-1.35	-0.96	-0.50	0.11	-1.24	-2.02	-0.59	-0.46	-0.93	-2.81	-0.79	0.86	1.28	1.06	1.45	1.20
	(0.17)	(3.07)	(0.34)	(0.39)	(0.20)	(0.70)	(0.32)	(0.17)	(0.18)	(0.43)	(0.25)	(0.32)	(0.29)	(6.59)	(1.66)	(0.36)	(0.58)	(3.59)	(0.20)	(0.12)
sch 43	-0.03	1.02	1.15	0.48	-0.74	-0.41	0.26	0.73	0.11	0.08	0.07	0.39	0.91	0.89	0.68	1.79	2.07	2.13	3.04	3.24
	(0.21)	(0.37)	(0.41)	(0.35)	(0.33)	(0.31)	(0.47)	(0.24)	(0.24)	(0.15)	(0.28)	(0.31)	(0.36)	(0.33)	(0.55)	(0.44)	(0.76)	(0.32)	(0.23)	(0.15)
sch 44	-1.10	-1.21	-0.97	-0.59	-1.35	-1.21	-0.36	-0.71	-1.29	-0.36	-0.02	0.36	-0.14	-0.74	0.59	1.12	-20.92	0.13	-0.13	-0.67
	(0.19)	(0.19)	(0.16)	(4.86)	(0.29)	(0.28)	(0.15)	(0.46)	(0.25)	(0.20)	(0.20)	(0.49)	(0.31)	(0.37)	(0.53)	(2.90)	(0.00)	(0.39)	(0.18)	(0.29)
sch 45	-1.55	-1.61	-1.05	-0.42	-1.11	-0.88	-0.07	0.64	-2.08	-0.79	-0.01	-0.91	-1.76	-0.15	-0.11	0.48	0.61	0.69	1.59	1.55
	(0.23)	(0.21)	(0.16)	(4.23)	(0.29)	(0.18)	(0.15)	(0.27)	(0.29)	(0.19)	(0.19)	(3.57)	(1.55)	(0.22)	(0.52)	(3.30)	(12.86)	(0.27)	(0.21)	(0.18)
sch 46	0.13	0.37	0.81	0.61	0.24	0.57	1.23	1.98	-0.77	0.48	1.00	1.12	0.34	0.66	1.42	1.25	2.69	2.39	3.40	3.56
	(0.15)	(0.11)	(0.12)	(0.57)	(0.20)	(0.18)	(0.13)	(0.26)	(0.20)	(0.15)	(0.17)	(0.29)	(0.23)	(0.20)	(0.50)	(0.49)	(2.17)	(0.31)	(0.17)	(0.21)
sch 47	0.53	0.47	0.34	-0.15	0.15	0.32	0.07	-0.46	-0.13	0.50	0.25	0.15	0.67	1.05	0.88	-0.25	0.96	-0.96	-1.78	-11.81
	(0.15)	(0.13)	(0.13)	(2.34)	(0.20)	(0.16)	(0.14)	(4.16)	(0.17)	(0.18)	(0.17)	(2.80)	(0.27)	(0.23)	(0.29)	(6.29)	(9.73)	(3.91)	(0.50)	(0.00)
Race	Black	Black	Black	Black	Hisp	Hisp	Hisp	Hisp	Hisp	Hisp	Hisp	Hisp	B/w	B/w	B/w	B/w	white	white	white	white
ELL	N	N	N	N	N	N	N	N	Y	Y	Y	Y	Y	Y	Y	Y	N	N	N	N
Income	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
# student school pairs	18,873	10,631	9,794	3,152	11,832	9,128	11,436	7,063	16,913	21,308	11,062	5,455	4,757	4,980	3,417	2,660	1,392	3,041	12,098	25,562

Note: School mean utilities, for each school and group. Bootstrapped standard errors in parenthesis

Table A.6: School Mean Utilities - Part 3

	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6	Group 7	Group 8	Group 9	Group 10	Group 11	Group 12	Group 13	Group 14	Group 15	Group 16	Group 17	Group 18	Group 19	Group 20
sch 48	0.08	1.14	1.49	-0.46	0.52	1.50	1.41	0.79	1.29	1.52	1.59	2.20	2.18	1.63	1.92	1.88	4.53	1.06	1.13	0.96
	(0.27)	(0.41)	(0.55)	(0.61)	(0.28)	(0.19)	(0.36)	(0.45)	(0.18)	(0.09)	(0.27)	(0.32)	(0.40)	(0.26)	(0.53)	(0.43)	(0.99)	(0.36)	(0.31)	(0.38)
sch 49	-0.48	-0.91	-1.28	-1.60	-0.54	-0.75	-1.39	-0.60	0.02	-0.35	0.51	0.13	0.18	0.03	-0.32	0.24	-1.75	-0.79	-1.70	-1.27
	(0.09)	(0.15)	(0.19)	(0.45)	(0.12)	(0.17)	(0.23)	(0.25)	(0.10)	(0.17)	(0.17)	(0.26)	(0.20)	(0.27)	(0.41)	(0.48)	(23.53)	(3.62)	(0.58)	(0.29)
sch 50	-0.85	-0.68	-0.94	-0.71	-0.64	-0.49	-0.29	-0.85	-0.43	0.38	0.42	-0.16	-0.42	0.31	0.93	-0.72	-1.11	-0.78	-0.05	-1.44
	(0.16)	(0.16)	(0.22)	(4.38)	(0.17)	(0.22)	(0.17)	(0.40)	(0.14)	(0.13)	(0.19)	(0.31)	(0.21)	(0.27)	(0.34)	(2.80)	(28.32)	(5.00)	(0.25)	(0.46)
sch 51	-0.73	-0.62	-0.43	0.04	-0.83	-0.82	0.26	0.38	-1.15	-0.10	0.06	-0.50	-0.60	0.12	0.74	-0.33	-0.94	0.05	1.70	0.98
	(0.16)	(0.17)	(0.18)	(0.56)	(0.19)	(0.27)	(0.16)	(0.28)	(0.18)	(0.15)	(0.23)	(0.33)	(0.29)	(0.29)	(0.33)	(0.52)	(10.56)	(0.48)	(0.18)	(0.15)
sch 52	-0.90	0.81	0.08	-1.91	0.12	0.42	0.14	0.22	0.11	0.59	0.32	0.88	0.91	0.31	0.90	1.13	3.94	0.77	0.74	-0.72
	(0.42)	(0.42)	(2.33)	(5.34)	(0.30)	(0.17)	(0.46)	(2.70)	(0.23)	(0.08)	(0.27)	(0.29)	(0.51)	(0.31)	(0.56)	(2.17)	(15.29)	(0.29)	(0.36)	(4.51)
sch 53	-1.09	0.21	0.55	-14.32	-0.15	0.46	-0.24	-0.53	-0.06	0.51	0.33	0.40	-0.18	0.52	0.13	1.23	2.42	0.46	0.13	-0.39
	(0.56)	(1.47)	(0.51)	(0.00)	(0.28)	(0.19)	(0.53)	(4.87)	(0.23)	(0.09)	(0.22)	(0.51)	(3.62)	(0.26)	(3.89)	(2.19)	(15.30)	(0.32)	(0.42)	(4.00)
sch 54	-0.72	-0.46	-0.17	-0.49	-0.57	0.31	-0.09	-0.44	0.50	-1.26	0.41	1.01	0.02	-1.06	-0.69	0.66	-0.14	0.20	-0.04	-0.28
	(0.17)	(0.43)	(0.22)	(0.36)	(0.21)	(0.50)	(0.18)	(0.21)	(0.16)	(0.41)	(0.13)	(0.22)	(0.34)	(2.14)	(0.38)	(0.31)	(7.17)	(5.43)	(0.20)	(0.11)
sch 55	-1.30	-1.55	-0.39	-0.85	-1.36	-1.25	-0.82	0.15	-0.97	-2.57	-0.63	0.68	0.09	-1.75	-0.54	0.97	1.73	-12.79	0.64	0.88
	(0.29)	(4.98)	(0.30)	(0.45)	(0.45)	(6.46)	(0.36)	(0.27)	(0.29)	(4.89)	(0.27)	(0.28)	(0.38)	(5.57)	(0.42)	(0.34)	(0.63)	(0.00)	(0.20)	(0.11)
sch 56	-0.86	0.33	-0.53	-1.42	-0.75	-0.33	-0.55	-0.69	-0.63	-0.44	-0.66	-0.39	-1.21	-1.00	-1.15	-1.84	-1.24	-1.04	-1.56	-2.45
	(0.15)	(0.36)	(0.45)	(0.55)	(0.18)	(0.29)	(3.53)	(0.38)	(0.15)	(0.17)	(0.35)	(0.32)	(0.40)	(0.48)	(5.66)	(7.09)	(7.79)	(5.89)	(3.88)	(4.06)
sch 57	-0.91	-0.84	-0.98	-1.28	-1.54	-0.89	-1.14	-2.00	-1.65	-3.28	-2.06	-16.52	-1.42	-3.02	-2.25	-0.50	-0.76	-17.30	-0.47	-0.61
	(0.14)	(0.30)	(0.44)	(3.77)	(0.24)	(0.52)	(1.41)	(2.65)	(0.18)	(3.26)	(1.53)	(0.00)	(0.31)	(6.72)	(6.72)	(5.71)	(23.87)	(0.00)	(0.37)	(0.24)
sch 58	-0.83	-0.87	-0.92	-0.63	-0.98	-1.03	-0.42	-0.05	-0.97	-0.90	-0.23	-0.56	-0.60	0.29	0.46	-0.55	1.01	1.42	1.02	0.27
	(0.23)	(0.28)	(0.27)	(1.91)	(0.29)	(0.34)	(0.28)	(0.34)	(0.25)	(0.29)	(0.32)	(0.45)	(0.46)	(0.39)	(0.60)	(3.01)	(3.73)	(0.60)	(0.34)	(0.17)
sch 59	-0.01	-0.18	-0.37	0.66	-0.59	-0.09	0.14	1.56	-0.44	0.22	0.35	0.44	-0.03	0.20	0.69	0.65	2.37	2.40	2.99	3.03
	(0.19)	(0.21)	(0.24)	(0.62)	(0.29)	(0.26)	(0.25)	(0.27)	(0.20)	(0.21)	(0.33)	(0.31)	(0.36)	(0.45)	(0.61)	(0.47)	(0.72)	(0.45)	(0.24)	(0.23)
sch 60	0.48	0.48	0.42	0.61	0.08	0.15	0.51	0.59	-0.47	-0.02	-0.10	-0.10	0.02	0.30	-0.03	-1.26	-23.40	-0.14	0.36	0.09
	(0.10)	(0.11)	(0.13)	(0.44)	(0.17)	(0.16)	(0.15)	(0.35)	(0.16)	(0.18)	(0.17)	(0.48)	(0.27)	(0.21)	(0.33)	(7.02)	(0.00)	(0.50)	(0.28)	(0.33)
sch 61	-0.43	1.07	0.96	-0.55	-0.88	0.07	0.37	-0.13	-0.52	0.96	0.32	0.17	-0.06	0.69	-0.93	0.67	1.11	-0.41	0.28	-1.29
	(0.28)	(0.38)	(1.53)	(0.58)	(0.31)	(0.27)	(0.60)	(3.15)	(0.33)	(0.15)	(0.28)	(2.42)	(1.52)	(0.37)	(6.27)	(5.91)	(17.17)	(0.43)	(0.32)	(5.52)
sch 62	0.26	0.89	2.06	-0.20	-0.08	0.99	2.16	0.93	0.10	1.28	0.60	0.39	0.82	1.68	1.14	0.73	3.02	1.72	2.10	1.93
	(0.20)	(0.47)	(0.54)	(0.56)	(0.25)	(0.28)	(0.38)	(0.48)	(0.23)	(0.14)	(0.29)	(2.37)	(0.39)	(0.32)	(0.53)	(3.83)	(0.49)	(0.32)	(0.28)	(0.28)
sch 63	-0.54	-0.55	-0.12	-0.47	-0.27	-0.32	0.12	0.27	-0.65	-0.61	0.51	-0.23	-0.25	0.94	0.83	-0.23	1.88	1.25	1.36	1.02
	(0.23)	(0.22)	(0.22)	(0.42)	(0.20)	(0.26)	(0.22)	(0.24)	(0.19)	(0.20)	(0.20)	(0.27)	(0.32)	(0.26)	(0.45)	(0.49)	(0.51)	(0.55)	(0.24)	(0.14)
sch 64	0.11	0.41	1.22	-0.48	-0.12	0.07	-0.18	0.25	0.04	0.99	0.43	0.09	-0.03	-0.39	-15.60	-0.11	-1.57	0.49	-0.08	-1.49
	(0.16)	(0.39)	(0.51)	(0.33)	(0.16)	(0.23)	(2.32)	(0.26)	(0.12)	(0.11)	(0.20)	(0.20)	(0.27)	(0.38)	(0.00)	(0.37)	(30.21)	(1.90)	(0.38)	(0.36)
sch 65	0.36	0.67	0.78	0.37	0.04	-0.15	-0.42	0.91	-0.49	-0.18	-0.36	0.25	0.05	0.20	0.24	1.28	0.78	1.30	2.43	2.13
	(0.15)	(0.34)	(0.51)	(0.32)	(0.18)	(0.30)	(2.01)	(0.24)	(0.18)	(0.18)	(0.29)	(0.26)	(0.29)	(0.38)	(0.61)	(0.27)	(2.08)	(0.42)	(0.20)	(0.10)
sch 66	0.17	-0.33	0.54	0.49	0.15	0.66	1.01	1.49	0.12	-1.03	-0.24	1.45	0.38	-1.50	-0.19	1.92	2.93	3.12	2.18	2.22
	(0.13)	(0.34)	(0.25)	(0.30)	(0.17)	(0.51)	(0.21)	(0.17)	(0.14)	(0.29)	(0.17)	(0.24)	(0.26)	(4.61)	(0.34)	(0.29)	(0.61)	(3.20)	(0.15)	(0.09)
sch 67	-0.28	-0.40	-0.04	-0.67	-0.19	-0.47	0.04	-0.71	-0.56	-0.29	-0.37	-0.55	-0.30	-0.82	-0.74	-0.97	-0.16	-1.19	-0.85	-2.32
	(0.13)	(0.36)	(0.36)	(0.33)	(0.16)	(0.32)	(0.50)	(0.41)	(0.12)	(0.18)	(0.24)	(0.30)	(0.26)	(0.48)	(2.95)	(3.75)	(3.05)	(7.38)	(3.62)	(4.42)
sch 68	-0.39	-0.56	-0.61	-0.96	-0.65	-0.91	-0.95	-1.91	-1.04	-0.72	-0.41	-1.80	-0.28	-0.04	-0.06	-15.54	-21.65	-2.07	-3.15	-2.08
	(0.18)	(0.15)	(0.14)	(6.49)	(0.24)	(0.16)	(0.19)	(6.02)	(0.21)	(0.26)	(0.19)	(6.19)	(0.42)	(0.33)	(0.36)	(0.00)	(0.00)	(7.47)	(6.36)	(5.52)
Race	Black	Black	Black	Black	Hisp	Hisp	Hisp	Hisp	Hisp	Hisp	Hisp	Hisp	B/w	B/w	B/w	B/w	white	white	white	white
ELL	N	N	N	N	N	N	N	N	Y	Y	Y	Y	Y	Y	Y	Y	N	N	N	N
Income	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4	Q1	Q2	Q3	Q4
# student school pairs	18,873	10,631	9,794	3,152	11,832	9,128	11,436	7,063	16,913	21,308	11,062	5,455	4,757	4,980	3,417	2,660	1,392	3,041	12,098	25,562

Note: School mean utilities, for each school and group. Bootstrapped standard errors in parenthesis

Table A.7: School Mean Utilities and School Characteristics - Independent Regressions

	Standardized $\delta_{jc}/\lvert\beta_c\rvert$				
	Black-ELL=0	Hisp-ELL=0	White-ELL=0	Hisp-ELL=1	B/W-ELL=1
% Scored Advanced-Proficient Math	0.012	0.015	0.018	0.011	0.009
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	[940]	[940]	[940]	[940]	[940]
% Scored Advanced-Proficient English	0.015	0.017	0.022	0.013	0.008
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	[944]	[944]	[944]	[944]	[944]
% of Black Students	-0.016	-0.028	-0.023	-0.018	-0.030
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
	[1,032]	[1,032]	[1,032]	[1,032]	[1,032]
% of Hispanic Students	-0.003	0.007	0.000	0.002	0.018
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
	[1,032]	[1,032]	[1,032]	[1,032]	[1,032]
% of White Students	0.025	0.031	0.032	0.020	0.019
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
	[1,032]	[1,032]	[1,032]	[1,032]	[1,032]
% Low-Income Students in Kindergarten	-0.011	-0.013	-0.012	-0.005	-0.005
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[1,024]	[1,024]	[1,024]	[1,024]	[1,024]

Note: Each coefficient is from an independent regression where the dependent variable is the standardized ratio δ_{jr}/β_c. Standard errors in parenthesis and sample size shown below in square brackets.

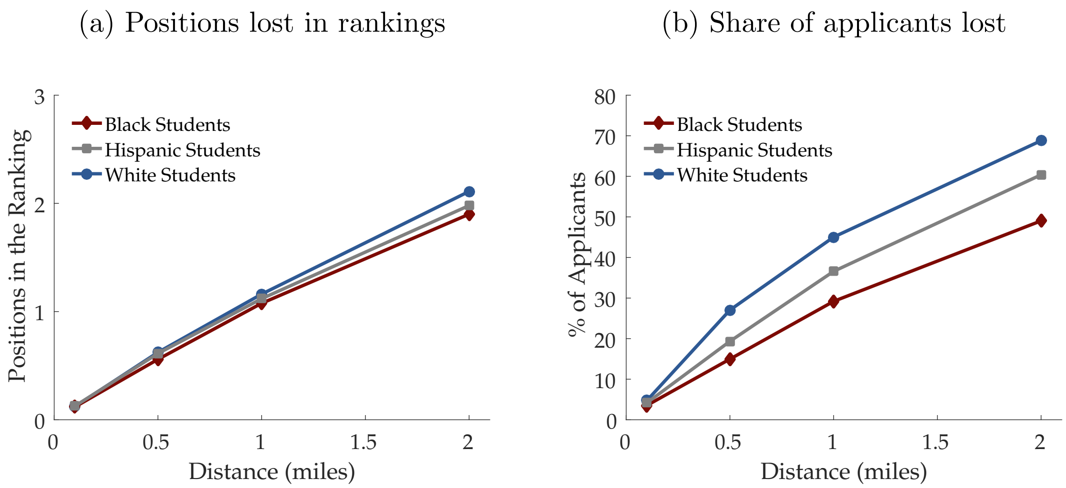
Table A.8: School Mean Utilities and School Characteristics - Pooled Regressions

	Standardized δ_{jc}/β_c				
	Black-ELL=0	Hisp-ELL=0	White-ELL=0	Hisp-ELL=1	B/W-ELL=1
% Scored advanced-proficient Math	0.0016	-0.0002	0.0041	0.0018	-0.0030
	(0.0019)	(0.0015)	(0.0015)	(0.0017)	(0.0015)
% Black students	0.0194	-0.0053	-0.0147	-0.0159	-0.0293
	(0.0058)	(0.0048)	(0.0047)	(0.0053)	(0.0045)
% white students	0.0146	0.0246	0.0598	0.0240	-0.0035
	(0.0070)	(0.0057)	(0.0057)	(0.0064)	(0.0055)
% Black students squared	-0.0004	-0.0002	0.0001	0.0001	-0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
% white students squared	0.0001	-0.0002	-0.0008	-0.0002	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
% Low-income in Kindergarten	-0.0013	-0.0031	-0.0008	0.0027	-0.0003
	(0.0018)	(0.0015)	(0.0015)	(0.0016)	(0.0014)
Observations	932	932	932	932	932
R^2	0.178	0.376	0.362	0.157	0.387

Standard errors in parentheses

Note: Coefficients from regressions between the standardized δ_j^r and school characteristics by race. Standard errors in parenthesis.

Figure A.5: Intensive and extensive margin effect of distance increase on applications



Note: The graphs show the intensive and extensive margin effects on rankings of an increase in the distance between schools and students. In panel (a) I plot the average number of positions that a school would lose if the distance between the school and the students increased by 0.1, 0.5, 1 and 2 miles. Here the average is taken across schools and students. In panel (b) I plot the average share of applicants lost if the distance between the school and the students increased by 0.1, 0.5, 1 and 2 miles, where the average is taken across schools. To model these changes I generate rankings using the parameters of the model where I increase the distance between each school and all students.

Figure A.6: Fit of Estimated Parameters -Black Students

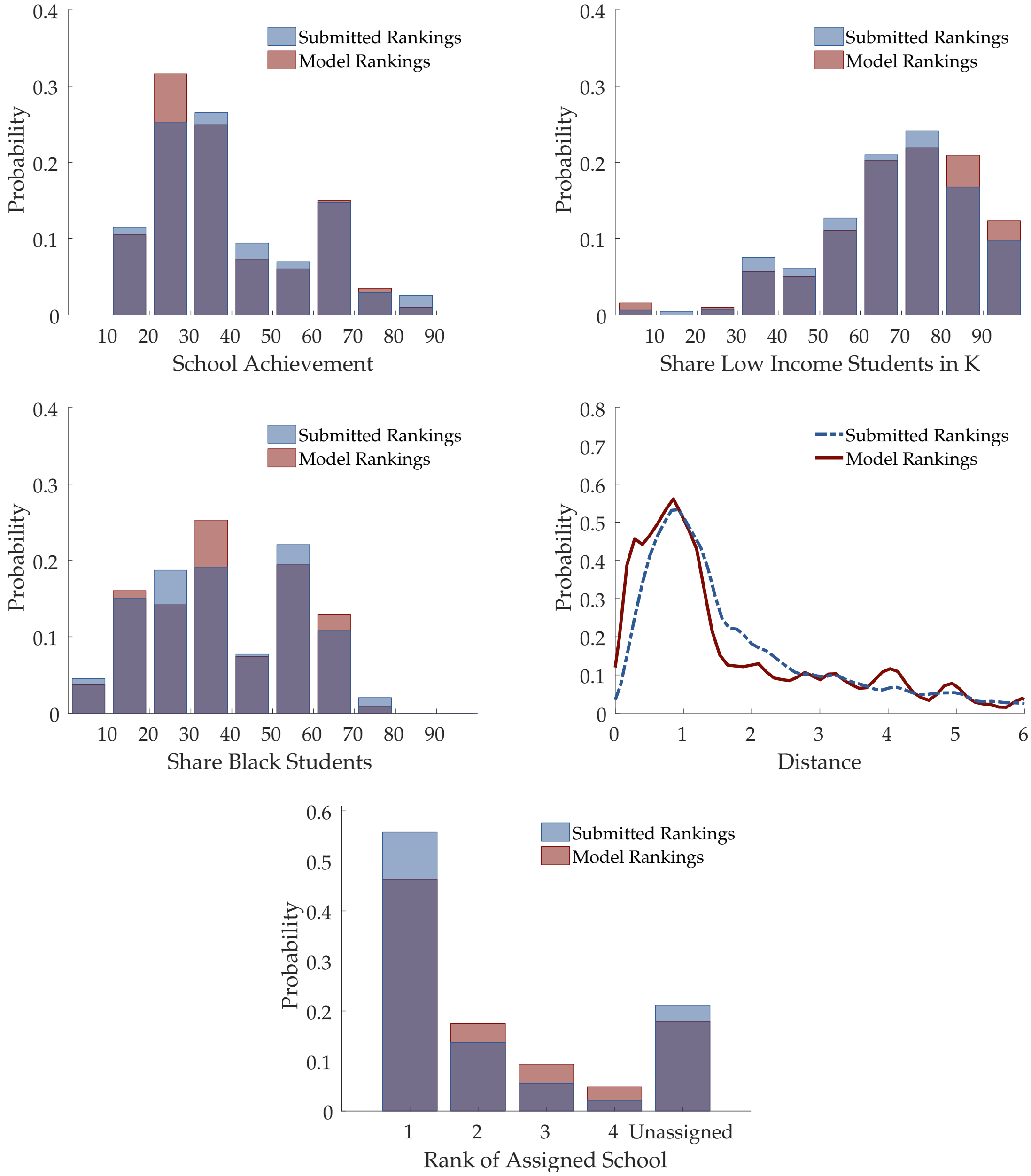


Figure A.7: Fit of Estimated Parameters - Hispanic Students

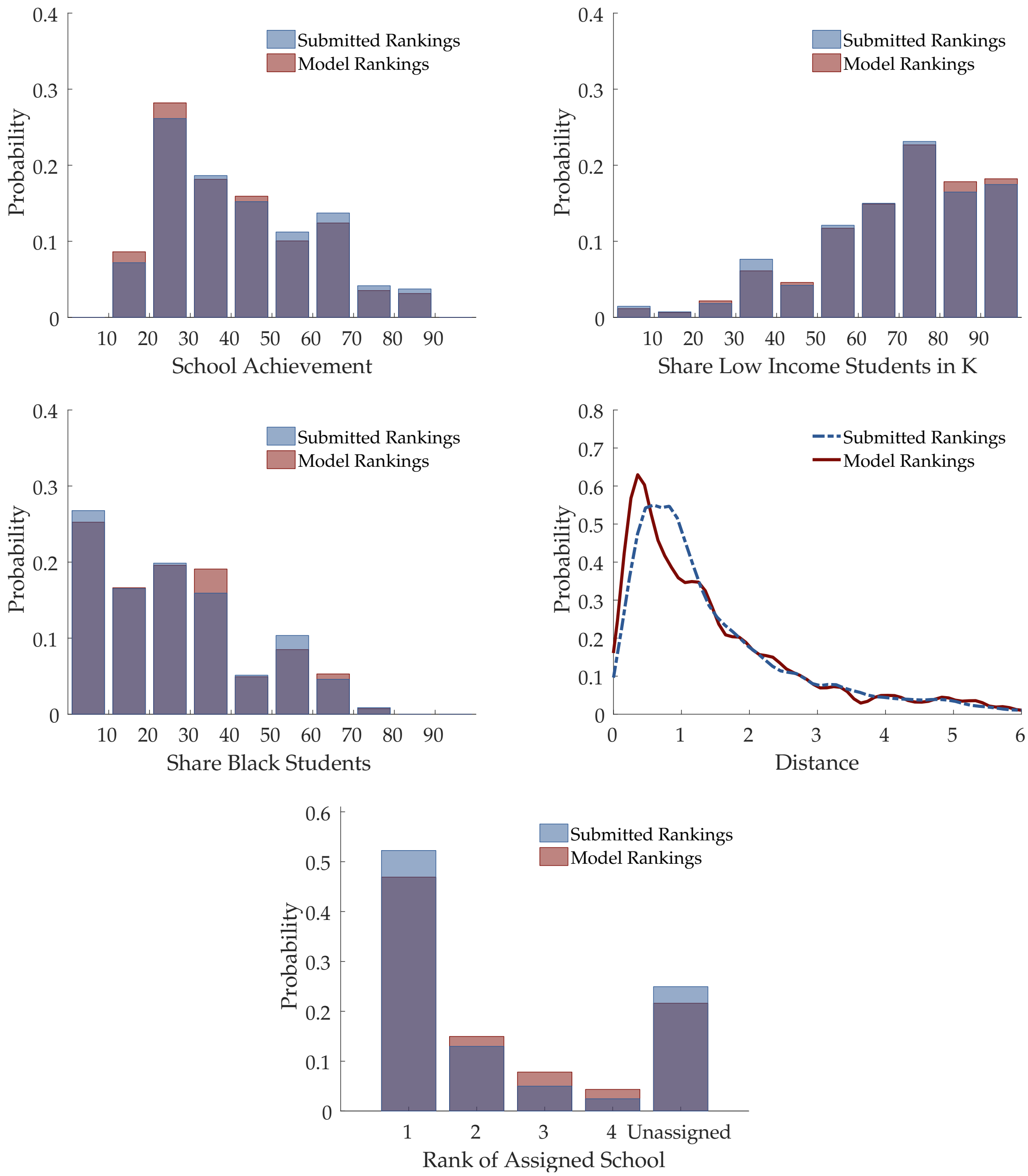
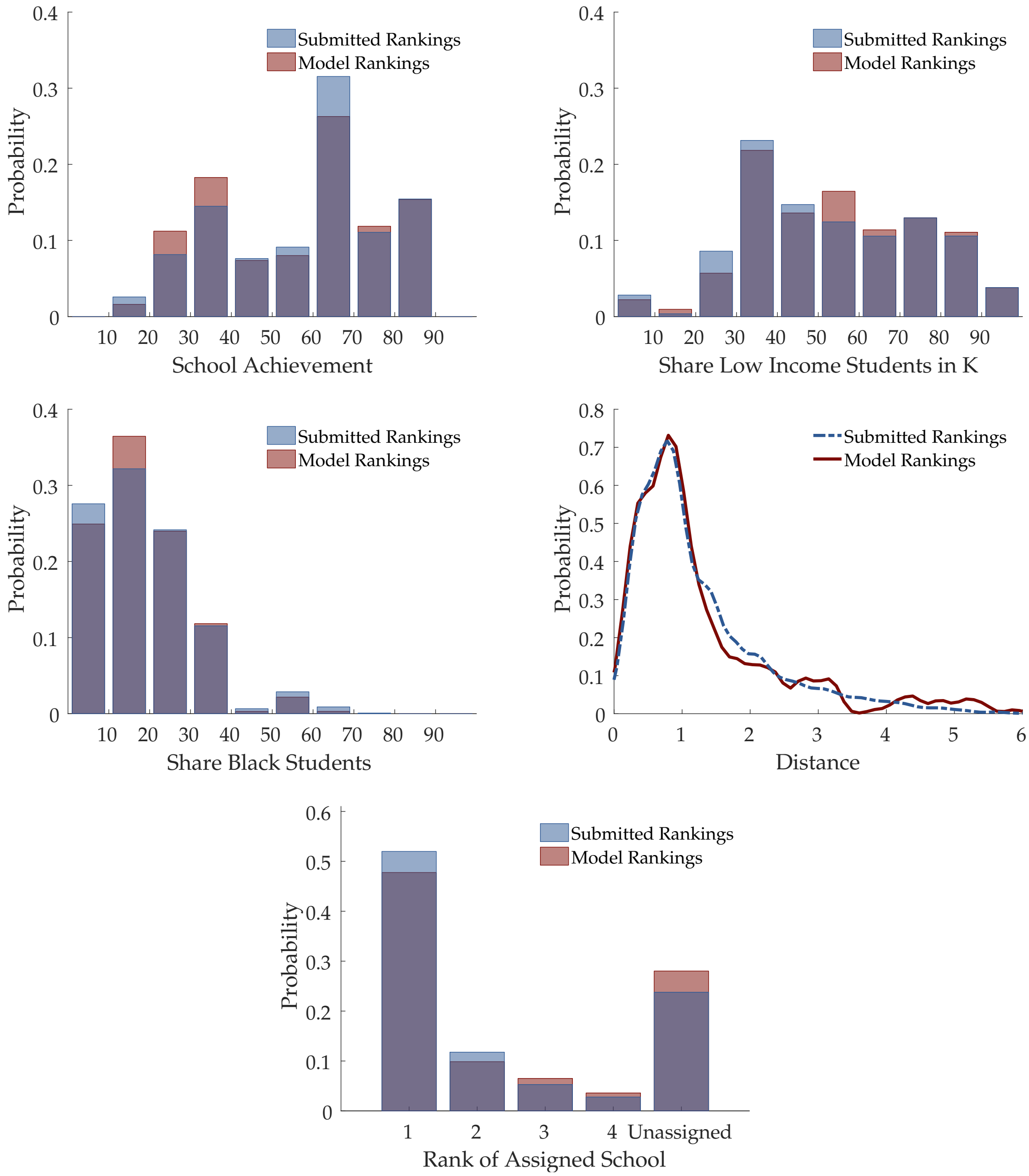


Figure A.8: Fit of Estimated Parameters - White Students

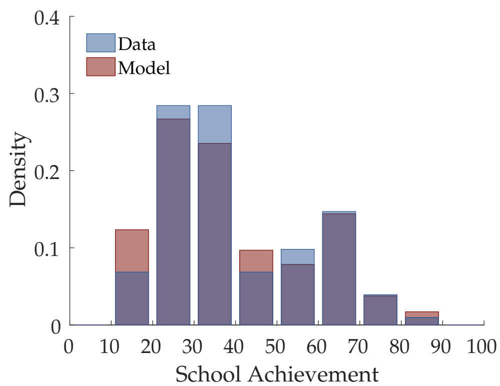


Note: Results for submitted rankings are obtained after running the DA using the rankings submitted by parents to BPS. Simulated rankings are obtained from rankings generated using demand parameters and realizations of ϵ, β_i^r and κ_i^r, and then running the DA algorithm.

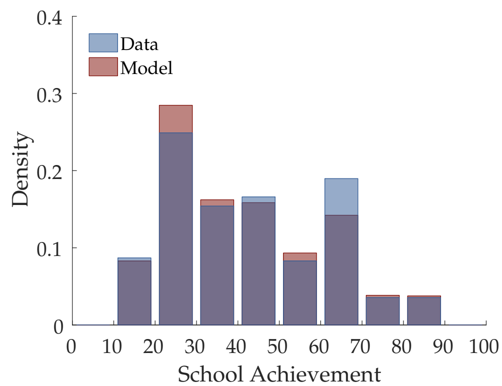
B Counterfactual Analysis

Figure B.9: Distribution of School Achievement of Siblings' School

(a) Black Students

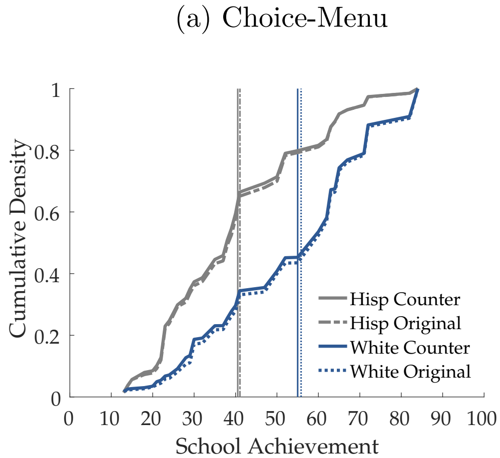


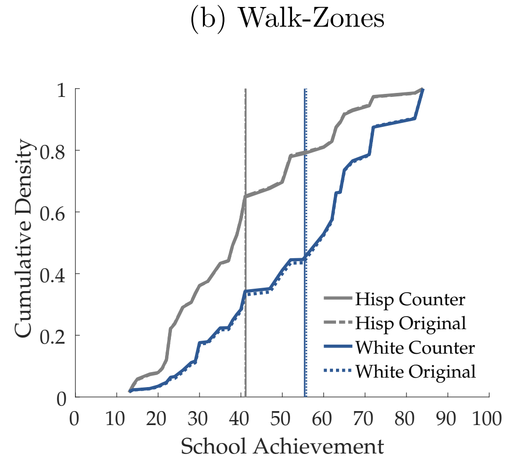
(b) Hispanic Students



Note: In blue, the histogram of school achievement at the schools where Black and Hispanic students have a sibling priority. In red, the histogram of school achievement for the predicted sibling's school for the same set of students in their original residential locations. The prediction is made running the DA algorithm with simulated rankings, assuming no student has a sibling priority. After running this assignment, I say that school j is the prediction of the sibling's school for students i, if i is assigned to school j. Intuitively, this would have been the assignment the older sibling would have gotten, assuming the family's preference parameters, information, and residence did not change.

Figure B.10: Eliminate location-specific rules - Hispanic Students





Note: Distribution of achievement in schools assigned to Hispanic and white students under a counterfactual assignment where choice-menu restrictions are eliminated (on the left), and walk-zone priorities are abolished (on the right).

C Maximum Likelihood Function

Let $R_i = (R_{i1}, \cdots, R_{il_i})$ be the rank-order list submitted and $\mathcal{J}_i$ the choice set of i. The conditional likelihood of R_i is

$$\mathcal{L}(R_i | \beta_c, \delta_{cj}) = \left[\prod_{k=1}^{l_i} \frac{\exp(u_{iR_{ik}})}{1 + \sum_{j \in \mathcal{J}_i \setminus \{R_{im}: m<k\}} \exp(u_{iR_{ij}})} \right] \left[\frac{1}{\sum_{j \in \mathcal{J}_i \setminus \{R_{im}: m<l_i\}} \exp(u_{iR_{ij}})} \right] \quad (6)$$

By maximum simulated likelihood, I find the values of $\{\beta_c, (\delta_{cj})_j\}_c$ that maximize

$$\prod_{i \in c(X_i)} \mathcal{L}(R_i | \beta_c, \delta_{cj})$$

for each c.

D Assignment Algorithm

With the exception of a couple of schools, half of the seats at each school are assigned using the priority order explained in the main text. This includes sibling and walk-zone priorities. For the second half of seats, the priority does not include any walk-zone considerations. In consequence, students with a sibling have the first priority and the rest have the second priority. Ties between groups are broken using a unique random number drawn for each student.

Now, since a student may be eligible for seats in both halves at each school, a precedence order across halves is established. This is, the rule that determines whether a student is first considered for the first or second half of the seats at a school. A student with a walk-zone priority will be considered for the walk-half first while a student outside the walk-zone is considered for the second half first. The DA algorithm, described below, is ran over school halves.

- *Step 1:* Applicants are sorted in priority order in their first ranked schools and students in excess of capacity are rejected. Those who are not rejected are provisionally admitted.
- *Step k:* For students rejected in step $k-1$, their next preferred option is considered. Each school ranks by priority order the set of provisionally admitted students jointly with those new students who are being considered in k. The program provisionally admits those with the highest priority and rejects students in excess of capacity. The algorithm stops when every rank list has been exhausted or when there are no rejections.

More details about the assignment algorithm can be found in Pathak and Shi 2013a and Pathak and Shi 2013b.