# Equilibrium Effects of Neighborhood Schools*

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November 12, 2025

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**Abstract**

Many public school districts allow families to enroll in schools outside their neighborhood. At the cost of higher transportation spending, choice programs aim to decouple educational opportunity from residential geography. This paper evaluates the impact of a return to neighborhood-based assignment following Seattle's re-introduction of neighborhood schools in 2010. We quantify the aggregate and distributional consequences of neighborhood assignment using an equilibrium model of joint residential and enrollment choices. Residential relocation responses limit the welfare costs of neighborhood assignment, reducing aggregate losses by roughly half. Lower housing costs fully offset welfare losses from restricted choice for low-income renters. Neighborhood assignment does not increase racial segregation or reduce the quality of schools attended by low-income families.

*This project would not have been possible without the assistance of staff from Seattle Public Schools, especially Hailey Karcher and Jay Freistadt. Han wishes to thank Parag Pathak, Josh Angrist and Dave Donaldson for their guidance and encouragement. We are also grateful to David Autor, Nikhil Agarwal, Arnaud Costinot, Bill Wheaton, Daniel Sturm, Chris Walters, Dean Li, Vincent Rollet, Laura Weiwu, Evan Soltas and participants in the MIT Labor Lunch. Eryn Heying, Jennifer Jackson, and Talia Gerstle provided invaluable administrative support. This paper reports on research conducted under data use agreements between MIT, the authors, and Seattle Public Schools; the views expressed are those of the authors alone. Han appreciatively acknowledges financial support from the George and Obie Shultz Fund and National Science Foundation Graduate Research Fellowship under Grant No. 2141064.

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## 1. Introduction

Concentrated disadvantage and residential segregation in American cities have prompted decades of effort to decouple educational opportunity from residential location. Court-mandated busing programs in the 1970s and 1980s attempted to overcome residential segregation by transporting students to schools in different neighborhoods. When busing programs ended in the 1990s, districts turned to voluntary school choice as a means to expand school access. The passage of the 2001 No Child Left Behind Act accelerated this shift, spurring widespread adoption of district-wide choice mechanisms.

Citing high transportation costs, many districts are now contemplating a return to neighborhood assignment.[1] Because choice systems promised to improve equity, proposals to abandon them raise distributional concerns. Yet the consequences of a return to neighborhood assignment are far from clear. Neighborhood assignment allows families to choose schools by sorting into neighborhoods, changing the terms of school choice rather than eliminating it. In equilibrium, housing costs rather than application priorities ration access to desirable schools. While this price rationing may improve allocative efficiency, it also makes school access contingent on ability to pay. Which form of choice is preferable—and for whom—turns on equilibrium effects in the interrelated markets for schools and housing.

This paper evaluates the aggregate and distributional consequences of a move to neighborhood schools using evidence from Seattle's sudden switch from district-wide choice to neighborhood assignment in 2010. Drawing on administrative student-level data from the district, our analysis shows the reform doubled the share of students attending their neighborhood school. We find household location choices respond strongly to neighborhood school characteristics, driving adjustments in housing prices across neighborhoods. To quantify the welfare implications of these enrollment, location, and price responses, we develop and estimate an equilibrium model of neighborhood and school choice. The estimated model indicates that most low-income households are better off under neighborhood assignment, a finding that challenges core arguments for public school choice.

Seattle's experience provides a unique opportunity to evaluate neighborhood assignment relative to contemporary forms of choice. While a number of other districts have also returned to neighborhood schools, most of these shifts accompanied the dismantling of court-ordered desegregation programs.[2] Research on these transitions documents that the end of race-based busing likely

[1] Major districts that have shifted toward guaranteeing assignments at local or neighborhood schools since 2000 include Minneapolis, Pinellas County (FL), Wake County (NC), and Jefferson County (KY). San Francisco has approved a return to neighborhood schools at the elementary school level, slated for adoption in the 2026-27 school year.

[2] As far as we know, Seattle's transition from district-wide choice to neighborhood schools is unique among major urban districts. Pinellas County, Florida ended its controlled choice plan in 2007 in favor of neighborhood assignment. San Francisco introduced attendance area priorities into its choice system in 2010 but provided overriding assignment priorities to students from disadvantaged census tracts.

deepened segregation and increased racial inequality (Lutz 2011, Reardon et al. 2012, Billings et al. 2014). The Seattle setting offers a distinct comparison between neighborhood assignment and voluntary choice, the prevailing solution for expanding school access.

The introduction of new attendance boundaries provides an opportunity to credibly identify housing market responses to the change in assignment policy. Our analysis shows that when application-based choice is restricted, households maintain access to desirable schools through residential mobility. This leads to price and sorting effects that are quantitatively important for welfare. After the reform, higher-income households sort into more desirable attendance areas where prices rise. Their exit from less desirable areas lowers housing costs for lower-income households—who are primarily renters—by enough to offset losses from reduced school choice. A model that holds residential choices and prices fixed incorrectly predicts net losses for low-income households, while overstating aggregate welfare losses.

We begin our analysis by presenting basic facts about how families use application-based choice when it is offered. Ranked-choice applications from the years preceding the reform show that only 31% of applicants list their neighborhood school as their top choice. Low-income applicants are least likely to rank their neighborhood school first. This points to substantial demand for school choice among applicants. Yet participation in the application process is incomplete and uneven. A quarter of households fail to submit applications, including over 40% of low-income families. Prevalent non-participation suggests that application barriers may prevent many families from fully realizing gains from district-wide choice.

Neighborhood school enrollment rises sharply after the reform, with the share of students attending their neighborhood school doubling from one-third to two-thirds. Our analysis shows that housing markets respond to the strengthened link between residence and schooling. Comparing the evolution of housing prices on opposite sides of attendance area borders, we show that neighborhood school characteristics capitalize strongly into transaction prices within six years of the reform. A one standard deviation increase in white enrollment raises prices by 3.2%, while an equivalent increase in math scores produces a premium of 1.8%. A complementary difference-in-differences specification shows increased price dispersion across neighborhoods, consistent with these boundary effects.

These price effects reflect residential sorting by families with school-age children toward neighborhoods with more desirable schools. Using student-level address data and a similar boundary discontinuity design, we find that a one standard deviation increase in the neighborhood school's white enrollment generates a 21% increase in the number of enrolled households in the attendance area. An equivalent difference in math scores leads to a 9% increase. The data also reveal heterogeneity in the school attributes valued by households. Higher-income households sort more strongly toward whiter schools, while low-income households respond more to test score performance.

To assess the welfare implications of these responses, we develop an equilibrium model of neighborhood and school choices. In our model, households choose where to live, trading off schooling opportunities against non-schooling amenities and housing costs. Once location choices are made, households with school-age children decide whether to submit a choice application, how to rank schools, and where to enroll. Housing prices and admissions rates clear the residential and schooling markets in equilibrium. To estimate the model, we use complementary information from both assignment regimes. Pre-reform ranked-choice applications identify preferences over schools, while post-reform boundary discontinuities in prices and sorting reveal preferences over neighborhoods and willingness to pay for schools.

Using the estimated model, we simulate the equilibrium allocation of pre-reform households under counterfactual neighborhood assignment. In aggregate, we estimate that returning to neighborhood schools leads to a decline in welfare for households with school-age children equivalent to \$16 per month. This loss closely matches our estimate of the district's per-pupil transportation cost savings of \$15 per month, suggesting that school choice roughly breaks even on net benefits. Allowing for location responses to non-marginal changes in school access is key to this conclusion—a version of the model holding locations fixed estimates much larger aggregate welfare losses of \$31 a month per student.

These aggregate effects mask substantial heterogeneity in impacts across income groups. Middle-income households lose most from the reform, and are willing to pay an average of \$29 per month to restore application-based assignment. High-income households also lose from restricted school choice on average, though losses are partially offset by guaranteed admission to schools in high-income areas that were previously oversubscribed. High-income homeowners in desirable attendance areas are further compensated by capital gains as their home equity rises. Low-income households, the majority of whom are renters, experience a moderate decline in school access. However, our estimates imply this loss is fully offset by declining housing costs in less desirable attendance areas. As a result, low-income households are indifferent to—or weakly prefer—neighborhood assignment.

The finding that low-income households are no better off under choice leaves open whether choice serves other equity goals, such as raising achievement for disadvantaged students or reducing segregation. We evaluate these alternative rationales by comparing enrolled school quality and segregation patterns across assignment regimes. Contrary to conventional wisdom, we find that neighborhood assignment neither exacerbates racial segregation nor shifts low-income students toward lower-quality schools. Both our model predictions and observed post-reform data show that low-income households attend schools with higher baseline math scores and modestly higher value-added under neighborhood assignment. These improvements are driven by students who don't apply under choice and are administratively assigned to schools worse than their neighborhood schools. We estimate slight increases in income-based segregation in both schools and neighborhoods, consistent with strengthened residential sorting by income. However, racial segregation in schools declines as neighborhood assignment shuts down application-based sorting. These findings collectively challenge the core arguments for school choice.

Our work contributes to a broad empirical literature documenting the effects of schools and school choice policies on student and housing market outcomes. A literature initiated by Black (1999) uses boundary designs to estimate the capitalization of school quality into housing prices (Boustan 2012, Reback 2005, Schwartz et al. 2014, Zheng 2022, Fack and Grenet 2010, Kane et al. 2006, Figlio and Lucas 2004). A concern in this literature is that boundaries are non-randomly placed. Our design compares homes sold near boundary lines before and after the reform that made these boundaries binding for school assignment, isolating the capitalization of schools from other unobserved features of houses and neighborhoods. A related strand of literature shows that households actively move in response to choice policies (Bibler and Billings 2020, Billings et al. 2018, Baum-Snow and Lutz 2011, Bergman et al. 2020), while other work examines the effects of school choice on student achievement (Abdulkadiroğlu et al. 2020, Angrist et al. 2022, Cullen et al. 2006, Deming et al. 2014, Hoxby 2000) and segregation in schools (Idoux 2022, Hastings et al. 2005). These outcomes have typically been studied independently, with evidence spread across different contexts. We observe price changes, residential sorting, and enrollment patterns in a single setting, and map these effects to household welfare using a structural model of preferences.

Our methodological approach builds on a literature estimating equilibrium models of neighborhood sorting, particularly Bayer et al. (2007), which embeds a boundary discontinuity design in a model of housing choices.[3] Our work also relates closely to two recent papers that take structural approaches to studying the consequences of public school choice. Park and Hahm (2023) develop and estimate a model of school choice with endogenous residential locations using data from New York City. They show that location responses dampen integration policies, but abstract from equilibrium adjustment of prices. Agostinelli et al. (2024) estimate a spatial equilibrium model of residential sorting and school choice using enrollment records from Wake County, NC. Relative to these papers, our data provide an unusually rich basis for estimating household preferences. Ranked-choice applications under choice reveal detailed information about preferences for schools, while price effects and residential sorting across attendance boundaries under neighborhood assignment identify money-metric scalings across the income distribution.[4]

Finally, this paper complements theoretical work on the welfare trade-offs between school choice and neighborhood schools in general equilibrium. A point of disagreement in this literature

[3]Following early empirical work by Epple and Sieg (1999), related equilibrium sorting models have been fruitfully used to evaluate education policies such as private school vouchers (Ferreyra 2007) and school finance reforms (Epple and Ferreyra 2008).

[4]Measuring the desirability of schools (and other public goods) is a long-standing issue in empirical models of equilibrium sorting. Prior studies have relied on a low-dimensional set of observed characteristics to capture school desirability, such as average test scores or racial composition. We use self-reports in the form of ranked-choice lists to better measure preferences, a solution in the spirit of Bergstrom et al. (1982).

is whether low-income households are better or worse off under neighborhood assignment. Models which hold fixed the price of housing in the district's least desirable neighborhoods typically conclude that choice is welfare improving for low-income households (Epple and Romano 2003, Grigoryan 2021). The pattern of welfare effects we document aligns instead with models in which public good provision (here, school choice) is costly for low-income households either due to taxation or equilibrium adjustment of housing costs. Epple and Romano (1996) show that middle-types benefit the most from the provision of a public good in a model in which low-types have low willingness to pay and high-types have access to private provision. Avery and Pathak (2021) illustrate this "ends-against-the-middle" conflict in a model of public school and residential choice. We provide the first empirical evidence supporting this distributional prediction.

The next section provides background on school choice in Seattle before and after the reform. Section 3 describes the data we use and descriptive features of our kindergarten sample. Section 4 documents how the reform impacted application rates and patterns of enrollment. Section 5 provides evidence on the housing market responses of households and prices to the reform. We proceed to develop our model of neighborhood sorting and school choice in Section 6. Section 7 outlines our estimation procedure. We quantify the welfare impacts of the district's return to neighborhood assignment in Section 8. Finally, Section 9 summarizes our findings and concludes.

## 2. Student assignment in Seattle

### 2.1. District-wide choice: 1999–2009

Seattle began offering district-wide choice in 1999, following the phase-out of the city's mandatory busing plan. The plan erased existing attendance areas in favor of a centralized assignment system that remained in operation until the 2009-10 school year. Families could rank up to 12 schools, and the district used the Deferred Acceptance (DA) algorithm to issue each applicant a single offer.[5] Common tie-breakers determined priority at oversubscribed schools: students with siblings enrolled at the school received first priority, students living in the school's geographic reference area received second priority, and remaining ties were broken by distance. The district provided yellow bus transportation to any school within a student's geographic cluster, with the district partitioned into eight clusters.

Families received application reminder letters in early fall. Households that did not submit applications in the main choice round enrolled through three alternative pathways. First, a small number of students with specialized needs received designated assignments through an independent special education office. Second, an early sibling application round allowed students to apply in advance to schools where an older sibling was enrolled, with nearly all such applications granted during our sample period. Third, students who did not submit any application and did not qualify for

[5]Seattle's adoption of DA in 1999 predates the first academic papers on the use of DA in the student assignment problem. The district referred to the algorithm as the Barnhart-Waldman amendment after the two school board members who proposed it based on their familiarity with the workings of the medical residency match.

special placement were administratively assigned by the district's central enrollment office. Based on conversations with district officials and examination of our data, administrators attempted to place students at undersubscribed schools near students' home residences when capacity allowed, though students had no guarantee of assignment to any particular school.

### 2.2. Return to neighborhood schools: 2010–present

In June 2009, district leaders approved a major reform titled the "New Student Assignment Plan." The reform emphasized a return to neighborhood schools and curtailed busing service, aiming to simplify enrollment and reduce transportation costs.[6] In October 2009, the district released new attendance area maps at each grade level. At the elementary school level, 58 schools became neighborhood schools with defined attendance areas, while the remaining 9 schools were designated "option" schools without attendance areas.

The new system guaranteed every student a seat at their assigned neighborhood school, and took effect starting in the 2010-11 school year. Families planning to attend their neighborhood school did not need to submit applications and could simply enroll directly. However, a centralized choice process remained available for families seeking re-assignment to other neighborhood schools or the nine option schools. These families could submit a rank-order list indicating their preferences, with assignments determined through a centralized match.[7] The district continued using DA to conduct this match during the first year of the new assignment plan but switched to the Boston mechanism starting in the 2011-12 school year. Yellow bus transportation was restricted to schools within a family's middle school service area, a considerably smaller geographic zone than the previous cluster-based system.[8] A "grandfathering" scheme minimized educational disruption by allowing students already enrolled at non-neighborhood schools to continue until the highest grade offered. As a result, we focus most of our analysis on students at the kindergarten level, the elementary entry grade.

## 3. Data and samples

Our data on students come from administrative files provided by Seattle Public Schools covering the 2002-03 through 2022-23 school years. Enrollment files identify students enrolled at each school, while application files record families' ranked school lists submitted through the centralized assignment mechanism.[9] The application files also provide detailed match information, including each

[6] The adoption of the plan was also spurred by the Supreme Court ruling in *Parents Involved in Community Schools v. Seattle School District No. 1*, which invalidated Seattle's use of a racial tie-breaker at oversubscribed high schools and prompted a broader re-evaluation of the district's choice system among district leaders.

[7] Both neighborhood and option schools offer sibling priorities to choice applicants. Option schools additionally give a "geographic zone" priority to students living sufficiently close to the school.

[8] In 2010, each middle school service area contained between 5 and 12 elementary schools, compared to the eight large clusters under the previous system.

[9] Application files are available for each annual match with the exception of 2010. The archived database containing the records from this year could not be located after an extensive search by multiple district personnel. This omission

student's priority level at every school, lottery tiebreakers for breaking ties, and final assignment outcomes. Both enrollment and application files contain demographic information (including race, gender, special education status) and exact residential addresses. We define distance to school as the minimum road distance (in miles) between residential and school addresses, computed using the TravelTime API. The district also provided state standardized test scores from 2004–2022.

To distinguish renters from homeowners, we rely on Seattle's Rental Registration and Inspection Ordinance (RRIO), a citywide registry of long-term rental properties initiated in 2014. We identify households as renters or homeowners based on the RRIO registration of their parcel of residence.[10] We further identify students living in project-based public housing by matching addresses to the Seattle Housing Authority's published housing inventory. Data on housing transactions are derived from King County Assessor records of all residential property sales from 1999–2023. These files include sale prices and physical characteristics of transacted homes. Following Bayer et al. (2007) we convert transaction prices of homes into monthly user costs of housing by estimating rent-price ratios in each of Seattle's six public-use microdata areas using data from the American Community Survey. This facilitates reporting of welfare impacts in terms of dollars per month. Since we do not have direct data on rents across neighborhoods, we use monthly user costs derived from changes in the prices of single-family homes as our primary measure of housing costs, as in Diamond et al. (2018).

We supplement housing transactions with tax assessment rolls which provide assessed values for the full housing stock regardless of whether properties transacted. We further incorporate residential address histories from Infutor, a consumer marketing firm. These data track moves for essentially all U.S. adults during our sample period, allowing us to link current student addresses to pre-enrollment locations. Finally, to construct counts of households with elementary school-age children enrolled outside the district, as well as households without enrollment-age children, we utilize block-group-level estimates of household counts from the American Community Survey. Appendix D provides additional detail on each of these data sources and our construction of key variables.

### 3.1. Income categories

Our student files do not contain direct information on household income.[11] To construct a measure of income, we rely on student-level residential address data matched to property assessment records from the county assessor's office. We use the 2004 assessed value of the matched parcel to divide households into three income groups, which we refer to throughout our analysis as "Low-

has minimal impact on our analysis, which relies almost entirely on application data from the pre-period.

[10] The share of students we designate as renters using this approach is close to corresponding estimates from the ACS. We identify 39% of enrolled kindergarten students as renters, while 2005-2009 ACS five-year estimates imply a rental rate of 37% for Seattle households enrolled in public elementary school.

[11] A common proxy for income is free or reduced price lunch status. While this indicator is not available in our setting, distributional analysis that extends beyond the binary contrast provided by lunch status still requires additional information.

", "Middle-" and "High-income." Since renters' incomes may differ systematically from homeowners at the same property value, we assign students to categories using tenure-specific income distributions from the 2005-2009 ACS. Specifically, we use the ACS data to compute the share of renters and homeowners in each income tercile among Seattle households with public elementary school children. We then assign students in our data to income categories based on property values, applying thresholds for renters and homeowners that match these ACS shares.

Finally, our estimation of neighborhood preferences relies on the residential locations of households in the period after the policy reform. Since the reform may have changed where households choose to live, we rely only on pre-reform address data to construct income categories for the post-reform sample. We link residential addresses for post-reform students to address histories from Infutor. The Infutor data allow us to determine the location of each household in 2008. We are able to match 86% of households in our primary post-reform sample consisting of kindergarten enrollees from 2013–2015 to pre-reform property values in this way. Appendix D provides additional detail on the construction of income categories and the procedure used to match students to Infutor residential histories.

### 3.2. Sample characteristics

Table 1 tabulates mean characteristics of kindergarten enrollees and applicants between 2006 and 2008, the pre-period of our study. Column 1 reports on the sample of enrolled kindergarteners used in our main analysis. We exclude a small number of students requiring specialized education support, who are placed through a special education office outside of the main match.[12]

The demographic composition of students in Seattle differs from most large urban districts in the U.S. The district has a relatively low share of Black and Hispanic students (29% combined) and an unusually high share of Asian students (17%), with white students comprising 45% of enrollment. Average household income is near $100,000, somewhat higher than typical for an urban district of its size. The three income groupings we construct differ sharply in composition and tenure. Low-income households are disproportionately Black (30%) and Hispanic (21%), while high-income households are predominantly white (71%). Eighty percent of low-income households rent, while nearly all high-income households own their homes. A subset of low-income renters, representing eighteen percent of low-income enrollees, reside in project-based public housing, where rents are largely insulated from market fluctuations. These differences in tenure are consequential for the welfare effects of changes in housing prices.

Column 5 shows that applicants differ systematically from enrolled students. Applicants are whiter (54% vs. 45%), wealthier ($111,000 vs. $98,000 in household income), and more likely to own homes (68% vs. 61%). These differences reflect that higher income enrollees are more likely to submit applications, as well as the fact that a substantial number of higher income applicants

[12] These students represent just 2.5% of district enrollment.

apply but do not ultimately enroll in the district. We discuss these application and enrollment patterns in more detail in the next section.

## 4. Descriptive application and enrollment patterns

### 4.1. Who exercises choice?

Over two-thirds of students attend non-neighborhood schools under district-wide choice. This is documented in Panel A of Table 2 using data on enrollments of kindergarten students between 2006 and 2008. Low-income households are least likely to enroll at their neighborhood school (26%), compared to 35% of middle-income households and 48% of high-income households. This pattern is reflected in distance to school: low-income students travel an average of 2.36 miles to school, compared to less than a mile for high-income students.

Panel A also shows how students enroll in the district. The majority of students (59%) apply through the district's main choice round, while a smaller share apply through an early round for families seeking co-placement with older siblings.[13] Over a quarter (27%) of enrolled students do not submit applications in either round. Non-applicants—along with a small share of applicants who fail to rank sufficient choices—are assigned administratively to schools with available capacity and within reasonable travel distance. Non-application rates vary dramatically by income, with 44% of low-income students failing to apply, compared to 26% of middle-income students and just 10% of high-income students.

Panel B examines preferences among applicants, revealing strong demand for out-of-neighborhood schools despite the participation gaps documented above. Most applicants eventually enroll in the district (82%), though high-income households are more likely to exit to private schools or other districts (24% vs. 11% of low-income applicants). Revealed preferences suggest substantial demand for non-neighborhood schools, with only 31% of applicants ranking their neighborhood school first under the incentive-compatible DA algorithm. This figure drops to 25% among low-income applicants. More than half (55%) of low-income applicants do not rank their neighborhood school anywhere in their list, compared to only a quarter (27%) of high-income applicants.

These patterns indicate that families—particularly low-income families—place significant value on accessing schools beyond their immediate neighborhood. Yet many families do not apply. While low-income households exhibit the strongest revealed preference for non-neighborhood schools when they do apply, they are also the least likely to submit applications. The high rate of non-application is likely a consequence of informational frictions and other difficulties associated with navigating the logistics of the choice process. A growing literature documents that such frictions disproportionately affect minority and lower-income households (Ladd 2002, Hastings

[13] A result of the early sibling round in Seattle is that the main application round features relatively fewer applicants with older siblings, whose preferences may be distorted by the convenience of being placed at the same school. Only 15% of main round applicants in this sample period are assigned a sibling priority at some school.

and Weinstein 2008, Kapor et al. 2020). As a result, while low-income families value school choice, barriers to application may prevent these families from fully realizing the potential gains.

### 4.2. Immediate consequences of the reform

Seattle's return to neighborhood assignment approximately doubled the share of kindergarten students attending their neighborhood school. Panel (a) of Figure 1 shows this shift. Prior to the reform, roughly a third of students enrolled at their assigned neighborhood school. In 2010, the first year of the reform, 70% of students enrolled at their neighborhood school, and this share rose to 75% by 2015. Panel (b) documents the corresponding reduction in travel distances, which fell from an average of 1.9 miles before the reform to 1.3 miles in 2010. By 2015, average distance to school declined to just 1.1 miles.

While the reform guaranteed neighborhood assignments, families retained the option to apply for seats at other schools with available capacity. Panel (c) shows that relatively few families exercised this option, with application rates falling by half or more across all income groups. Low-income submission rates fall from 48% to just 25% by 2010, while middle- and high-income rates drop from over 60% to 31%.[14] This decline reflects both reduced seat availability for out-of-neighborhood applicants and guaranteed neighborhood placement, which eliminated the need for families satisfied with their zoned school to apply.

### 4.3. Changes in segregation and enrolled school quality

Neighborhood assignment raises concerns about segregation and school quality for disadvantaged students. Table 3 examines the consequences of the reform for these outcomes using three approaches. Column 1 reports observed 2008 enrollments under choice, while columns 4-6 report observed enrollments in 2010, 2012, and 2014—the initial years under neighborhood assignment. To isolate the mechanical effects of the policy from time-varying confounds, columns 2-3 simulate counterfactual allocations of the 2008 cohort.[15] Column 2 assigns students directly to schools based on 2010 attendance boundaries. Column 3 uses a Deferred Acceptance algorithm that assigns students to their closest available school while respecting school capacity constraints, following Angrist et al. (2022).[16] These simulations isolate the allocation effect of neighborhood assignment absent residential responses.

To quantify the unevenness of minority students across schools, we rely on two indices of segregation. The variance ratio index measures district-wide sorting of minority (Black or

[14] In the 2009 application cycle, the district pushed back the choice deadline by over a month to give families additional time to make choices after a number of schools were slated to close, leading to slightly elevated application rates.

[15] This is the same sample of enrolled students we use for estimation and counterfactual simulation of the structural model.

[16] Specifically, we construct rank order lists for students that list schools in ascending order of distance, and priorities for schools that rank students the same way. To account for randomness arising from lottery tie-breaking, we simulate the match 100 times with new tie-breaker draws in each iteration.

Hispanic) students, defined as

$$\text{VR} = \frac{\text{Var}(s_j)}{s(1-s)}$$

where $s_j$ is the share minority of school $j$, $s$ is the district-wide average, and the variance is weighted by school enrollment (Massey and Denton 1988). This index measures dispersion in minority shares across schools, expressed as a fraction of the maximum variance.[17] To measure concentration in the most segregated schools, we report minority isolation, defined as the share of minority students attending schools that are more than 70% minority. Segregation measures for low-income students are constructed analogously.

Both neighborhood assignment schemes and observed post-reform enrollments suggest that neighborhood assignment reduces racial segregation. The minority variance ratio drops from 0.26 to 0.18 (attendance areas) or 0.14 (DA), and minority isolation falls from 19% to near zero. Post-reform measures are qualitatively consistent with this pattern. Minority segregation declines, though less dramatically than predicted. Meanwhile, segregation of low-income students appears largely unaffected.

Notably, low-income students enroll in higher performing schools under both neighborhood assignment schemes. Average math scores improve from -0.28$\sigma$ to -0.26$\sigma$ (attendance areas) or -0.20$\sigma$ (DA), with modest value-added gains.[18] Post-reform data corroborate these improvements. Figure A.1 shows that this gain in attended school quality is driven by the large share of low-income students who do not apply. Non-applicants are administratively assigned to schools that are substantially lower performing and have lower value-added than their neighborhood schools. By guaranteeing neighborhood assignment, the reform eliminates these adverse placements.

For middle- and high-income students, however, predicted and observed post-reform school characteristics diverge. Simulations forecast small declines in test score levels with negligible value-added changes, suggesting reassignment to schools with different peer composition but similar effectiveness. Instead, the post-reform data show that high-income students attend schools with unchanged average scores post-reform, while middle-income students attend slightly higher-performing schools. This divergence provides an initial indication that middle- and high-income households relocated strategically to preserve school access, offsetting the mechanical effects of partial equilibrium reassignments.

[17] The variance ratio can equivalently be expressed as the difference in average school minority shares faced by minority and non-minority students: $\text{VR} = \text{E}\big[s_{n(i)}|m_i = 1\big] - \text{E}\big[s_{n(i)}|m_i = 0\big]$, where $m_i$ indicates minority status.

[18] We estimate value-added for each elementary school using the risk-controlled value-added model introduced in (Angrist et al. 2020). This involves including controls for school assignment risk (computed by simulating the centralized match) in a regression of 4th grade standardized math scores on school-specific enrollment indicators and controls for student demographics. Figure D.1 validates these value-added estimates using offer instruments leveraging quasi-experimental variation in assignments.

## 5. Residential responses to neighborhood schools

### 5.1. Housing price effects

To evaluate the reform's effects on housing prices across neighborhoods, we employ a boundary discontinuity design (BDD) that isolates the capitalization effects of schools from the influence of other unobserved neighborhood amenities. Each boundary in our analysis separates a pair of attendance areas corresponding to distinct schools. We estimate how school characteristics capitalize into house prices by comparing the discontinuities in prices at attendance boundaries to associated changes in neighborhood school attributes.

Compared to Black (1999) and subsequent studies, we leverage the fact that attendance boundaries did not impact school assignment prior to the reform, allowing us to account for pre-existing differences in the housing stock and neighborhood amenities near boundaries.[19] The process by which boundaries were drawn was not random, raising the possibility that houses and neighborhoods on opposite sides may differ in unobserved ways. Conditioning on baseline housing prices mitigates this concern by absorbing persistent spatial variation, as well as increasing the statistical precision of our capitalization estimates.

Our main specification relates the price of house $h$ sold in period $t$, denoted $p_{ht}$, to $Q_h$, a characteristic of the neighborhood school whose attendance area includes $h$. To pool data across boundaries, we assign positive distance to the side of the boundary zoned to the higher performing school and stack observations across the boundaries in our sample. To allow capitalization effects to evolve over time, we divide the sample into a pre-reform period (2006–2008) and two post-reform periods (2010–2012 and 2013–2015). We exclude 2009 as a transition year, during which the new attendance areas were announced but the assignment policy had not yet been implemented. This yields the following estimating equation:

$$p_{ht} = \beta_{2006} Q_h + \sum_{\tau \in \{2010, 2013\}} \beta_\tau (1[t = \tau] \times Q_h) + \theta_{b(h)t} + f_{b(h)t}(D_h) + \mathbf{W}'_h \phi_t + \varepsilon_{ht}, \qquad (1)$$

where $Q_h$ denotes a characteristic (such as test scores or demographic composition) of the neighborhood school that house $h$ is assigned to by the 2010 boundaries, and $\theta_{b(h)t}$ denote boundary-by-period fixed effects which equal one if $h$ is within a quarter mile of boundary $b$ in period $t$ and zero otherwise. With the inclusion of these fixed effects, identifying variation in $Q_h$ arises only from crossing boundaries.[20] To control for differences in slopes across boundaries, the equation

[19]Figure A.3 shows minimal systematic differences in the physical characteristics of homes sold near boundaries in the pre-period. We do, however, find some evidence of pre-existing demographic imbalances: using 2005–2009 ACS estimates of block-group-level demographics, Figure A.4 shows that the higher-performing and whiter sides of boundaries tend to be slightly whiter but less college educated.

[20]Equation (1) can be equivalently cast as an instrumental variables regression with $Q_h$ and its period interactions as endogenous variables and boundary side indicators (and analogous period interactions) as instruments. The single-equation formulation is identical because $Q_h$ is perfectly predicted by side indicators within each boundary. This also implies that $\beta_{2006}$ can be interpreted as a weighted average of boundary-specific discontinuities, and the post-period

includes piecewise-linear controls for signed distance from the nearest boundary ($D_h$) through the function $f_{b(h)t}(D_h) = \upsilon^{-}_{bt} D_h + \upsilon^{+}_{bt} D_h \cdot 1[D_h > 0]$. We additionally control for $\mathbf{W}_h$, a vector of house characteristics.[21] The coefficients $\beta_\tau$ for $\tau \in \{2010, 2013\}$ measure the capitalization of $Q_h$ in the post-periods relative to the 2006 baseline, and are the parameters of interest.

To estimate this equation, we use transaction data on single-family homes from the King County Assessor's Office. As is standard practice in hedonic analysis, we focus on single-family homes because they constitute the majority of the housing stock in residential neighborhoods with families and because condominium and apartment transactions often reflect investor rather than household demand (Bishop et al. 2020). We focus on boundaries with more than 10 sales within 0.1 miles on either side during the pre-period (2006-2008), which excludes boundaries coinciding with natural barriers and major highways where housing transactions are sparse. We restrict our primary analysis to elementary school boundaries as attendance areas matter the most for newly enrolled students.[22]

Figure 2 illustrates the variation in school assignments underlying our main specification Equation 1 by plotting mean math scores of neighborhood and enrolled schools in distance bins near boundaries for years both before (panel (a)) and after the reform (panel (b)). In both periods, the black points show that attendance boundaries are associated with an average discontinuity in neighborhood school math test scores of around $0.4\sigma$. However, panel (a) shows that, prior to the introduction of attendance boundaries, students on both sides of the district's eventual boundaries attend schools with comparable test score performance.

In contrast, panel (b) shows that in the post-reform years the discontinuity in neighborhood school performance translates into a corresponding discontinuity in enrolled school performance: students residing on the side zoned to the higher-performing school attend schools with scores an average of $0.25\sigma$ higher than students living on the opposite side of the boundary. The appearance of a discontinuity in enrolled school characteristics confirms that the reform substantially changed the types of schools attended by students residing across boundaries that had previously carried no assignment significance.

Figure 3 provides visual evidence that the discontinuity in school access which emerges translates into housing prices. The figure plots average sale prices in distance bins on either side of attendance boundaries residualized by house characteristics.[23] Panel (a) shows that housing prices are continuous across boundaries in the pre-reform period, but exhibit a discontinuity of around \$10,000 post-reform in panel (b). The post-reform price jump at attendance boundaries indicates

coefficients as the difference of this weighted average in each period relative to $\beta_{2006}$ (Angrist and Imbens 1995).

[21] The characteristics in $\mathbf{W}_h$ include the age of the home, a quadratic control for square footage, the assessed value of the property in 2004, the number of stories, bedrooms, and bathrooms, distance to waterfront, and distance to the nearest major road.

[22] Our estimates reveal strong capitalization of elementary school characteristics into housing prices, with effects indistinguishable from zero for middle and high schools.

[23] These characteristics are identical to those included in $\mathbf{W}_h$ in Equation (1).

that housing prices adjusted in response to the change in expected school assignments associated with the establishment of boundaries.

OLS estimates of the capitalization coefficients in Equation (1) are consistent with this graphical evidence, as reported in Table 4. In the pre-period (2006–2008), prices do not vary across the eventual locations of attendance area boundaries in a way correlated with any of the four school attributes we examine. The discontinuities associated with each characteristic are small and statistically indistinguishable from zero. In the post-period, however, significant capitalization effects emerge. For sales between 2013 and 2015, we estimate that a one standard deviation increase in the share white of the neighborhood school leads to a price increase of \$16,300. This represents a 3.2% increase in price, an effect comparable in magnitude to that found in Black (1999). The same increase in the share of students that are low-income depresses home prices by \$18,400, while a one standard deviation increase in average math scores increases prices by \$9,800. Price effects associated with causal value-added are positive but small and indistinguishable from zero. This may reflect the fact that value-added estimates are not publicly advertised, making it difficult for households to incorporate this information into their housing decisions.[24]

Panel B adds controls for Census block group demographics, including racial composition, educational attainment, and median household income. As argued by Bayer et al. (2007), neighborhood demographics can vary substantially even within narrow bandwidths since households endogenously sort across established boundaries. In their application, controlling for demographics reduces capitalization estimates by roughly half. In contrast, the inclusion of demographic controls leaves our estimates largely unchanged. For example, the estimated effect of share white decreases only modestly to \$14,600, while the estimated capitalization of math scores is essentially unaffected. This stability likely reflects the fact that we examine newly instituted boundaries, minimizing the potential for entrenched demographic sorting across attendance areas to confound the price effects we observe. In the next section, however, we show that sorting does occur among the small subset of households with enrollment-age children, for whom boundaries are most consequential.

The price discontinuities that emerge along boundary lines result in an overall increase in the dispersion of housing prices across the district. We document this citywide effect of the assignment reform by comparing changes in home prices between more- and less-desirable attendance areas over time using the following event study specification:

$$\ln p_{ht} = \sum_{\tau \neq 2009} \delta_\tau \cdot (1[t = \tau] \times T_h) + \omega_t + \mathbf{W}_h' \phi_t + \epsilon_{ht}, \tag{2}$$

[24] Kuminoff and Pope (2014) emphasize that capitalization effects based on comparisons across different points in time are difficult to interpret unless the hedonic price function is time invariant, which will not generally be the case after large shocks. This concern is mitigated in our context since the price effects we estimate in the pre-period are small. We clarify the mapping between price effects and preferences in the discussion of the structural model in Section 6.

where $T_h$ indicates whether house $h$ is zoned to an attendance area that ranks above or below the district median on a measure of desirability at baseline–specifically the share of white students, mean math scores, and mean residential income. The coefficients $\delta_\tau$ trace the evolution of the price premium associated with the group indicated by $T_h$ over time.

Plotting estimates of $\delta_\tau$ from Equation (2), Figure 4 shows that relative prices evolve across attendance areas in ways consistent with our boundary discontinuity estimates. Panel (a) shows that attendance areas with above-median share white schools experience relative price increases of 3-5 percent. Panel (b) reveals somewhat smaller increases of 1-2 percent in attendance areas of schools with above-median math scores. Finally, panel (c) shows that areas with lower baseline residential income become relatively more affordable, exhibiting a relative price decline of 3-5 percent. These effects emerge gradually after 2009, flattening by 2015.[25]

While we do not have comparable microdata on market rents, Figure A.5 performs a similar analysis using block-group level rent estimates from the 2000 Decennial Census and American Community Survey. Although fewer data points are available and estimates are substantially noisier, changes in rents after 2010 broadly mirror the housing price results, suggesting that changes in attendance area desirability capitalize into both sale prices and rental rates following the introduction of neighborhood schools.

### 5.2. Residential sorting effects

Because neighborhood assignment changes the set of schools that are accessible in different areas of the district, households may make different residential choices in response to the change in assignment policy. To isolate the effect of school access from other features of neighborhoods that may influence residential choices, we build on the same boundary discontinuity design as in Section 5.1. As motivation for this approach, Figure 5 provides visual evidence that households with children enrolling in the district sort toward more desirable attendance areas following the introduction of the assignment reform. Panel (a) shows that, in the pre-period, the density of enrolled students is nearly continuous across future attendance boundaries, whereas substantially more households locate on the side zoned to higher-performing schools between 2013 and 2015, shown in panel (b).

To quantify the magnitude of residential sorting, we estimate a specification analogous to Equation 1 that relates the log count of enrolled students residing on each side of attendance area boundaries to baseline school characteristics. Letting $s$ index boundary-side pairs, our estimating equation is:

$$\ln N_{gst} = \lambda_{2006} Q_s + \sum_{\tau \in \{2010, 2013\}} \lambda_\tau \cdot (1[t = \tau] \times Q_s) + \theta_{b(s)t} + \mathbf{W}'_s \phi_t + \nu_{st} \tag{3}$$

[25] The district introduced a substantial set of boundary revisions ahead of the 2015-16 school year, complicating analysis of longer-run effects.

where $\ln N_{gst}$ is the log number of enrolled students in group $g$ residing on boundary-side $s$ in period $t$. As before, $Q_s$ denotes the baseline characteristic of the neighborhood school assigned to side $s$, and $\theta_{b(s)t}$ are boundary-by-period fixed effects that control for time-varying conditions at each boundary. The vector $\mathbf{W}_s$ includes controls for average characteristics of the housing stock derived from assessor property records and neighborhood demographics from ACS block-group estimates. The semi-elasticities $\lambda_\tau$ are identified from the extent to which sorting toward higher-quality attendance areas intensifies post-reform.[26]

Panel A of Table 5 examines sorting by neighborhood school math scores. In the pre-period, differences across boundaries are small, though there is some evidence that low-income households are more likely to reside on the lower-performing side. Sorting emerges clearly in the post-period. Between 2013 and 2015, a one standard deviation increase in baseline math scores is associated with a 9% increase in enrolled students overall. Notably, this effect is largest for low-income households, who exhibit a 13% increase, while effects are statistically insignificant for other households.

Middle- and high-income households also engage in residential sorting, but place greater weight on peer race, as shown in Panel B. In aggregate, a one standard deviation increase in share white is associated with a 21% increase in enrolled students at the boundary by 2013–2015. Middle- and high-income households drive most of the sorting on this dimension, with semi-elasticities of 0.23 and 0.27 respectively. Low-income households show positive, but more modest responses. Across all groups, these effects are smaller and often insignificant between 2010 and 2012, pointing to gradual rather than instantaneous residential adjustment.[27]

Since we observe only enrolled students rather than all school-age households, we caution that the estimates in Table 5 combine residential sorting with potential differences in enrollment rates across boundaries. If families are more likely to enroll when residing in desirable attendance areas, this would lead to an overestimate of the degree of sorting. Two factors weigh against this interpretation. First, the effects emerge gradually over several years, consistent with residential relocation rather than immediate enrollment shifts. Second, private school enrollment is just 21% at the elementary level and concentrated among higher-income households. Differential enrollment propensities may modestly affect estimated magnitudes for these groups, but are therefore unlikely to explain the broad sorting patterns we observe.[28] We account for selection into enrollment in

[26] A limitation of estimating Equation (3) via OLS is that it requires dropping boundaries with zero counts. We further exclude boundaries with fewer than 5 enrolled students on any side to avoid logging small counts. The structural model of neighborhood choice we develop in Section 6 addresses this limitation by modeling household location decisions directly.

[27] Table A.4 presents analogous results which split households by race and tenure. Consistent with the correlation of tenure and income, renters move toward higher scoring schools. White households sort most strongly toward same-race peers. Table A.3 demonstrates robustness of these estimates to the inclusion of controls for neighborhood demographics, dropping of elementary school boundaries coincident with middle school boundaries, and narrower bandwidths.

[28] Sorting across boundaries is also unlikely to be explained by differential changes in housing supply. Using data on new residential building permits, Table A.2 shows that counts of newly permitted residential units are not systematically correlated with neighborhood school characteristics in our sample period.

estimation of our structural model, described in Section 7.

## 6. A model of school and neighborhood choice

We consider a public school district located in a region inhabited by a measure 1 of households. We differentiate between households with and without children of imminent elementary school age. We assume each household of the former kind has a single child, and we refer to these agents as enrollment-age households or students interchangeably. Enrollment-age households make residential, application, and enrollment choices in four stages. Other households make only residential choices. We begin by describing the decisions made by enrollment-age households, reserving discussion of other households for last.

In the first stage, enrollment-age households choose a neighborhood $n \in \{0, 1, \ldots, N\}$ to live in, where neighborhoods 1 through $N$ are located in the district and neighborhood 0 is an outside alternative corresponding to eight surrounding school districts.[29] In the second stage, students decide whether and how to apply to public schools indexed by $j \in \{1, \ldots, J\}$. Students who choose to apply incur a disutility $c_i$, capturing frictions faced by parents in the process of researching and ranking schools.[30] Applicants submit rank order lists that maximize expected utility. In the third stage, students are assigned a single best offer by the district's centralized assignment mechanism. Students who do not receive an offer to one of their ranked schools, including non-applicants, are administratively placed at a school indexed by $j = J + 1$. Finally, in the fourth stage, students decide whether to enroll in their assigned district school, or to leave for an outside school indexed by $j = 0$. Figure 6 summarizes the timing of decisions for enrollment-age households.

We accommodate differences in preferences by income by placing households into income categories indexed by $g$. The indirect utility of enrollment-age household $i$ belonging to group $g$ when living in neighborhood $n$ is

$$v_{in} = \gamma_g S_{in} - \alpha_g p_n + \mathbf{a}_n' \varphi_g + \xi_n + \eta_{in}$$

where $\mathbf{a}_n$ is a vector of observed neighborhood characteristics, $\xi_n$ represents an unobserved vertical dimension of neighborhood quality, and $S_{in}$ is the expected value from schooling detailed below. The cost of housing appears as a monthly user cost, $p_n$, which enters negatively to capture the disutility of housing costs for renters and new home-buyers.[31] We adjust for capital gains and losses

[29] These districts are the Bellevue, Mercer Island, Shoreline, Highline, Renton, Tukwila, Northshore, and Lake Washington School Districts. The combined population of these districts is slightly larger than the population of Seattle (770k compared to 609k according to the 2010 Census).

[30] Kapor et al. (2020) rationalize non-application by introducing a cost of receiving a placement from the match. Introducing placement costs can rationalize short application lists, where an upfront application cost does not. However, associating this cost with the application decision is more natural when even non-applicants may receive assignments in the form of administrative placements.

[31] We convert sale prices to monthly user costs by calculating monthly rental equivalents for each property using data from the ACS. Appendix D provides additional detail on this procedure.

experienced by existing homeowners in our welfare accounting in Section 8.[32] The parameter $\gamma_g$ captures the sensitivity of group $g$ to school access, while $\alpha_g$ allows for differential sensitivity to prices. We set the deterministic utility of the outside neighborhood to zero, which provides the location normalization for the neighborhood utilities.

The utility associated with enrolling in school $j \in \{1, \ldots, J\}$ is

$$u_{ijn} = \delta_{gj} + \sum_{\ell,k} \beta^{\ell,k} x_i^{\ell} w_j^k + \sum_k \lambda_i^k w_j^k + \mathbf{d}'_{jn} \tau_g + \varepsilon_{ij}$$

This formulation allows for both observed and unobserved heterogeneity in parental preferences for schools. First, we allow group preferences to differ flexibly across schools through the inclusion of group-by-school mean utilities $\delta_{gj}$. The first summation term allows preferences for observable characteristics of schools, $w_j^k$ to depend on observed student characteristics $x_i^{\ell}$.[33] This term allows for heterogeneity in preferences that are not based on income, for instance preferences for same race peers. The second summation term incorporates unobserved preference heterogeneity in the form of random coefficients on observed school characteristics. Finally, we allow utilities to depend on the difficulty of commuting through the vector $\mathbf{d}_{jn}$. We include in $\mathbf{d}_{jn}$ road distance, measured in miles, as well as the interaction between road distance and whether yellow bus transportation is offered to $j$ from $n$.

We allow two other schooling options in addition to the $J$ regular public schools. First, non-applicants and applicants who do not receive an offer to one of their ranked schools are administratively placed. We model administrative placement as assignment to a school whose appeal is equivalent to a weighted average of the utilities of the schools a student could be assigned to. We assign the index $J + 1$ to the administrative school, which offers utility:

$$u_{i,J+1,n} = \sum_{j=1}^{J} \rho_{jn}(u_{ijn} - \varepsilon_{ij}) + \varepsilon_{i,J+1},$$

where $\rho_{jn}$ are neighborhood specific placement probabilities. We do not explicitly model how administrative placements are determined, instead setting these rates equal to observed pre-period placement probabilities in our empirical application.[34] We treat administrative assignment as a distinct school with its own additive idiosyncratic shock $\varepsilon_{i,J+1}$ to rationalize the submission of lists ranking every observed administrative assignment destination above one or more other schools.

[32] We additionally treat households living in public housing as insulated from changes in $p_n$.

[33] We measure observed characteristics using pre-reform data, treating these attributes as fixed proxies for observed and unobserved dimensions of school appeal. We discuss the rationale for abstracting from endogenous changes in school attributes in more detail below.

[34] Placement probabilities are estimated using data from the kindergarten match between 2006 and 2008. Since the number of students in each of the neighborhoods we define later is relatively small, we form these placement probabilities at the attendance area level.

Second, households may elect to enroll outside the district, through private schools, home-schooling, or non-resident enrollment in a neighboring public school district. We treat this bundle of non-district options as a single outside alternative, indexed by $0$. While we treat utilities for public schools as known at the time of application, we allow for uncertainty about the appeal of non-district enrollment. In particular, we specify utility from non-district enrollment as

$$u_{i0} = \varepsilon_{i0} + \zeta_i,$$

where $\varepsilon_{i0}$ is a preference known at stage 2 and $\zeta_i$ is a shock learned at the start of stage 4. The inclusion of the additional shock $\zeta_i$ represents uncertainty about the attractiveness of non-district enrollment that is only realized after households decide whether to apply to public schools. Behaviorally, it rationalizes why students may receive an assignment to one of their ranked schools, but decide not to enroll. The deterministic component of utility associated with non-district enrollment is normalized to zero, which provides the location normalization for the school utilities.

Households without enrollment-age children do not engage in the district's choice process and make a single decision about where to live. Indirect utility for these households is:

$$v_{in} = \gamma S_n - \alpha_g p_n + \mathbf{a}_n' \varphi_g + \xi_n + \eta_{in}.$$

The primary difference relative to the earlier specification is the schooling term. We allow $S_n$ to enter into the utility of households without enrollment-age children to accommodate the potential for indirect benefits of school access to neighborhoods (Hilber and Mayer 2004). However, since these households do not make application and enrollment choices, we model this value in a more stylized way. In particular, we assume non-enrollment households have a common valuation of the schooling amenity in each neighborhood equal to the average school access for enrollment-age households, denoted by $S_n$.

### 6.1. Household choices

Here we detail the set of choices enrollment-age households make in each stage, proceeding in reverse order.

**Stage 4. Enrollment decision** Let $z_i \in \{1, \ldots, J, J+1\}$ denote $i$'s assigned public school. With an offer in hand, students decide whether to enroll at $z_i$, or at the non-district option $0$. Students take up their offer and enroll in the district when

$$u_{iz_in} \geq u_{i0} = \varepsilon_{i0} + \zeta_i. \tag{4}$$

We denote the expected utility of this stage by

$$\bar{u}_{iz_in}(\varepsilon_i) \equiv \mathrm{E}[\max\{u_{iz_in}, \varepsilon_{i0} + \zeta_i\}|\varepsilon_i]. \tag{5}$$

where $\varepsilon_i = (\varepsilon_{i0}, \varepsilon_{i1}, \ldots, \varepsilon_{iJ}, \varepsilon_{i,J+1})$, and the expectation is taken over $\zeta_i$.

**Stage 3. Formation of ranked-order lists** Offers are determined by a Deferred Acceptance match. To streamline our discussion, we rely on the fact that DA assignments can be represented in terms of market clearing cutoffs (Azevedo and Leshno 2016, Agarwal and Somaini 2018). Concretely, we define applicant $i$'s rank at school $j$, $rank_{ij}$, to be the sum of an integer priority and a fractional tie-breaker. The school $j$ cutoff $\kappa_j$ is equal to the rank of the last applicant receiving an assignment there, $\min_{i:z_i=j}\{rank_{ij}\}$. In line with previous work, we assume the market is sufficiently large that individual applicants treat cutoffs as fixed. We let $\mathcal{J}_i$ denote this set of feasible schools for each applicant. We additionally specify that this set always contains the administrative assignment school $J+1$.

Because some schools in Seattle use lottery tie-breaking, applicants do not have perfect knowledge of $\mathcal{J}_i$. Applicants are also unlikely to know the exact realization of preferences and priorities of other applicants in the match, which introduces a second source of uncertainty. We assume applicants have rational expectations over their realization of $\mathcal{J}_i$ conditional on knowledge of their own priorities, and the distribution of preferences and priorities in the population. We detail how we estimate these expectations in Section 7.

Students who have chosen to submit an application form a rank order list $\mathbf{r}_i = (r_{i1}, \ldots, r_{iK_i})'$. Our key assumption is that students rank schools optimally according to their preferences. This implies that students rank schools in order of true preference, and that applicants rank a school if and only if it is preferable to administrative assignment. Under DA, this implies each applicant is offered the school they most prefer among the feasible schools in $\mathcal{J}_i$.

**Stage 2. Decision to apply** Students decide whether to submit an application with assignment probabilities in mind. Application is associated with a cost $c_i$. Students choose to apply when,

$$\mathrm{E}\left[\max_{j\in\mathcal{J}_i(\kappa)} \bar{u}_{ijn}(\varepsilon_i)|\varepsilon_i\right] - c_i > \bar{u}_{i,J+1}(\varepsilon_i), \tag{6}$$

where the expectation is taken over $\boldsymbol{\kappa}$, the vector of equilibrium school cutoffs.

**Stage 1. Neighborhood choice** In the initial stage, students choose a neighborhood to live in. Let $n_i$ denote the neighborhood chosen by $i$. The chosen neighborhood satisfies

$$v_{in_i} > v_{im} \, \forall m \neq n_i, \tag{7}$$

where the inclusive value of application and enrollment stages in each neighborhood is given by,

$$S_{in} = \mathrm{E}\left[\max\left\{\max_{j\in\mathcal{J}_i(\kappa)} \bar{u}_{ijn}(\varepsilon_i) - c_i, \bar{u}_{i,J+1}(\varepsilon_i)\right\}\right], \tag{8}$$

with the expectation taken over $\boldsymbol{\varepsilon}_i, \lambda_i, c_i, \boldsymbol{\kappa}$, and $\zeta_i$.

## 6.2. Equilibrium

We take the capacity of each school $Q_j$ and housing supply in each neighborhood $H_n$ to be exogenous throughout our analysis.[35] An equilibrium is defined as follows:

**Definition 6.1** *An equilibrium in the model consists of cutoffs* $\boldsymbol{\kappa} = (\kappa_1, \ldots, \kappa_J)$*, prices* $\boldsymbol{p} = (p_1, \ldots, p_N)$*, and individual application, enrollment, and neighborhood choices such that:*

- *Cutoffs for each school* $j \in [1, \ldots, J]$ *respect capacity constraints:*

$$\sum_i A_i \cdot 1\left\{j = \arg\max_{k\in\mathcal{J}_i(\boldsymbol{\kappa})} u_{ikn_i}\right\} \leq Q_j,$$

  *where* $A_i$ *is an indicator taking value 1 when* $i$ *chooses to apply.*
- *Prices in each neighborhood* $n \in [1, \ldots, N]$ *clear the housing market:*

$$\sum_i 1\left\{n = \arg\max_m v_{im}(\boldsymbol{p})\right\} \leq H_n.$$

- *Application choices, enrollment decisions, and neighborhood choices, are individually optimal, satisfying Equations* (4)*,* (6)*, and* (7)*.*

## 6.3. Parameterization

For both enrollment-age and non-enrollment households, we classify households into three income groups, so that $g \in \{\text{low, middle, high}\}$. We place parametric restrictions on the errors in the model which facilitate estimation. In the neighborhood choice stage, we assume idiosyncratic neighborhood preferences follow

$$\eta_{in} \sim \text{Gumbel}(0, 1).$$

[35] Seattle's zoning regime concentrates post-2000 development in scattered "urban villages," designated growth centers that constrain where housing supply can respond to localized demand shifts. Consistent with this, we find no evidence that newly permitted units locate in desirable attendance areas (Table A.2). Baum-Snow and Han (2024) estimate tract-level supply elasticities for Seattle in the bottom-quartile values among urban tracts, ranging between 0.09 and 0.13.

This provides the scale normalization for the neighborhood utilities. In the school choice stages, we assume

$$\varepsilon_{ij} \sim \mathcal{N}(0,1) \quad \lambda_i \sim \mathcal{N}(0,\Sigma) \quad \zeta_{in} \sim \mathcal{N}(0,\sigma^2_{\zeta,g}) \quad c_i \sim \mathcal{N}(\bar{c}_g,\sigma^2_{c,g}) \quad \delta_{gj} \sim \mathcal{N}(\bar{\delta}_g,\sigma^2_{\delta,g}).$$

Unit variance of the school specific shocks $\varepsilon_{ij}$ provides the scale normalization. This set of parameterizations facilitates estimation of this stage of the model using Markov Chain Monte Carlo methods. Taking advantage of our data on rank choice lists, we leave the variance of random coefficients $\Sigma$ unrestricted, allowing for arbitrary correlation between random coefficients on school characteristics.

### 6.4. Discussion

The model above abstracts from changes in the desirability of neighborhoods and schools due to changes in demographic composition. We make this choice because demographic characteristics are likely correlated with unobserved attributes—making it difficult to distinguish whether families value peer composition per se or unobserved features composition may proxy for. Importantly, enrollment-age households comprise only 11% of households in the region, so residential sorting due to changes in student assignment policies minimally impact the overall neighborhood composition. Moreover, we find neither systematic changes in student segregation by income or race across schools, nor shifts in composition at high-performing schools, as discussed in Section 4. This evidence suggests treating these characteristics as endogenous would not substantially alter the conclusions of the analysis.

Following much of the literature, we assume that the idiosyncratic preferences for schools $(\varepsilon, \lambda)$ are independent of distance conditional on observable characteristics of students. A notable exception is Park and Hahm (2023), who relax this assumption in a model with three unobserved discrete types. If households take unobserved components of preferences for schools into account when choosing where to live, our analysis may underestimate the extent to which households are able to express preferences for schools through their residential choices. The advantage of treating unobserved preferences for schools as independent of location choices is that it enables separate estimation of the neighborhood and school choice stages. This allows us to incorporate richer unobserved heterogeneity in preferences for schools, including random coefficients on school characteristics. Accurately accounting for this preference heterogeneity is key to gauging the welfare impacts of restrictions in school choice sets.[36] This separability also facilitates estimation of school and neighborhood preferences using data and identifying variation from different assignment regimes. The next section discusses our estimation approach in more detail.

[36]Previous iterations of the model estimated without random coefficients and with Gumbel-distributed $\varepsilon_{ij}$ suggested implausibly large reductions in household welfare associated with restricting school choice. This issue is related to the "new goods" problem noted in the industrial organization literature.

## 7. Estimation

Our estimation proceeds in two stages. First, we estimate the parameters associated with the school choice stages of the model using a Bayesian Markov-Chain Monte Carlo (MCMC) procedure. Using these estimates, we form the inclusive value of schooling $S_{in}$ across neighborhoods for each household $i$. In the second stage, we estimate the neighborhood choice parameters $\gamma_g$ and $\alpha_g$ by GMM, matching on observed differences in the density of enrolled households across attendance boundaries. This two-stage estimation procedure allows us to use samples from different assignment regimes to learn about the preferences of households in each stage. Specifically, we use data from years of district-wide choice to learn about parental preferences for schools, while we use data on household sorting across attendance boundaries after the introduction of neighborhood schools to infer how families trade-off school access and housing costs.

### 7.1. Constructing neighborhoods

We start by partitioning the district into a set of neighborhoods. We take as our starting point 2010 Census block groups. We intersect block group shapes with the district's 2008 reference areas, and then with the district's 2010 attendance areas. This generates a set of neighborhoods experiencing relatively well-defined changes in school access. We consolidate a small number of resulting areas of less than .025 square miles with the smallest adjoining neighborhood in the same elementary school attendance area. This procedure defines a total of 865 neighborhoods partitioning the district, providing sufficient geographic resolution to capture spatial distinctions in pre-period priorities and post-period attendance areas, without introducing unnecessary finite sample bias from specification of an overly granular model (Dingel and Tintelnot 2025). Figure A.8 maps the areas we construct against the district's attendance areas.

### 7.2. Admissions probabilities

As discussed above, we assume that applicants rationally forecast their chances of admission subject to two sources of uncertainty. First, lottery tie-breaking at options schools introduces a stochastic element into the match, affecting cutoffs at all schools. Second, we assume applicants do not have perfect knowledge of the set of other applicants in the match, knowing only the joint distribution of preferences and priorities in the population. We estimate these beliefs by bootstrapping the 2008 match. In each iteration, we draw a bootstrap sample of applicants and new lottery tie-breakers. We compute admissions cutoffs to be the lowest ranked student assigned to a given school. We set household beliefs about the set of schools where they are eligible to be the realizations of $\mathcal{J}_i$ generated by this bootstrap procedure.

### 7.3. Estimating school preferences

Our estimation procedure for the school choice part of the model builds on applications of Markov Chain Monte Carlo (MCMC) techniques to demand estimation, including in school choice settings

(Abdulkadiroğlu et al. 2017, Kapor et al. 2020, Idoux 2022). This approach is particularly useful here because the likelihood of the application and enrollment stages in our model is expensive to compute directly. However, the parametric assumptions we impose make it relatively easy to sample from the conditional distributions of parameters and utilities.

A key feature of our model is the possibility of non-application. While we directly observe non-applicants that eventually enroll in the district, our data do not include students that neither apply nor enroll.[37] Ignoring this selection into our data leads to severe underestimation of the relative attractiveness of leaving the district. To account for this group of students, we supplement our district sample with counts of students not enrolled in public school from the ACS. Using the 2005-2009 5-year estimates we obtain the number of elementary school-age children not enrolled in public school in each of Seattle's six Public-use Microdata Areas (PUMAs), by race, gender, and income.[38] Our estimation sample comprises kindergarten applicants and enrollees for the school year starting in fall 2008 and non-district enrollees matching the counts described above in each neighborhood.

To implement the MCMC approach, we use data augmentation to draw latent utilities, application costs, and random coefficients for each household that are consistent with the choices we observe in the data. In each iteration, we start by drawing school utilities $u_{ij}$ for each student. The assumption that students submit utility maximizing applications imposes a set of $J$ linear inequalities on the utilities of each schooling alternative for applicants. Given a rank-order list $\mathbf{r}_i = (r_{i1}, \ldots, r_{iK_i})$, these inequalities are:

$$\begin{aligned}
u_{ir_{ik}} &> u_{ir_{i,k+1}} \quad \text{for all } k < K_i && \text{(Ranked schools)} \\
u_{ir_{iK_i}} &> u_{i,J+1} && \text{(Administrative placement)} \\
u_{i,J+1} &> u_{ij} \quad j \in \{1, \ldots, J\} \setminus \{r_{i1}, \ldots, r_{iK_i}\} && \text{(Unranked schools)}
\end{aligned}$$

where the dependence on $n$ is suppressed. The first set of conditions derives from our assumption that students rank schools truthfully, in line with the incentive properties of the Deferred Acceptance algorithm. The remaining conditions are a consequence of the assumption that applicants rank their most preferred options, stopping when they prefer administrative assignment ($u_{i,J+1}$) to the remaining alternatives.

The decisions to apply and enroll provide two additional sets of inequalities. With an offer

[37] Past work on public school choice has largely restricted attention to public school enrollees. Idoux (2022) studies exit from NYC high schools using data on applicants. However, enrollment rates among applicants may differ from those among non-applicants.

[38] PUMAs are the finest geographic identifiers in the ACS public microdata, but are considerably coarser than the neighborhoods we construct. We assign ACS households to neighborhoods within a PUMA using the distribution of households across our neighborhoods implied by block-level counts from the 2010 Census.

to school $j$ in hand, the utility of enrollees satisfy

$$u_{ij} > \varepsilon_{i0} + \zeta_i \qquad \text{(Enrollment)}$$

with the reverse holding for students that decline to enroll. Finally, the application decision imposes that, for applicants,

$$\mathrm{E}\left[\max_{j \in \mathcal{J}_i} \bar{u}_{ij}\right] - c_i > \bar{u}_{i,J+1} \qquad \text{(Application)}$$

where $\bar{u}_{ij}$ is the inclusive value of the enrollment stage given in (5). The reverse condition is true for non-applicants.

The computationally intensive step in the procedure is sampling from the posterior distribution of school utilities subject to these constraints. The typical approach uses a Gibbs sampler to iterate over the set of schools $j$ for each student, drawing each $u_{ij}$ from a normal distribution truncated by a small number of ranking constraints. To improve mixing and achieve faster convergence to the target distribution, we utilize Hamiltonian Monte Carlo (HMC) techniques for drawing from multivariate normal distributions truncated by linear constraints (Pakman and Paninski 2014).[39] This delivers joint draws from of the full vector of school utilities for each student conditional on all the constraints described above except for the non-linear application decision inequality. To impose the application constraint, we rely on a simple accept-reject loop over the HMC draw.

We sample from the posterior distributions of the remaining latent variables and structural parameters using standard Gibbs steps. To facilitate this, we specify diffuse priors following Abdulkadiroğlu et al. (2017) and Rossi et al. (1996). We detail these remaining update steps and our choice of priors in Appendix B.

### 7.4. Estimating neighborhood preferences

Our approach to estimating neighborhood preference parameters builds on the two step procedure proposed by Bayer et al. (2007), in which neighborhood mean utilities are estimated in a first step, then decomposed into price and non-price components using a linear instrumental variables regression. The primary point of departure is the presence of a neighborhood specific amenity $S_{in}$ that is heterogeneous across households.[40] To accommodate this feature of our model, we proceed

[39] Hamiltonian Monte Carlo sampling methods generate proposals by treating the log of the probability distribution as minus the potential energy of a particle, and forward simulating the position of the particle by integrating its Hamiltonian equations of motion. In the special case when the target distribution is a multivariate normal distribution truncated by linear or quadratic inequalities, the Hamiltonian path has a closed-form solution allowing exact computation of travel time to each constraint. Upon reaching a constraint, the velocity of the particle is reflected. Since all dynamics and reflections are computed exactly, every proposal lies in the feasible region.

[40] Starting with the 2011-12 school year, the district switched from a Deferred Acceptance mechanism to the non strategy-proof Boston mechanism. We abstract from this change in our estimation and compute the inclusive value of schooling in each neighborhood $S_{in}$ as if assignment were coordinated using Deferred Acceptance throughout our sample period. The greatly restricted scope of choice in the post-period implies that differences in school access arising

using an iterated GMM procedure. We utilize data on residential choices for households enrolling in kindergarten between 2013 and 2015, when the reform had been in place for several years.

As in Bayer et al. (2007), we distinguish the component of household utility that is common to groups:

$$v_{in} = \gamma_g S_{in} + \mu_{gn} + \eta_{in}$$
$$\text{where} \quad \mu_{gn} \equiv -\alpha_g p_n + \mathbf{a}_n' \varphi_g + \xi_n.$$

We start by constructing neighborhood choice shares. To account for households enrolling in non-district schools who we do not directly observe, we utilize the enrollment rates implied by the school choice stage of the model. Specifically, we form neighborhood choice shares for each group by inverse-probability weighting observed enrollees:

$$\hat{s}_{gn} = \frac{\sum_{i \in g} y_{in} / \hat{\omega}_{in}}{\sum_{i \in g} 1 / \hat{\omega}_{in}},$$

where $y_{in}$ is an indicator taking value 1 when $i$ lives in $n$ and $\hat{\omega}_{in}$ are predicted enrollment probabilities.

The next step of the estimation iterates over the sensitivity to school access $\gamma_g$ and the components of $\mu_{gn}$. Using the group choice shares $\hat{s}_{gn}$, an inner loop solves for $\mu_{gn}$ using the Berry (1994) contraction. An outer loop estimates the preference parameters using variation in school access and prices arising from boundaries. To operationalize the procedure, we define the following structural residual in each iteration $k$,

$$e_{gn}^{(k)} \equiv \mu_{gn}^{(k)}(\gamma_g^{(k)}) + \alpha_g^{(k)} p_n - \mathbf{a}_n' \varphi_g^{(k)}.$$

This residual contains the common unobserved component of neighborhood quality $\xi_n$, as well as remaining unexplained variation in $\mu_{gn}^{(k)}$. The outer loop updates $\alpha_g$, $\varphi_g$, and $\gamma_g$ by targeting the following conditional moments in each boundary $b$:

$$\mathrm{E}\left[\mathbf{q}_{gn} e_{gn}^{(k)} | n \in \mathcal{N}_b(h)\right] = 0 \tag{9}$$
$$\mathrm{E}\left[S_{in} e_{gn}^{(k)} | n \in \mathcal{N}_b(h)\right] = 0, . \tag{10}$$

The vector $\mathbf{q}_{gn}$ includes neighborhood characteristics $\mathbf{a}_n$ and excluded price instruments. We include in $\mathbf{a}_n$ the share white of the neighborhood, the average income of the neighborhood, nearest distance to water, nearest distance to a major road, average square footage of housing units, and the share of housing units that are single-family. The notation $\mathcal{N}_b(h) \equiv \{n : \text{dist}(n, b) \leq h\}$

from the change in mechanism are likely to be minor.

denotes the set of neighborhoods falling within a distance $h$ of boundary $b$.[41] These moments pin down preferences by forcing choice residuals to be orthogonal to price instruments, observed neighborhood characteristics, and school access within a narrow band of each boundary. We iterate the procedure until convergence.

To form instruments for housing prices, we use the fact that groups place differential value on the set of schools accessible on either side of a given boundary. The value of school access for other groups affects equilibrium prices but does not feature in each household's own utility conditional on their access $S_{in}$. To operationalize this idea, we form average levels of school access for each group across neighborhoods, denoting these quantities by $\bar{S}_{gn}$.[42] This yields two excluded instruments for price $(\bar{S}_{hn})_{h \neq g}$. Finally, given estimates of $\alpha_g$ and $\varphi_g$, we estimate $\xi_n$ using an inverse-variance weighted projection of the residual $\hat{e}_{gn}$ on neighborhood fixed effects.

The validity of our price instruments hinge on correct specification of schooling access across groups. Our detailed data on application and enrollment decisions allow us to estimate fine distinctions in what different types of households value in schools. However, misspecification of $S_{in}$ may introduce correlation between estimated measures of access for other groups and unobserved neighborhood quality $\xi_n$. As a robustness check, we also estimated a version of the model using more traditional BLP instruments based on land use and characteristics of the housing stock in a "donut" surrounding each neighborhood. While these instruments also have well-documented weaknesses, the resulting estimates were qualitatively similar to those we report using the instruments described above, with somewhat weaker first-stage F-statistics.[43]

To construct counterfactuals, we also need to specify preferences for non-enrollment-age households. Since we do not have data on the counts of these households across fine geographies by income, we parameterize the access and price sensitivities for these households in a more stylized manner. Because we expect price sensitivity to depend largely on income, we set $\alpha_g$ equal to the estimated coefficient for enrollment households of the same income category. As described in Section 6, we allow non-enrollment households to place value on the average level of school access in each neighborhood, but specify a common sensitivity $\gamma$. We estimate this sensitivity using block-level estimates of the number of non-enrollment households in each neighborhood derived from the ACS. Appendix Section D.4 details the procedure used to construct these counts.

### 7.5. School preference estimates

Estimates of the parameters of the school choice stage of the model reported in Table 6, reveal several intuitive patterns. First, we estimate significant disutilities for distance for all groups. The

[41] Following the baseline specifications in Section 5.2, we set the bandwidth $h$ to be a quarter of a mile.

[42] The instruments we construct have some resemblance to Hausman-type instruments in the industrial organization literature. Rather than relying on demand shifts in other markets, the variation derives here from shifts in demand from other groups in the same market.

[43] Specifically, we constructed instruments based on the share of area zoned to residential and commercial use, the share of water, and the total number of single and multi-family units within a 2-3 mile ring of each neighborhood. We controlled for the values of these same characteristics in the 2 mile region surrounding each neighborhood.

availability of yellow bus service dissipates roughly a third of this disutility. The mean values of the deterministic utilities $\delta_{gj}$ are informative about the attractiveness of public schools relative to non-district options, such as private schools. Mean utilities are strongly decreasing in income, implying that non-district options are more relevant for higher income households. Projecting group utilities on school characteristics yields coefficients that broadly align with the configuration of sorting effects by income in Section 5.2. Higher-income households place more weight on peer demographics, while lower-income households place relatively greater value on test score performance.

We estimate highly significant variances of random coefficients on school characteristics, pointing to substantial unobserved heterogeneity in preferences for schools. In line with high rates of non-application, we estimate large average application costs that decline in income. Interpreting the average cost for low-income households in terms of disutility of distance, our estimates imply costs equivalent to 3.7 miles of additional travel to school. Costs for middle- and high-income households, by contrast, correspond to only 1.8 miles and .8 miles of travel, respectively.

### 7.6. Neighborhood preference estimates

Estimates of neighborhood preferences are reported in Table 7. As expected, estimates of the marginal utility weight for schools ($\gamma_g$) and prices ($\alpha_g$) are positive for all three income groups. To facilitate interpretation, we convert the coefficient $\gamma_g$ into a measure of willingness to pay by dividing it by $\alpha_g$. We estimate that low-income households are willing to pay around \$110 a month for a one standard deviation increase in neighborhood school access, while estimates for middle- and high-income households are approximately \$260 and \$360 a month, respectively. [44]

These estimates of willingness to pay are in line with more recent studies that estimate willingness to pay for enrolled households.[45] Notably, our estimates are substantially higher than reported in studies that do not distinguish households with school-age children. For instance, Bayer et al. (2007) estimate that households at large have an average marginal willingness to pay of \$17 a month for a one standard deviation increase in neighborhood school test scores. Our estimates of willingness to pay for non-enrollment-age households fall closer to this mark.

## 8. Welfare effects of neighborhood schools

### 8.1. Simulating neighborhood assignment

To quantify the welfare consequences of neighborhood schools, we simulate neighborhood assignment for the 2008 cohort of kindergarten age households in Seattle. In particular, we set $\rho_j = 1$ for each student's neighborhood school and $\rho_j = 0$ elsewhere, guaranteeing assignment at

[44] We also report first-stage F-statistics for our price instruments, which range from 7.2 for middle-income households to 11.2 for low-income households.

[45] Park and Hahm (2023) estimate a median WTP of \$154 for a one standard deviation increase in school test scores for enrolled households in New York City.

neighborhood schools. Following the 2010 reform, students may still apply to non-neighborhood schools with available capacity, with applications coordinated through Deferred Acceptance.

As in our estimation, the sample consists of households enrolled in both public and private schools. We fix parameter estimates at their posterior means, using the Hamiltonian Monte Carlo sampler described in Section 7.3 to draw latent school utilities from the distribution of $\mathbf{u}_i$ conditional on observed application and enrollment decisions. We draw latent utilities for neighborhoods from the conditional distribution of $\eta_{in}$ given the observed neighborhood choice.[46]

We start by specifying the measure of enrollment- and non-enrollment-age households of each income category in the market. To do so, we rely on 2005–2009 ACS 5-year microdata files which provide demographic and income information for households in the greater Seattle area. We define households as enrollment-age if they have at least one child aged 6 or under. Enrollment-age households defined in this way represent 11.4% of all households in the combined region.

Computing the counterfactual equilibrium requires solving for admissions cutoffs and housing prices. We solve for these quantities using a nested fixed point iteration, taking observed cutoffs and prices in 2013 as our initial guess. In each iteration, we use the current value of admissions cutoffs to compute the inclusive value of schooling $S_{in}$ for enrollment-age households in each neighborhood. Since the expected utility from schooling does not admit a simple closed-form expression, we compute $S_{in}$ by drawing school utilities $\mathbf{u}_{in}$ for each student-neighborhood pair, forming application choices, and simulating the Deferred Acceptance match. An inner loop solves for market clearing prices in each neighborhood given the resulting values of $S_{in}$. Section B.4 describes the procedure in detail.

### 8.2. Welfare impacts

The removal of choice has heterogeneous effects on the value of school access $S_{in}$ across neighborhoods and income groups. Figure 7 illustrates these changes by mapping shifts in equilibrium school access for each income group. Panel (a) shows that low-income households see a decline in school access in most neighborhoods, with the notable exception of the attendance area belonging to Stevens Elementary—a small historic school near downtown. Panel (b) reveals a sharp north-south divide for middle-income households: school access falls sharply in the heavily minority South End, but increases in whiter neighborhoods north of the Lake Washington Ship Canal. Panel (c) shows that high-income households gain substantially in Capitol Hill and Northeast Seattle, neighborhoods home to several of the district's highest-performing schools. However, high-income households residing elsewhere, including in southern waterfront neighborhoods, experience sizable losses.

Equilibrium changes in the cost of housing across neighborhoods further tilt the distributional effects. Panel (d) of Figure 7 maps these changes. Unsurprisingly, the geographic pattern of

[46]For each household, we sample 1,000 draws after a burn-in period of 200 draws.

changes in housing costs closely mirrors that of changes in school access. Costs fall in most areas in the south of the district where schools enroll larger shares of minority and low-income students. Costs rise in the desirable high performing schools in the northeast. By raising prices in expensive areas and lowering them in affordable ones, neighborhood assignment widens housing price dispersion across the district.

Figure 8 shows the net effects of the reform on household welfare, decomposing the aggregate effect into contributions from changes in school access, housing costs, and residential relocation. We start by quantifying the change in school access for each group holding household locations fixed. For all three groups, returning to neighborhood assignment implies substantial losses in school access, as shown by the blue bars. Across all enrollment-age households living in the district, neighborhood schools lead to an average decline in access worth a little over \$30 a month. This loss is smallest for low-income households who experience declines of slightly less than \$20 a month, and largest for middle-income households who lose the equivalent of \$45 a month. Losses for high-income households are partially offset by gains for households zoned to the district's most desirable neighborhood schools, but are still substantial at \$36 a month.

Continuing to hold household locations fixed, the red bars show the changes in household welfare resulting from adjustments in equilibrium prices. We abstract temporarily from capital gains and losses for incumbent homeowners, treating households as renters and entrants who prefer lower housing costs, all else equal. Housing costs fall by an average of \$4 a month district-wide, reflecting the slightly decreased school access in Seattle as a whole relative to the surrounding districts. Within Seattle, the decline in housing costs in low-income neighborhoods almost fully compensates low-income households for the loss of school access. Price increases in desirable attendance areas exacerbate losses for high-income renters and home-buyers, while middle-income households are largely unaffected on net. The next section discusses how accounting for home owning alters these conclusions.

The final set of bars (purple) in Figure 8 show the improvements in welfare resulting from allowing households to move in response to the change in assignment policy. In aggregate, residential relocations offset over a third of the static decline in access. These gains are relatively minor for low-income households, but middle- and high-income households recoup \$18 and \$24 dollars a month respectively by moving to more preferred areas. Appendix Figure A.9 maps changes in the geographic distribution of enrollment-age households across the district.

The top of Table 8 tabulates the total welfare impacts for each group, and compares these effects to those inferred by a model ignoring residential adjustment.[47] Ignoring price effects and moves to preferred schools overstates the true welfare loss from switching to neighborhood schools by a factor of two.

[47] These effects are quantitatively similar to the changes in school access represented by the first set of bars in Figure 8, but are based on a distinct set of changes in $S_{in}$ computed in an equilibrium where households do not move.

### 8.3. Incumbent homeowners and non-enrollment households

The accounting in Figure 8 abstracts from the possibility that not all households prefer lower home prices. Existing homeowners may benefit from increases in home values, while residents in public housing are insulated from changes in market rents. To properly incorporate these effects, we split enrollment-age households into three groups based on their street address in the Infutor data at the start of 2003, five years prior to the policy counterfactual.[48] We identify households as incumbent homeowners if they lived at a non-rental Seattle address at this point in time, and public housing residents as households living in public housing projects managed by the Seattle Housing Authority. The remaining households consist of renters and new homebuyers.

We treat changes in the prices of homes owned by incumbents as capitalized gains that are realized only when a household moves. For any incumbent household choosing a location different than their 2003 address, we convert the full lump-sum change in home value into a monthly flow equivalent using a standard annuity formula and an annual discount rate of 3%. For households who do not change locations, we first compute the expected present value of future sale using sale-time distributions estimated using our transactions data in each neighborhood, before converting to a monthly flow in the same way.

Table 8 tabulates total changes in welfare accounting for differences in pre-existing housing status. Relative to the impacts shown in Figure 8, the primary consequence is that high-income households with elementary age children experience smaller losses thanks to appreciation in the prices of homes zoned to desirable neighborhood schools. Decreases in home prices owned by low- and middle-income incumbents lead to small marginal losses. Notably, the welfare gain from overall increases in home values (\$2.3 a month) does not fully offset the accompanying increase in monthly user costs (\$4.1 a month) for homeowning households. This is explained in part by the relatively long average time between sales we observe in our sample (8.4 years).

Around a fifth of low-income households are residents of public housing. Unlike other low-income renters, these households do not benefit directly from the decline in market rents in low-income areas. These households experience losses of \$6 a month from the restriction in out-of-neighborhood enrollment. In spite of this, low-income households as a group continue to weakly prefer neighborhood assignment. This result contrasts with the predictions of theoretical models of jurisdictional sorting which predict gains for low-income households from school choice, including Epple and Romano (2003) and Grigoryan (2021). Common to these two models is an assumption that housing prices in the lowest quality neighborhoods are fixed under changes in assignment policy. Our empirical results show that declining market rents in low-income neighborhoods are consequential for welfare. More broadly, the pattern of welfare effects we estimate supports an "ends-against-the-middle" interpretation of the distributional consequences of school choice, as

[48] A five-year lag aligns with the timing in our reduced form estimates of residential responses between 2013 and 2015, four to six years after the (factual) reform.

argued by Avery and Pathak (2021) and Epple and Romano (1996).

The last column reports on welfare impacts for households without enrollment-age children. Since these households have much weaker preferences for school access, welfare impacts are smaller and largely track changes in prices. We estimate a small gain of \$4 a month for renters and new homebuyers, and small losses of \$2 a month for incumbent homeowners. On average, welfare for non-enrollment-age households rises by a little less than a dollar a month.

### 8.4. A simple cost-benefit analysis

What do these estimates imply about whether school choice is worth the cost? In Appendix D, we conduct a back-of-the-envelope estimation of the district's transportation savings from returning to neighborhood schools. Table D.1 documents the district's annual transportation expenditures from 2004 to 2016. Prior to 2010, the district spent approximately \$640 a student on transportation costs. This figure falls to around \$550 a few years after the reform. Using the share of students eligible for bus transportation at each grade level, we estimate slightly larger per-pupil savings at the elementary level. On a monthly basis, we place transportation cost savings associated with the reform at around \$15 a month per elementary school student.[49]

This estimate of per-pupil savings for the district is remarkably similar to the average welfare cost of \$16 a month we estimate for households. This suggests that school choice brings benefits to families that perhaps justify the costly transportation required. However, our estimates imply that low-income households do not see these benefits. This is consequential because district budgets are tight. As a school board representative put it at a meeting introducing the new attendance area maps in November 2009: "Every dollar we spend on transportation is money we can't spend in the classroom" Record (2009). Our results suggest low-income families would likely prefer the district spend on instruction rather than on choice. Accounting for equilibrium residential adjustment is key to this conclusion. As reflected by the estimates in Column (2) of Table A.5, ignoring these forces leads to estimates of welfare losses for all income groups which exceed the estimated cost savings to the district.

### 8.5. Effects on segregation and enrolled school quality

While parental welfare is a key consideration in gauging the appeal of different assignment policies, choice policies are often justified on other grounds—particularly their potential to improve educational outcomes for disadvantaged students and reduce segregation. Critics of neighborhood assignment argue it concentrates low-income students in lower-quality schools and increases racial stratification. These concerns extend beyond equity. As argued by Bénabou (1996), stratification can reduce the efficiency of human capital formation.

[49] Note that these numbers represent transportation savings apportioned across all enrolled students, including those not riding the bus. In 2007, a year when data are available, 32% of students were bused to school, so that savings per rider are likely closer to \$50 a month. Relatedly, the per-pupil cost estimate does not include savings from reductions in private commutes.

Table 9 shows these concerns did not materialize in Seattle. Comparing post-reform segregation to 2008 levels, both the variance ratio and minority isolation indicate declining racial segregation in schools after the reform. Residential segregation across neighborhoods also declines. While these patterns may partially reflect the secular increase in non-minority enrollment districtwide, our counterfactual simulations using pre-reform data predict effects in the same direction. This supports the conclusion that neighborhood assignment reduced racial segregation in both schools and neighborhoods.

Meanwhile, measures of the segregation of low-income students increase modestly in both schools and neighborhoods. The most notable change is increased residential isolation, consistent with the patterns of sorting by income group across attendance areas we document. However, we emphasize that these indices report on the segregation of same-age peers while broader measures of neighborhood segregation are less affected.

To examine how the reform shifted students across the quality distribution of schools, the bottom half of the table shows changes in the average baseline math scores and value-added of enrolled schools. Low-income students experience modest improvements in school quality, both in terms of peer achievement and value-added. In contrast, changes in these measures are small and statistically insignificant for middle- and high-income students. Despite restricted choice, school quality remains largely unchanged for middle- and high-income students, consistent with residential relocation offsetting potential losses in access to effective schools for these households.

Finally, for the majority of the segregation and attended school characteristics reported in this table, changes predicted by the model align closely with observed changes in these quantities observed post-reform. This is apparent when comparing columns (2) to (4) to column (5). Because we use only pre-reform data to estimate application costs and preferences for schools, this provides an out-of-sample validation of the application and enrollment stages of the model.

## 9. Conclusion

This paper evaluates the welfare consequences of neighborhood assignment relative to public school choice by studying Seattle's return to neighborhood assignment in 2010. Our results show that when application-based choice is restricted, households choose schools by choosing neighborhoods, moving toward the attendance areas of desirable schools. This behavioral response mitigates the loss in household welfare from the restriction in school choice. Notably, low-income households mostly prefer neighborhood assignment, as equilibrium prices fall in low-income areas of the district.

Our results raise the bar for school choice policies. As transportation costs rise, arguments for choice typically rest on distributional grounds. Our analysis suggests the primary beneficiaries from choice are middle- and high-income families, who are able to live in more affordable neighborhoods while attending desirable non-neighborhood schools when choice is offered. Complementing a growing literature on the barriers faced by low-income and minority households in the application process, this paper provides empirical evidence that choice also generates unintended consequences for these families in the housing market by compressing the distribution of housing costs in the district.

Several important questions remain beyond our scope. Our analysis captures short-run equilibrium responses but abstracts from longer-run dynamics. Over time, neighborhood assignment may change the reputation and character of neighborhoods through entrenched residential sorting or alter school quality through shifts in teacher composition. The relative benefits of choice may also differ at higher grade levels, where households may be less mobile and schools may offer more specialized curricula. Finally, neighborhood assignment may foster greater parental and community investment in local schools. We hope to explore these issues in future research.

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Table 1: Seattle Kindergarten Sample (2006-2008)

| | Enrollees | | | | |
|---|---|---|---|---|---|
| | All (1) | Low-income (2) | Middle-income (3) | High-income (4) | Applicants (5) |
| *Demographics* | | | | | |
| White | 0.45 | 0.19 | 0.43 | 0.71 | 0.54 |
| Asian | 0.17 | 0.21 | 0.21 | 0.10 | 0.17 |
| Black | 0.17 | 0.30 | 0.16 | 0.04 | 0.11 |
| Hispanic | 0.12 | 0.21 | 0.10 | 0.05 | 0.10 |
| Female | 0.50 | 0.50 | 0.52 | 0.50 | 0.47 |
| Special Education | 0.04 | 0.05 | 0.05 | 0.04 | 0.06 |
| *Housing and income* | | | | | |
| Renter | 0.39 | 0.80 | 0.33 | 0.04 | 0.32 |
| Public housing | 0.06 | 0.18 | 0.00 | 0.00 | 0.05 |
| Property value ($) | 352,550 | 137,080 | 304,375 | 566,439 | 387,280 |
| Imputed household income ($) | 97,730 | 27,954 | 79,408 | 184,448 | 110,766 |
| *N* | 11,393 | 3,868 | 3,643 | 3,882 | 8,235 |

*Notes.* This table reports on characteristics of SPS kindergarten enrollees and applicants in the school year starting fall 2006 through the school year starting fall 2008. Students are assigned to income categories using an imputation procedure based on property values, as described in the text. Estimates of average household income are derived from 2005–2009 ACS 5-year estimates. Property values and incomes reported in 2010 dollars. The sample in Column (5) consists of applicants in the district's main choice round (and excludes early sibling applicants).

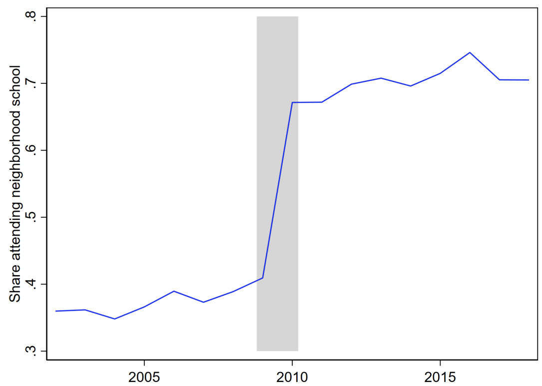
Table 2: Enrollment and Application Patterns Under Choice (2006-2008)

| | All<br>(1) | Low-income<br>(2) | Middle-income<br>(3) | High-income<br>(4) |
|---|---|---|---|---|
| | | *Panel A. Enrollees* | | |
| Distance to enrolled school (miles) | 1.57 | 2.36 | 1.35 | 0.98 |
| Enrolled at neighborhood school | 0.36 | 0.26 | 0.35 | 0.48 |
| Applied in main round | 0.59 | 0.48 | 0.62 | 0.68 |
| Applied in early sibling round | 0.14 | 0.08 | 0.12 | 0.22 |
| Did not apply | 0.27 | 0.44 | 0.26 | 0.10 |
| Administrative placement | 0.29 | 0.46 | 0.28 | 0.12 |
| *N* | 11,393 | 3,868 | 3,643 | 3,882 |
| | | *Panel B. Applicants* | | |
| Enrolled | 0.82 | 0.89 | 0.85 | 0.76 |
| Neighborhood school ranked 1st | 0.31 | 0.25 | 0.29 | 0.37 |
| Neighborhood school ranked in top 3 | 0.55 | 0.42 | 0.52 | 0.66 |
| Neighborhood school not ranked | 0.39 | 0.55 | 0.42 | 0.27 |
| *N* | 8,235 | 2,110 | 2,633 | 3,492 |

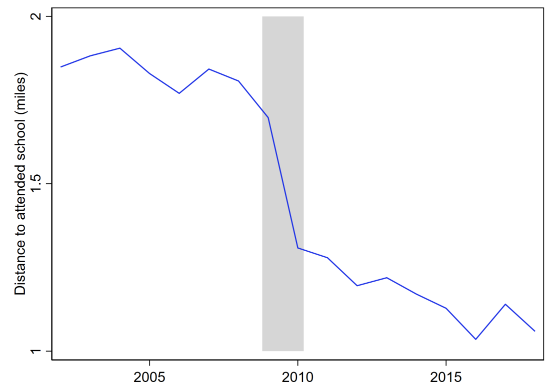
*Notes.* This table reports on enrollment and application patterns for kindergarten students between 2006 and 2008 under district-wide choice. The sample in Panel A consists of kindergarten students enrolled in the district. Panel B reports on kindergarten applicants in the main choice round (excluding early sibling applicants).

Figure 1: Elementary School Choice in Seattle at Kindergarten

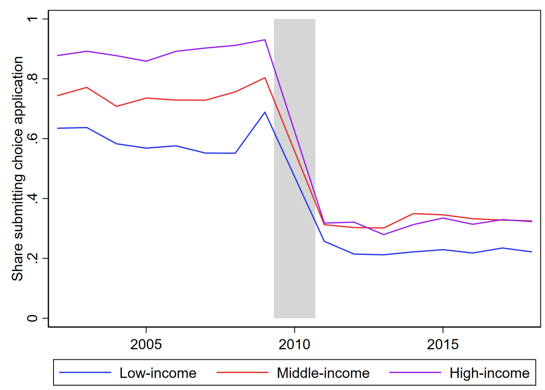
(a) Share attending neighborhood school




(b) Distance to attended school




(c) Application rates by income category




*Notes:* This figure shows the consequences of the 2010 reform on enrollment and application rates. Panel (a) plots the share of kindergarteners attending their neighborhood school, according to the newly adopted attendance area maps. Panel (b) plots average road distance to enrolled schools. Panel (c) plots the share of enrolled students submitting choice applications.

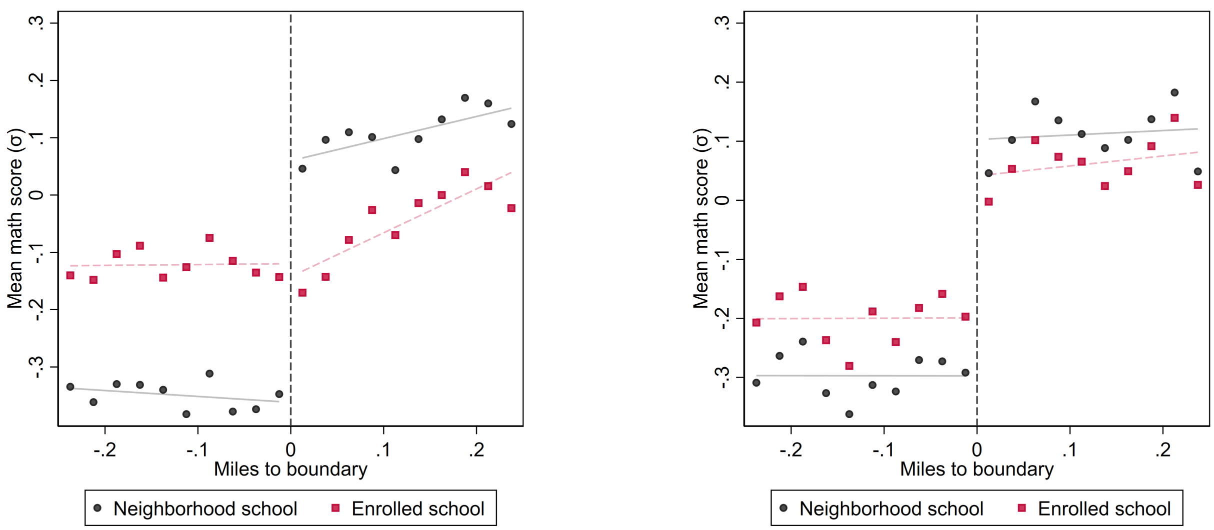
Table 3: Simulated and Observed Neighborhood Assignment

| | 2008 | | | | | |
|---|---|---|---|---|---|---|
| | Observed (1) | Zone Assign (2) | Proximity DA (3) | 2010 (4) | 2012 (5) | 2014 (6) |
| Distance (miles) | 1.86 | 0.76 | 1.05 | 1.36 | 1.24 | 1.21 |
| $N$ | 3,965 | 3,965 | 3,965 | 4,333 | 4,897 | 4,714 |
| *School segregation* | | | | | | |
| Low-income variance ratio | 0.23 | 0.24 | 0.20 | 0.24 | 0.24 | 0.25 |
| Low-income isolation | 0.20 | 0.19 | 0.14 | 0.25 | 0.18 | 0.22 |
| Minority variance ratio | 0.26 | 0.18 | 0.14 | 0.24 | 0.21 | 0.25 |
| Minority isolation | 0.19 | 0.00 | 0.03 | 0.12 | 0.09 | 0.18 |
| *Low-income students* | | | | | | |
| Distance (miles) | 2.32 | 0.85 | 1.36 | 1.57 | 1.42 | 1.33 |
| Math scores | -0.28 | -0.26 | -0.20 | -0.23 | -0.20 | -0.19 |
| Value-added | -0.03 | -0.01 | 0.01 | -0.00 | 0.01 | 0.01 |
| *Middle-income students* | | | | | | |
| Distance (miles) | 1.89 | 0.74 | 1.09 | 1.45 | 1.25 | 1.30 |
| Math scores | -0.07 | -0.13 | -0.11 | -0.06 | -0.04 | -0.01 |
| Value-added | 0.05 | 0.04 | 0.03 | 0.06 | 0.07 | 0.07 |
| *High-income students* | | | | | | |
| Distance (miles) | 1.41 | 0.71 | 0.73 | 1.08 | 1.05 | 0.99 |
| Math scores | 0.24 | 0.19 | 0.19 | 0.23 | 0.22 | 0.22 |
| Value-added | 0.18 | 0.18 | 0.16 | 0.18 | 0.17 | 0.17 |

*Notes.* This table reports district-wide segregation and characteristics of attended schools by student income category. Column 1 reports observed enrollments under Seattle's district-wide choice system in 2008. Column 2 reports on a counterfactual assignment of the 2008 cohort to schools based on 2010 neighborhood school attendance areas. Column 3 reports on a simulated Deferred Acceptance assignment in which students rank schools in order of ascending road distance from their home, and schools rank students analogously. Columns 4-6 report on observed enrollments in the initial years of the neighborhood school scheme. Value-added refers to causal impacts of schools on 4th grade math scores estimated using the risk-controlled approach of Angrist et al. (2020), detailed in Section D.6.

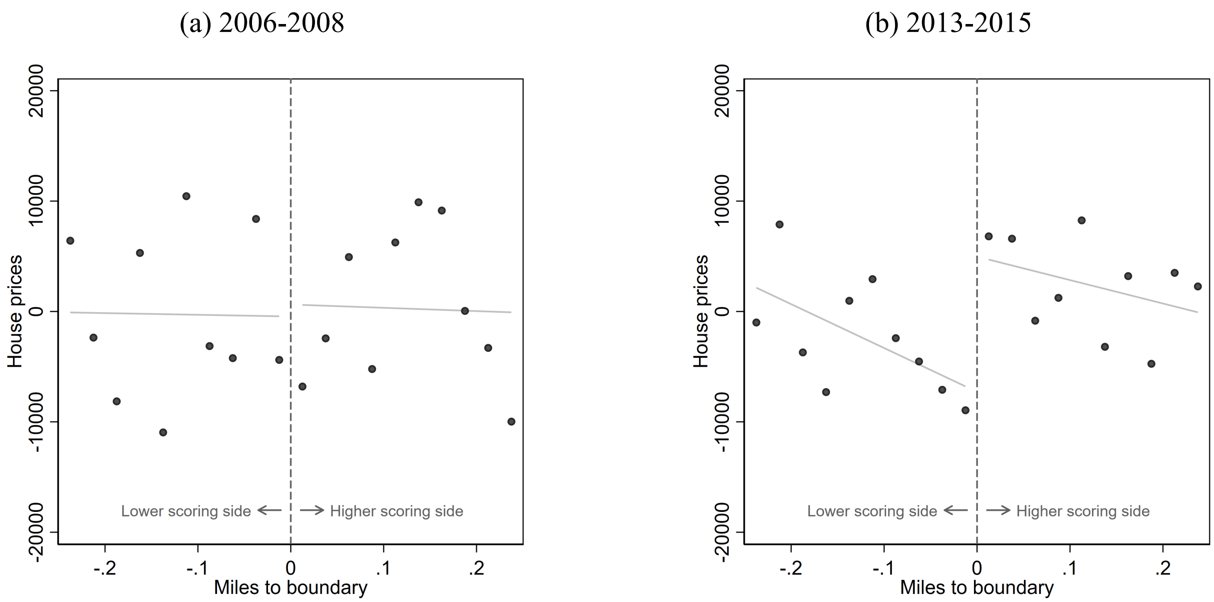
Figure 2: Discontinuities in Math Scores of Neighborhood and Enrolled Schools

(a) 2006-2008 (b) 2013-2015




*Notes:* This figure illustrates how attendance area boundaries create variation in the average math scores of schools where kindergarten students enroll. Boundaries are organized so that the side of the boundary zoned to the school with higher baseline math scores is assigned positive distance (on the right side of each panel). Panel (a) shows average math scores of enrolled schools for students living within a quarter mile of an (eventual) attendance area boundary in the pre-period. Panel (b) shows average math scores for both enrolled and assigned neighborhood schools in the post-period. Scores are reported in standard deviation units ($\sigma$).

Figure 3: Effects of Neighborhood School Math Scores on House Prices




*Notes:* This figure shows prices of single-family homes sold near elementary school attendance boundaries before and after the reform. Boundaries are organized so that the side of the boundary zoned to the school with higher baseline math scores is assigned positive distance (as illustrated in Figure 2). Each panel plots binned average house prices, residualized by a vector of house characteristics. Panel (a) shows sales from 2006–2008, prior to the establishment of the boundaries. Panel (b) shows post-reform sales from 2013–2015.

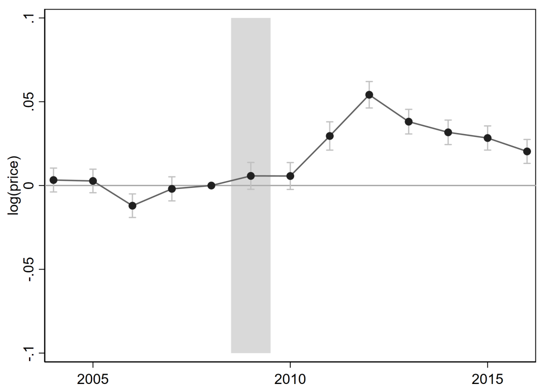
Table 4: Capitalization Effects of Neighborhood School Characteristics

| | Share white (1) | Share low-income (2) | Math scores (3) | Value-added (4) |
|---|---|---|---|---|
| | *Panel A. No demographic controls* | | | |
| School characteristic | -1,156 | 7,929 | -4,111 | 4,147 |
| | (8,724) | (8,163) | (6,674) | (4,666) |
| School characteristic × Post (2010-2012) | 14,223* | -8,325 | 3,799 | -5,259 |
| | (4,770) | (5,437) | (5,329) | (5,048) |
| School characteristic × Post (2013-2015) | 16,306*** | -18,438*** | 9,786** | -2,636 |
| | (3,362) | (3,828) | (3,122) | (4,617) |
| Boundaries | 63 | 63 | 63 | 63 |
| *N* | 29,916 | 29,916 | 29,916 | 29,474 |
| | *Panel B. Demographic controls* | | | |
| School characteristic | -2,694 | 8,803 | -3,961 | 6,191 |
| | (8,541) | (7,470) | (5,844) | (4,356) |
| School characteristic × Post (2010-2012) | 13,324** | -8,324 | 4,376 | -7,041 |
| | (4,829) | (5,249) | (5,141) | (4,712) |
| School characteristic × Post (2013-2015) | 14,558*** | -13,469** | 9,895** | -1,627 |
| | (3,856) | (4,503) | (3,362) | (3,853) |
| Boundaries | 63 | 63 | 63 | 63 |
| *N* | 29,916 | 29,916 | 29,916 | 29,474 |

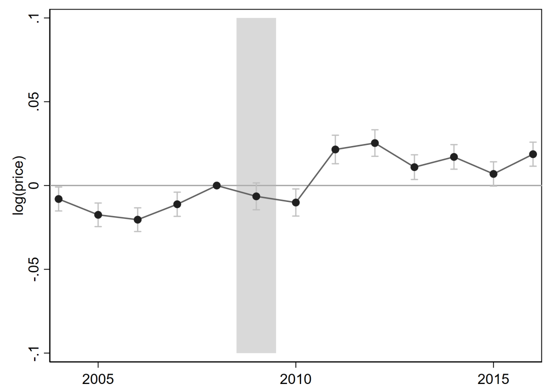
*Notes.* * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This table reports boundary discontinuity estimates of the effect of neighborhood school characteristics on the transaction prices of homes sold (in 2010 dollars) from Equation (1). The sample consists of transactions of single family homes within a quarter mile of elementary school boundaries in each of the three time periods (2006–2008, 2010–2012, and 2013–2015). All specifications include linear controls for distance from the boundary on each side, boundary fixed effects, and controls for housing characteristics. Standard errors clustered at the attendance area level in parentheses. Value-added refers to causal effects on 4th grade math scores estimated using the risk-controlled approach of Angrist et al. (2020).

Figure 4: Housing Prices across Attendance Areas

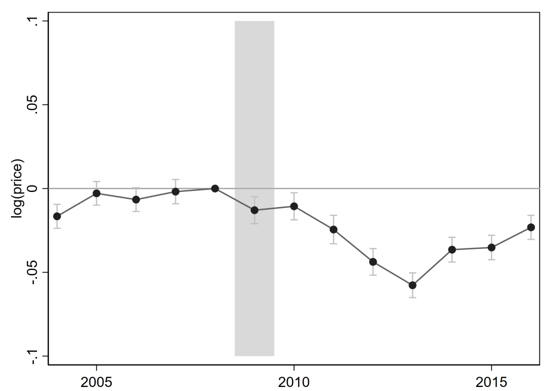
(a) Above-median school share white




(b) Above-median school math scores



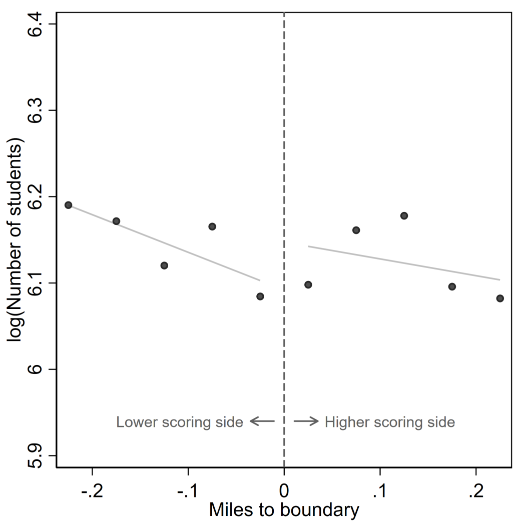

(c) Below-median residential income



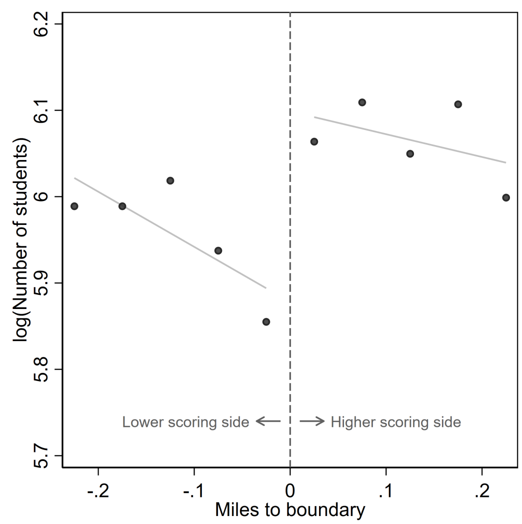

*Notes:* These figures compare transaction prices of single-family homes across attendance areas by plotting difference-in-differences estimates from Equation (2). Panel (a) compares attendance areas zoned to schools with an above-median share of white students at baseline to other attendance areas. The treated group in Panel (b) consist of attendance areas zoned to schools with above-median 4th grade math scores. Panel (c) compares attendance areas with below-median residential income (based on 2005-2009 ACS estimates) to areas with above-median baseline income.

Figure 5: Residential Sorting across Boundaries

(a) Counts of enrolled students: 2006-2008




(b) Counts of enrolled students: 2013-2015



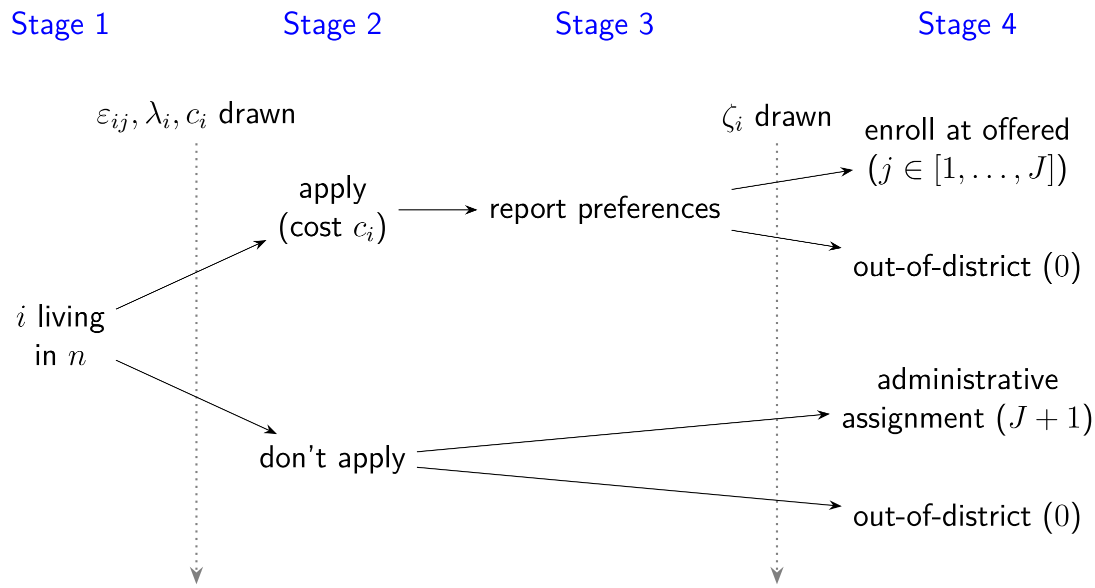

*Notes:* This figure shows counts of enrolled students living near attendance boundaries. Boundaries are organized so that the side of the boundary zoned to the school with higher baseline math scores is assigned positive distance (on the right side of each panel). Panel (a) plots log counts of enrolled students in each distance bin prior to the establishment of boundaries between 2006–2008. Panel (b) plots log counts under neighborhood assignment between 2013–2015.

Table 5: Residential Sorting Effects

| | All (1) | Low-income (2) | Mid-income (3) | High-income (4) |
|---|---|---|---|---|
| | | *Panel A. Math scores* | | |
| Math scores | -0.038 | -0.115 | -0.020 | 0.137 |
| | (0.032) | (0.058) | (0.036) | (0.077) |
| Math scores $\times$ Post (2010–2012) | 0.079* | 0.176** | 0.010 | -0.049 |
| | (0.030) | (0.050) | (0.041) | (0.041) |
| Math scores $\times$ Post (2013–2015) | 0.092* | 0.128** | 0.059 | -0.027 |
| | (0.036) | (0.047) | (0.042) | (0.046) |
| *N* | 19,884 | 5,953 | 7,388 | 5,374 |
| | | *Panel B. Share white* | | |
| Share white | -0.049 | -0.129 | 0.037 | 0.028 |
| | (0.055) | (0.070) | (0.062) | (0.087) |
| Share white $\times$ Post (2010–2012) | 0.114* | 0.155* | 0.134 | 0.017 |
| | (0.057) | (0.072) | (0.082) | (0.145) |
| Share white $\times$ Post (2013–2015) | 0.210*** | 0.096** | 0.226** | 0.273** |
| | (0.058) | (0.033) | (0.078) | (0.078) |
| *N* | 19,884 | 5,953 | 7,388 | 5,374 |

*Notes.* * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This table reports estimates of the semi-elasticity of residential location with respect to differences in neighborhood school characteristics from Equation (3). Each coefficient derives from an individual regression of log counts on school characteristics, boundary fixed effects, and controls for neighborhood characteristics. The sample consists of households enrolled in kindergarten at a district school in each of the three periods (2006–2008, 2010–2012, and 2013–2015). The bandwidth is a quarter mile on each side of the boundary. We drop boundaries with fewer than five students on a given side. Standard errors clustered at the attendance area level reported in parenthesis.

Figure 6: Model Timing




*Notes:* This figure shows the timing of decisions and preference shocks for households with school-age children described in Section 6. Here, $\varepsilon_{ij}$ are idiosyncratic preference shocks, $\lambda_i$ are random coefficients on school characteristics, $c_i$ is the fixed cost of application, and $\zeta_i$ is an enrollment-time shock to the out-of-district schooling option.

Table 6: Estimates of School Preferences

| | All (1) | Low-income (2) | Middle-income (3) | High-income (4) |
|---|---|---|---|---|
| Distance disutility | -0.45 (0.004) | -0.34 (0.006) | -0.42 (0.008) | -0.54 (0.005) |
| Distance $\times$ Bus offered | 0.15 (0.005) | 0.13 (0.009) | 0.11 (0.008) | 0.19 (0.008) |
| *Deterministic utilities ($\delta_{gj}$)* | | | | |
| Mean | 0.12 (0.07) | 1.17 (0.09) | 0.30 (0.11) | -0.76 (0.15) |
| Standard deviation | 0.80 (0.05) | 0.57 (0.06) | 0.68 (0.07) | 1.06 (0.11) |
| Projection on characteristics: | | | | |
| Share white | 0.36 (0.03) | -0.03 (0.04) | 0.26 (0.03) | 0.71 (0.04) |
| Math scores | 0.29 (0.02) | 0.15 (0.03) | 0.28 (0.03) | 0.39 (0.03) |
| Value-added | 0.15 (0.01) | 0.14 (0.02) | 0.12 (0.02) | 0.17 (0.02) |
| *Application costs (c)* | | | | |
| Mean | 0.78 (0.02) | 1.28 (0.04) | 0.77 (0.04) | 0.45 (0.03) |
| Standard deviation | 0.29 (0.02) | 0.35 (0.07) | 0.30 (0.03) | 0.25 (0.02) |
| Enrollment shock ($\sigma_\varsigma$) | 1.08 (0.05) | 0.90 (0.06) | 0.81 (0.09) | 1.33 (0.16) |
| *Random coefficients (Variances)* | | | | |
| Share white | 0.74 (0.05) | | | |
| Math scores | 0.14 (0.02) | | | |
| Value-added | 0.09 (0.0091) | | | |
| *N* | 4,855 | 1,362 | 1,556 | 1,937 |

*Notes.* This table reports estimates of the parameters in the school choice stage of the model. The scale normalization is given by the variance of the idiosyncratic school preference shock $\varepsilon_{ij}$. Value-added refers to causal effects on 4th grade math scores estimated using the risk-controlled approach of Angrist et al. (2020). Standard errors in parentheses.

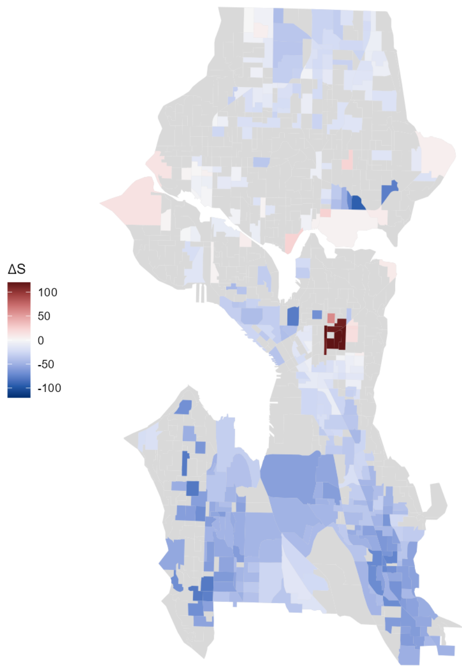
Table 7: Estimates of Neighborhood Preferences

| | Low-income (1) | Middle-income (2) | High-income (3) |
|---|---|---|---|
| *Panel A. Enrollment-age households* | | | |
| School access ($\gamma_g$) | 0.27 | 0.29 | 0.24 |
| | (0.13) | (0.11) | (0.10) |
| Housing price sensitivity ($\alpha_g$) | 0.24 | 0.11 | 0.066 |
| | (0.069) | (0.040) | (0.036) |
| WTP for school access | 111 | 264 | 360 |
| ($ per month) | (66) | (150) | (289) |
| First-stage F | 11.2 | 7.2 | 8.0 |
| *N* | 1,362 | 1,556 | 1,937 |
| *Panel B. Other households* | | | |
| School access ($\gamma_g$) | | 0.02 | |
| | | (0.04) | |
| WTP for school access | 9 | 19 | 31 |
| ($ per month) | (34) | (76) | (128) |
| *N* (ACS) | 83,807 | 74,190 | 30,355 |

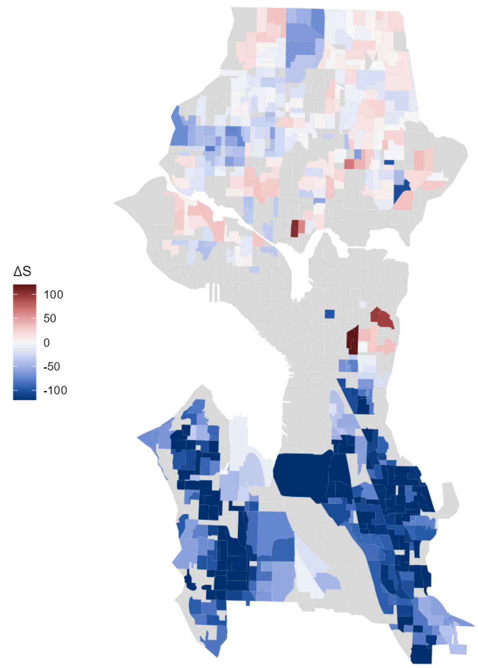
*Notes*. This table reports neighborhood choice parameters estimated using data on residential locations of households in neighborhoods within a quarter mile of a boundary. The sample in Panel A consists of households enrolled in SPS kindergarten between 2013 and 2015. The estimation adjusts for rates of non-enrollment as described in the text. Panel B reports our estimate of school access sensitivity for non-enrollment-age households, using 2012-2016 5-year ACS estimates of block group level counts. Price sensitivities for non-enrollment-age households are set equal to the estimates for the corresponding group in Panel A. WTP is computed as $\gamma_g/\alpha_g$, with delta method standard errors reported in parentheses. Monthly housing costs are in units of $100. School access is normalized to unit variance across neighborhoods within each group.

Figure 7: Equilibrium Effects on School Access and Housing Costs

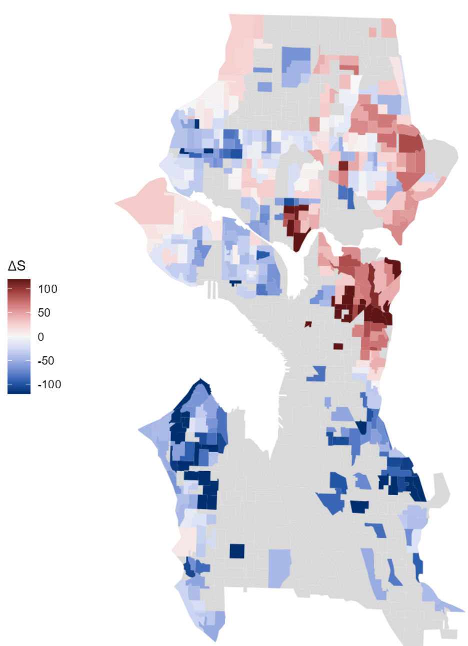
(a) Change in school access: low-income



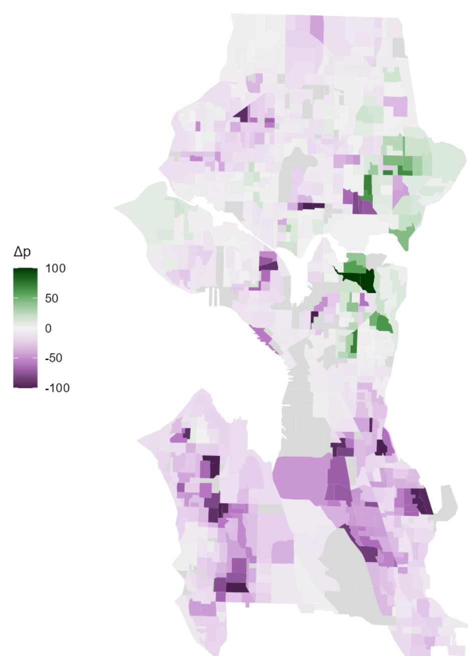

(b) Change in school access: middle-income




(c) Change in school access: high-income

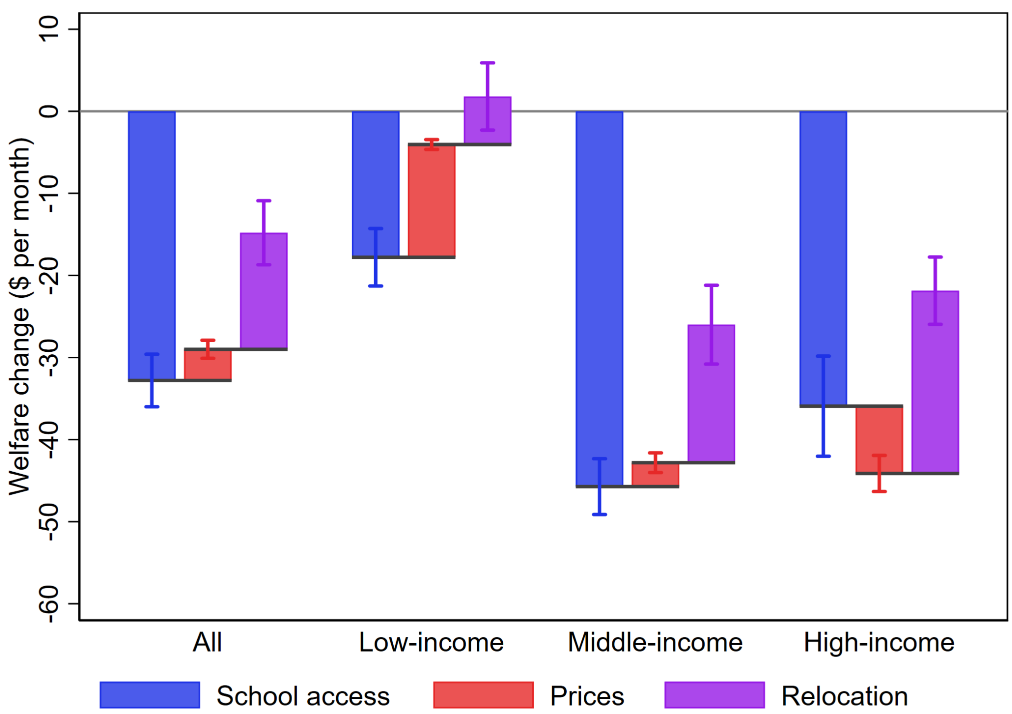



(d) Change in rents




*Notes:* This figure plots changes in equilibrium school access and rents across Seattle neighborhoods. Panels (a)–(c) plot the change in school access in units of dollars per month. Panel (d) plots the change in equilibrium monthly housing costs.

Figure 8: Welfare Effects of Neighborhood Schools




*Notes:* This figure depicts the change in welfare for kindergarten households in 2008 under counterfactual neighborhood assignment. The blue bars depict the change in welfare associated with changes in equilibrium school access, holding household locations fixed. The red bars depict the additional impact of changes in equilibrium housing costs, again holding locations fixed. Note that these bars abstract from capital gains (and losses) accruing to existing homeowners. Finally, the purple bars show the change in welfare resulting from moves to new neighborhoods.

Table 8: Welfare Effects of Neighborhood Schools by Income and Housing Status

| | Enrollment-age households | | | | |
|---|---|---|---|---|---|
| | All (1) | Low-income (2) | Middle-income (3) | High-income (4) | Other households (5) |
| Welfare ($ per month) | -16.3 | +0.4 | -28.5 | -18.3 | +0.8 |
| | [3.3] | [2.4] | [2.4] | [6.2] | [0.1] |
| *N* | 29,116 | 7,812 | 10,080 | 11,224 | 269,422 |
| *Renters & new homebuyers* | | | | | |
| Housing costs | -9.7 | -15.7 | -4.9 | +1.2 | -6.6 |
| | [0.5] | [0.4] | [0.6] | [1.2] | [0.1] |
| Welfare | -13.5 | +4.2 | -31.2 | -26.7 | +3.9 |
| | [2.4] | [2.5] | [4.3] | [13.8] | [0.2] |
| *N* | 10,926 | 5,046 | 4,272 | 1,608 | 129,880 |
| *Incumbent homeowners* | | | | | |
| Housing costs | +4.1 | -8.3 | -2.0 | +9.3 | -2.2 |
| | [0.5] | [1.1] | [0.5] | [0.6] | [0.1] |
| Capital gains | +2.3 | -6.2 | -1.4 | +5.7 | -1.5 |
| | [0.4] | [0.8] | [0.4] | [0.5] | [0.1] |
| Welfare | -19.5 | -6.8 | -26.5 | -16.9 | -2.1 |
| | [3.7] | [6.1] | [5.0] | [6.4] | [0.2] |
| *N* | 16,540 | 1,116 | 5,808 | 9,616 | 117,581 |
| *Public housing residents* | | | | | |
| Welfare | -6.2 | -6.2 | – | – | -0.9 |
| | [8.4] | [8.4] | | | [0.3] |
| *N* | 1,650 | 1,650 | 0 | 0 | 4,061 |

*Notes.* This table reports on the welfare effects of neighborhood assignment for households residing in Seattle in 2008. All effects are reported in dollars per month. Incumbent homeowners are households identified as owning homes in 2003. We compute capital gains by converting changes in home prices to monthly flows using expected time to sale and a discount rate of 3%, as described in the text. Public housing residents refers to households living in project-based public housing managed by the Seattle Housing Authority (SHA) in 2010. The count of non-enrollment households living in public housing is based on subtracting the count of enrollment-age households in public housing from the total number of SHA units available in 2010.

Table 9: Effects on Segregation and School Quality

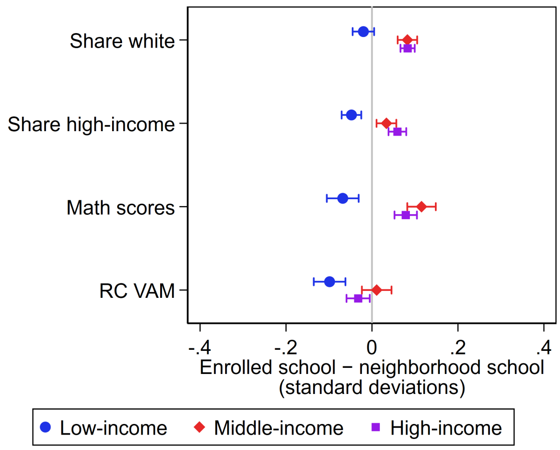
| | Choice | Neighborhood Schools | | | |
|---|---|---|---|---|---|
| | Observed (2008) (1) | Observed (2010) (2) | Observed (2012) (3) | Observed (2014) (4) | Model Prediction (2008) (5) |
| *Residential Segregation* | | | | | |
| Low-income Variance Ratio | 0.23 | +0.02 | +0.01 | +0.00 | +0.03 [0.01] |
| Low-income Isolation | 0.12 | +0.12 | -0.01 | +0.06 | +0.10 [0.03] |
| Minority Variance Ratio | 0.27 | -0.03 | -0.07 | -0.04 | -0.00 [0.01] |
| Minority Isolation | 0.21 | -0.11 | -0.14 | -0.11 | -0.04 [0.03] |
| *School Segregation* | | | | | |
| Low-income Variance Ratio | 0.23 | +0.01 | +0.01 | +0.02 | +0.03 [0.01] |
| Low-income Isolation | 0.20 | +0.05 | -0.02 | +0.02 | +0.04 [0.02] |
| Minority Variance Ratio | 0.26 | -0.02 | -0.05 | -0.01 | -0.01 [0.01] |
| Minority Isolation | 0.19 | -0.07 | -0.10 | -0.01 | -0.07 [0.03] |
| *Low-income* | | | | | |
| Enrolled math scores | -0.28 | +0.05 | +0.08 | +0.09 | +0.07 [0.02] |
| Enrolled value-added | -0.03 | +0.03 | +0.04 | +0.04 | +0.02 [0.01] |
| *Middle-income* | | | | | |
| Enrolled math scores | -0.07 | +0.01 | +0.03 | +0.06 | +0.04 [0.02] |
| Enrolled value-added | 0.05 | +0.01 | +0.02 | +0.02 | -0.02 [0.01] |
| *High-income* | | | | | |
| Enrolled math scores | 0.24 | -0.01 | -0.02 | -0.02 | -0.01 [0.01] |
| Enrolled value-added | 0.18 | +0.00 | -0.01 | -0.01 | +0.01 [0.00] |

*Notes.* This table reports observed and predicted levels of school and residential segregation in the district, as well as average quality of schools attended by each income group. Column (1) reports on the observed sample of enrolled kindergarten students in 2008 under district wide choice. The numbers in the remaining columns indicate changes relative to this baseline. Columns (2) - (4) report on kindergarten entering classes of 2010, 2012 and 2014, in that order. Column (5) reports the changes in each measure predicted by counterfactual neighborhood assignment using data on 2008 kindergarten households.

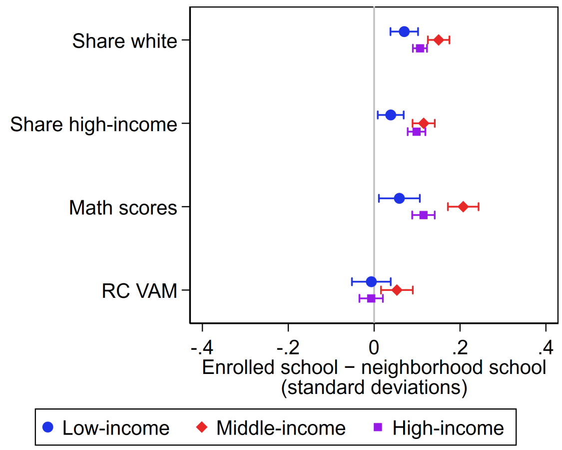
## Appendix A. Additional Exhibits

Figure A.1: Enrolled vs Neighborhood School Quality Under Choice (2006-2008)

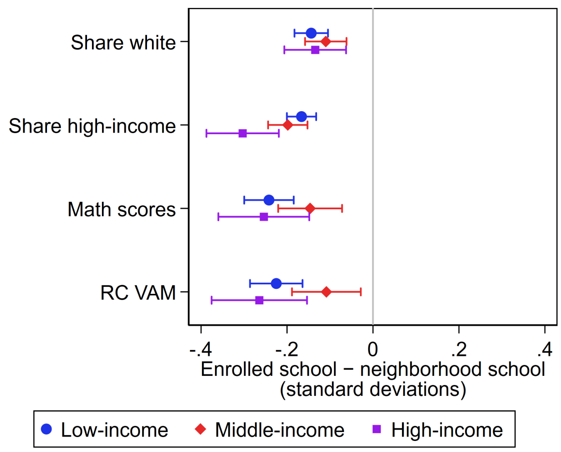
(a) All enrollees




(b) Applicants



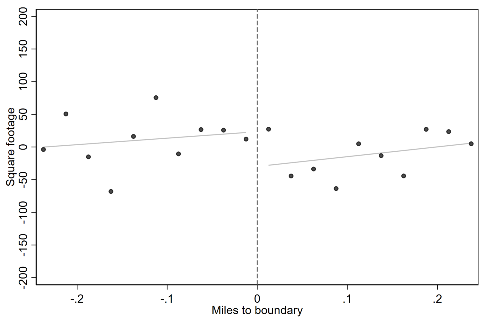

(c) Non-applicants



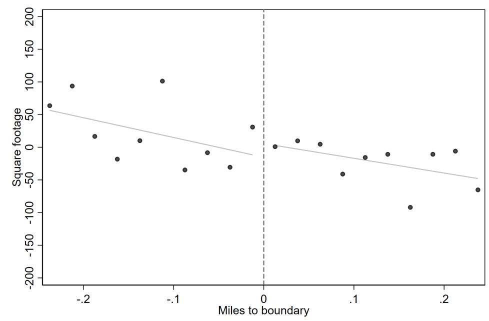

*Notes:* This figure plots average differences in the characteristics of enrolled and zoned schools, by student income group. All school characteristics normalized to unit standard deviation across schools. RC VAM refers to causal value-added on 4th grade standardized math exams, estimated using the risk-controlled value-added model introduced in Angrist et al. (2020).

Figure A.3: Characteristics of Homes Sold Near Boundaries: 2006-2008

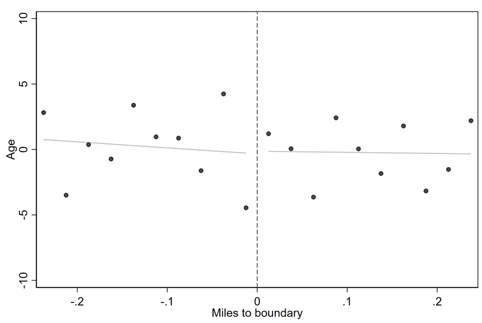
(a) Square footage, by school math scores



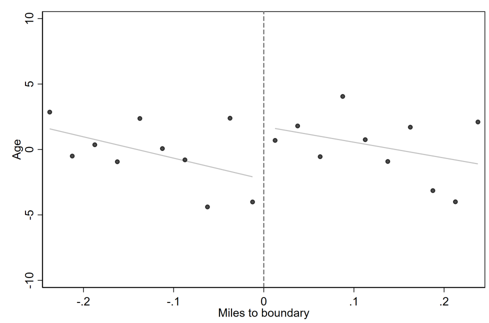

(b) Square footage, by school share white



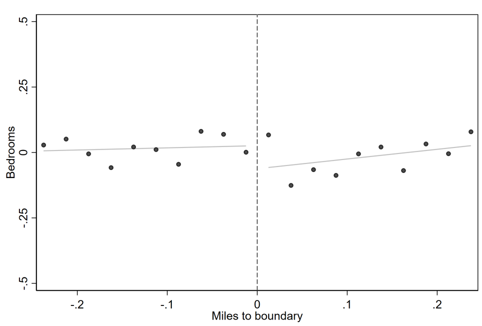

(c) Age, by school math scores



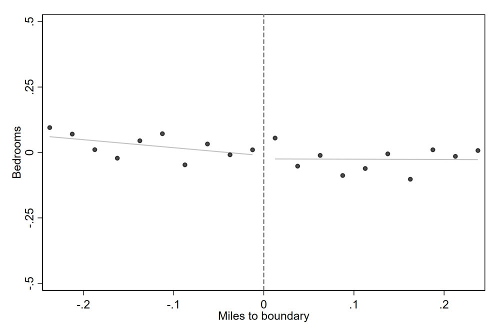

(d) Age, by school share white




(e) Number of bedrooms, by school math scores



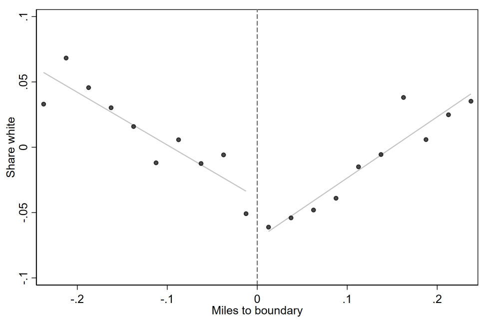

(f) Number of bedrooms, by school share white



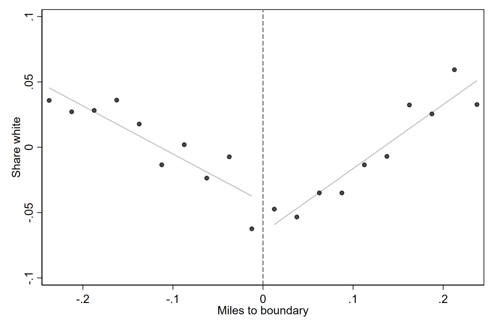

*Notes:* This figure shows physical characteristics of homes sold near the location of eventually established elementary school attendance boundaries, using transactions from 2006–2008. The panels in the left column organize boundaries by baseline math scores of the zoned neighborhood schools, with higher performing sides assigned positive distance. The panels in the right column organize boundaries by school share white, with higher share white sides assigned positive distance.

Figure A.4: Neighborhood Demographics Near Boundaries: 2005-2009 ACS

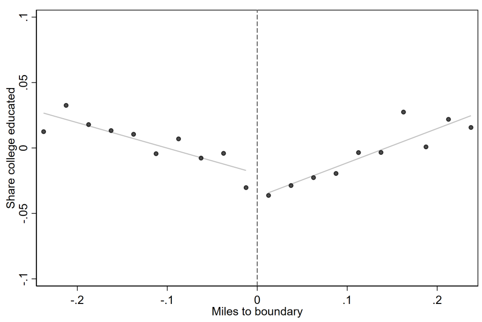
(a) Share white, by school math scores



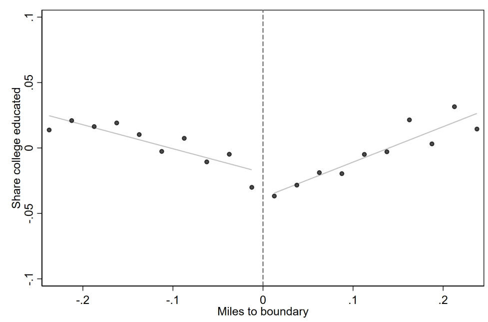

(b) Share white, by school share white




(c) Share college educated, by school math scores




(d) Share college educated, by school share white




*Notes:* This figure shows neighborhood demographics near the location of eventually established elementary school attendance boundaries, using block-group level estimates from the 2005-2009 American Community Survey. The panels in the left column organize boundaries by baseline math scores of the zoned neighborhood schools, with higher performing sides assigned positive distance. The panels in the right column organize boundaries by school share white, with higher share white sides assigned positive distance.

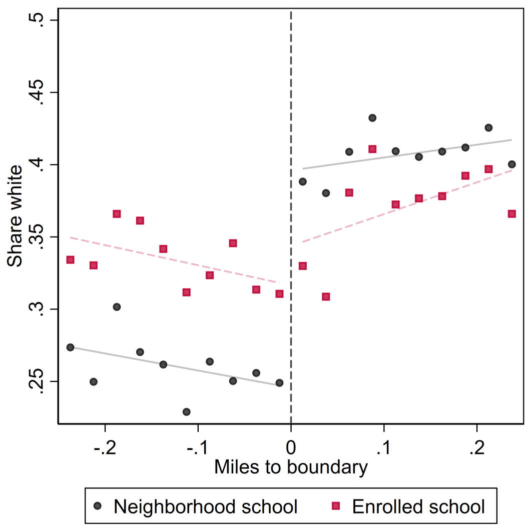
Table A.1: Changes in Characteristics of Sold Homes

| | Share white (1) | Share low-income (2) | Math scores (3) | Value-added (4) |
|---|---|---|---|---|
| | *Panel A. Square footage* | | | |
| Square footage (2006–2008) | 22.282 | -11.409 | -26.937 | -25.229 |
| | (54.957) | (39.193) | (27.241) | (28.745) |
| Sqft × Post (2010–2012) | 5.695 | -26.539 | 76.124** | 58.975 |
| | (48.112) | (37.415) | (27.769) | (34.047) |
| Sqft × Post (2013–2015) | -52.600 | 70.462* | -28.248 | -40.744 |
| | (30.466) | (29.200) | (23.099) | (28.058) |
| Boundaries | 63 | 63 | 63 | 63 |
| *N* | 29,916 | 29,916 | 29,916 | 29,474 |
| | *Panel B. Age* | | | |
| Age (2006–2008) | 5.333 | 0.650 | -1.242 | -3.901 |
| | (3.418) | (2.935) | (1.842) | (2.090) |
| Age × Post (2010–2012) | -4.497 | 4.003 | -3.335 | 0.350 |
| | (2.270) | (2.638) | (2.032) | (2.476) |
| Age × Post (2013–2015) | -5.373* | 2.548 | -2.581 | 0.785 |
| | (2.212) | (2.188) | (1.781) | (1.640) |
| Boundaries | 63 | 63 | 63 | 63 |
| *N* | 29,916 | 29,916 | 29,916 | 29,474 |
| | *Panel C. Bedrooms* | | | |
| Bedrooms (2006–2008) | -0.016 | -0.003 | -0.060 | 0.003 |
| | (0.045) | (0.038) | (0.037) | (0.031) |
| Bedrooms × Post (2010–2012) | -0.009 | 0.018 | 0.069 | -0.013 |
| | (0.066) | (0.048) | (0.041) | (0.039) |
| Bedrooms × Post (2013–2015) | -0.084 | 0.096* | 0.014 | -0.050 |
| | (0.052) | (0.044) | (0.038) | (0.032) |
| Boundaries | 63 | 63 | 63 | 63 |
| *N* | 29,916 | 29,916 | 29,916 | 29,474 |

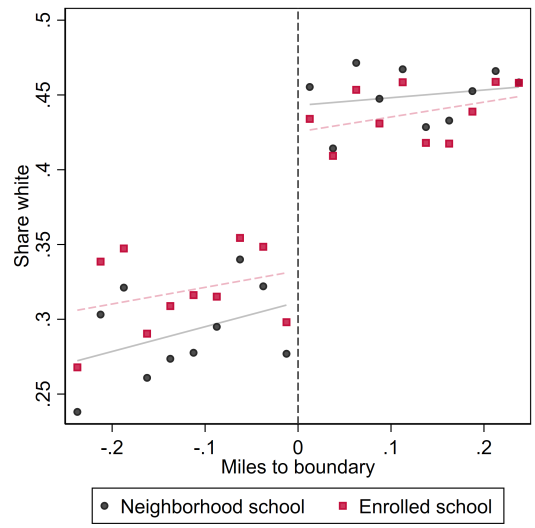
*Notes.* $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. This table reports estimates of discontinuities in physical housing characteristics from models analogous to Equation (1). Characteristics replace price as the dependent variable in these specifications. Standard errors clustered at the attendance area level reported in parenthesis.

Figure A.2: Capitalization of Neighborhood School Demographics

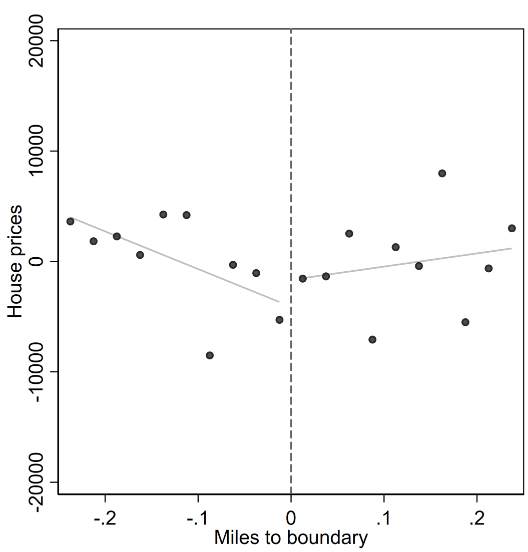
(a) Enrolled school share white: 2006-2008



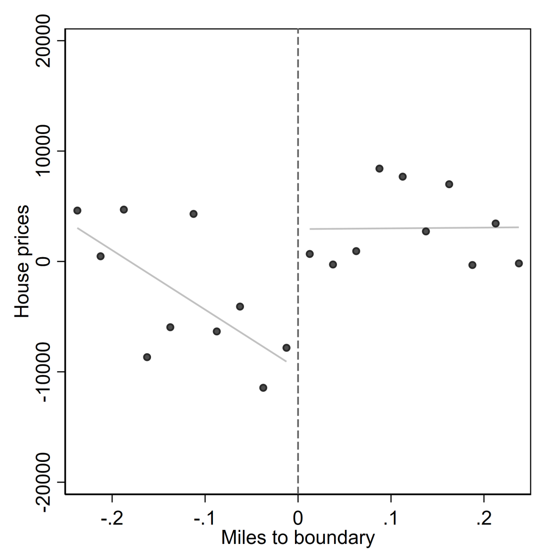

(b) Enrolled school share white: 2013-2015




(c) House prices: 2006-2008




(d) House prices: 2013-2015




*Notes:* This figure plots conditional means of the share white of the enrolled school and house prices, by bins of distance to an attendance area boundary. Boundaries are organized so that the side of the boundary zoned to the school with higher share white is assigned positive distance (on the right side of each panel). Panel (a) plots average share white for enrolled students living within a quarter mile of an (eventual) attendance area boundary in the pre-period. Panel (c) plots binned average house prices, residualized by a vector of house characteristics. Panels (b) and (d) plot the same outcomes in the post-period.

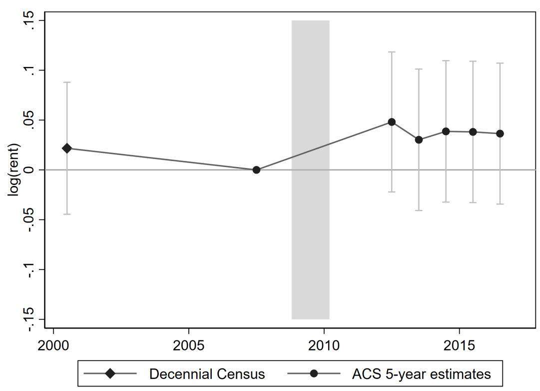
Table A.2: Changes in Number of Homes Sold and New Residential Units

| | Share white (1) | Share low-income (2) | Math scores (3) | Value-added (4) |
|---|---|---|---|---|
| | *Panel A. Number of home sales* | | | |
| log(Sales) (2006–2008) | -0.138** | 0.028 | -0.072* | -0.033 |
| | (0.049) | (0.063) | (0.029) | (0.046) |
| log(Sales) × Post (2010–2012) | -0.003 | 0.030 | -0.017 | -0.010 |
| | (0.036) | (0.034) | (0.020) | (0.025) |
| log(Sales) × Post (2013–2015) | 0.088* | -0.051 | 0.054 | -0.002 |
| | (0.041) | (0.048) | (0.034) | (0.029) |
| *N* | 29,922 | 29,922 | 29,922 | 29,481 |
| | *Panel B. Newly Permitted Residential Units* | | | |
| log(Units) (2006–2008) | -0.150 | 0.138 | -0.043 | -0.083 |
| | (0.153) | (0.118) | (0.101) | (0.097) |
| log(Units) × Post (2010–2012) | 0.063 | 0.000 | -0.269 | -0.201 |
| | (0.226) | (0.220) | (0.168) | (0.161) |
| log(Units) × Post (2013–2015) | 0.043 | 0.087 | -0.165 | 0.122 |
| | (0.273) | (0.198) | (0.146) | (0.125) |
| *N* | 20,007 | 20,007 | 20,007 | 19,908 |

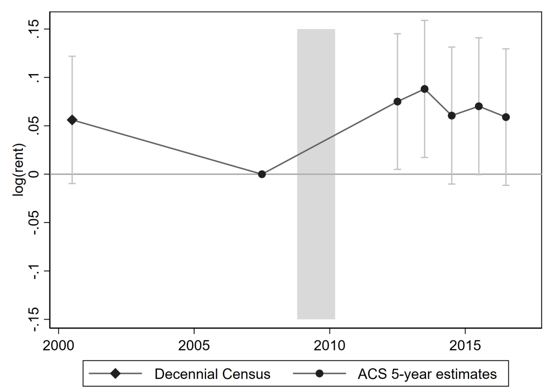
*Notes.* $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. This table reports estimates of discontinuities in the log number of sales (Panel A) and log number of newly permitted residential units (Panel B) using the specification described by Equation (3). Each coefficient derives from an individual regression of log counts on school characteristics, boundary fixed effects, and controls for neighborhood characteristics. The bandwidth is a quarter mile on each side of the boundary. Standard errors clustered at the attendance area level in parentheses. Value-added refers to causal effects on 4th grade math scores estimated using the risk-controlled approach of Angrist et al. (2020).

Figure A.5: Rents across Attendance Areas

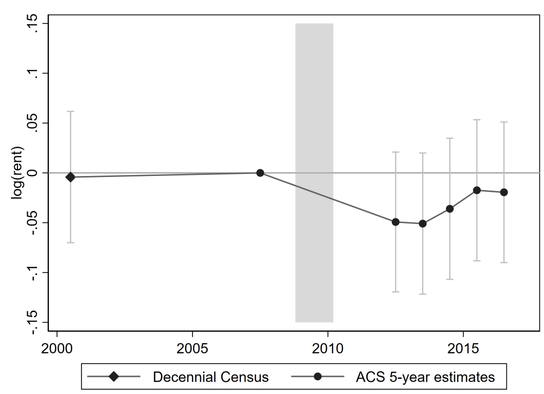
(a) Above-median school share white




(b) Above-median school math scores



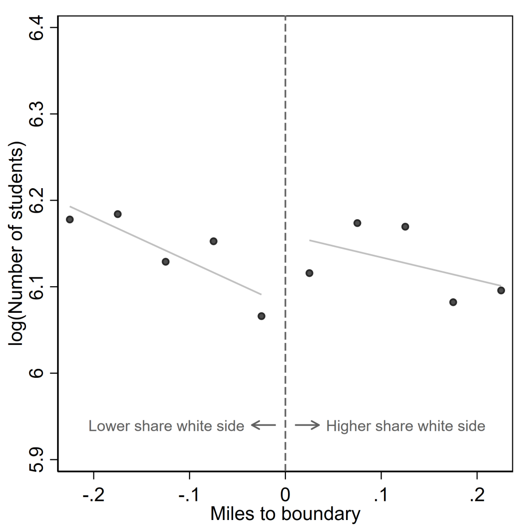

(c) Below-median residential income



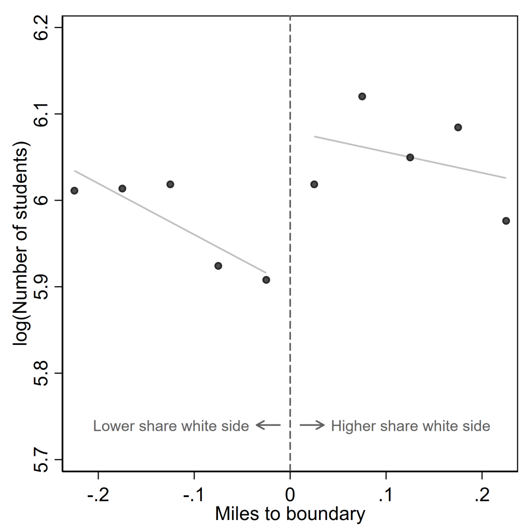

*Notes:* These figures plot estimates from event study specifications comparing the evolution of rents across attendance areas. Panel (a) compares attendance areas zoned to schools with above-median share white at baseline to other attendance areas. The treated group in Panel (b) consists of attendance areas zoned to schools with above-median 4th grade math scores. Panel (c) compares attendance areas with below-median residential income (based on 2005-2009 ACS estimates) to areas with above-median baseline income. Rent data come from the 2000 decennial census and subsequent ACS 5-year estimates (2005-2009, 2010-2014, 2011-2015, 2012-2016, 2013-2017, and 2014-2018). Due to the overlapping nature of ACS 5-year estimates, post-2010 observations are not independent.

Figure A.6: Residential Sorting by School Share White

(c) By neighborhood school share white: 2006-2008




(d) By neighborhood school share white: 2013-2015




*Notes:* This figure shows counts of enrolled students living near attendance boundaries. Boundaries are organized so that the side of the boundary zoned to the school with higher baseline share white is assigned positive distance (on the right side of each panel). Panel (a) plots counts of enrolled students in distance bins prior to the establishment of boundaries between 2006–2008. Panel (b) plots counts of enrolled students under neighborhood assignment between 2013–2015.

Table A.3: Sorting Estimation Robustness Exercises

| | Math scores | | | Share white | | |
|---|---|---|---|---|---|---|
| | Demographic Controls (1) | No Middle School Boundaries (2) | Bandwidth .125 miles (3) | Demographic Controls (4) | No Middle School Boundaries (5) | Bandwidth .125 miles (6) |
| All | 0.097* | 0.098* | 0.083* | 0.252*** | 0.287*** | 0.236** |
| | (0.046) | (0.039) | (0.041) | (0.061) | (0.063) | (0.082) |
| *N* | 18,069 | 16,830 | 9,804 | 18,069 | 16,830 | 9,804 |
| Low-income | 0.114* | 0.127** | 0.216* | 0.160*** | 0.130** | 0.231 |
| | (0.045) | (0.045) | (0.092) | (0.038) | (0.040) | (0.148) |
| *N* | 5,235 | 4,890 | 2,981 | 5,235 | 4,890 | 2,981 |
| Middle-income | 0.072 | 0.063 | -0.005 | 0.184** | 0.215* | 0.123 |
| | (0.049) | (0.048) | (0.073) | (0.069) | (0.084) | (0.122) |
| *N* | 6,465 | 6,190 | 2,852 | 6,465 | 6,190 | 2,852 |
| High-income | -0.025 | -0.033 | -0.168 | 0.219** | 0.263** | 0.309* |
| | (0.070) | (0.047) | (0.106) | (0.076) | (0.086) | (0.144) |
| *N* | 5,300 | 4,755 | 2,333 | 5,300 | 4,755 | 2,333 |

*Notes.* * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This table reports estimates of household sorting effects from three variants of our main specification. The first three columns examine sorting on school math scores, while the last three columns examine sorting on school share white. Columns (1) and (4) include controls for block-group level neighborhood demographics from ACS 5-year estimates. Columns (2) and (5) drop the set of elementary school boundaries coinciding with middle school attendance areas. Columns (3) and (6) use a bandwidth half that of the main estimates (corresponding to one-eighth of a mile).

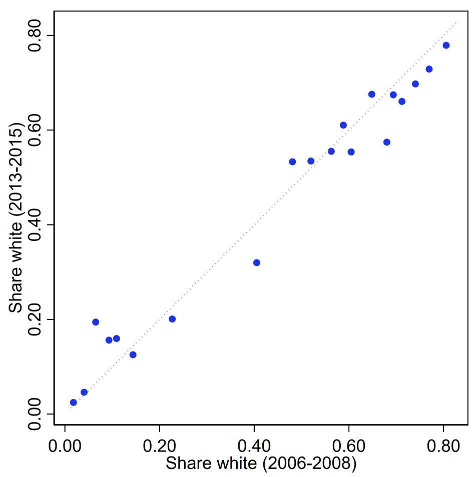
Table A.4: Sorting Estimates for Other Demographic Groups

| | All (1) | White (2) | Minority (3) | Renters (4) | Owners (5) |
|---|---|---|---|---|---|
| | | | *Panel A. Math scores* | | |
| Math scores | -0.038 | 0.020 | -0.096 | -0.099$^{*}$ | 0.024 |
| | (0.032) | (0.045) | (0.053) | (0.040) | (0.035) |
| Math scores × Post (2010–2012) | 0.079$^{*}$ | -0.038 | 0.097 | 0.037 | -0.013 |
| | (0.030) | (0.039) | (0.060) | (0.065) | (0.035) |
| Math scores × Post (2013–2015) | 0.092$^{*}$ | -0.086$^{*}$ | 0.183$^{*}$ | 0.193$^{**}$ | -0.036 |
| | (0.036) | (0.041) | (0.075) | (0.058) | (0.036) |
| *N* | 19,884 | 10,862 | 5,899 | 6,258 | 12,526 |
| | | | *Panel B. Share white* | | |
| Share white | -0.049 | 0.019 | -0.109$^{*}$ | -0.109 | -0.024 |
| | (0.055) | (0.085) | (0.051) | (0.080) | (0.063) |
| Share white × Post (2010–2012) | 0.114$^{*}$ | 0.182$^{*}$ | 0.103 | 0.172$^{*}$ | 0.076 |
| | (0.057) | (0.083) | (0.079) | (0.079) | (0.053) |
| Share white × Post (2013–2015) | 0.210$^{***}$ | 0.307$^{***}$ | 0.143 | 0.237$^{**}$ | 0.205$^{***}$ |
| | (0.058) | (0.080) | (0.126) | (0.079) | (0.053) |
| *N* | 19,884 | 10,862 | 5,899 | 6,258 | 12,526 |

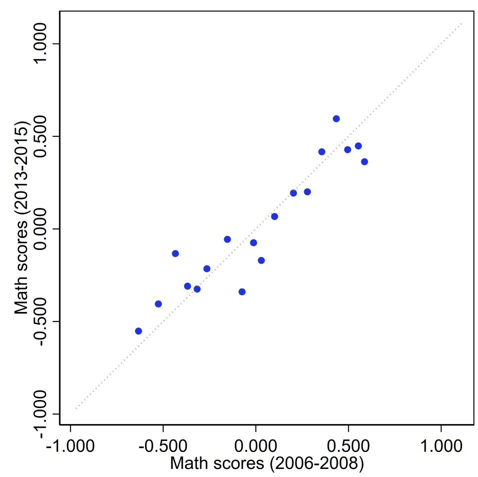
*Notes.* $^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$. This table reports coefficients from Equation (3) corresponding to the semi-elasticity of the residential choices of enrolled households with respect to differences in neighborhood school characteristics across attendance boundaries. Each coefficient derives from an individual regression of log counts on school characteristics, boundary fixed effects, and controls for neighborhood characteristics. The bandwidth is a quarter mile on each side of the boundary. The sample in each specification consists of the set of boundaries with at least five observations on either side. Standard errors clustered at the attendance area level reported in parenthesis.

Figure A.7: Stability of School Characteristics

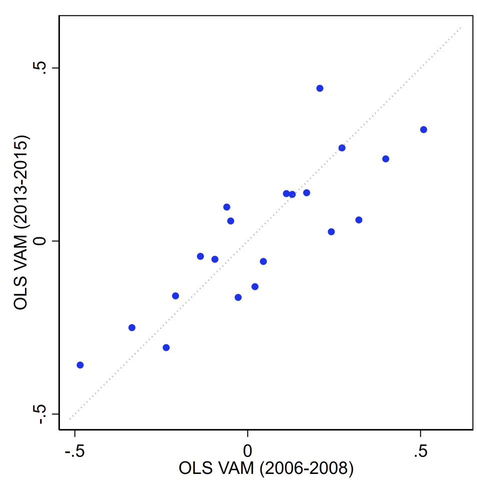
(a) Share white




(b) Math scores

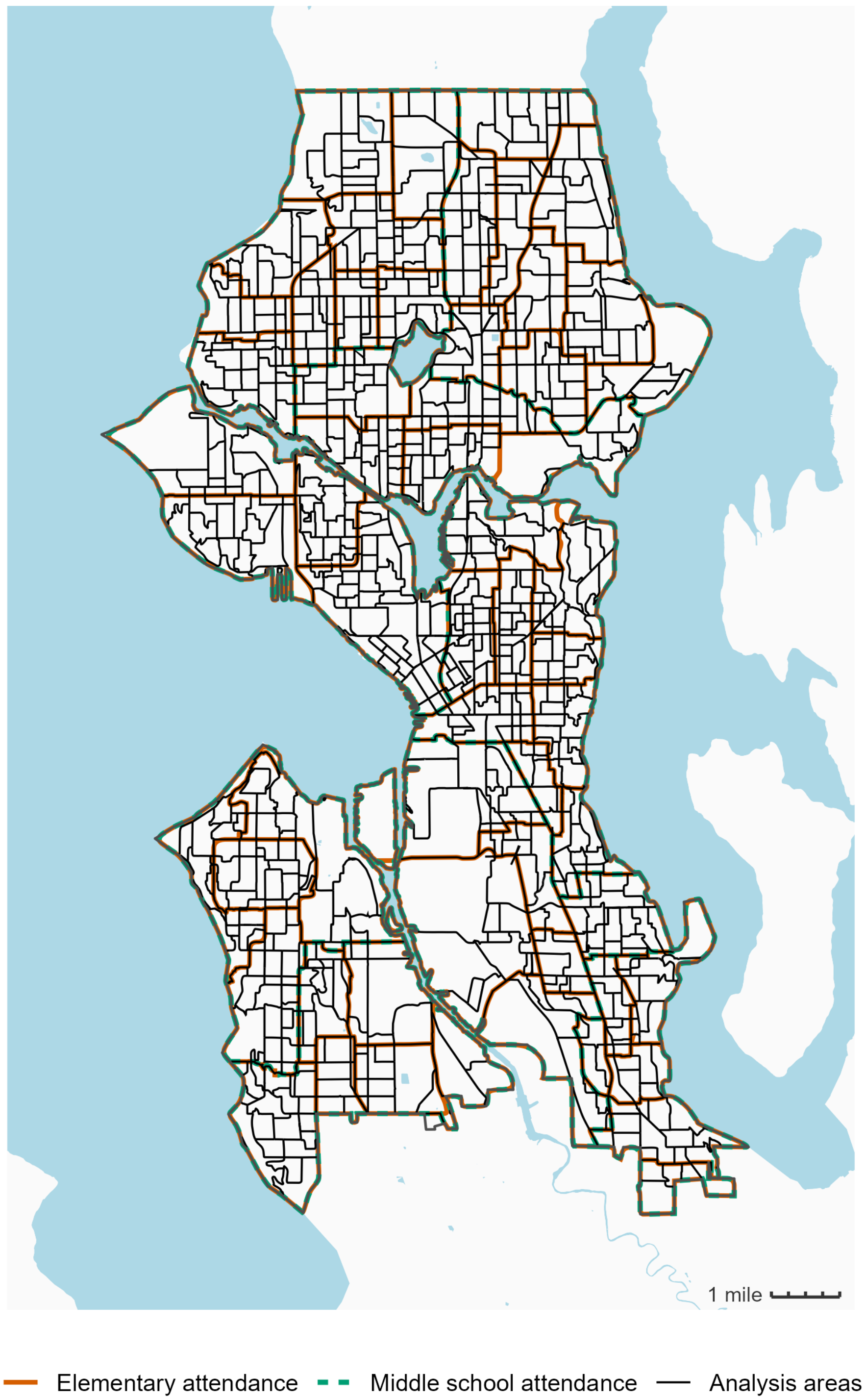



(c) Conventional value-added




*Notes:* This figure plots elementary school characteristics under neighborhood assignment (2013-2015) and district-wide choice (2006-2008). Panel (a) plots school enrollment share white. Panel (b) shows changes in average math scores. Panel (c) plots value-added estimates which control for student demographics and residential property value. Unlike our main value-added specification, these estimates do not include assignment risk controls, since few students face assignment risk in the post-period. See Section D.6 for additional detail on our value-added estimation procedure and validation of each specification.

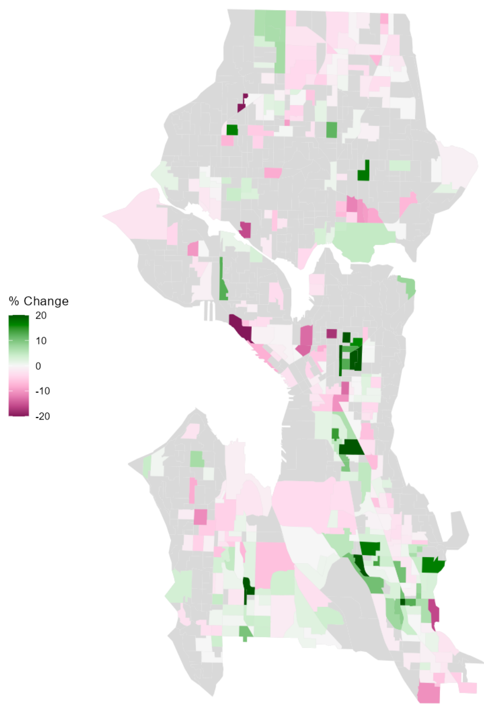
Figure A.8: Attendance Areas and Neighborhoods



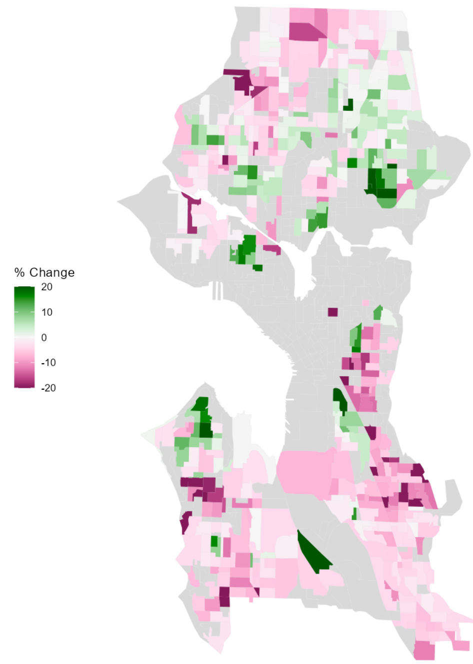

*Notes:* This figure shows elementary and middle school attendance areas in Seattle introduced by the reform in 2010. Outlines of the neighborhood definitions we constructed are overlaid in black.

Figure A.9: Equilibrium Effects on Locations for Enrollment-Age Households

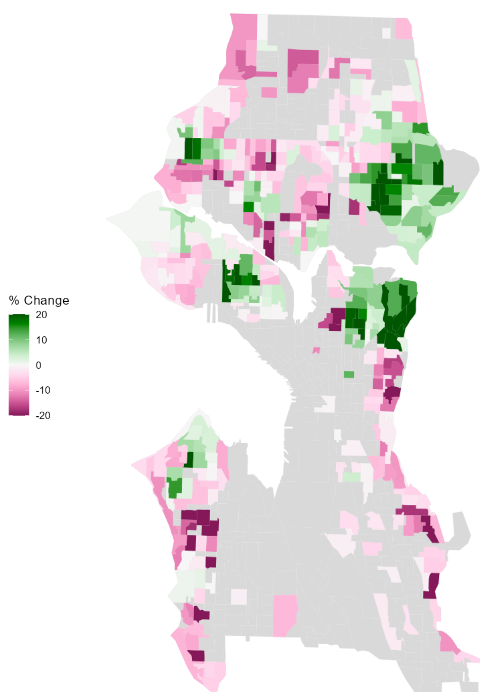
(a) Low-income




(b) Middle-income

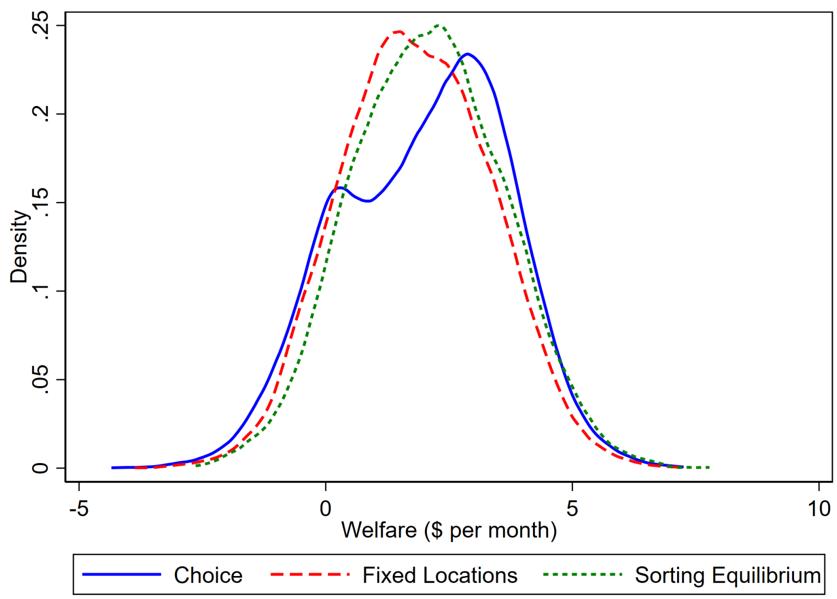



(c) High-income




*Notes:* This figure plots changes in the density of enrollment-age households across neighborhoods under counterfactual neighborhood assignment for each of the three income groups.

Figure A.10: Distribution of Welfare for Low-income Households




*Notes:* This figure plots the distribution of student welfare (excluding non schooling amenities and idiosyncratic neighborhood preferences) for the sample of low-income 2008 kindergarteners under (1) district-wide choice, (2) a model holding household locations fixed, and (3) the full model allowing for residential adjustment.

Table A.5: Welfare Impacts of Neighborhood Assignment

| | District-wide Choice (Observed) (1) | Neighborhood Guarantee: Fixed Locations (2) | Neighborhood Guarantee: Sorting Equilibrium (3) | Guarantee + Less Busing: Sorting Equilibrium (4) |
|---|---|---|---|---|
| Welfare | | -31.15 [2.47] | -15.62 [2.42] | -15.54 [2.56] |
| Rent | 1827.19 | - | -3.89 [.86] | -4.28 [1.16] |
| Enrollment Rate | 0.67 | -0.01 [0.00] | -0.03 [0.01] | -0.03 [0.01] |
| Application Rate | 0.61 | -0.43 [0.00] | -0.43 [0.00] | -0.43 [0.00] |
| *Low-income* | | | | |
| Welfare | | -15.79 [2.58] | +2.45 [2.51] | +2.79 [2.87] |
| Rent | 1346.58 | - | -12.74 [3.60] | -12.68 [4.51] |
| *Middle-income* | | | | |
| Welfare | | -42.73 [2.65] | -27.19 [2.26] | -26.60 [6.01] |
| Rent | 1639.62 | - | -4.91 [3.56] | -4.99 [1.74] |
| *High-income* | | | | |
| Welfare | | -34.93 [6.82] | -22.12 [6.78] | -22.82 [7.04] |
| Rent | 2295.36 | - | +8.80 [3.02] | +8.72 [3.76] |

*Notes.* This table reports on welfare and allocations of the 2008 kindergarten cohort under simulated neighborhood assignment. Column (1) reports observed levels of rents, enrollment rates, and application rates in 2008. Column (2) reports on changes in each variable under an assignment scheme guaranteeing enrollment at neighborhood schools, but holding household locations fixed. Column (3) reports on a full sorting equilibrium. Column (4) additionally implements the reduction in busing implemented by SPS in 2010. Standard deviations across simulation draws reported in brackets.

## Appendix B. Computation

This section provides details on the MCMC procedure used to estimate the school choice stage of the model. Recall that utility associated with enrolling in school $j \in \{1, \ldots, J\}$ is

$$u_{ijn} = \delta_{gj} + \sum_{\ell,k} \beta^{\ell,k} x_i^\ell w_j^k + \sum_k \lambda_i^k w_j^k + \mathbf{d}'_{jn}\tau_g + \varepsilon_{ij}$$

with the following parameterizations:

$$\delta_{gj} \sim \mathcal{N}\left(\bar{\delta}_g, \sigma^2_{\delta,g}\right), \quad \lambda_i \sim \mathcal{N}(0, \Sigma_\lambda), \quad \zeta_{in} \sim \mathcal{N}(0, \sigma^2_{\zeta,g}), \quad c_i \sim \mathcal{N}(\bar{c}_g, \sigma^2_{c,g})$$

We specify priors for the variance components, and the elements of $\beta$, $\tau$ and $\delta$ as

$$\begin{gathered}\beta_k \sim \mathcal{N}\left(0, \bar{\sigma}^2_\beta\right), \quad \tau_{g,k} \sim \mathcal{N}\left(0, \bar{\sigma}^2_\tau\right), \quad \bar{\delta}_k \sim \mathcal{N}\left(0, \bar{\sigma}^2_\delta\right), \quad \bar{c}_g \sim \mathcal{N}\left(0, \bar{\sigma}^2_c\right), \\ \Sigma_\lambda \sim \text{IW}\left(\bar{\Sigma}_\lambda, \nu_\lambda\right), \quad \sigma^2_{\zeta,g} \sim \text{IW}\left(\bar{\sigma}^2_\zeta, \nu_\zeta\right), \quad \sigma^2_{\delta,g} \sim \text{IW}\left(\tilde{\sigma}^2_\delta, \nu_\delta\right), \quad \text{and} \quad \sigma^2_{c,g} \sim \text{IW}\left(\tilde{\sigma}^2_c, \nu_c\right),\end{gathered}$$

where IW is the inverse Wishart distribution. Following Rossi et al. (1996) and Abdulkadiroğlu et al. (2017), we specify diffuse priors as follows:

$$\begin{aligned}\bar{\sigma}^2_\beta = \bar{\sigma}^2_\tau = \bar{\sigma}^2_\delta = \bar{\sigma}^2_c &= 100 \\ \left(\bar{\sigma}^2_\zeta, \nu_\zeta\right) = \left(\tilde{\sigma}^2_\delta, \nu_\delta\right) = \left(\tilde{\sigma}^2_c, \nu_c\right) &= (1, 2) \\ \left(\bar{\Sigma}_\lambda, \nu_\lambda\right) &= \left(\left(3 + \text{dim}\left(\lambda_i\right)\right) I_{\text{dim}(\lambda_i)}, 3 + \text{dim}\left(\lambda_i\right)\right),\end{aligned}$$

where $I_k$ is the identity matrix of dimension $k$.

### B.1. Constraints

As described in the main text, observed application and enrollment choices imply the following constraints on the school utilities $\mathbf{u}_i$. For rank-order list $\mathbf{r}_i = (r_{i1}, \ldots, r_{iK_i})$:

$$\begin{aligned} u_{ir_{ik}} &> u_{ir_{i,k+1}} \quad \text{for all } k < K_i && \text{(Ranked schools)} \\ u_{ir_{iK_i}} &> u_{i,J+1} && \text{(Administrative placement)} \\ u_{i,J+1} &> u_{ij} \quad j \in \{1, \ldots, J\} \setminus \{r_{i1}, \ldots, r_{iK_i}\} && \text{(Unranked schools)} \end{aligned}$$

where the dependence on $n$ is suppressed. With an offer to school $j$ in hand, the utility of enrollees satisfy

$$u_{ij} > \varepsilon_{i0} + \zeta_i \qquad \text{(Enrollment)}$$

with the reverse holding for students that decline to enroll. Finally, the application decision imposes that, for applicants,

$$\mathrm{E}\left[\max_{j\in\mathcal{J}_i}\bar{u}_{ij}\right]-c_i>\bar{u}_{i,J+1} \qquad \text{(Application)}$$

where $\bar{u}_{ij}$ is the inclusive value of the enrollment stage given in (5). The reverse condition is true for non-applicants.

### B.2. Sampling procedure

The sampling procedure iterates through the following steps. Let the vector $\mathbf{u}_i = (u_{i1},\ldots,u_{iJ},u_{i,J+1},u_{i0})$ collect the school utilities for individual $i$. In each iteration $s$,

1. Draw $\mathbf{u}_i^{s+1}$ and $\zeta_i^{s+1}$ conditional on $\zeta_i^s,\sigma_\zeta^s,c_i^s,\delta^s,\beta^s,\tau_{g(i)}^s$ and the constraints above. This involves iterating over the following steps:
   - Propose new values of $\mathbf{u}_i^{s+1}$ by drawing from its posterior distribution given $\zeta_i^s,\sigma_\zeta^s,c_i^s,\delta^s,\beta^s,\tau_{g(i)}^s$, and the enrollment and ranking constraints. This is conducted via a Hamiltonian Monte Carlo procedure described below.
   - Draw $\zeta_i^{s+1}$ conditional on $u_{i0}^{s+1}$ and $\sigma_\zeta^s$
   - Accept the proposed utilities for individuals satisfying the application constraint. Repeat these steps for remaining students until everyone passes, or 100 attempts have been made
2. Draw $\beta^{s+1}$ from its posterior distribution conditional on $\mathbf{u}^{s+1},\lambda^s,\tau^s,\delta^s$
3. Draw $\delta^{s+1}$ and $\bar{\delta}_g^{s+1}$ from their posterior distributions conditional on $\mathbf{u}^{s+1},\lambda^s,\tau^s,\beta^{s+1},\bar{\delta}_g^s,\sigma_\delta^s$
4. Draw $\tau^{s+1}$ from its posterior distribution conditional on $\mathbf{u}^{s+1},\lambda^s,\beta^{s+1},\delta^{s+1}$
5. Draw $c_i^{s+1}$ and $\bar{c}_g^{s+1}$ from their posterior distributions conditional on $\mathbf{u}^{s+1},\zeta^{s+1},\sigma_\zeta^s,\bar{c}_g^s,\sigma_{c,g}^s$
6. Draw $\lambda_i^{s+1}$ from its posterior distribution conditional on $\mathbf{u}_i^{s+1},\delta^{s+1},\beta^{s+1},\tau^{s+1},\Sigma_\lambda^s$
7. Draw $\sigma_\zeta^{s+1}$ from its posterior distribution conditional on $\zeta^{s+1}$
8. Draw $\sigma_\delta^{s+1}$ from its posterior distribution conditional on $\delta^{s+1}$ and $\bar{\delta}_g^{s+1}$
9. Draw $\sigma_c^{s+1}$ from its posterior distribution conditional on $c_i^{s+1}$ and $\bar{c}_g^{s+1}$
10. Draw $\Sigma_\lambda^{s+1}$ from its posterior distribution conditional on $\lambda^{s+1}$

Each of the steps above are standard Gibbs-sampler updates with the exception of step 1. We draw the vector school utilities for each student $\mathbf{u}_i^{s+1}$ using a procedure based on a Hamiltonian Monte Carlo (HMC) sampling step which allows us to target the joint posterior distribution of these utilities conditional on the parameters, latent variables, and constraints. Specifically, the distribution of school utilities conditional on the full data is a multivariate normal distribution truncated by ranking and enrollment constraints which are linear in the elements of $\mathbf{u}_i^{s+1}$, and the non-linear application constraint.

To construct proposals consistent with all constraints except the application constraint, we adopt the procedure for efficient sampling from multivariate normal distributions truncated by linear constraints via HMC introduced by Pakman and Paninski (2014). We start by initializing the procedure at a starting position that is consistent with the linear constraints implied by the ranking and enrollment choices of each student. We construct this initial point by assigning a value of 1 to the school ranked first by each applicant, and values decreasing in increments of .05 to each subsequent ranked school. We assign the utility of 0 to the administrative option and of -.1 to all unranked schools. Finally, we assign the value -.2 to the out-of-district school if a student enrolls in the district, and the value 1.1 if a student does not enroll.

As is standard, the procedure casts the log of the target probability distribution as minus the potential energy of a particle. The initial position of the particle is given by the current guess of utilities $\mathbf{u}_i^s$, while its initial velocity is sampled from a normal distribution $\mathcal{N}(0, I_{J+2})$. The proposal $\mathbf{u}_i^s$ is given by the particle's location after a fixed time $T$, which is solved for by integrating the Hamiltonian equations of motion. The linear constraints are accommodated by treating them as "walls," against which the particle bounces off elastically. In the special case when the target distribution is a multivariate normal distribution truncated by linear constraints, the Hamiltonian equations of motion can be integrated exactly. This implies that proposals constructed this way are always feasible, improving run-time and reducing the need to tune an acceptance step as in typical HMC applications.

We write the $J + 1$ restrictions implied by the reported rank-order list and the enrollment decision in matrix form as

$$\Gamma_i \, \mathbf{u}_i \geq \mathbf{0}$$

where each row of the matrix $\Gamma_i \in \mathbb{R}^{(J+1)\times(J+2)}$ corresponds to a single linear constraint. Letting $e_j$ denote the standard basis vector of dimension $J + 2$, the first $K_i - 1$ rows impose the ordering constraints between ranked schools and take the form $\left(e_{r_{ik}} - e_{r_{i,k+1}}\right)'$ for $k < K_i$. The constraint that ranked schools are preferred to administrative placement corresponds to a row of the form $(e_{r_{iK_i}} - e_{J+1})'$. The next $J - K_i$ rows impose that administrative assignment is preferred to unranked schools and take the form $(e_{J+1} - e_j)'$ for each $j \notin \{r_{i1}, \ldots, r_{iK_i}\}$. Finally, letting $z$ denote the offered school, the enrollment constraint is a single row taking the form $(e_z - e_{J+2})'$ if the student enrolls and $(e_{J+2} - e_z)'$ if the student does not.

To improve numerical stability, we conduct the sampling in whitened coordinates so that the target becomes a truncated (multivariate) standard normal. Specifically, we define

$$\tilde{\mathbf{u}} \equiv L_i^{-1}(\mathbf{u}_i - \mu_i), \qquad \tilde{\Gamma}_i \equiv \Gamma_i L_i,$$

where $L_i$ is the Cholesky factor of $\Sigma_i$.

Pakman and Paninski (2014) show that the Hamiltonian dynamics admit a closed-form trajectory. We reproduce the relevant results here using our notation. With initial location $\tilde{\mathbf{u}}(0)$ and velocity $\mathbf{v}(0) \sim \mathcal{N}(\mathbf{0}, I)$, the position and velocity of the particle at time $t$ are given by

$$\tilde{\mathbf{u}}(t) = \mathbf{v}(0)\sin t + \tilde{\mathbf{u}}(0)\cos t, \qquad \mathbf{v}(t) = \mathbf{v}(0)\cos t - \tilde{\mathbf{u}}(0)\sin t.$$

These expressions also makes it possible to solve for the time before the particle reaches a constraint. For constraint $\ell$, define the signed distance

$$K_\ell(t) \equiv \tilde{\Gamma}_{i,\ell\cdot}\,\tilde{\mathbf{u}}(t) = \underbrace{\tilde{\Gamma}_{i,\ell\cdot}\mathbf{v}(0)}_{b_{1\ell}}\sin t + \underbrace{\tilde{\Gamma}_{i,\ell\cdot}\tilde{\mathbf{u}}(0)}_{b_{2\ell}}\cos t.$$

This distance can be expressed as $K_\ell(t) = \rho_\ell \cos(t + \kappa_\ell)$, where $\rho_\ell = \sqrt{b_{1\ell}^2 + b_{2\ell}^2}$ and $\kappa_\ell$ satisfies

$$\cos\kappa_\ell = \frac{b_{2\ell}}{\rho_\ell}, \qquad \sin\kappa_\ell = -\frac{b_{1\ell}}{\rho_\ell}.$$

The roots of $K_\ell(t) = 0$ in $[0, 2\pi)$ are then given by

$$t_\ell^{(1)} = \tfrac{\pi}{2} - \kappa_\ell, \qquad t_\ell^{(2)} = \tfrac{3\pi}{2} - \kappa_\ell,$$

with the time to nearest impact given by the smallest positive root across all constraints,

$$t^{\text{hit}} = \min_\ell \{\, t_\ell^{(1)},\, t_\ell^{(2)} :\ t > 0 \,\}.$$

At an impact with constraint $h$, we update the velocity by reflecting it across the plane of the constraint, $\tilde{\Gamma}_{i,h\cdot}\tilde{\mathbf{u}} = 0$:

$$\mathbf{v}^{\text{ref}}(t^{\text{hit}}) = \mathbf{v}(t^{\text{hit}}) - 2\,\frac{\tilde{\Gamma}_{i,h\cdot}\,\mathbf{v}(t^{\text{hit}})\tilde{\Gamma}'_{i,h\cdot}}{\|\tilde{\Gamma}_{i,h\cdot}\|^2}.$$

We continue the same sinusoidal motion from the new initial condition $\big(\tilde{\mathbf{u}}(t^{\text{hit}}), \mathbf{v}^{\text{ref}}(t^{\text{hit}})\big)$, continuing to reflect at constraints until the horizon $T$. Following the recommendation in the paper we set $T = \pi/2$. The proposal $\mathbf{u}_i^\star$ is then obtained by unwhitening via $\mathbf{u}_i^\star = \mu_i + L_i\,\tilde{\mathbf{u}}(T)$.

### B.3. Convergence

We ran the MCMC procedure for 50,000 iterations, discarding the first 25,000 iterations as burn-in. To assess convergence, we ran two parallel chains from dispersed starting values and computed the Potential Scale Reduction Factor (PSRF) for each parameter, following Gelman and Rubin (1992). The PSRF compares the variance between chains to the variance within a chain, with values close to one indicating convergence. Figures B.1 and B.2 show trace plots and report PSRFs for the full set of estimated parameters. We estimate PSRFs very close to one for the majority of parameters, and below 1.1 for all parameters, providing strong indication of convergence to the target distribution.

### B.4. Counterfactual procedure

Our counterfactual exercise simulates school and neighborhood allocations under neighborhood assignment for the set of households enrolling in kindergarten in 2008. We start by drawing latent variables $\mathbf{u}_i, c_i, \lambda_i$, and $\eta_i$ from their full conditional distributions for each kindergarten household, using the sampling procedure described above. We fix remaining model parameters at their posterior means. We generate 1,200 draws in this manner, discarding the first 200 as burn-in.

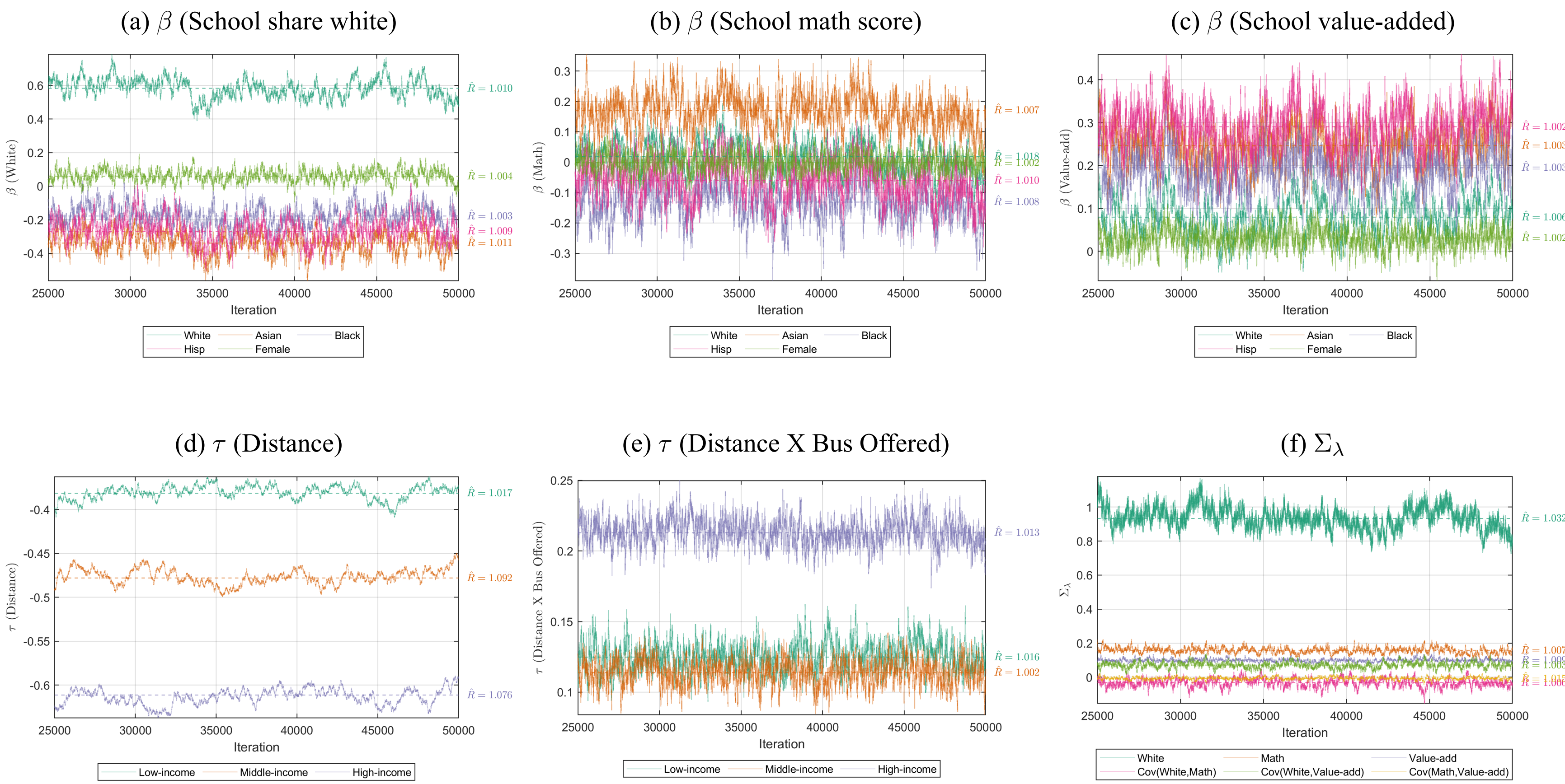
Next, we solve for equilibrium housing prices $\{p_n\}_0^N$ and school admissions cutoffs $\{\kappa_j\}_1^J$ using a nested fixed-point algorithm. The inner loop solves for market-clearing prices by computing analytic neighborhood choice shares integrating over the distribution of latent variables. This requires computing expected school access $S_{in}$ in each neighborhood. Expected school access depends only on income groups and the observable student characteristics in $\mathbf{x}_i$ which include four race dummies and gender. We therefore compute $S_{in}$ for each combination of income group, race, and gender via simulation. We draw latent variables from their unconditional distributions 250 times for each type. For each draw of application costs and preference shocks, we simulate application choices, taking as given current guess of admissions cutoffs. Using simulated rank choice lists, we conduct the DA match and simulate enrollment decisions. We average the resulting utility across iterations to obtain $S_{in}$.

We obtain region-level counts of enrollment- and non-enrollment-age households by income group from the 2005–2009 5-year ACS estimates. Enrollment households are further broken down by the gender and ethnicity of the kindergarten-age child. Using the counts of each type of household in the region, and values of school access, we solve for the prices which clear the housing market in each neighborhood. The outer loop iterates over admissions cutoffs. For each draw of latent variables, we determine which kindergarten households submit applications and how they rank schools, then simulate the DA algorithm. We update cutoffs by averaging the marginal admitted student's rank across draws. We iterate until both prices and cutoffs converge.

We compute expected welfare for each kindergarten household by averaging over school enrollment and neighborhood choice outcomes across latent draws, given the solved equilibrium prices and cutoffs. To net out the effect of model misprediction in our welfare comparisons, we follow this procedure for both the observed district-wide choice regime and the counterfactual neighborhood assignment regime. This allows us to report welfare impacts as the difference in welfare under each equilibrium.

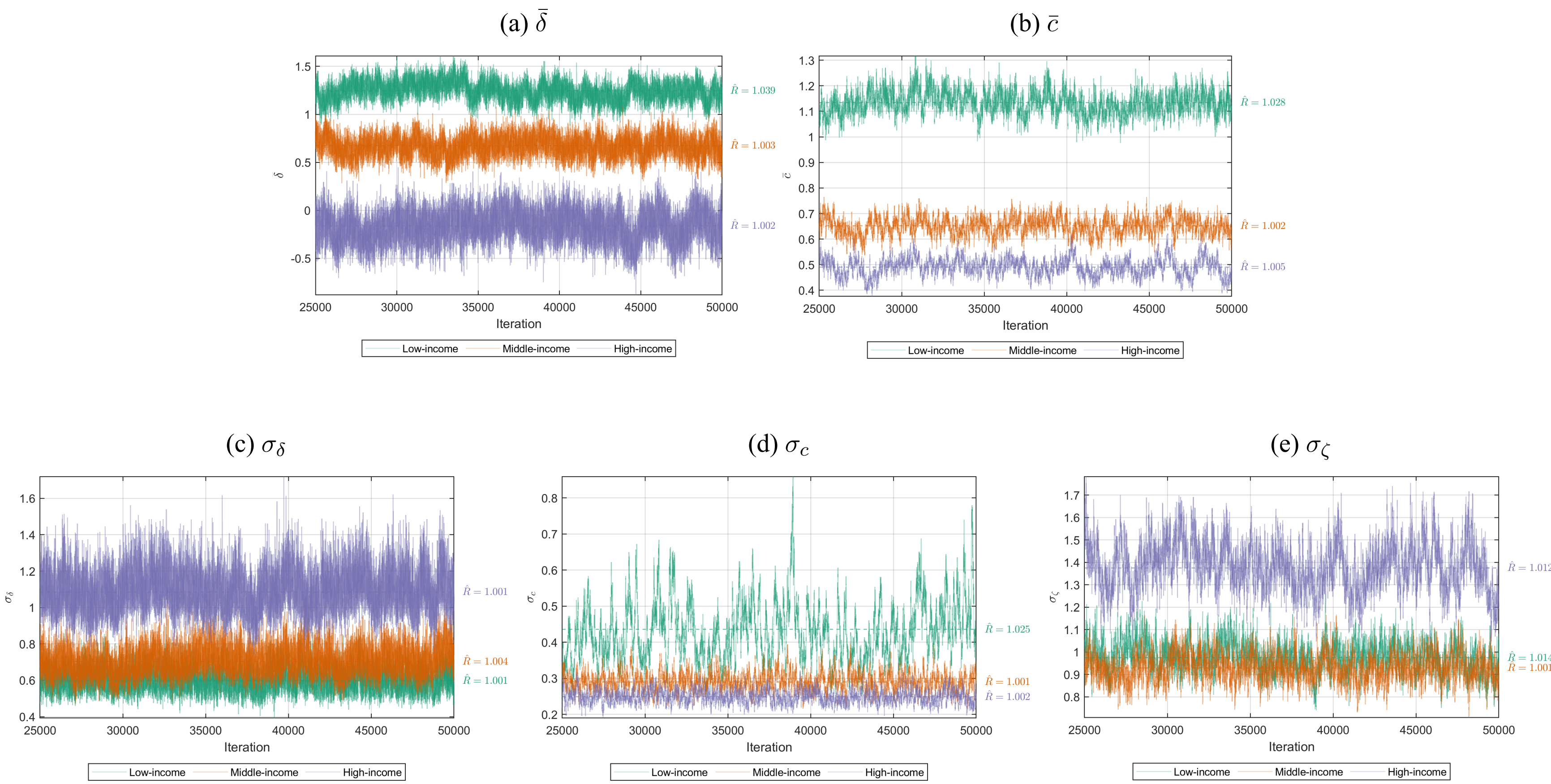
Finally, four new attendance area schools were phased in during the 2010 and 2011 school years: McDonald, Sandpoint, Rainier View and Viewlands. We reassign these attendance areas to existing schools. We assign the Rainier View attendance area to Emerson and the Viewlands attendance area to Broadview-Thomson, following the district's implementation in the 2010 school year. We assign the attendance areas associated with McDonald and Sandpoint to the neighborhood school with greatest overlap in pre-period reference areas (Green Lake for McDonald and View Ridge for Sandpoint).

Figure B.1: MCMC Trace Plots




*Notes:* This figure shows trace plots for each parameter across MCMC iterations. The first 25,000 iterations are discarded as burn-in (not shown). Dashed horizontal lines mark posterior means. The right margin of each plot reports the Gelman-Rubin PSRF convergence statistic ($\hat{R}$).

Figure B.2: MCMC Trace Plots (cont.)




*Notes:* This figure shows trace plots for each parameter across MCMC iterations. The first 25,000 iterations are discarded as burn-in (not shown). Dashed horizontal lines mark posterior means. The right margin of each plot reports the Gelman-Rubin PSRF convergence statistic ($\hat{R}$).

## Appendix C. Derivations

This section derives a sufficient statistics representation of the change in ex-ante welfare associated with a change in the distribution of school access across neighborhoods from $S_{in}$ to $S'in$. Under our assumption that neighborhood preference shocks are iid Type-I Extreme Value, the change in welfare can be written as a function of level changes in school access and level changes in prices across neighborhoods. First, note that neighborhood choice probabilities are given by

$$\pi_{in} = \frac{\exp\left(\mathbf{a}_n^\top \varphi_i + \xi_n + \gamma_g S_{in} - \alpha_g p_n\right)}{\sum_k \exp\left(\mathbf{a}_k^\top \varphi_i + \xi_k + \gamma_g S_{ik} - \alpha_g p_k\right)}$$

and the ex-ante welfare of household $i$ is given by the log-sum formula

$$W_i = \log\left(\sum_k \exp\left(\mathbf{a}_k^\top \varphi_i' + \xi_k + \gamma g S_{ik} - \alpha_g p_k\right)\right).$$

Let the notation $\Delta x \equiv x' - x$ indicate level changes in variables, the change in $W_i$ can be expressed as

$$\begin{aligned}
\Delta W_i &= \log\left(\frac{\sum_k \exp\left(\mathbf{a}_k'^\top \varphi_i' + \xi_k' + \gamma_g S_{ik}' - \alpha_g p_k'\right)}{\sum_k \exp\left(\mathbf{a}_k^\top \varphi_i' + \xi_k + \gamma_g S_{ik} - \alpha_g p_k\right)}\right) \\
&= \log\left(\sum_k \pi_{ik} \frac{\exp\left(\mathbf{a}_k'^\top \varphi_i' + \xi_k' + \gamma g S_{ik}' - \alpha_g p_k'\right)}{\exp\left(\mathbf{a}_k^\top \varphi_i' + \xi_k + \gamma_g S_{ik} - \alpha_g p_k\right)}\right) \\
&= \log\left(\sum_k \pi_{ik} \frac{\exp\left(\Delta \mathbf{a}_k^\top \varphi_i' + \Delta \xi_k + \gamma_g \Delta S_{ik}\right)}{\exp(\alpha_g \Delta p_k)}\right)
\end{aligned}$$

In the absence of changes in non-schooling amenities, this simplifies to

$$\Delta W_i = \log\left(\sum_k \pi_{ik} \frac{\exp(\Delta S_{ik})^{\gamma_g}}{\exp(\Delta p_k)^{\alpha_g}}\right)$$

The derivation of this expression is a variant of the exact-hat algebra popularized by Dekle et al. (2008) for models of international trade. We work in level changes, rather than relative changes as in the exact-hat formulation, since we assume preference shocks are Type I rather than Type II extreme value. To interpret this expression, note that the ratio $\frac{\exp(\Delta S_{in})^{\gamma_g}}{\exp(\Delta p_n)^{\alpha_g}}$ is the change in indirect utility associated with neighborhood $n$. Intuitively, neighborhood appeal rises if households place positive value on school access ($\gamma_g > 0$) and school access improves ($\Delta S_{in} > 0$), while neighborhoods become less attractive if prices rise ($\Delta p_n > 0$). The change in ex-ante welfare relates to the sum of these changes in neighborhood-specific utilities. In a discrete choice setting, changes in the value of an alternative are more valuable when it is more likely to be chosen. In the logit model, the appropriate importance weight is the choice probability in the original equilibrium, $\pi_{in}$. This expression can be used to assess the welfare impact of any shift in the distribution of school access across neighborhoods, when non-schooling amenities are unchanged.

Under some additional assumptions and parameterizations, the change in school access admits a particularly simple form when the counterfactual is strict neighborhood assignment. First, we assume that the matching algorithm implements a stable match. Second, we assume students rank schools truthfully, such as when students apply optimally to a Deferred Acceptance match. When the matching is stable, the allocation can be described by school-specific cutoffs (Azevedo and Leshno 2016). Let $\mathcal{J}_i$ denote the set of schools where $i$ clears the cutoff. Third, we assume that applicants know exactly which schools they clear the cutoff at and always qualify at their neighborhood school. Finally, we set $\lambda_i = 0$ (no random coefficients) and assume $\varepsilon_i$ and $\zeta_i$ are each distributed Gumbel(0,1), realized after application choices are made.

In this case, the (net of application cost) utility associated with school access can be written

$$\begin{aligned} S_{in} &= \mathrm{E}\left[ p(A_i = 1) \cdot \log\left( 1 + \sum_{j \in \mathcal{J}i} \exp(u_{ij}) \right) + (1 - p(A_i = 1)) \cdot \log(1 + \exp(u_{i,J+1})) \right] \\ &= \mathrm{E}\left[ \log\left( (1 + \sum_{j \in \mathcal{J}i} \exp(u_{ij}))^p (1 + \exp(u_{i,J+1}))^{1-p} \right) \right] \end{aligned}$$

where $A_i$ is an indicator taking value 1 when $i$ applies. When feasible sets $\mathcal{J}i$ are known, we can drop the expectation. It follows that,

$$\begin{aligned} \Delta S_{in} &= \log \frac{1 + \exp(u_{in})}{(1 + \sum_{j \in \mathcal{J}i} \exp(u_{ij}))^p (1 + \exp(u_{i,J+1}))^{1-p}} \\ &= \log \frac{\tau_{n,n} + \tau_{n,0}}{(\tau_{n,J+1} + \tau_{n,0})^{1-p}}. \end{aligned}$$

where $\tau_{n,j}$ denotes the share of students in $n$ enrolling in $j$. This shows the change in school access associated with a move to strict neighborhood assignment can be expressed as a function of four sufficient statistics in the initial period. Neighborhood assignment is more harmful in areas where the share of students attending their neighborhood school $\tau_n$ is low. Areas with high private school attendance $\tau_0$ are partially insulated from the restriction of public school choice. The denominator captures the fact that non-applicants under school choice are administratively assigned. Neighborhood assignment can be beneficial as a result in areas where application rates $p$ are low, and where few students are assigned administratively ($\tau_{J+1}$).

## Appendix D. Data

### D.1. District data

We construct our primary analysis sample using student-level files provided by the district. The analysis sample consists of every student enrolled in kindergarten in our sample period (2006–2015) as well as applicants who did not ultimately enroll. Enrolled students come from the enrollment histories from the district's start of school year head-count, while information on applicants comes from application records. Both sets of files provide information on student race, gender, current grade, and residential coordinates. In a small number of cases, demographic and residential information differs between the enrollment and application records; in these cases we utilize information from the enrollment files. An additional file provides a crosswalk between de-identified student IDs and street addresses, with unit numbers where applicable.

From the application files, we drop a small number of applications submitted after the application deadline (∼1.5% of all applications) that were not considered in the match. From the enrollment files, we drop students enrolled in five school codes associated with non-traditional instructional settings or which function as administrative codes to track students not directly enrolled in the district (∼0.9% of enrolled students).[50] We additionally drop students enrolled in dedicated special education programs not part of the main match (∼2.5% of students).

We match each student to post-reform attendance areas by joining residential coordinates to attendance area shapefiles provided by the district. We also compute the minimum distance of each student address to nearby attendance areas to facilitate analysis of boundary discontinuities. The measure of commuting distance we use in our analysis is computed as the minimum road distance between each residential student address and school site coordinates provided by the district, using the TravelTime API. A small set of schools experience temporary relocations during our sample period. Since most of these relocations are relatively brief (e.g., 1–2 years) and households are presumably informed that relocations are temporary, we use the permanent location of the school when computing distance in these cases.

The application files contain information on the priorities of each student at each school in their list. The processing order of priorities was inferred by referencing historical enrollment guides and through consultation with district staff. In the pre-period, the order of priorities was sibling, reference area, and special program (for students attending closing schools), with remaining ties broken by a continuous-valued tiebreaker. Option schools used lottery tie-breaking while remaining schools used (straight-line) distance tie-breaking. Both types of tie-breakers are provided in the application files. The priorities in the post-reform match followed the same order but without the reference area priority. The tie-breaking scheme remained the same. Though applicants were not required to rank their neighborhood school, those who did were granted a dominating priority

[50] These codes are Special Education Home Instruction, Residential Consortium, the Experimental Education Unit, Private/Parochial Special Education, and Cascade Parent Partnership.

that guaranteed assignment. Using these rules, we are able to replicate close to all assignments. In 2008, our main estimation cohort, we replicate the correct program assignment for 95.5% of applicants. In 2011, the district reverted to the Boston mechanism. We replicate an average of 96.6% of assignments between 2011 and 2015 under this regime.

The district also provided a file containing standardized test scores during our sample period. Our analysis focuses on fourth-grade state standardized assessments. Through spring 2009, students took the Washington Assessment of Student Learning (WASL) in Grade 4. This test consists of Reading, Writing, and Mathematics sections. Our analysis focuses on the Mathematics section of this exam. Beginning in 2010, Washington replaced WASL with the Measurements of Student Progress (MSP). Fourth-grade testing under MSP consisted of Reading and Mathematics, with a separate Writing assessment administered through 2013. Starting in the 2014–15 school year, Washington adopted the Smarter Balanced Assessment (SBA) aligned to the Common Core State Standards. At Grade 4, SBA covers English Language Arts (ELA)—which integrates reading and writing—and Mathematics. Our data contain both raw student scores and codes for whether students met or exceeded grade-level expectations. We normalize raw scores to standard deviation units in each test-year (denoted in the text by $\sigma$). Attrition from these exams is relatively minor, with only 5.6% of 4th grade students missing exam outcomes during our sample period.

### D.2. Infutor data

Infutor is a private consumer-identity provider that aggregates residential address information from sources including phone books, voter files, magazine subscriptions, credit header files, and property deeds records, linking them into person-level longitudianl profiles. We utilize the Total Consumer History Plus file, which provides full names and up to three aliases for each individual. Individuals are associated with up to 10 past addresses, dating back to the mid 1990's. Each address record includes an effective date indicating when the individual was first observed at that location. We geocoded all addresses using ArcGIS software.

We take several steps to clean the data. Most importantly, we deduplicate records that appear to be associated with the same individual. Our deduplication procedure operates within each unique address in the dataset. For each address, we compile a list of all person IDs observed at that location at any point in time, then conduct a fuzzy match on first and last names within this list. Specifically, we use the bigram algorithm implemented by the Stata `matchit` program to assign a similarity score (ranging from 0 to 1) between each individual's first name and all other first names associated with the address, including name aliases. We repeat this process for last names and sum the two scores to obtain a combined similarity measure. Based on manual inspection of the data, we classify record pairs with a combined name similarity score exceeding 1.6 as matches representing the same individual. We consolidate these matches across the full dataset by reassigning person IDs to the maximum ID within each matched set. To resolve potential chains of matches—where person A matches person B at one address, and person B matches person C at another address—we iterate this ID reassignment procedure until no further updates occur.

Individuals are sometimes assigned the same effective date at different addresses. To resolve these conflicts, we rely on address type codes provided by Infutor, prioritizing addresses that are more likely to be residential. Specifically, when multiple addresses share the same effective date, we drop observations flagged as business complex, PO box, or firm addresses if a residential address is available. We also consolidate address observations within 500 feet of each other to catch duplicate records caused by data entry errors, such as missing or mislabeled unit numbers. For remaining records with conflicting effective dates, we retain addresses in the order provided in the Infutor file. Finally, we identify and drop address changes likely associated with short-term relocations, such as visiting family or work trips. Specifically, we remove address records where an individual moves to a new address but subsequently returns to their previous address, treating the intermediate address as temporary.

### D.3. Income imputation

To assign households to income categories, we start by linking student addresses to tax parcel centroids using a nearest parcel match with a 200-foot tolerance. We merge parcel assessment values from assessor files obtained from the King County Assessor's office (KCA). From the assessor files we retain the observed value of the parcel in 2004 to fix baseline asset values. For students matched to apartment buildings, we divide the total assessment of the building by the number of units recorded in the KCA's building characteristics files. We flag parcels as rental housing if the address matches a record in Seattle's Rental Registration & Inspection Code (RRIO). We flag parcels as public housing if the address is part of the inventory of project-based public housing operated by the Seattle Housing Authority (SHA) in 2010 (obtained through the SHA website using the Wayback Machine).

The second step of the procedure uses data from the 2005–2009 American Community Survey to estimate the share of renters and homeowners in the enrolled sample who should be assigned to each of the three income categories. To do this, we identify in the ACS microdata the sample of households living in Seattle with children enrolled in public elementary. We compute terciles of household income in this sample, then obtain the shares of renting and owning households in each tercile. We apply these tenure-by-tercile shares to the enrolled sample, ordering the latter by property assessment values. Households matched to public housing are treated as low-income renters by construction.

For post-reform samples of enrolled students, we apply the procedure above to the residential addresses of these households in 2008. To link households to past addresses, we merge student records with the Infutor data. To do so, we start by geocoding the universe of street addresses in the Infutor data. We match each student address from the district files to the list of Infutor addresses within 200 feet. We adjudicate between matches by first prioritizing exact street address and unit number matches, followed by exact street address matches, and finally by distance. To identify students with a specific Infutor individual, we use longitudinal information on the post-enrollment address histories of students in the district files. We associate students with the address-matched Infutor individual with the largest number of matching future locations, breaking ties using the number of years with matching addresses, followed by the length of total observation in the Infutor data. We match 86% of students to pre-reform addresses using this procedure. We then place households into the three income groups by applying the categorization procedure above to the 2008 addresses.

### D.4. Household counts

Our estimation of locational preferences relies on counts of households across neighborhoods in the post-reform period. To form these counts, we start with block-group level counts of total households from the 2012–2016 5-year ACS estimates. This data breaks households into 15 income categories that we map to our three income groups. The block-group level counts do not distinguish between households with and without children, however. We estimate the number of enrollment households in each block-group using the distribution of entering kindergarteners between 2013 and 2015 from the SPS files. We adjust the SPS kindergarten counts using estimated enrollment rates in each neighborhood to obtain counts of all kindergarten households. Finally, we obtain block-group level estimates of enrollment-age households by scaling the kindergarten counts to match the total count of enrollment-age households in Seattle from the same ACS file.

To estimate counts of non-enrollment-age households, we subtract our estimate of enrollment-age households in each block group from total households. Finally, we assign block-group level counts to areas. For block groups straddling multiple area geographies, we apportion counts using weights based on block-level population counts from the 2010 Decennial Census. Estimates of both enrollment-age and non-enrollment-age household counts by income group for the adjoining districts comprising the outside neighborhood are obtained from ACS microdata. Since this is a large region, PUMA-level geographies suffice. We use the nine 2010 PUMAs covering the Bellevue, Mercer Island, Shoreline, Highline, Renton, Tukwila, Northshore, and Lake Washington school districts.

### D.5. Transportation costs

Information on annual district-wide transportation expenditure comes from the National Center for Education Statistics Common Core of Data. Table D.1 reports both total and per-pupil expenditure

In the years leading up to the reform (2006-2008), the district spent an average of $640 per pupil per year. In the post-period (2013-2015), spending averaged $555. This suggests a relatively modest savings of $7 a month per pupil. However, these district-wide numbers reflect continuing costs of busing middle- and high-school students who are more likely to attend school further away from home, and therefore understate the degree to which transportation was restricted at the elementary school level.

Table D.1: Student Transportation Expenditure and Enrollment (2010 Dollars)

| | Total Transportation Expenditure (1) | Total Enrollment (2) | Expenditure per Pupil (3) | Expenditure per Elementary Pupil (4) |
|---|---|---|---|---|
| 2004 | 28,969,473 | 46,746 | 620 | 490 |
| 2005 | 26,836,631 | 46,085 | 582 | 456 |
| 2006 | 27,885,425 | 46,113 | 605 | 503 |
| 2007 | 29,604,597 | 45,581 | 649 | 527 |
| 2008 | 31,055,076 | 45,968 | 676 | 553 |
| 2009 | 30,778,709 | 46,522 | 662 | 518 |
| 2010 | 31,132,000 | 47,735 | 652 | 496 |
| 2011 | 29,306,919 | 49,269 | 595 | 434 |
| 2012 | 28,899,823 | 50,655 | 571 | 401 |
| 2013 | 30,540,027 | 50,509 | 605 | 373 |
| 2014 | 27,206,340 | 52,834 | 515 | 328 |
| 2015 | 29,125,408 | 53,317 | 546 | 353 |
| 2016 | 30,045,423 | 54,215 | 554 | 344 |

*Notes.* Data come from the National Center for Education Statistics Common Core of Data. Expenditures inflation adjusted to 2010 dollars.

To allocate the drop in total expenditure across grades, we identify students eligible for busing in each year. Under the operating assumption that busing costs per eligible student are roughly similar across grades, we assign transportation expenditures to grades in proportion to the share of busing eligible students in each grade. Finally, we divide by the number of students in each grade to obtain a grade-specific measure of per-pupil expenditure. We report these figures for elementary grades in Column (4). Using these estimates, per-pupil spending per elementary school student falls from \$528 pre-reform to \$361 after the reform, implying savings of \$15 a month per pupil.

### D.6. Value-added estimates

To obtain estimates of school value-added, we estimate a risk-controlled value-added model (VAM) as suggested by Angrist et al. (2020). This approach uses propensity score conditioning to eliminate selection bias unaccounted for by conventional controls. We implement this procedure using an OLS regression of the form:

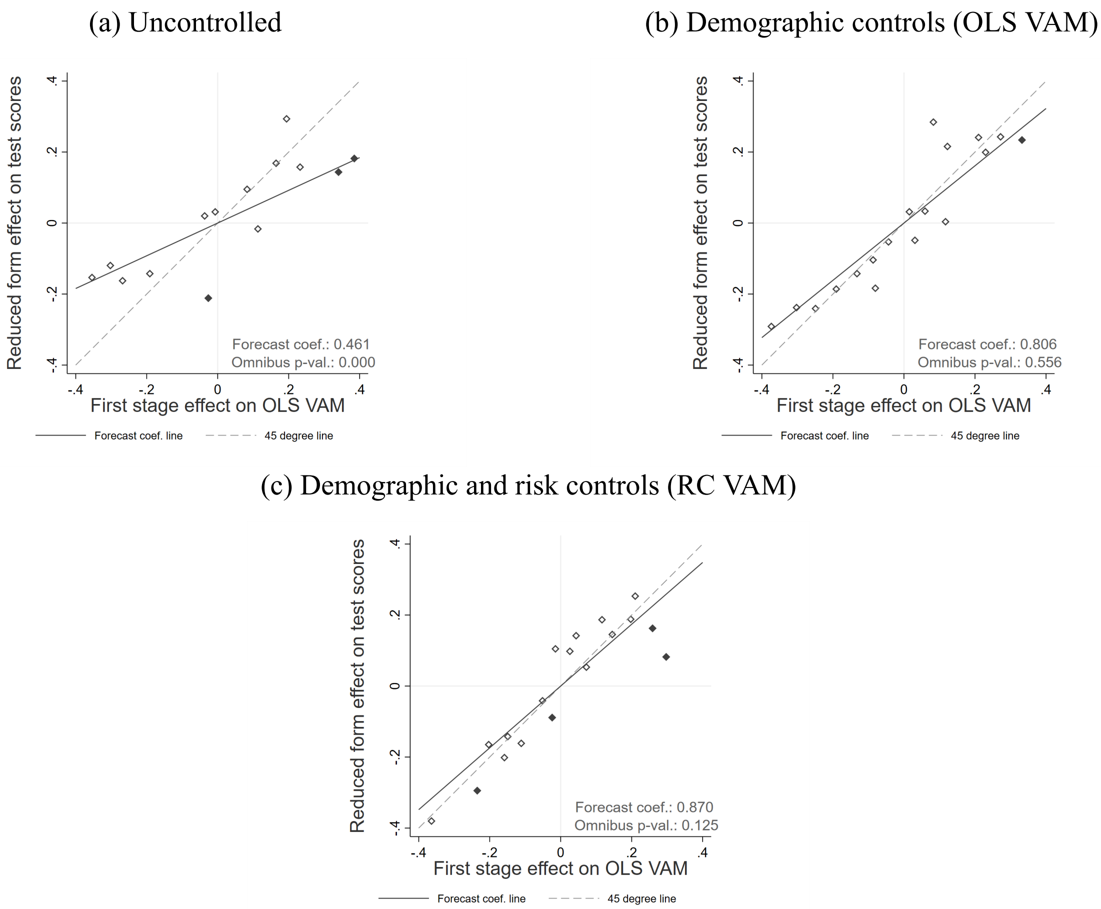
$$Y_i = \alpha_0 + \sum_j \alpha_j D_{ij} + X_i'\Gamma + g(P_i) + \iota_i$$

where $Y_i$ are 4th grade math scores and $D_{ij}$ indicates enrollment at school $j$. The vector $X_i$ includes controls for gender, four race dummies, an indicator for whether the student's household are renters, and residential property values (as a proxy for income). The function $g(P_i)$ parameterizes control for the vector of propensity scores $P_i$. We include in this function linear terms in the propensity score for assignment to each school, and a set of dummies when propensity scores take value zero.

We test the predictive validity of these value-added estimates in Seattle using the testing framework introduced in Angrist et al. (2016), which evaluates whether VAM estimates predict the causal effects of randomized school offers. These tests center on a projection of $Y_i$ on value-added estimates $\hat{\alpha}_j$, instrumented by risk-adjusted school offers $Z_{ij} - P_{ij}$. Figure D.1 provides a graphical summary of this test for our preferred estimates, as well as simpler models with fewer controls. Specifically, the figure plots the reduced form effects of offers binned by value-added on test scores against the first-stage effects on predicted value-added $\hat{\alpha}_j$.

When value-added is estimated perfectly, the points in this figure should fall exactly on the 45-degree line. The forecast bias coefficient reports the slope of the line of best fit, while the omnibus statistic combines a test for whether this slope differs from one and an assessment of whether deviation of individual points from the line can be explained by sampling error. While estimates of value-added that control for neither demographics nor assignment risk (panel (a)) are badly biased according to both statistics, models which include either demographic controls alone (panel (b)) or both demographic and assignment risk controls (panel (c)) perform quite well. Our preferred estimates shown in panel (c) exhibit a forecast coefficient of 0.87, and fail to reject the omnibus test for VAM validity.

Figure D.1: Validation of Value-added Estimates




*Notes:* This figure provides a graphical summary of the predictive validity of our value-added estimates. The sample consists of kindergarten students enrolled between 2006 and 2008. The dependent variable is 4th grade math test scores. The forecast coefficient and omnibus p-values are computed as described in Angrist et al. (2020)