School Segregation, Student Achievement, and Parental Preferences*

Sam Stemper†

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Abstract

For most American K-12 students, where you live determines where you go to school. At the local level, school assignment is dictated by school attendance zones: boundaries that assign households to schools. I use geospatial data on the boundaries used to assign over 10 million public school students across 1,401 districts to identify changes in school assignment and estimate how these changes affect school segregation, student achievement, and neighborhood house prices. Comparing districts that impose integrative versus segregationary rezoning schemes shows that rezoning decisions can change segregation substantially, and that reductions in segregation narrow test score gaps between White and Black students. I find evidence that house prices fall in neighborhoods that are newly-assigned to schools with higher shares of Black and Hispanic students.

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†Harvard University. Email: stemper@g.harvard.edu

For most American K12 students, where you live determines where you go to school. While the boundaries that delineate school districts are largely unchanged since the 1970s,[1] school attendance zones *within* districts change periodically—for example when school districts open a new school or close an existing one. By changing the allocation of students to schools, redrawing school attendance zones can change levels of segregation across schools, and some school administrators view school attendance boundaries as a tool in their efforts to achieve more racial balance across schools.[2]

There is good reason why promoting racial integration by changing school boundaries may be desirable policy for local school districts. First, changing school boundaries imposes no direct financial burden on school budgets.[3] Second, historical evidence from large-scale desegregation orders suggests that Black-White integration often increases academic and labor market outcomes for Black students, with little evidence of negative effects on White students (Guryan (2004), Johnson (2011)). While many district-level case studies find similar results (Billings et al. (2014), for example), little is known about the frequency, determinants, and effects of local changes in school assignment.

These changes are not without controversy. By definition, this approach to racial integration necessitates reassigning White students to different schools: schools that typically have relatively higher shares of minority students. In many anecdotal reports, school administrators note that many White parents raise concerns about integration. In one notable example, Minneapolis' plan to desegregate high school attendance "landed with a thud," for White parents; house sales increased in areas newly zoned for predominantly Black high schools, and many White parents opted for alternative high schools, including charters.[4]

This paper assesses how changes to school attendance zones affect these multiple margins of education: school segregation, student achievement, and the housing market. My empirical

[1]This followed a wave of consolidation between 1938 and 1970, in which over 100,000 American school districts were eliminated through consolidation (Duncombe and Yinger (2007)). More recently, there have been a number of notable school district successions, though the overall scale of succession activity is small: 44 successions between 2000 and 2014 (Houck and Murray (2019)) relative to over 10,000 school districts in the US.

[2]In one notable example, a 2007 Supreme Court ruling (Parents Involved in Community Schools v. Seattle School District No. 1) struck down assignment plans from Seattle, Washington and Louisville, Kentucky, where administrators used individualized racial classifications to reduce racial segregation through school assignment.

[3]These changes may affect district finances indirectly, via changes in student enrollment or house prices and subsequent property tax revenue.

[4]"In Minneapolis Schools, White Families Are Asked to Help Do the Integrating," The New York Times, November 27, 2021.

approach combines hyper-local data on neighborhood racial composition across the US with data on school attendance zones over time. Linking these two allows me to identify *changes* to school attendance zones, and to predict, for each change, whether it is likely to increase or decrease levels of racial segregation ex-ante. This design enables me to estimate the effects of changes in school attendance boundaries along multiple margins, isolating variation that is unrelated to underlying changes in neighborhood composition or student achievement.

I focus on the role of segregation between Black students and White students.[5] I do so for two reasons. First, Black students were historically exposed to de jure segregation for much of the 20th century. While the laws establishing racial segregation in schools were outlawed following Brown v. Board of Education in 1954, many argue that informal systems of segregation maintained racial inequalities in access to education throughout the 20th century (Rothstein (2017)). Second, despite substantial progress in equalizing financial resources across districts since the 1970s (documented in Jackson et al. (2015) and elsewhere), racial achievement gaps between Black and White students have changed only slightly over the same period (Hanushek et al. (2019), Lee (2002)). I test the degree to which changes in school boundaries increase or decrease these stubborn achievement gaps.

I present three main findings. First, while changes in school attendance boundaries are somewhat rare (Between 10 and 20 percent of diverse, multi-school districts in my sample meaningfully change their boundaries between 2013 and 2015.), these changes to school boundaries can meaningfully affect district-level segregation. Compared to districts that introduce segregationary boundary changes, districts that introduce more integrative boundaries see a 0.02 reduction in Black-White dissimilarity—over 10% of one standard deviation in my sample. These changes persist for the duration of my sample: four years after school attendance boundaries changed.

Second, segregationary changes in school attendance boundaries increase district-level achievement gaps between White and Black students. Specifically, comparisons of test scores in districts that introduced segregationary versus integrative boundary changes show that integration decreases tests score gaps, and vice versa. My preferred specifications estimate effects of boundary changes on within-district test score gaps for Black and White students, which absorb common, district-specific test score shocks. This approach is attractive in my setting because changes in

[5]As described later, Appendix A reports results with respect to Hispanic and White students.

school boundaries may have effects on achievement that arise purely due to the disruption of prior school assignment patterns. Focusing on within-district gaps isolates the differential effect that changes in within-district segregation has on gaps between Black and White students within a district.

Effect sizes for White-Black gaps are reasonably large in magnitude—0.04 standard deviations—roughly 6 percent of the average White-Black test score gap in my sample. A decomposition exercise shows that effects are driven primarily by reductions in the scores of Black students whose districts become more segregated. Throughout, effects of segregation on the scores of White students are generally small and statistically insignificant. In assessing mechanisms that explain these effects, I find suggestive evidence for changes in racial gaps in school quality and school operations, such as access to new school facilities and exposure to suspensions.

Finally, I analyze the effect of these changes on housing markets. When diverse districts adjust school boundaries, these changes imply that some areas of the district are assigned to schools with higher or lower shares of Black and Hispanic students than under previous boundary regimes. I estimate how changes in school assignment affect house prices over time, finding that areas that are newly-assigned to schools with 10% higher share of underrepresented minority ("URM"[6]) students in 2015 see their house prices fall by 2 percent by 2018. These effects do not appear to be driven by changes in school quality, as measured by prior test scores.

My work highlights the promise and perils of local desegregation efforts. Attempts to desegregate schools via changes in school boundaries indeed decrease within-district segregation substantially. Moreover, districts that implement such changes, on average, see reductions in long-standing racial achievement gaps (relative to schools that impose segregationary changes). However, my results with respect to house prices suggest that these changes have distributional consequences; neighborhoods reassigned to schools with more White students see house prices increase, while neighborhoods reassigned to schools with more URM students see house prices fall.

This work relates to the large literature on education, race, and segregation in America. As documented in Reardon and Owens (2014) and elsewhere, racial and economic segregation in the

[6]Throughout this paper, I use URM to refer to Black and Hispanic students together.

US fell substantially in the 1960s and 1970s, but has either stagnated or increased since.[7] Much prior work has documented the effects of historical school desegregation in on academic achievement and other outcomes (Guryan (2004), Hanushek et al. (2009), Johnson (2011)). Within this literature, my work relates most close to studies involving local changes such as school boundary changes and busing. The effects of these policies on a local and national scale are documented in Billings et al. (2014) and Monarrez (2021), both of which suggest that locally-induced changes to school segregation have large and persistent impacts on achievement gaps between Black and White students.

Moreover, my work relates to house prices and the characteristics of local schools. A long literature uses house prices to infer how parents value school quality (Black (1999), Bayer et al. (2007), Cellini et al. (2010)). In the same respect, if parents are willing to pay for their children to attend schools with a higher share of White students, local house prices will reflect these preferences. Using data from Atlanta, Georgia and Columbus, Ohio, respectively, Clotfelter (1975) and Gill (1983) present evidence consistent with this hypothesis: areas exposed to more racial integration have lower house prices. My results move beyond individual metropolitan areas and demonstrate that this is a common phenomenon, not specific to any particular period or setting.

This paper proceeds as follows. In the section immediately below, I introduce my data sources. Next, I describe my methodology for identifying and characterizing changes in school boundaries, and estimating the effects of changes in school boundaries on students, districts, and neighborhoods. I then present my results and conclude.

1 Data Sources

Below, I briefly describe my data sources.

School Attendance Boundaries Data

My school boundaries data come from two rounds of the School Attendance Boundary Survey ("SABS"), a survey conducted by the National Center for Education Statistics ("NCES"). These data contain, for all US public school districts, school-specific boundaries for school years beginning in 2013 and 2015. The data also contain grade ranges for each school; because each districts

[7] Reardon and Owens (2014) note that different approaches to measuring segregation lead to different conclusions about whether school segregation has been stagnant since the 1970s or whether it has increased.

has different grade structures (For example, some districts separate schools serving grades K-5 into two schools serving K-2 and 3-5, while others do not.), I focus on grade-specific attendance boundaries for students in grades 3 through 8.

Neighborhood Racial Composition Data

I use data from the 2010 US Census to measure neighborhood racial composition. Specifically, I use 2010 Census SF1 data from IPUMS (Manson (2020)), which reports population by race for each census block in the US. Census blocks are the smallest geographic unit used by the United States Census Bureau, and the median block in 2010 had a population of 3.[8] In cities, where most of my sample districts are located, census blocks are typically city blocks bounded on all sides by streets.

School Enrollment and Operations Data

I collect school enrollments by grade and race using data from the National Center for Education Statistics ("NCES") Common Core of Data. This data reports, for every public school in the country, the number of students in each grade, separately by race. I use this data to measure the racial composition of schools and districts and segregation within each district.

In addition I collect data on school operations from the Office of Civil Rights Civil Rights Data Collection ("CRDC"). This survey collects data on the educational practices of individual schools, such as the number of suspensions or the presence of gifted and talented programs. These data are available only in odd-numbered years between 2009 and 2017. I aggregate these data to the district-by-grade-by-year level by taking the enrollment-weighted average of individual school practices, both overall and specific to Black and White students separately.

Test Score Data

I measure student achievement using test score data from the Stanford Education Data Archive ("SEDA") (Reardon et al. (2021)). SEDA data contains, for each district, grade, and year between 2009 and 2017, average normed test scores in math and reading and language arts ("RLA") on state-administered standardized tests. In addition to mean test scores for each district overall, SEDA data reports mean scores for specific student subgroups, as well as gaps between large subgroups. I focus particularly on test scores for Black and White students, and the White-Black

[8] The interquartile range was 0 to 28 and the 95th percentile was 119.

test score gap.

Local Real Estate Data

I use data from the Federal Housing Finance Agency ("FHFA") to measure house prices. FHFA data reports "house price indexes that measure changes in single-family home values."[9] FHFA house price indices are based on a repeat-sales statistical technique that measures changes in house prices for the same property over time. I use FHFA's tract-level data, which reports the average change in house prices for each year at the census tract level. I construct a house price index for each census tract, which is pegged to 1 as of 2012 and moves according to annual changes in all other years. I drop any tracts that do not report prices for all years between 2008 and 2018. Census tracts are larger than census blocks, typically containing between 1,200 and 8,000 people, and I discuss my approach to mapping these tracts to school districts in the section below.

I combine this tract-level price data with data from the Home Mortgage Disclosure Act ("HMDA"), provided publicly by the Consumer Financial Protection Bureau.[10] These data include individual-level information for all applications for first lien, owner-occupied, 1 to 4 family homes. This data has been used in assessing other educational contexts in Friday and Smith (2021), and Bhutta et al. (2017) report that HMDA data covers roughly 90% of home purchases reported in consumer credit files. I aggregate this data to the tract-by-year level, and count the number of home mortgage applications overall and separately by race. In my analyses, I transform these counts using the inverse hyperbolic sine transformation, which approximates a log function but can handle zeros.

2 Methodology

Broadly, my methodology has three parts. First, I combine data on school attendance boundaries and neighborhood racial composition to identify and characterize changes in school attendance boundaries between 2013 and 2015. Specifically, I identify changes in school boundaries, and predict whether they are likely to increase or decrease segregation using data on neighborhood racial composition. Second, I estimate district-by-grade level difference-in-differences models to measure the effect of changes in school attendance boundaries on district level outcomes: namely, lev-

[9] "House Price Index," FHFA.

[10] "Download HMDA data," Consumer Financial Protection Bureau.

els of district segregation and test scores. Finally, I use a similar methodology to identify changes in school assignment at the neighborhood level, and test whether house prices change in response to school reassignment.

2.1 Identifying Changes in School Attendance Boundaries with Geospatial Data

I combine two sets of data to identify and characterize district-level changes to school attendance boundaries: a two-year panel of school attendance boundaries, and data from the 2010 US Census that captures local racial composition across neighborhoods in the US. With these data, I identify anticipated effects of school attendance boundary changes on district segregation.

I identify the effects of school boundary changes as follows. First, for each school and year (2013 and 2015), I calculate the set of census blocks that fall within the school's attendance boundary. In practice, this calculation is a spatial merge: identifying the set of census block centroids that fall within a given school attendance boundary.

I denote whether a census block b falls in school attendance boundary s for grade g in year t with a binary indicator p_{bsgt}. All values of p_{bsgt} are equal to either 1 or 0. In practice, because census blocks are extremely small, nearly all census blocks fall entirely within one attendance boundary.

Next, I use this mapping to predict each school's racial composition. For each school, I calculate its predicted attendance by race, assuming that each school's attendance reflects the demographics of the census blocks it contains. Denote the predicted number of Black students enrolled in school s in grade g in year t as $\hat{Black}_{sgt}$, and denote the equivalent number of White students as $\hat{White}_{sgt}$, respectively. I calculate predicted enrollments according to the equations below.

$$\hat{Black}_{sgt} = \underbrace{Enr_{sg,2013}}_{\text{Observed 2013 Enrollment}} \times \underbrace{\frac{\sum p_{bsgt} \times BlackPop_{2010b}}{\sum p_{bsgt} \times TotPop_{2010b}}}_{\text{Predicted \% Black from Overlapping Census Blocks}}$$

$$\hat{White}_{sgt} = \underbrace{Enr_{sg,2013}}_{\text{Observed 2013 Enrollment}} \times \underbrace{\frac{\sum p_{bsgt} \times WhitePop_{2010b}}{\sum p_{bsgt} \times TotPop_{2010b}}}_{\text{Predicted \% White from Overlapping Census Blocks}}$$

I use 2013 enrollments rather than contemporaneous enrollments because the latter may be affected by changes in school choice that arise in response to boundary changes. For schools that do not appear in 2013 data (schools that opened after between 2013 and 2015, for example), I set $Enr_{sg,2013}$ equal to the median value (among 2013 schools) for district s and grade g.

I use these school-level predicted enrollments by race to estimate the predicted level of segregation within a district. Throughout, I focus on Black-White segregation and measure within-district segregation using the Black-White dissimilarity index, calculated below.

$$\hat{Diss}_{dgt} = \frac{1}{2}\sum_{i=1}^{n}\left|\frac{\hat{White}_{sgt}}{\hat{White}_{dgt}} - \frac{\hat{Black}_{sgt}}{\hat{Black}_{dgt}}\right|$$

Here, $\hat{White}_{dgt}$ represents the predicted number of White students enrolled in district d in grade g in year t, and similarly for $\hat{Black}_{dgt}$.

Dissimilarity indices fall between between 0 and 1 and represent the share of Black students that would need to be reallocated to equalize their share across schools. Dissimilarity indices also have the attractive property that, unlike indices of exposure or isolation, they are invariant to changes in the share of Black students in the population (Reardon and Owens (2014)). Because districts vary substantially in the share of Black enrollment, dissimilarity indices provide a more reasonable measure for comparisons across districts.[11]

To quantify the effect of changes in school boundaries on levels of segregation, I estimate $\hat{Diss}_{dgt}$ for 2013 and 2015 for all districts in my sample. Doing so allows me to identify districts in which changes to school boundaries are likely to meaningfully change levels of Black-White segregation within a district. In turn, these predicted changes allow me to compare the changes in district outcomes—namely, observed segregation and test scores—in districts where school boundary changes were likely to increase or decrease segregation to the changes in other districts.

Figure 1 shows how this works in practice, taking District of Columbia Public Schools in Washington, DC as an example. As is the case in many American cities, the Black population in Washington, DC is concentrated geographically. Panel A shows the Black population share of each census block in Washington, DC in 2010. Neighborhoods in the eastern side of Washington, DC are predominantly Black; neighborhoods further west are predominantly non-Black. In Panel

[11] In Appendix Table B3, I show how my main results differ when I use alternative measures of segregation; the isolation index and the variance ratio. Using these indices produces results that are qualitatively similar but slightly less precise.

B, I show the boundaries for schools serving 8th graders in Washington, DC as of 2013. For each school, I predict the Black share of enrollment based on the the racial composition of overlapping census blocks in Panel A. Between 2013 and 2015, District of Columbia Schools dramatically redrew their school attendance boundaries, after an "emotional 10-month process, in which residents [...] worried about [...] children's academic opportunities and their real estate values."[12] The resulting boundaries (as of 2015) are shown in Panel C. For 8th graders, these changes decreased predicted district-level segregation, by equalizing the predicted share of Black students across middle schools in the district. I measure the magnitude of this change (and changes for all other districts in my data) using dissimilarity indices of predicted enrollment counts. Here, predicted dissimilarity fell from 0.65 to 0.61.

This approach is an approximation, and assumes that the racial distribution of a given school mirrors the racial distribution of the census blocks within a school's boundary. This assumption does not hold perfectly, and there are two sources of prediction error that may lead to errors in my measure of segregation. First, census block racial composition may change between 2010 (the year of my population estimates) and 2013 or 2015 (the years of my school boundaries data). Second, my neighborhood racial data reflects population-level racial composition, which may differ from the racial composition of students attending local public schools. For example, my estimates do not account for differences that arise due to private schooling or charter school enrollment. I discuss the consequences of these errors in my estimation in Section 2.5 below.

Despite the potential for these prediction errors, this approach is desirable for multiple reasons. First, because population counts at the census block are held constant at their 2010 values, changes in $\hat{Black}_{sgt}$ and $\hat{White}_{sgt}$ between 2013 and 2015 are driven entirely by changes in school attendance boundaries, rather than endogenous migration or enrollment decisions that may arise in response to changes in school boundaries. Second, my approach to identification relies on *changes* in school assignments between 2013 and 2015. Across numerous measures, these changes do not predict the evolution of outcomes prior to 2013. They do, however, strongly predict changes in segregation following 2013. The methods used to estimate these changes are described in the section that follows.

[12]"DC Mayor Gray adopts new school boundary recommendations," The Washington Post, August 21, 2014.

2.2 Estimating Effects of Boundary Changes on District Outcomes

I combine data on changes to school attendance zones with a panel of outcomes—namely, segregation and test scores—at the district-by-grade level between 2008 and 2018. With these data, I estimate the effect of changes to school boundaries on within-district segregation and test scores through a difference-in-differences design that compares districts that implemented different school boundary changes over time. I operationalize my approach as follows.

First, I define the predicted change in Black-White dissimilarity between 2013 and 2015 as follows:

$$\Delta \hat{Diss}_{dg} = \hat{Diss}_{dg,2015} - \hat{Diss}_{dg,2013}. \tag{1}$$

$\Delta \hat{Diss}_{dg}$ represents, for district d and grade g, the predicted change in Black-White dissimilarity between 2013 and 2015. Absent any prediction errors in my estimates of school-level enrollment, $\Delta \hat{Diss}_{dg}$ would represent the exact change in segregation between these two years.

To measure the effects of $\Delta \hat{Diss}_{dg}$ on district segregation over time, I estimate difference-in-differences models, comparing districts with different values of $\Delta \hat{Diss}_{dg}$ over time. My identifying equation interacts my measure of predicted changes in segregation between 2013 and 2015, $\Delta \hat{Diss}_{dg}$, with binary variables identifying time periods, controlling for district-by-grade and year fixed effects.

To produce my main estimates, I use the two-way fixed-effects specification below.

$$y_{dgt} = \gamma_{dg} + \rho_t + \beta\big(\Delta \hat{Diss}_{dg} \times \mathbb{1}[t > 2013]\big) + \varepsilon_{dgt} \tag{2}$$

Here, y_{dgt} represents some outcome at the district-grade-year level; for example, the average White-Black test score gap or Black-White dissimilarity. γ_{dg} represents fixed-effects for district-grade pairs, ρ_t represents year fixed-effects, and $\mathbb{1}[t > 2013]$ is an indicator for post-2013 years. My coefficient of interest is β, which measures the effect of changes in predicted segregation in years after 2013.

Throughout, I cluster standard errors at the district level. For regressions in which mean test scores (or gaps) are outcomes, I precision-weight, weighting each observation by the inverse of estimated variance of the mean test score.[13] To limit the effect of large outliers, I winsorize $\Delta\hat{Diss}_{dg}$ such that no value is above 0.1 or below -0.1. This change affects less than 2 percent of the district-grade combinations in my data, but limits the degree to which large outliers drive my results.

In addition to estimates of Equation 2 in tables, I present visual evidence in the form of event studies, which show the dynamic effect of changes in predicted segregation on district-by-grade outcomes. I estimate these effects simply by changing the indicator variable in Equation 2, $\mathbb{1}[y > 2013]$, to individual indicators for each year other than 2012, indicated below by τ_t.

$$y_{dgt} = \gamma_{dg} + \rho_y + \sum_{k \neq 2012} \beta_t \left(\Delta\hat{Diss}_{dg} \times \mathbb{1}[t = k]\right) + \varepsilon_{dgt} \tag{3}$$

Here, the β_t coefficients trace the path of $\Delta\hat{Diss}_{dg}$ across individual years. Omitting 2012 implies that estimates of β_t should be interpreted relative to their 2012 values.

2.3 Estimating Effects of Boundary Changes on Neighborhood House Prices

Thus far, the analyses described above analyze outcomes of school districts over time. However, my approach additionally allows me to measure how school assignment affects outcomes of neighborhoods over time. In particular, I test whether neighborhoods that were newly-assigned to relatively more or less non-White schools in 2015 experienced lower house price returns in subsequent years.

My approach relies on the school-specific predictions described above, which combine 2013 and 2015 school boundaries with data on 2010 neighborhood racial composition to predict each school's enrollment by race. I aggregate these predictions to estimate changes in the predicted racial composition of school assignments at the census tract level. (FHFA housing data is reported at the tract level.)

My approach to estimating these changes is as follows. First, for each block-grade combination, I calculate the average predicted racial composition of assigned schools. (For block-grades assigned to only one school this is simply equal to the racial composition of their assigned school.) Second, I aggregate these block-grade level averages to the tract level, calculating the average

[13] As I show in Appendix Table B1 (and describe later in text), using equal weights produces qualitatively similar results that are less precise.

block-level URM share, weighted by population. This process produces, for each census tract and grade represented in my data, the predicted URM share of assigned schools. I denote this value for tract c in grade g in year t as $\%\hat{URM}_{cgt}$.

Similar to the analyses above, I quantify changes in neighborhood school assignment by measuring the difference between 2015 assignments and 2013 assignments. In particular, I define the predicted change in tract-level school assignment between 2013 and 2015 as follows:

$$\Delta\%\hat{URM}_{cg} = \%\hat{URM}_{cg,2015} - \%\hat{URM}_{cg,2013}. \tag{4}$$

$\Delta\%\hat{URM}_{cg}$ represents the predicted change the URM share of schools assigned to students in tract c and grade g.

I estimate effects of these changes using a difference-in-differences strategy similar to the district-level estimates described above. Census tract-level data allows me to control for district-by-year effects, which absorb some year-specific local shocks. I estimate a difference in differences models of the form below.

$$y_{ct} = \gamma_{cg} + \rho_{d(c)t} + \beta\big(\Delta\%\hat{URM}_{cg} \times \mathbb{1}[t > 2013]\big) + \varepsilon_{cgt} \tag{5}$$

Here, y_{ct} represents the house price index for tract c in year t, which is pegged to 1 in 2012. γ_{cg} represent tract-by-grade fixed effects. In addition, the presence of multiple observations per district per year allows me to control for district-by-year fixed effects $\rho_{d(c)t}$. β represents the effect of $\Delta\%\hat{URM}_{cg}$ in post-2013 years relative to their values in 2012 and earlier. I cluster standard errors at the district level and weight each regression by 2010 census tract populations. (Similar to above, I additionally report the results of an event study specification that estimates year-specific coefficients β_t.[14])

I report results for all districts, as well as results with two different sample restrictions, in attempt to exclude districts that are poorly-suited for estimation in this context. In the first set of restrictions, I exclude tracts for which less than 95 percent of the tract population falls within

[14]This regression equation is shown below:

$$y_{ct} = \gamma_{cg} + \rho_{d(c)t} + \sum_{t \neq 2012} \beta_t\big(\Delta\%\hat{URM}_{cg} \times \mathbb{1}[y = t]\big) + \varepsilon_{cgt} \tag{6}$$

district-by-grade specific school boundaries (in both 2013 and 2015 data)[15] and exclude tracts from very large districts (districts that overlap with more than 50 tracts[16]), where district-by-year fixed effects explain relatively less of the variation in house prices.[17] In my second set of results, I additionally exclude tract-grade combinations that are covered by more than one school.

2.4 Sample Description

Not all districts in the US are well-suited for this study. For one, many districts have only one school for each grade (one elementary, one middle, and one high school), meaning there are mechanically no changes in school attendance boundaries from year to year. Second, many school districts have little racial diversity and therefore limited data on racial segregation and racial gaps in standardized test scores. For this reason, I restrict my sample to district-grade combinations for which (a) SABS data is available for both 2013 and 2015, (b) SABS data indicates the district had at least 2 non-open enrollment schools in 2013 and 2015, (c) NCES data indicates that the district-grade enrolled at least 20 Black students and 20 White students in 2013, (d) SEDA data reports White-Black gaps for at least one year. Altogether, these restrictions produce a set of 6,630 district-grade combinations from 1,401 unique districts.

Figure 2 shows where these districts are located in the US. The size of each point in Figure 2 reflects district total enrollment in eligible grades in 2013. My sample includes most districts in the American South, where districts are generally quite large and district boundaries usually correspond to county lines. Outside of the American South, my sample consists primarily of urban and suburban districts near large cities such as Chicago, Minneapolis, or Seattle.

Table 1 displays summary statistics for these districts. All characteristics in Table 1 are as of 2013 and are measured at the district-by-grade level. The average district-grade in my data enrolls roughly 1,500 students, about 302 of whom are Black, 513 of whom are Hispanic, and 612 of whom are White. Altogether, these district-grades enroll over 10 million students, more than 20% of total K12 public enrollment in the US.[18]

[15] These scenarios arise when census tracts are bisected by two or more districts.

[16] This reduces the number of tracts in my data substantially, but reduces the number of unique districts in my data only slightly.

[17] For example, the Los Angeles Unified School District covers over 500 unique census tracts, whereas many other districts cover fewer than 10 tracts.

[18] "Table 203.20. Enrollment in public elementary and secondary schools, by region, state, and jurisdiction: Selected years, fall 1990 through fall 2023," NCES.

For districts in my sample, mean math test scores are approximately zero—equal to national normed averages. However, these averages mask large differences across racial groups. The mean district in my sample has a White-Black test score gap equal to 0.65 standard deviations, driven both by White relative over-performance and Black relative under-performance. Measures of school operations are shown in Panel C.

In my main results, I focus on Black-White segregation and test score gaps. In Appendix A, I repeat my main analyses for Hispanic-White segregation and test score gaps. I follow the same approach to sample construction and estimation, and find that, similar to my main results, changes in school boundaries meaningfully affect levels of segregation within school districts. However, unlike the results described below, I find limited evidence for changes in White-Hispanic test score gaps following changes in district-level segregation.

2.5 Threats to Identification

Prior to presenting results, I briefly discuss two threats to identification in my context: the potential for prediction errors in my predictions of school racial composition and issues with estimating difference-in-difference models with a continuous treatment variable.

2.5.1 Prediction Errors in School Racial Composition

First, as described above, my method for predicting school racial composition is prone to prediction errors that arise due to differences between neighborhood racial composition and school attendance. In Figure 3 I assess the accuracy of these predictions.

In Panel A of Figure 3, I compare the predicted Black share of school enrollments to the actual Black share of school enrollments, separately in 2013 and 2015. On average, my method is highly predictive of school enrollments; actual Black shares rise monotonically with predicted Black shares. However, for schools with predicted Black shares above 0% and below 100%, my method typically under-estimates the Black share of students, indicated by predictions that fall above the 45-degree line. While I cannot test for the sources of this bias directly, this pattern would arise in the presence of (a) differential population growth: areas with non-zero Black populations in 2010 tend to have larger Black populations (and therefore a larger school-age Black population) in 2013 and 2015, or (b) age bias: within a census block, school-age residents are typically more likely to be Black than all residents. While prediction error generally biases my estimates of school Black population shares downwards, the magnitude of this underestimate is reasonably consistent between 2013 and 2015, providing support for my use of *changes* in boundaries to identify *changes* in segregation.

In Panel B of Figure 3, I compare predicted levels of segregation to actual levels of segregation, separately in 2013 and 2015. For both years, my prediction are, on average, quite accurate; means only deviate slightly from the 45 degree line. As I show in the section that follows, changes in predicted segregation between 2013 and 2015 are also highly predictive of changes in actual segregation over this period.

2.5.2 Difference-in-Difference Methodology

Recent work on difference-in-differences with continuous treatments demonstrates that identification in this context relies on stronger assumptions than the typical binary treatment case. Namely, Callaway et al. (2021) show that difference-in-differences coefficients in continuous treatment settings (such as β in Equation 2) identify the average treatment effect only under a "strong parallel trends" assumption, which states that differences in potential outcomes are equal across all *doses* of treatment. While this assumption cannot be evaluated directly, I show that I obtain qualitatively similar results under alternative definitions of treatment: namely, discretizing $\Delta \hat{Diss}_{dg}$ in Equation 2 to represent groups of districts based on ranges of $\Delta \hat{Diss}_{dg}$.

Second, I test whether districts that imposed segregationary versus integrative rezoning schemes exhibit differential trends in student demographics prior to 2013. To do so, I interact $\Delta \hat{Diss}_{dg}$ with a year trend and estimate the equation below using only pre-2013 data.

$$y_{dgt} = \gamma_{dg} + \rho_t + \beta(\Delta \hat{Diss}_{dg} \times Year_t) + \varepsilon_{dgt} \tag{7}$$

Here, β measures the differential trends between segregationary and integrative districts.

Appendix Table B2 displays my results. In Panel A, I estimate Equation 7. The results indicate that pre-2013 trends in student characteristics are not systematically different between districts with high- versus low-predicted changes in segregation between 2013 and 2015. Panel B of Appendix Table B2 additionally tests whether student characteristics change in response to changes in predicted segregation *after* 2013. The results indicate that changes in segregation do not generate significant changes in student characteristics.

Finally, estimating my event study specification, Equation 3, allows me to test for the presence of pre-trends prior to 2013 by via coefficients on pre-2013 treatment interaction terms. My estimates, as shown in my event study results below, generally find that these terms are insignificant and small.

3 Results

3.1 Characterizing District Boundary Changes

I first characterize the nature of boundary changes before assessing the impact of boundary changes on segregation and other outcomes. Figure 4 shows the distribution of predicted changes in Black-White Dissimilarity, $\Delta \hat{Diss}_{dg}$, separately for grades 3 to 5 and grades 6 to 8. For visual clarity, I winsorize the distribution, grouping districts above 0.04 or below -0.04 together.

Figure 4 shows that, in most year-grade combinations, districts do not change boundaries to meaningfully affect segregation. I use 0.02 as a threshold for meaningful changes in Black-White dissimilarity.[19] Intuitively, this level of change indicates that the share of Black students that would need to be reallocated to equalize their share across schools increased or decreased by 2 percentage points. At this threshold, roughly 13 percent of districts meaningfully change in their school boundaries for grades 3-5 between 2013 and 2015. For grade ranges 6-8, this number is 17 percent. For both grade ranges in Figure 4, the share of segregationary versus integrative boundary changes are roughly equal.

Boundary changes don't arise randomly, and I next explore the determinants of school boundary changes at the district-grade level. To do so, I estimate linear probability models in which the outcome is a binary variable indicating whether a district-grade increased segregation or whether a district-grade decreased segregation. I include three explanatory variables that are often cited in local press coverage of school boundary changes:

[19] I select this threshold after examining the relationship between predicted changes in segregation and actual changes in segregation. Appendix Figure B1 displays this relationship visually. For districts with changes that are predicted to change dissimilarity by less than 0.02, there is little systematic relationship between predicted changes and actual changes. Above that threshold, predicted changes in segregation tend to translate into actual changes, albeit at magnitudes less than one-to-one.

- The change in the number of schools between 2013 and 2015 as a share of total schools in 2013, which captures the effect of school openings and closures
- The growth in log district enrollments between 2010 and 2012, which captures the effect of expanding enrollments
- A "crowding index," equal to the difference in school-level enrollment growth at the 75th and 25th percentile, which captures the rate of differential growth across schools within a district-grade combination; this index is highest when a district has some schools that are growing rapidly and others that are shrinking

To simplify exposition and to limit the influence of outliers, I convert the latter two indices above into percentiles ranging from 0 to 1. In all regressions, I include controls for the log of 2013 enrollment and the log of the number of schools in 2013.

Table 2 displays results. Columns 1 through 4 of Table 2 display linear probability models that predict whether a district introduced boundary changes that increase predicted segregation between 2013 and 2015. Increases in the number of schools increase segregation; my estimates suggest that a 50 percent increase in the number of schools (from 2 schools to 3 for example) increases the probability of increasing segregation by over 5 percentage points. District-level growth does not appear to play a significant role in determining which districts increase segregation; estimated coefficients are positive but statistically indistinguishable from zero. District crowding appears to play a positive role; district-grades that experience an increase in crowding are more likely to introduce segregationary boundary changes in the following years. Moving from the lowest to highest rate of district crowding increases the probability of introducing segregationary boundary changes by roughly 4 percentage points. Column 4 presents the results of a "horse race" regression, in which the relationships mentioned above remain reasonably consistent.

Columns 5 through 8 of Table 2 display results of a similar exercise, instead predicting whether a district imposed changes that decrease predicted segregation. Here, neither changes in the number of schools, district growth, or district crowding bear a significant relationship with desegregationary boundary changes. Instead, district sizes appear to play a role; districts with high enrollment and districts with a relatively larger number of schools are more likely to impose desegregationary changes between 2013 and 2015.

The determinants of boundary changes are not limited to district-by-grade specific phenomena; instead, they are often part of broader district plans to change boundaries. To show this, I estimate correlation coefficients of each district's change in predicted segregation across grade levels. Table 3 displays the results of this exercise. As seen in Table 3, correlations in changes between adjacent grades (grade 3 to grade 4 or grade 7 to grade 8, for example) are often above 0.7. These correlations fall as the distance between grades increases. However, even the most distant grade pairs still bear positive correlation coefficients. In my districts, these grade pairs are rarely served by the same sets of schools, but still appear to move (partially) in tandem, suggesting that district-specific planning may explain some of the variation in my measure of predicted segregation. This is consistent with evidence in Macartney and Singleton (2018), who find that school board members have large effects on levels of district segregation.

3.2 Boundary Changes and District Segregation

Next, I show that predicted changes in segregation correspond strongly to observed changes in subsequent years. Figure 5 summarizes my main results.

Panel A of Figure 5 displays changes to mean levels of segregation (relative to 2012) for each year, separately for three discrete groups identified above: districts whose boundary changes were predicted to decrease segregation, districts whose boundary changes were predicted to have no effect on segregation, and districts whose boundary changes were predicted to increase segregation. As seen in Panel A, levels of segregation for these three groups move roughly in tandem between 2008 and 2012. However, starting in 2013, these groups diverge substantially, all in their predicted directions. In 2016, differences between desegregating and segregating districts were between 0.02 and 0.03, over 10% of one standard deviation in Table 1.

Panel B of Figure 5 displays the corresponding event study estimates, where the effect of school boundary changes is identified based on a continuous treatment measure, $\Delta \hat{Diss}_{dg}$. Similar to the evidence in Panel A, pre-2013 estimates in Panel B show no significant "pre-trends." Moreover, effects after 2013 in Panel A show that predicted changes in segregation have significant effects on post-2013 levels of segregation. By 2016, a one-unit increase in predicted segregation change generates a change in actual segregation of roughly 0.25. This effect size is reasonably consistent from 2015 to the final year in my data, 2018.

I present these results formally in Table 4. Panel A of Table 4 presents results using all districts, while Panel B restricts the sample to only the district-grades that were predicted to change levels of segregation. Column 1 of Panel A uses the continuous treatment measure of predicted changes in segregation, $\Delta \hat{Diss}_{dg}$, to estimate effects on segregation. Consistent with Figure 5, I find that a one-unit increase in $\Delta \hat{Diss}_{dg}$ increases actual post-2013 segregation by 0.2 units.

Columns 2 through 5 run similar models, replacing my continuous treatment measure with discrete groups of district-grades indicating whether a district-grade is predicted to increase or decrease segregation. Column 2 identifies these districts using a cutoff of 0.01. Treatment coefficients indicate that, consistent with my predictions, "increasing" districts do indeed increase, and vice versa. Columns 3, 4, and 5 repeat this process at cutoffs of 0.02, 0.05, and 0.1 respectively, finding increasing effects sizes at each larger cutoff.

Panel B of Table 4 repeats this process but restricts the set of district-grades "increasing" or "decreasing" districts. Here, coefficients identify the difference in segregation between these two sets of districts after 2013. The estimates in Panel B are similar to those in Panel A.

Panel B provides a useful reference point for gauging effect sizes. How much can school boundaries alone change levels of segregation? The results in Panel B suggest that school districts indeed have substantial influence over segregation via school boundaries. Column 3 of Panel B indicates that even relatively common changes in school boundaries can increase short-term segregation by 0.02, equal to 12.5 percent of one standard deviation in Table 1. This is not a particularly high threshold; the sample in Column 3 includes over 15 percent of my sample. At higher thresholds, effect sizes are much larger, but include a smaller set of districts.

3.3 Effect of Segregation on Student Achievement

I next assess whether these changes in segregation affect achievement of Black and White students (and gaps between these groups). In Figure 6 I present visual evidence of effects on test scores in the form of event study plots showing estimates from Equation 3. The left two panels of Figure 6 show effects on test score levels, separately for Black and White students. These series largely move in tandem prior to 2013. In the years immediately following 2013, both series exhibit declines in RLA. In math, the scores of White students remain constant while the scores of Black students decline.

The left panel of Figure 6 formalizes this comparison by testing the over-time evolution of test score gaps rather than test score levels. This comparison generates clearer evidence of differential effects of segregation on Black and White students. In the years following an increase in segregation, test score gaps in math between White and Black students grow. Results in ELA show little strong evidence of large changes.

The corresponding point estimates for these effects, which are estimated via a single difference-in-differences coefficient from Equation 2, are shown in Table 5. Because samples change slightly relative to estimates based on the all available data (Test score data is not available for all district-grades in all years.), Panel A of Table 5 shows difference-in-differences estimates of effects on segregation, as measured by Black-White dissimilarity. Similar to the event study results in Figure 5 and Table 4, Panel A indicates that predicted changes in segregation predict actual changes in segregation over time, and that this relationship is reasonably consistent across grade ranges.

Panels B and C of Table 5 shows estimates of the effects on test scores of White students and Black students, respectively. Effects for White students are generally imprecise; effects on the scores of Black students are uniformly negative but still noisy. Panel D makes the comparison between White and Black students within each district explicitly, presenting effects on test score gaps between White and Black students. The results in Panel D suggest that segregation increases test score gaps between White and Black students. These effects are concentrated in relatively earlier grades; grades 3 to 5 exhibit large and statistically significant increases in test score gaps whereas grades 6 to 8 do not.

In some instances, effect sizes with respect to Black and White test scores are not consistent with effect sizes on White-Black test score gaps (that is, effects on White-Black gaps are not equal to effects on White students minute effects on Black students). This is driven by changes in weighting across regressions: each regression is precision weighted, where each observation is weighted by the inverse of the variance of the estimated mean test score (or test score gap). Thus, districts with a large number of White students and relatively fewer Black students will be weighted more heavily in estimates of effects on White test scores and less heavily in estimates of effects on Black test scores. Appendix Table B1 shows how estimated effects change when test scores are weighted equally across all observations. Effects using equal weights are qualitatively similar to those in Table 5.

In Table 6, I present a decomposition of these results. Specifically, I estimate Equation 2 using discrete definitions of treatment, where district-grades for which $\Delta \hat{Diss}_{dg}$ is greater than 0.02 or less than -0.02 are identified as increasing or decreasing districts, respectively. The interaction terms $Post \times Increase$ and $Post \times Decrease$ compare test scores in increasing and decreasing districts to districts with no predicted change in segregation.

The results in Panel A confirm that these groups exhibited changes in segregation consistent with their expected signs. Panel B estimates effects on the test scores of White students. I find some evidence for negative effects in districts that increase *and* decrease segregation, particularly in RLA. As noted above, the changes in school boundaries that give rise to changes in predicted segregation may have effects on achievement independent of their effects on segregation. For example, opening and closing schools, or shifting students across schools more generally, appears to lower test scores of White students irrespective of effects on segregation.

Focusing on differences between Black and White students is more informative. Panel C shows that, following segregationary changes in school boundaries, test scores of Black students decrease by roughly 0.03 standard deviations in Math and RLA. Alternatively, changes that decrease segregation have little effect on test scores. Panel D shows effects on White-Black test score gaps. Increases in segregation increase test score gaps between White and Black students, and vice versa. Again, these effect sizes are particularly large in earlier grades, with little evidence of large effects in grades 6 to 8.

In sum, this decomposition of my difference-in-differences results suggests that changes in White-Black test score gaps are driven by differences in Black students' responses to boundary changes. While any boundary changes impose disruption and lead to small reductions in the scores of White students, test scores of Black students fall most sharply in response to school boundary changes that increase segregation, and much less in response to school boundary changes that decrease segregation.

Table 6 also provides a sense the magnitude of my effect sizes. Relative to schools that decrease segregation, segregationary changes in school boundaries increase the White-Black test score gap by nearly 0.04 standard deviations. This corresponds to roughly 6 percent of the average gap in Table 1: 0.65 standard deviations. The estimates in Billings et al. (2014) also provide useful point of reference. The authors find that a large-scale rezoning of schools in Charlotte-Mecklenburg

Schools "widened the racial gap in high school math scores by about 0.025 standard deviations." The magnitude of my test score effects is in line with this estimate, despite much smaller average effect on levels of segregation.[20] However, I note that test score outcomes in Billings et al. (2014) are captured in high school; my results suggest that changes in segregation in earlier grades may generate larger changes in test scores.

I test for the presence of heterogeneity in Appendix Table B4. To do so, I estimate effects after limiting my sample to subsamples of districts based on composition and enrollment levels prior to 2013. Broadly, I find the most evidence for effects on test score gaps in districts with high shares of Black students and higher enrollments. However, the results in Appendix Table B4 likely reflect changes in precision, rather than changes in effect sizes; estimated effect sizes for districts with relatively fewer Black students and relatively lower enrollments are very imprecise, and the confidence intervals have significant overlap with main estimates.

3.3.1 Mechanisms

What explains the changes in test score gaps that arise in response to changes in school boundaries? To test for potential mechanisms, I test whether changes in segregation generate changes in school resources or school operations, and whether these changes differ between Black and White students. To do so, I estimate Equation 2 using discrete definitions of treatment. As before, district-grades for which $\Delta \hat{Diss}_{dg}$ is greater than 0.02 or less than -0.02 are identified as increasing or decreasing districts, respectively. For all outcomes, I estimate effects on all students, effects on White and Black students separately, and effects on within-district gaps between White and Black students.

My results are shown in Table 7. Column 1 estimates effects on the share of students in a new school, where "new" refers to schools that first appear in NCES data after 2013. Increases and decreases in segregation are accompanied by increases in the share of students new schools. Both changes increase the share of students attending new schools by 1.5 to 2.5 percentage points. This is true of both White and Black students separately (shown in Panels B and C, respectively). However, in districts that decrease segregation, Black students are 0.4 percentage points more

[20] Billings et al. (2014) Appendix Figure A2 shows that the district dissimilarity indices in Charlotte-Mecklenburg schools increased by roughly 0.1 units between the year preceding rezoning and the year following.

likely to be assigned to new schools (relative to their White peers in the same district) in the following years, as indicated by the coefficient on *Post* × *Decrease* in Panel D.

In Column 2 I test whether changes in segregation affect the assignment of students to historically higher-scoring schools. I refer to the assignment of students to historically higher-scoring schools as "assignment quality." To construct a measure of assignment quality at the school-level, I use school-by-grade level proficiency data from EdFacts from 2009 to 2012. I construct residual proficiency rates for each school by regressing proficiency rates on state-by-year-by-grade fixed effects and controls for enrollment and racial composition.[21] Then, I take average residuals at the school level, and standardize this series to be mean 0 and standard deviation 1. Denote each school's "quality" as q_s. To calculate an annual measure of assignment quality, I take the enrollment weighted average of q_s in each year.[22]

The results in Column 2 suggest that increases and decreases in segregation are coincident with increases in the assignment quality for all students. Effect sizes are particularly large for Black students in districts that impose segregationary boundary changes, suggesting that local administrators may face a tradeoff between increasing the assignment of students to historically higher-achieving schools and reducing segregation. However, effects on White-Black differences in assignment quality (in Panel D) are zero, suggesting that changes in test scores are not driven by reassignment of White students to historically higher-scoring schools or reassignment of Black students to historically lower-scoring schools.

Columns 3 through 6 investigate whether changes in segregation are coincident with changes in school operations, based on CRDC data. These data are available only for odd-numbered years between 2009 and 2017, which explains changes in the number of observations across columns and also leads my estimates to be more imprecise than others.

Column 3 estimates effects on total suspensions per pupil. My effects on all students, and Black and White students separately are imprecise, but I find suggestive evidence that decreases in segregation affect differences in Black-White exposure to suspensions. In particular, districts that decrease segregation have smaller differences in White minus Black suspension exposure; these effects are driven by reductions in exposure of Black students to suspensions (shown in

[21]Specifically, I reress school-by-grade level proficiency on the log of grade-specific enrollment, the grade-specific share of students who are Hispanic, White, and Black, and state-by-year-by-grade fixed effects.

[22]This measure does not attempt to quantify the quality of new schools, which are excluded from this analysis.

Panel B). The magnitude of this change in gaps is 0.003, roughly 6 percent of the sample mean (for all students) in Table 1. Columns 4 and 5 estimate effects on exposure to gifted and talented programs and inexperienced teachers, showing little evidence of changes in access to either.

In other contexts, these inputs are shown to be meaningful determinants of achievement. For example, Lafortune and Schönholzer (2021) find that access to new school facilities has large effects on student achievement in Los Angeles and Bacher-Hicks et al. (2019) find long-term impacts of high suspension rates on academic and later-life outcomes. While my work cannot disentangle the effects of these school inputs from other effects of segregation (namely, differences in peers or differences in unobserved measures of school quality), these results suggest that changes in within-district access to school quality may drive some of the changes in test score gaps I observe.

3.4 House Price Effects of School Reassignment

The results in the preceding section suggest that school boundary changes can meaningfully affect racial achievement gaps: following changes to school zones, districts where these changes increased segregation see their racial test score gaps expand, relative to districts where changes decreased segregation. In this section, I investigate the effects of changes in school zones within a district. In particular, I test whether neighborhoods that were newly-assigned to schools with higher shares of URM students experience house price declines in subsequent years. To do so, I compare the evolution of house prices in census tracts that saw increases in the URM share of assigned schools to tracts that saw decreases in the URM share of assigned schools.

My main results are presented visually in Figure 7. The coefficients plotted in Figure 7 are β_t in Equation 6, which represent the effect of changes in the URM share of a neighborhood's assigned school on local house prices. The first series shows results with respect to all districts. Using all districts, estimated effects are extremely noisy. This is, in part, due to the inclusion of very large districts where district-by-year fixed effects explain relatively little of the tract level variation in house prices. Limiting my sample to small districts increases my precision substantially. The results indicate that a 100% increase in the predicted URM share of a neighborhood's assigned school leads to a house price decline approaching 20% in subsequent years. Observed changes in school assignment are never this extreme—more realistically, my estimates suggest that reassignment to a school with 10% more URM students leads to a 2% reduction in house prices. This effect exhibits little evidence of pre-trends, and grows in the years following 2014. Figure 7 additionally displays results after making an additional sample restriction: excluding tracts that are covered by more than one school. These results are consistent those described above: reassignment to a higher-URM school leads to reductions in house prices.

I summarize these results in Table 8. Column 1 of Table 8 estimates the house price effects of reassignment to higher-URM schools, limiting my sample to small districts. Consistent with Figure 7, these estimates suggest that changes to school zones that increase the URM share of a neighborhood's predicted school assignment decreases house prices. Average post-2013 effects suggest that reassignment to a school with 10% more URM students leads to a 1.6% decrease in house prices.

Investigating further, Columns 2, 3, and 4 of Table 8 estimate the effects of various other changes to the characteristics of assigned schools. In Columns 2 and 3, I estimate separate effects for changes in the predicted share of Black and Hispanic students, respectively. The results suggest that the negative house price effects in Column 1 are driven primarily by changes in the predicted share of Hispanic students. (Corresponding event study coefficients for these estimates can be found in Column 1 of Appendix Table B6 and Column 1 of Appendix Table B7.)

In Column 4 of Table 8, I assess whether house prices increase in response to assignment to a historically higher-performing school. To do so, I use the measures of school quality described in Section 3.3.1, where each school's quality is measured based on pre-2013 test score performance from EdFacts, residualized against state-by-year-by-grade fixed effects and controls for enrollment and racial composition. My measure of school quality q_s is equal to average school-level residuals, normalized to have mean 0 and standard deviation 1. (I set q_s equal to 0 for new schools.) The results in Column 4 of Table 8 show little relationship between changes in school quality and changes in house prices. (However, event study coefficients, shown in Appendix Table B8 show some evidence of increases in later years.) Results in Column 5 indicates that the effects of changes in predicted racial composition are robust to the inclusion of measures of school quality.

Columns 5 through 9 of Table 8 repeat these analyses after making an additional restriction: excluding tracts that are covered by more than one school. Broadly, these results are consistent those in Columns 1 through 5: effects of URM share and Hispanic share are consistently negative, while effects on Black share and school quality are insignificant.

To address potential concerns about the use of continuous measure in difference-in-differences models, in Appendix Figure B2 I show that my results are robust to a discrete specification that estimates effects on districts that increased and decreased segregation separately, using various thresholds to define these groups. Across a number of different thresholds and using my preferred data restrictions, the estimates in Appendix Figure B2 show that census tracts that are newly assigned to higher (lower) URM schools have lower (higher) house prices in subsequent years. Moreover, the results in Appendix Figure B2 indicate that the effects I measures are driven equally by two forces: house price increases in tracts where assigned school URM share decreases and house prices decreases in tracts where assigned school URM share increases.

I report event study coefficients for all four measures of changes in Appendix Tables B5 through B8. These tables additionally report the results of analyses of mortgage volumes in HMDA data. Broadly, I find little evidence that neighborhood mortgage volumes, both overall volumes and race-specific volumes, respond systematically in response to changes in the racial makeup of assigned schools, but I note that standard errors in these regressions tend to be larger than those in house price regressions. Mortgage volumes do appear to increase in response to neighborhood reassignment to historically higher-scoring schools, as shown in Table B8. The magnitudes in Column 2 suggest that a one standard deviation increase in test-based school quality increases the inverse hyperbolic sine of mortgage volumes by 0.06, equivalent to an increase from 100 to 106.2 mortgages per tract per year, or from 1,000 to 1,061.8 mortgages per tract per year. However, I caution that these results do vary across years and across samples.

Broadly, the limited effects of school quality is likely due, in part, to the role of new schools. Many of the changes in school assignment I observe are driven by opening new schools, which are not captured by my approach to quantifying school quality using prior test scores. Still, my results point to the persistent role of race in the American markets for education. I provide a broader discussion and conclude in the section below.

4 Conclusion

Racial segregation is a persistent characteristic of K12 education in the US, affected by multiple boundaries that assign students to schools, translating residential segregation into school segregation. In this paper, I examine the effects on one particular set of boundaries—school attendance zones—and identify how changes in these boundaries affect districts, students, and neighborhoods.

The results in this paper suggest that district-level rezoning can meaningfully change levels of segregation in a district, and that these changes have the potential to shrink or exacerbate long-standing gaps in standardized test performance between Black and White students. Importantly, these gaps are driven primarily by differential responses of Black students to segregationary versus integrative changes, with little evidence of differential effects for White students. I find suggestive evidence that these changes also affect within-district gaps in school quality: decreases in segregation increases Black students' representation in new schools and decreases their exposure to suspensions, relative to their White peers in the same district.

These changes also have consequences for neighborhood house prices: neighborhoods that are newly-assigned to schools with higher shares of URM students in 2015 see their house prices fall substantially in subsequent years, suggesting that parents have strong preferences for the racial composition of their children's schools. To the degree that these changes are predictive of longer-term changes in neighborhood demographics—specifically, whether these house price reductions reflect or precede "White flight"—some attempts at desegregation may be doomed to fail: attempts to integrate via school boundaries may be met with changes in residential segregation that ultimately resegregate schools. I find little evidence of this phenomenon in school enrollment data, but I caution that my results are over a relatively short period: four years following changes to school boundaries.

My work highlights important avenues for future work. First, my effects include only a small set of boundary changes over a relatively short period. Incorporating data for a larger set of boundary changes may help identify the effects of local segregation with more precision and over a longer period. Moreover, longer-term changes may better identify the degree to which changes in school assignment precede "White flight" or other changes in local demographics. Finally, while my work suggests potential mechanisms through which local segregation affects achievement, I cannot fully disentangle the effects of changes in school quality versus changes in peers. Future work may better disentangle these two, offering policy guidance for reducing achievement gaps.

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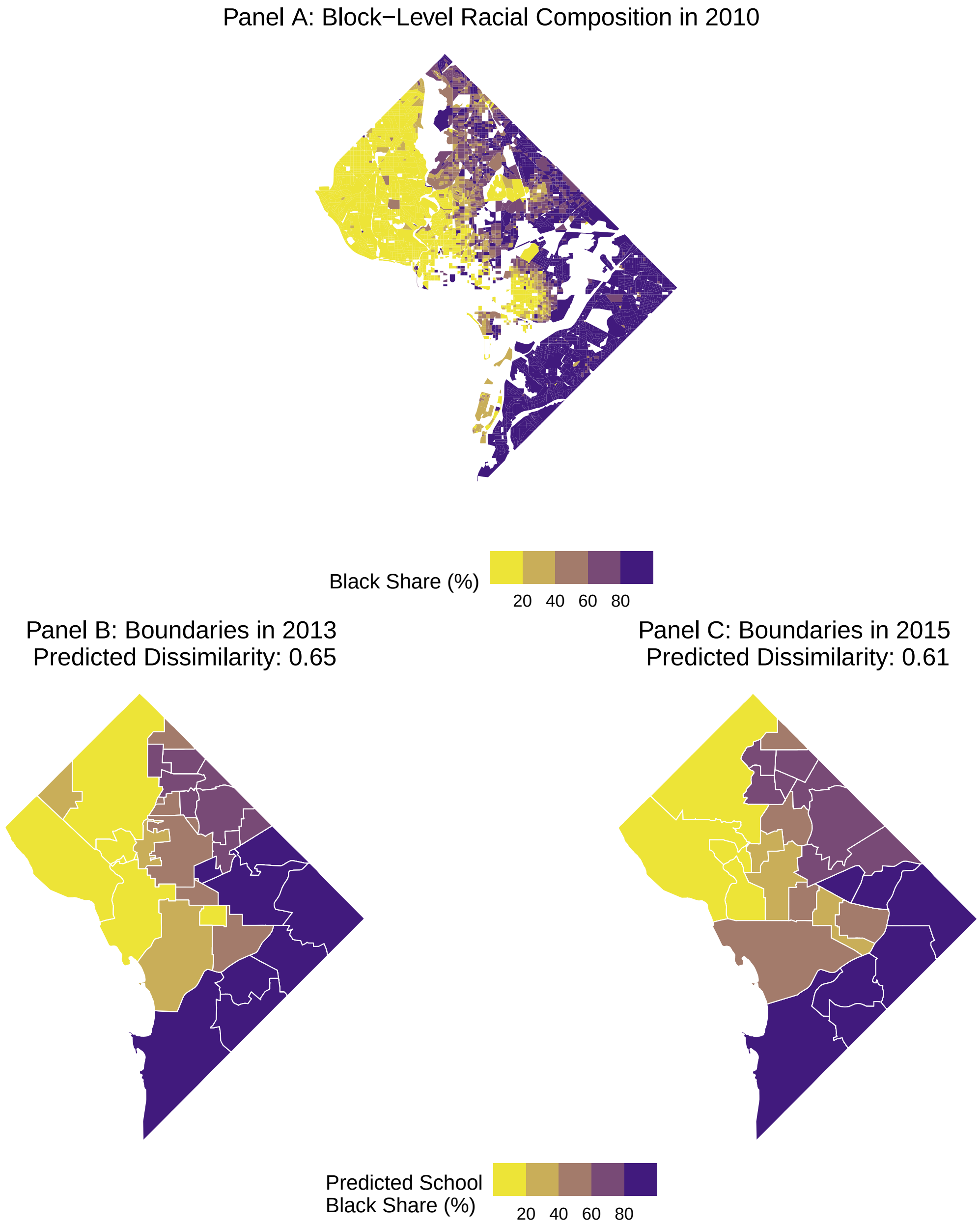
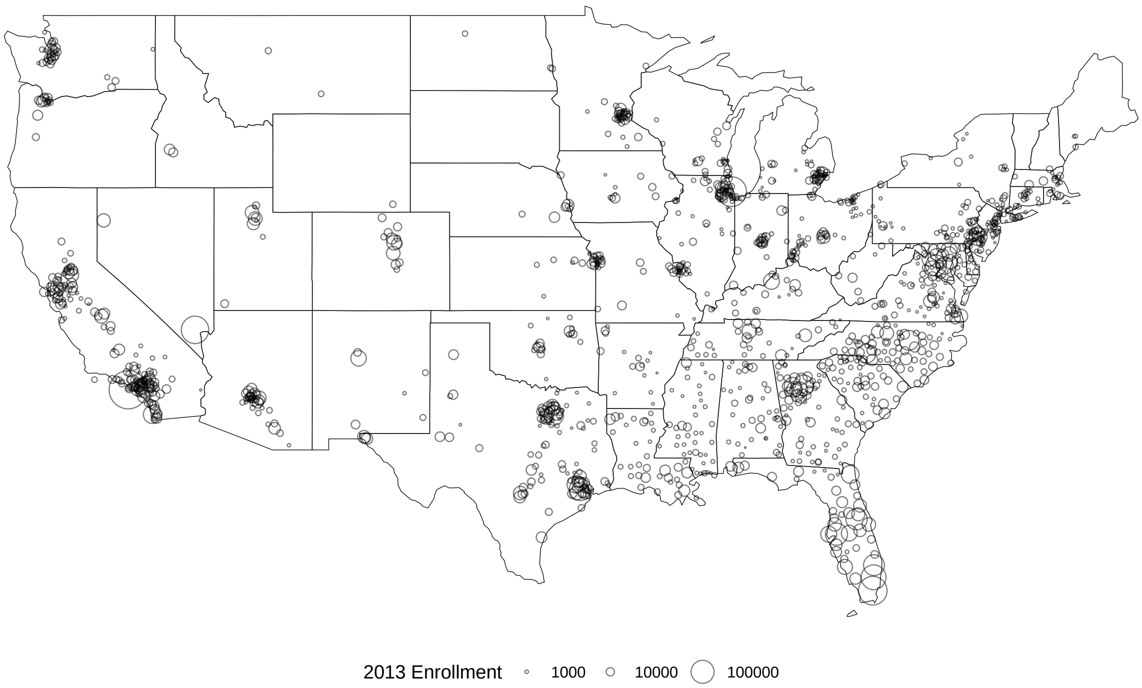
Figure 1: Changes in School Zones for 8th Grade in Washington, DC



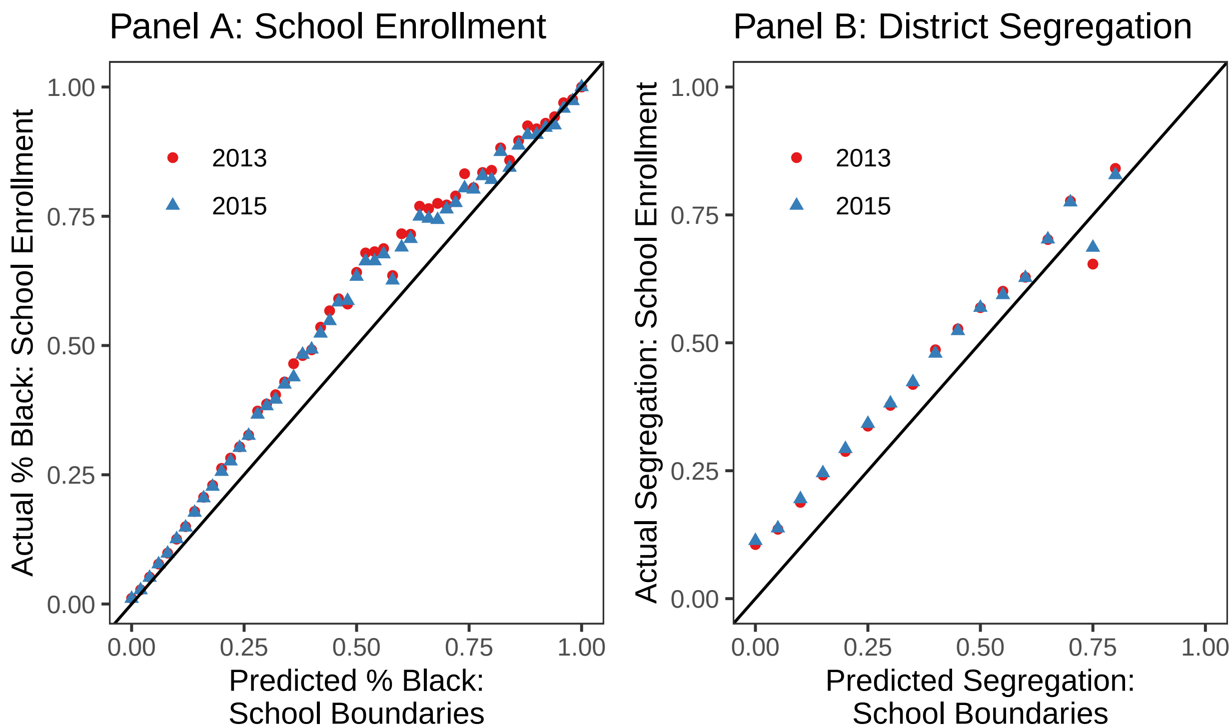
Notes: Figure displays changes in the school attendance zones for 8th grade students in District of Columbia Public Schools. Panel A displays, for each relevant census block, the Black share of the population from the 2010 Census. Panel B displays school attendance boundaries in 2013, and the corresponding predicted share of Black students (based on the population of overlapping census blocks). Panel C displays school attendance boundaries in 2015, and the corresponding predicted share of Black students (based on the population of overlapping census blocks).

Figure 2: Map of Sample School Districts



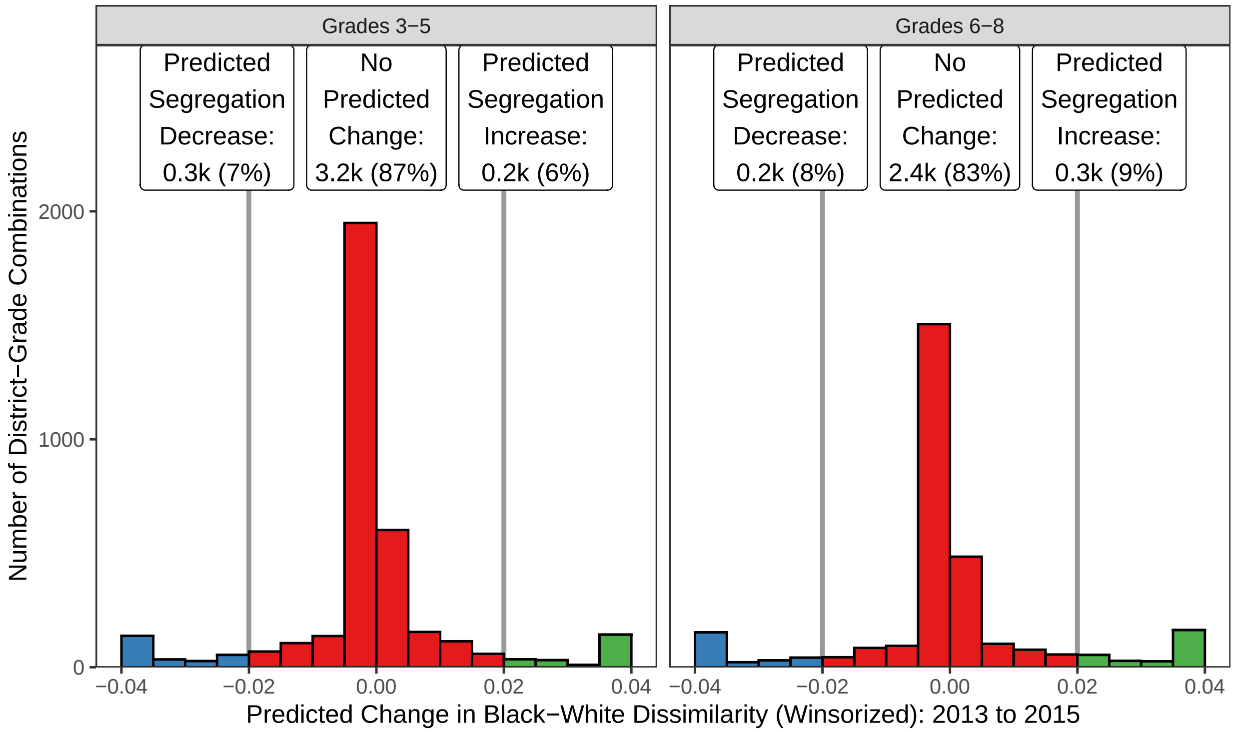
Notes: Figure displays the location of districts in my sample. These districts are those for which, in at least one grade in grades 3 through 8, (a) SABS data is available for both 2013 and 2015, (b) SABS data indicates the district had at least 2 non-open enrollment schools in 2013 and 2015, (c) NCES data indicates that the district-grade enrolled at least 20 Black students and 20 White students in 2013, (d) SEDA data reports White-Black gaps for at least one year. Altogether, these restrictions produce a set of 1,401 districts. Figure omits 2 districts in Alaska and 1 district in Hawaii.

Figure 3: Predicted versus Actual Enrollment and Segregation



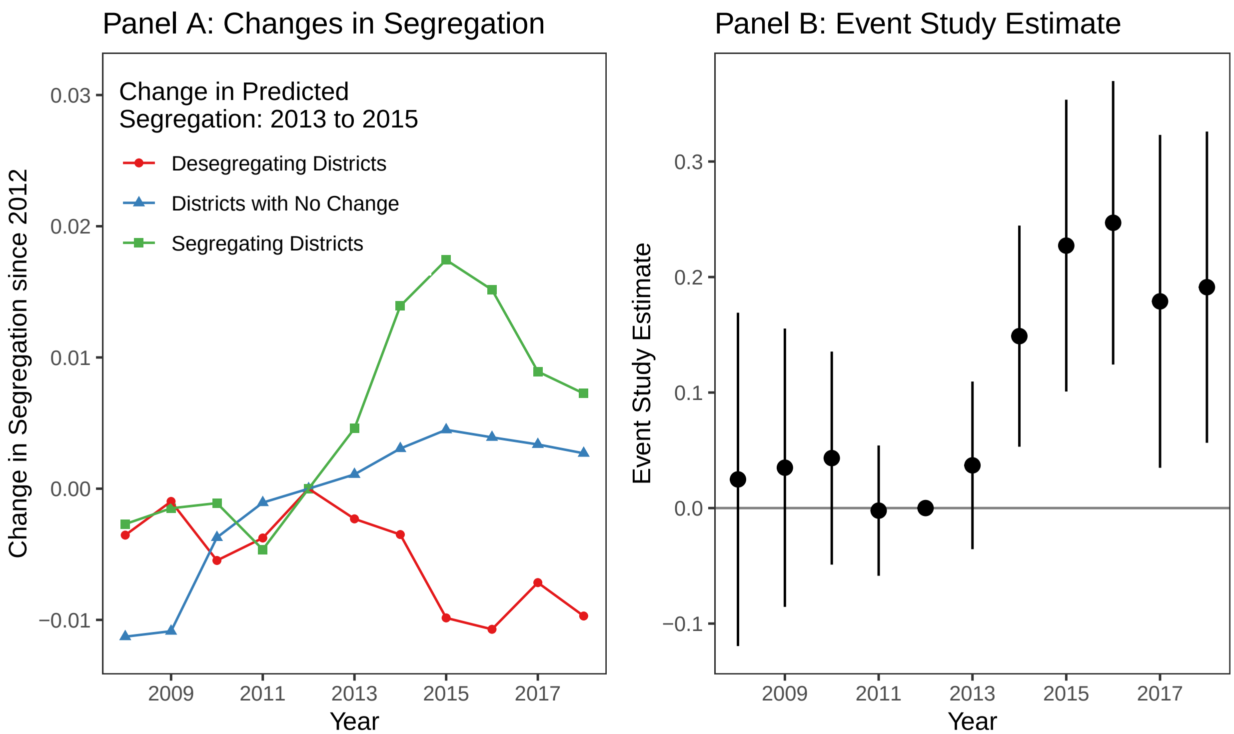
Notes: Figure displays the relationship between predicted racial measures (based on school boundaries) and actual measures (based on reported enrollments). Panel A displays the relationship between predicted and actual racial composition at the school level. Panel B displays the relationship between predicted and actual racial segregation at the district-by-grade level. Segregation is measured using White-Black dissimilarity, which is bounded between zero and one and represents the share of Black students that would need to be reallocated to equalize their share across schools.

Figure 4: Distribution of $\Delta \hat{Diss}_{dg}$: Predicted Change in District Dissimilarity



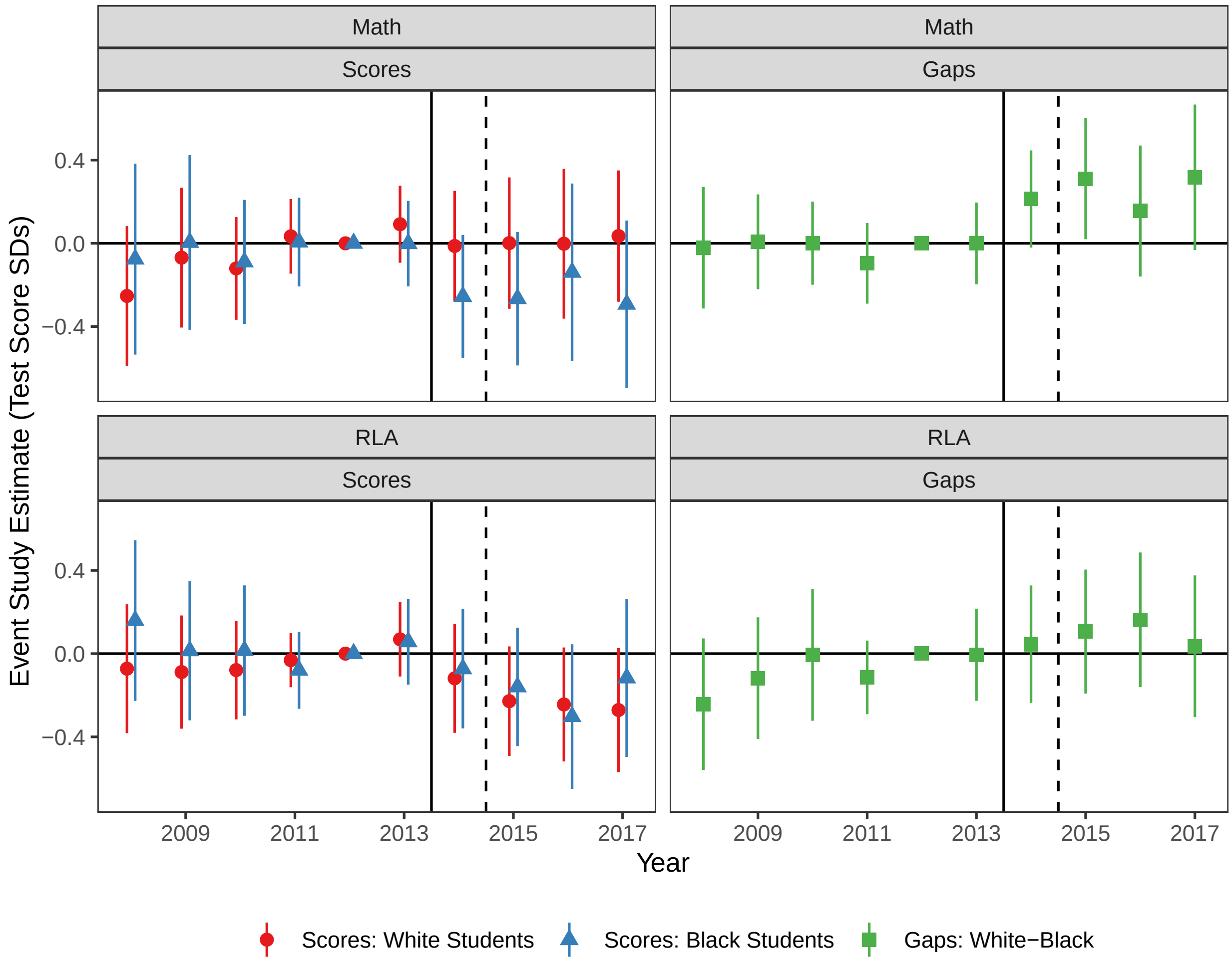
Notes: Figure displays the distribution of $\Delta \hat{Diss}_{dg}$, a measure of the anticipated effects of changes in school boundaries on segregation in the district, separately for different grade ranges. Segregation is measured using Black-White dissimilarity, which is bounded between zero and one and represents the share of Black students that would need to be reallocated to equalize their share across schools. In the figure, $\Delta \hat{Diss}_{dg}$ is winsorized to trim its distribution to 0.04 from above and -0.04 from below.

Figure 5: Effect of Changes in School Attendance Boundaries on District Segregation



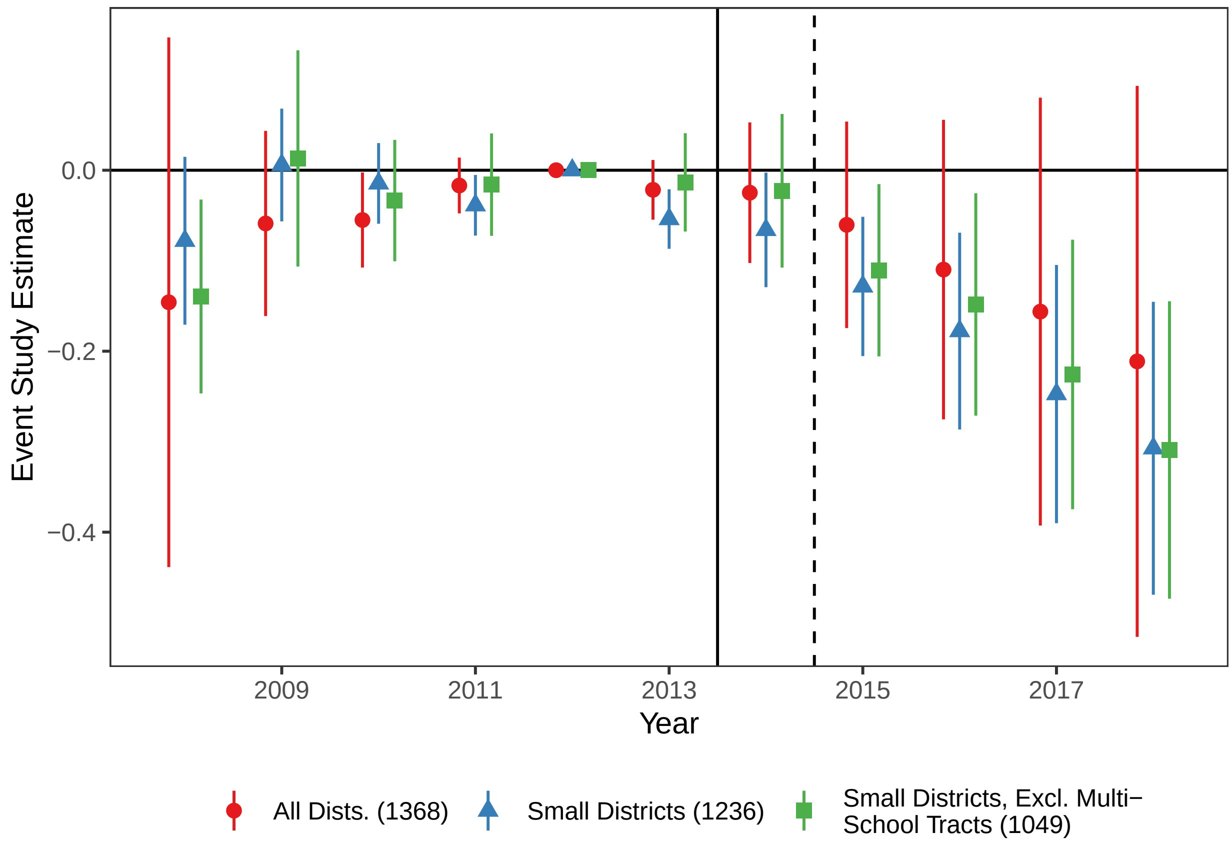
Notes: Figure displays estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on actual segregation. Panel A displays changes in actual segregation since 2012 for three groups, defined based on their predicted changes in segregation (based on school boundaries) between 2013 and 2015. Panel B displays estimates of Equation 3. Coefficients measure the effect of predicted segregation changes between 2013 and 2015 on actual segregation over time. Confidence bands reflect 95 confidence intervals. Segregation is measured using Black-White dissimilarity, which is bounded between zero and one and represents the share of Black students that would need to be reallocated to equalize their share across schools.

Figure 6: Effect of Changes in Segregation on Test Scores of Black and White Students



Notes: Figure displays estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on test scores of Black and White students. Coefficients measure the effect of predicted segregation changes between 2013 and 2015 on test scores and test score gaps over time. Reported coefficients are β_t in Equation 3. All test score regressions are precision weighted, where each observation is weighted by the inverse of the variance. All test score outcomes are in standard deviation units. Confidence bands reflect 95 confidence intervals.

Figure 7: Effect of Neighborhood School Assignment Changes on House Prices



Notes: Figure displays the difference-in-differences estimates of the effect of changes in the URM share of neighborhood schools (based on changes in school attendance boundaries) on house prices. Reported coefficients are β_t in Equation 6. All regressions are weighted by census tract population in 2010.

Table 1: District-Grade Characteristics as of 2013

Statistic	N	Mean	St. Dev.	Pctl(25)	Pctl(75)
Panel A: District-by-Grade Characteristics					
Total Enrollment	6,630	1,519.22	2,688.53	459	1,636
Total Black Enrollment	6,630	302.50	737.39	44	233
Total Hispanic Enrollment	6,630	513.17	1,564.95	57	464.8
Total White Enrollment	6,630	612.45	744.12	191	705.8
Black-White Dissimilarity	6,630	0.32	0.16	0.21	0.43
Predicted Black-White Dissimilarity	6,630	0.24	0.14	0.14	0.33
Predicted Black-White Change (2013-2015)	6,630	-0.0002	0.03	-0.0004	0.001
Panel B: Test Scores					
Mean Math Score (SDs)	4,431	0.004	0.37	-0.25	0.25
Mean Math Score (SDs): Black Students	4,309	-0.42	0.30	-0.63	-0.21
Mean Math Score (SDs): White Students	4,360	0.24	0.34	0.01	0.46
Mean White-Black Math Score Gap (SDs)	4,248	0.65	0.28	0.47	0.82
Panel C: School Operations					
Suspensions PP	6,450	0.05	0.07	0.01	0.07
Sch. has Gifted Talented	6,614	0.80	0.36	0.86	1.00
Share Inexp. Teachers	6,604	0.06	0.05	0.03	0.08

Notes: Table displays summary statistics for district-by-grades in my sample. All data is as of 2013. Missing test score data reflect non-reporting by districts due to state data availability and anonymization rules for small student populations.

Table 2: Determinants of School Boundary Changes

	Increase Segregation (0/1)				Decrease Segregation (0/1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ No. Schools ′13 to ′15 (%)	0.112**			0.115**	0.058			0.059
	(0.049)			(0.049)	(0.045)			(0.046)
District Growth Index ′10 to ′12		0.014		0.013		−0.026		−0.026
		(0.019)		(0.019)		(0.020)		(0.020)
Crowding Index ′10 to ′12			0.039**	0.042***			0.007	0.008
			(0.016)	(0.016)			(0.015)	(0.015)
log(Enrollment ′12)	−0.016	−0.011	−0.005	−0.013	−0.040***	−0.034**	−0.036***	−0.036***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.014)	(0.014)	(0.014)	(0.014)
log(No. Schools ′13)	0.024*	0.016	0.006	0.014	0.035**	0.029*	0.029*	0.031**
	(0.012)	(0.013)	(0.013)	(0.012)	(0.014)	(0.015)	(0.015)	(0.015)
No. Districts	1401	1401	1401	1401	1401	1401	1401	1401
Dep. Var. Mean	0.074	0.074	0.074	0.074	0.075	0.075	0.075	0.075
Observations	6,630	6,630	6,630	6,630	6,630	6,630	6,630	6,630
R^2	0.010	0.005	0.006	0.012	0.007	0.006	0.005	0.008
Adjusted R^2	0.009	0.003	0.005	0.010	0.006	0.005	0.004	0.006

Notes: Table displays estimates of the determinants of school boundary changes. Outcome variables are binary variables indicating whether a district increased or decreased predicted segregation between 2013 and 2015. Δ No. Schools represents the change in the number of schools between 2015 and 2013, as a share of total schools in 2013. District Growth Index represents the growth in log district enrollments between 2010 and 2012. Crowding Index is equal to the difference in school-level enrollment growth at the 75th and 25th percentile. Both District Growth Index and Crowding Index are converted to percentiles ranging from 0 to 1. *** p<0.01; ** p<0.05; * p<0.1.

Table 3: Correlation Coefficients: Changes in Predicted Segregation Across Grade Levels

	3	4	5	6	7	8
3		0.885	0.772	0.307	0.229	0.216
4	0.885		0.828	0.344	0.255	0.251
5	0.772	0.828		0.311	0.235	0.215
6	0.307	0.344	0.311		0.566	0.536
7	0.229	0.255	0.235	0.566		0.906
8	0.216	0.251	0.215	0.536	0.906	

Notes: Table displays correlation coefficients between changes in predicted segregation across different grades within the same district.

Table 4: Effects of School Attendance Boundary Changes on Segregation

	Dep. Var.: Dissimilarity Index				
	Continuous Treatment	Discrete $\Delta \hat{Diss}_{dg}$ Cutoff			
		0.01	0.02	0.05	0.1
Panel A: All Districts					
	(1)	(2)	(3)	(4)	(5)
Post $\times \Delta \hat{Diss}_{dg}$	0.201^{***}				
	(0.062)				
Post $\times$ Increase		0.004	0.006	0.012	0.025
		(0.003)	(0.004)	(0.008)	(0.020)
Post $\times$ Decrease		-0.007^{**}	-0.013^{***}	-0.018^{**}	-0.051^{***}
		(0.003)	(0.004)	(0.008)	(0.013)
Share Increasing	-	0.12	0.074	0.029	0.007
Share Decreasing	-	0.121	0.075	0.028	0.007
No. Districts	1401	1401	1401	1401	1401
Observations	72,903	72,903	72,903	72,903	72,903
R^2	0.865	0.865	0.865	0.865	0.865
Adjusted R^2	0.851	0.851	0.851	0.851	0.851
Panel B: Only Changers					
Post $\times$ Increase		0.011^{**}	0.019^{***}	0.030^{**}	0.076^{***}
		(0.004)	(0.006)	(0.012)	(0.025)
Share Increasing	-	0.12	0.074	0.029	0.007
Share Decreasing	-	0.121	0.075	0.028	0.007
No. Districts	-	526	363	157	44
Observations	-	17,600	10,890	4,158	1,045
R^2	-	0.877	0.871	0.856	0.843
Adjusted R^2	-	0.865	0.858	0.841	0.826

Notes: Table displays difference-in-differences estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on actual segregation. Reported coefficients are β in Equation 2. Columns 2 to 5 use discrete variables to identify districts with predicted increases or decreases in segregation. To limit the effect of large outliers, I winsorize $\Delta \hat{Diss}_{dg}$ such that no value is above 0.1 or below -0.1. Panel B restricts the estimation sample to only districts with predicted increases or decreases in segregation. *** p<0.01; ** p<0.05; * p<0.1.

Table 5: Effect of School Boundary Changes on Test Scores of Black and White Students

	Grades 3-8		Grades 3-5		Grades 6-8	
	Math	RLA	Math	RLA	Math	RLA
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Black-White Dissimilarity						
Post x $\Delta \hat{Diss}_{dg}$	0.210***	0.210***	0.189**	0.189**	0.233**	0.233**
	(0.071)	(0.071)	(0.091)	(0.091)	(0.097)	(0.097)
Observations	51,284	51,284	30,115	30,115	21,169	21,169
Panel B: White Students' Test Scores						
Post x $\Delta \hat{Diss}_{dg}$	0.051	−0.184	0.297	−0.143	−0.091	−0.198
	(0.154)	(0.119)	(0.245)	(0.169)	(0.183)	(0.142)
Observations	51,284	51,284	30,115	30,115	21,169	21,169
Panel C: Black Students' Test Scores						
Post x $\Delta \hat{Diss}_{dg}$	−0.219	−0.178	−0.223	−0.168	−0.196	−0.170
	(0.179)	(0.169)	(0.258)	(0.232)	(0.187)	(0.191)
Observations	51,284	51,284	30,115	30,115	21,169	21,169
Panel D: White-Black Test Score Gap						
Post x $\Delta \hat{Diss}_{dg}$	0.272**	0.164	0.371**	0.220	0.194	0.115
	(0.117)	(0.152)	(0.160)	(0.164)	(0.157)	(0.181)
Observations	51,284	51,284	30,115	30,115	21,169	21,169

Notes: Table displays difference-in-differences estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on test scores of Black and White students. Reported coefficients are β in Equation 2. To limit the effect of large outliers, I winsorize $\Delta \hat{Diss}_{dg}$ such that no value is above 0.1 or below -0.1. All test score regressions are precision weighted, where each observation is weighted by the inverse of the variance. All test score outcomes are in standard deviation units. *** p<0.01; ** p<0.05; * p<0.1.

Table 6: Discrete Effects of School Boundary Changes on Test Scores of Black and White Students

	Grades 3-8		Grades 3-5		Grades 6-8	
	Math	RLA	Math	RLA	Math	RLA
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Black-White Dissimilarity						
Post x Increase	0.008*	0.008*	0.008	0.008	0.008	0.008
	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)
Post x Decrease	−0.011**	−0.011**	−0.010*	−0.010*	−0.014**	−0.014**
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
p(Inc. = Dec.)	0.003	0.003	0.039	0.039	0.015	0.015
Observations	51,284	51,284	30,115	30,115	21,169	21,169
Panel B: White Students' Test Scores						
Post x Increase	0.004	−0.025***	0.027	−0.027**	−0.007	−0.020*
	(0.013)	(0.009)	(0.020)	(0.012)	(0.016)	(0.011)
Post x Decrease	−0.017	−0.016	−0.025	−0.030**	−0.008	−0.002
	(0.016)	(0.011)	(0.022)	(0.014)	(0.017)	(0.012)
p(Inc. = Dec.)	0.281	0.484	0.074	0.858	0.974	0.219
Observations	51,284	51,284	30,115	30,115	21,169	21,169
Panel C: Black Students' Test Scores						
Post x Increase	−0.026*	−0.031**	−0.027	−0.039**	−0.015	−0.019
	(0.015)	(0.013)	(0.020)	(0.016)	(0.018)	(0.019)
Post x Decrease	−0.003	−0.012	−0.006	−0.020	0.010	0.001
	(0.015)	(0.010)	(0.022)	(0.016)	(0.018)	(0.012)
p(Inc. = Dec.)	0.235	0.231	0.461	0.37	0.279	0.349
Observations	51,284	51,284	30,115	30,115	21,169	21,169
Panel D: White-Black Test Score Gap						
Post x Increase	0.026**	0.018	0.023	0.015	0.024	0.017
	(0.012)	(0.014)	(0.014)	(0.015)	(0.015)	(0.017)
Post x Decrease	−0.011	−0.006	−0.030***	−0.023**	−0.001	0.004
	(0.008)	(0.008)	(0.011)	(0.010)	(0.012)	(0.011)
p(Inc. = Dec.)	0.007	0.127	0.003	0.029	0.164	0.5
Observations	51,284	51,284	30,115	30,115	21,169	21,169

Notes: Table displays difference-in-differences estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on test scores of Black and White students. District-grades for which $\Delta \hat{Diss}_{dg}$ is greater than 0.02 or less than -0.02 are identified as increasing or decreasing districts, respectively. Rows for p(Inc. = Dec.) displays p-values of a test for the equality of displayed coefficients. All test score regressions are precision weighted, where each observation is weighted by the inverse of the variance. All test score outcomes are in standard deviation units. *** p<0.01; ** p<0.05; * p<0.1.

Table 7: Effect of Changes in Segregation on School Operations

	Share in New School	Assignment Quality	Total Suspensions PP	Gifted & Talented	Share Inexp. Teachers
	(1)	(2)	(3)	(4)	(5)
Panel A: Effects on All Students					
Post x Increase	0.021***	0.018***	−0.006	−0.008	−0.002
	(0.004)	(0.007)	(0.004)	(0.015)	(0.003)
Post x Decrease	0.016***	0.014*	−0.001	−0.017	−0.004
	(0.004)	(0.008)	(0.003)	(0.019)	(0.004)
p(Inc. = Dec.)	0.405	0.713	0.272	0.671	0.644
Observations	71,496	70,386	32,634	32,899	32,886
Panel B: Effects on White Students					
Post x Increase	0.020***	0.018**	−0.006	−0.010	−0.003
	(0.004)	(0.008)	(0.003)	(0.015)	(0.003)
Post x Decrease	0.014***	0.004	0.001	−0.020	−0.003
	(0.004)	(0.009)	(0.003)	(0.019)	(0.003)
p(Inc. = Dec.)	0.245	0.236	0.113	0.638	0.901
Observations	71,490	70,340	32,627	32,892	32,879
Panel C: Effects on Black Students					
Post x Increase	0.019***	0.022***	−0.006	−0.011	−0.002
	(0.004)	(0.008)	(0.004)	(0.016)	(0.003)
Post x Decrease	0.018***	0.012	−0.003	−0.015	−0.004
	(0.004)	(0.010)	(0.003)	(0.020)	(0.004)
p(Inc. = Dec.)	0.879	0.468	0.452	0.85	0.594
Observations	71,474	70,239	32,622	32,889	32,875
Panel D: Within-District White - Black Difference					
Post x Increase	0.001	−0.005	−0.0004	0.002	−0.0003
	(0.002)	(0.007)	(0.001)	(0.004)	(0.001)
Post x Decrease	−0.004**	−0.005	0.003***	−0.004	0.001
	(0.002)	(0.010)	(0.001)	(0.003)	(0.001)
p(Inc. = Dec.)	0.085	0.964	0.031	0.256	0.318
Observations	71,471	70,209	32,615	32,882	32,868

Notes: Table displays difference-in-differences estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on school operations. District-grades for which $\Delta \hat{Diss}_{dg}$ is greater than 0.02 or less than -0.02 are identified as increasing or decreasing districts, respectively. Rows for p(Inc. = Dec.) displays p-values of a test for the equality of displayed coefficients. "New" schools refers to schools that first appear in NCES after 2013. Assignment quality measures the assignment of students to historically higher-scoring schools; units are in standard-deviations of school quality. Columns 3 through 5 use data from CRDC, which is reported only in odd years between 2009 and 2017. *** p<0.01; ** p<0.05; * p<0.1.

Table 8: Effect of Neighborhood School Assignment Changes on House Prices

	Small Districts					Small Districts, Excl. Multi-School Tracts				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\Delta \hat{URM}_{tg}$ x Post	-0.16^{***}				-0.15^{***}	-0.13^{**}				-0.14^{**}
	(0.05)				(0.05)	(0.06)				(0.06)
$\Delta \hat{Black}_{tg}$ x Post		-0.02					0.03			
		(0.04)					(0.06)			
$\Delta \hat{Hispanic}_{tg}$ x Post			-0.36^{***}					-0.36^{***}		
			(0.11)					(0.09)		
$\Delta \hat{Quality}_{tg}$ x Post				0.01	0.002				0.002	-0.003
				(0.01)	(0.01)				(0.01)	(0.01)
No. Districts	1236	1236	1236	1236	1236	1049	1049	1049	1049	1049
Observations	749,375	749,375	749,375	749,375	749,375	279,708	279,708	279,708	279,708	279,708
R^2	0.95	0.95	0.95	0.95	0.95	0.96	0.96	0.96	0.96	0.96
Adjusted R^2	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94	0.94

Notes: Table displays the difference-in-differences estimates of the effect of changes in the characteristics of neighborhood schools (based on changes in school attendance boundaries) on house prices. Reported coefficients are β in Equation 5. All regressions are weighted by census tract population in 2010. $\hat{URM}_{tg}$ represents the predicted change in the share of URM students for schools attended by students in grade g in census tract t between 2013 and 2015 (and similarly for $\hat{Black}_{tg}$ and $\hat{Hispanic}_{tg}$). $\hat{Quality}_{tg}$ represents the predicted change in the school quality for schools attended by students in grade g in census tract t, where each school's quality is measured based on pre-2013 test score performance from EdFacts, residualized against state-by-year-by-grade fixed effects and controls for enrollment and racial composition. I calculate school quality as the average school-level residuals, normalized to have mean 0 and standard deviation 1.

Appendix A Effects on Hispanic-White Segregation and Test Score Gaps

Table A1: Effects of School Attendance Boundary Changes on Segregation

	Dep. Var.: Dissimilarity Index				
	Continuous Treatment	Discrete $\Delta \hat{Diss}_{dg}$ Cutoff 0.01	0.02	0.05	0.1
Panel A: All Districts					
	(1)	(2)	(3)	(4)	(5)
Post $\times \Delta \hat{Diss}_{dg}$	0.183***				
	(0.059)				
Post × Increase		0.004	0.006	0.011	0.030**
		(0.003)	(0.004)	(0.008)	(0.014)
Post × Decrease		−0.005	−0.006	−0.022**	−0.040**
		(0.003)	(0.004)	(0.009)	(0.016)
Share Increasing	-	0.101	0.061	0.022	0.006
Share Decreasing	-	0.098	0.059	0.017	0.004
No. Districts	1830	1830	1830	1830	1830
Observations	93,296	93,296	93,296	93,296	93,296
R^2	0.847	0.847	0.847	0.847	0.847
Adjusted R^2	0.831	0.831	0.831	0.831	0.831
Panel B: Only Changers					
Post × Increase		0.010**	0.012**	0.032***	0.069***
		(0.004)	(0.006)	(0.012)	(0.022)
Share Increasing	-	0.101	0.061	0.022	0.006
Share Decreasing	-	0.098	0.059	0.017	0.004
No. Districts	-	580	399	141	39
Observations	-	18,576	11,173	3,627	920
R^2	-	0.846	0.831	0.782	0.754
Adjusted R^2	-	0.830	0.813	0.760	0.725

Notes: Table displays difference-in-differences estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on actual segregation. Reported coefficients are β in Equation 2. Columns 2 to 5 use discrete variables to identify districts with predicted increases or decreases in segregation. Panel B restricts the estimation sample to only districts with predicted increases or decreases in segregation. *** p<0.01; ** p<0.05; * p<0.1.

Table A2: Effect of School Boundary Changes on Test Scores of Hispanic and White Students

	Grades 3-8		Grades 3-5		Grades 6-8	
	Math	RLA	Math	RLA	Math	RLA
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Hispanic-White Dissimilarity						
Post x $\Delta \hat{Diss}_{dg}$	0.168***	0.168***	0.190**	0.190**	0.156***	0.156***
	(0.054)	(0.054)	(0.091)	(0.091)	(0.057)	(0.057)
Observations	62,695	62,695	38,917	38,917	23,778	23,778
Panel B: White Students' Test Scores						
Post x $\Delta \hat{Diss}_{dg}$	0.035	−0.132	0.085	−0.121	0.005	−0.131
	(0.105)	(0.141)	(0.203)	(0.164)	(0.118)	(0.169)
Observations	62,695	62,695	38,917	38,917	23,778	23,778
Panel C: Hispanic Students' Test Scores						
Post x $\Delta \hat{Diss}_{dg}$	−0.008	−0.051	−0.0005	−0.128	−0.024	−0.009
	(0.168)	(0.147)	(0.238)	(0.171)	(0.203)	(0.181)
Observations	62,695	62,695	38,917	38,917	23,778	23,778
Panel D: White-Hispanic Test Score Gap						
Post x $\Delta \hat{Diss}_{dg}$	0.082	0.015	0.148	0.074	0.048	−0.011
	(0.082)	(0.091)	(0.118)	(0.119)	(0.108)	(0.115)
Observations	62,695	62,695	38,917	38,917	23,778	23,778

Notes: Table displays difference-in-differences estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on test scores of Hispanic and White students. Reported coefficients are β in Equation 2. All test score regressions are precision weighted, where each observation is weighted by the inverse of the variance. All test score outcomes are in standard deviation units. *** p<0.01; ** p<0.05; * p<0.1.

Appendix B Additional Tables and Figures

Table B1: Effect of School Boundary Changes on Test Scores of Black and White Students (Equal-Weighted)

	Grades 3-8		Grades 3-5		Grades 6-8	
	Math	RLA	Math	RLA	Math	RLA
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Black-White Dissimilarity						
Post x $\Delta \hat{Diss}_{dg}$	0.210***	0.210***	0.189**	0.189**	0.233**	0.233**
	(0.071)	(0.071)	(0.091)	(0.091)	(0.097)	(0.097)
Observations	51,284	51,284	30,115	30,115	21,169	21,169
Panel B: White Students' Test Scores						
Post x $\Delta \hat{Diss}_{dg}$	0.123	−0.159	0.178	−0.191	0.064	−0.126
	(0.158)	(0.131)	(0.241)	(0.178)	(0.193)	(0.154)
Observations	51,284	51,284	30,115	30,115	21,169	21,169
Panel C: Black Students' Test Scores						
Post x $\Delta \hat{Diss}_{dg}$	−0.079	−0.276*	−0.106	−0.377**	−0.051	−0.169
	(0.171)	(0.143)	(0.248)	(0.192)	(0.206)	(0.167)
Observations	51,284	51,284	30,115	30,115	21,169	21,169
Panel D: White-Black Test Score Gap						
Post x $\Delta \hat{Diss}_{dg}$	0.200	0.119	0.283	0.208	0.113	0.026
	(0.124)	(0.129)	(0.183)	(0.178)	(0.170)	(0.150)
Observations	51,284	51,284	30,115	30,115	21,169	21,169

Notes: Table displays difference-in-differences estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on test scores of Black and White students. Reported coefficients are β in Equation 2. All test score outcomes are in standard deviation units. *** p<0.01; ** p<0.05; * p<0.1.

Table B2: Effect of Changes in Segregation on Changes in Student Characteristics

	% Black	% White	% Hispanic	log(Enrollment)
	(1)	(2)	(3)	(4)
Panel A: Tests for Pre-Trends (Pre-2013 Data)				
Year x $\Delta \hat{Diss}_{dg}$	-0.017^{*}	0.009	0.001	0.014
	(0.009)	(0.012)	(0.007)	(0.023)
Observations	27,589	27,589	27,589	27,589
Panel B: Tests for Changes in Demographics (All Data)				
Post x $\Delta \hat{Diss}_{dg}$	-0.036	0.060	-0.036	0.042
	(0.029)	(0.044)	(0.026)	(0.120)
Observations	51,284	51,284	51,284	51,284

Notes: Panel A displays results of tests for pre-trends, limiting data to pre-2013 observations. Reported coefficients in Panel A are β in Equation 7. Panel B displays difference-in-differences estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on student characteristics. Reported coefficients in Panel B are β in Equation 2. *** p<0.01; ** p<0.05; * p<0.1.

Table B3: Effects of School Boundary Changes on Test Scores of Black and White Students (Alternative Segregation Measures)

	Dissimilarity Index		Isolation Index		Variance Ratio	
	Math	RLA	Math	RLA	Math	RLA
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Black-White Segregation						
Post x $\Delta \hat{Seg}_{dg}$	0.185***	0.185***	0.012	0.012	0.271*	0.271*
	(0.059)	(0.059)	(0.057)	(0.057)	(0.142)	(0.142)
Observations	51,284	51,284	51,284	51,284	51,284	51,284
Panel B: White Students' Test Scores						
Post x $\Delta \hat{Seg}_{dg}$	-0.0003	-0.057^{**}	-0.101	-0.110^{*}	-0.062	-0.104
	(0.039)	(0.029)	(0.076)	(0.058)	(0.076)	(0.063)
Observations	51,284	51,284	51,284	51,284	51,284	51,284
Panel C: Black Students' Test Scores						
Post x $\Delta \hat{Seg}_{dg}$	-0.063	-0.053	-0.063	-0.050	-0.135^{*}	-0.077
	(0.040)	(0.037)	(0.051)	(0.061)	(0.077)	(0.077)
Observations	51,284	51,284	51,284	51,284	51,284	51,284
Panel D: White-Black Test Score Gap						
Post x $\Delta \hat{Seg}_{dg}$	0.050**	0.033	0.053	0.029	0.113**	0.076
	(0.025)	(0.030)	(0.057)	(0.076)	(0.052)	(0.077)
Observations	51,284	51,284	51,284	51,284	51,284	51,284

Notes: Table displays difference-in-differences estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on test scores of Black and White students. Reported coefficients in Columns 1 and 2 are β in Equation 2. Columns 3 and 4 estimate an identical model, replacing $\Delta \hat{Diss}_{dg}$ with the predicted change in isolation index of segregation. Columns 5 and 6 estimate an identical model, replacing $\Delta \hat{Diss}_{dg}$ with the variance ratio index of segregation. In all regressions, all measures of segregation are divided by the sample standard deviation in 2012, so coefficients represent the effect of a predicted one standard deviation increase in segregation. In Panel A, outcome measures are are divided by the sample standard deviation in 2012 as well. All test score regressions are precision weighted, where each observation is weighted by the inverse of the variance. All test score outcomes are in standard deviation units. *** p<0.01; ** p<0.05; * p<0.1.

Table B4: Heterogeneous Effects of School Boundary Changes on Test Scores of Black and White Students

	% Black > 0.15		% Black ≤ 0.15		Dist. Enr. > 10000		Dist. Enr. ≤ 10000	
	Math	RLA	Math	RLA	Math	RLA	Math	RLA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Black-White Dissimilarity								
Post x $\Delta \hat{Diss}_{dg}$	0.186*	0.186*	0.275**	0.275**	0.144**	0.144**	0.256**	0.256**
	(0.104)	(0.104)	(0.114)	(0.114)	(0.067)	(0.067)	(0.117)	(0.117)
Observations	25,199	25,199	25,194	25,194	26,139	26,139	25,145	25,145
Panel B: White Students' Test Scores								
Post x $\Delta \hat{Diss}_{dg}$	0.033	−0.258	0.157	−0.026	−0.008	−0.223	0.143	−0.107
	(0.202)	(0.159)	(0.222)	(0.200)	(0.193)	(0.147)	(0.232)	(0.206)
Observations	25,199	25,199	25,194	25,194	26,139	26,139	25,145	25,145
Panel C: Black Students' Test Scores								
Post x $\Delta \hat{Diss}_{dg}$	−0.202	−0.186	0.015	−0.235	−0.282	−0.135	0.025	−0.242
	(0.196)	(0.201)	(0.386)	(0.365)	(0.202)	(0.185)	(0.397)	(0.380)
Observations	25,199	25,199	25,194	25,194	26,139	26,139	25,145	25,145
Panel D: White-Black Test Score Gap								
Post x $\Delta \hat{Diss}_{dg}$	0.338**	0.221	0.199	0.138	0.287**	0.175	0.146	0.113
	(0.144)	(0.199)	(0.175)	(0.213)	(0.143)	(0.192)	(0.179)	(0.224)
Observations	25,199	25,199	25,194	25,194	26,139	26,139	25,145	25,145

Notes: Table displays difference-in-differences estimates of the effect of changes in predicted segregation (based on changes in school attendance boundaries) on test scores of Black and White students. Reported coefficients are β in Equation 2. All test score regressions are precision weighted, where each observation is weighted by the inverse of the variance. All test score outcomes are in standard deviation units. *** p<0.01; ** p<0.05; * p<0.1.

Table B5: Effect of Neighborhood School Assignment Changes on House Prices

	Small Districts				Small Districts, Excl. Multi-School Tracts			
	HPI	IHS(Total)	IHS(White)	IHS(URM)	HPI	IHS(Total)	IHS(White)	IHS(URM)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta U\hat{R}M_{tg}$ x 2008	-0.08^{*}				-0.14^{**}			
	(0.05)				(0.05)			
$\Delta U\hat{R}M_{tg}$ x 2009	0.01				0.01			
	(0.03)				(0.06)			
$\Delta U\hat{R}M_{tg}$ x 2010	-0.01				-0.03			
	(0.02)				(0.03)			
$\Delta U\hat{R}M_{tg}$ x 2011	-0.04^{**}				-0.02			
	(0.02)				(0.03)			
$\Delta U\hat{R}M_{tg}$ x 2013	-0.05^{***}	0.09	0.12	0.10	-0.01	0.12	0.15	-0.54
	(0.02)	(0.08)	(0.16)	(0.24)	(0.03)	(0.16)	(0.29)	(0.35)
$\Delta U\hat{R}M_{tg}$ x 2014	-0.07^{**}	-0.01	0.16	-0.21	-0.02	-0.13	0.28	-0.97^{***}
	(0.03)	(0.12)	(0.13)	(0.27)	(0.04)	(0.17)	(0.17)	(0.35)
$\Delta U\hat{R}M_{tg}$ x 2015	-0.13^{***}	-0.03	0.28	-0.04	-0.11^{**}	-0.03	0.34	-0.14
	(0.04)	(0.11)	(0.20)	(0.24)	(0.05)	(0.17)	(0.25)	(0.34)
$\Delta U\hat{R}M_{tg}$ x 2016	-0.18^{***}	0.04	0.14	-0.03	-0.15^{**}	-0.10	0.02	-0.26
	(0.06)	(0.11)	(0.18)	(0.28)	(0.06)	(0.14)	(0.34)	(0.35)
$\Delta U\hat{R}M_{tg}$ x 2017	-0.25^{***}	-0.19	-0.21	0.22	-0.23^{***}	-0.36^{*}	-0.33	0.11
	(0.07)	(0.14)	(0.20)	(0.31)	(0.08)	(0.22)	(0.31)	(0.40)
$\Delta U\hat{R}M_{tg}$ x 2018	-0.31^{***}				-0.31^{***}			
	(0.08)				(0.08)			
No. Districts	1236	1236	1236	1236	1049	1049	1049	1049
Observations	749,375	408,714	408,714	408,714	279,708	152,542	152,542	152,542
R^2	0.95	0.95	0.95	0.88	0.96	0.94	0.95	0.88
Adjusted R^2	0.94	0.93	0.93	0.84	0.94	0.92	0.92	0.82

Notes: Table displays the difference-in-differences estimates of the effect of changes in the URM share of neighborhood schools (based on changes in school attendance boundaries) on house prices and mortgage volumes. IHS refers to the inverse hyperbolic sine transformation. Reported coefficients are β_t in 6. All regressions are weighted by census tract population in 2010. $U\hat{R}M_{tg}$ represents the predicted change in the share of URM students for schools attended by students in grade g in census tract t between 2013 and 2015.

Table B6: Effect of Neighborhood School Assignment Changes on House Prices

	Small Districts				Small Districts, Excl. Multi-School Tracts			
	HPI	IHS(Total)	IHS(White)	IHS(URM)	HPI	IHS(Total)	IHS(White)	IHS(URM)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta \hat{Black}_{tg}$ x 2008	−0.01				−0.12**			
	(0.04)				(0.05)			
$\Delta \hat{Black}_{tg}$ x 2009	−0.03				−0.03			
	(0.03)				(0.05)			
$\Delta \hat{Black}_{tg}$ x 2010	−0.02				−0.05			
	(0.03)				(0.05)			
$\Delta \hat{Black}_{tg}$ x 2011	−0.05*				−0.04			
	(0.03)				(0.04)			
$\Delta \hat{Black}_{tg}$ x 2013	−0.01	0.07	0.15	−0.11	0.02	0.002	−0.17	−1.25**
	(0.02)	(0.11)	(0.18)	(0.40)	(0.03)	(0.25)	(0.41)	(0.49)
$\Delta \hat{Black}_{tg}$ x 2014	0.01	0.03	0.19	−0.17	0.08	−0.07	0.42*	−1.27**
	(0.04)	(0.18)	(0.16)	(0.41)	(0.05)	(0.26)	(0.25)	(0.50)
$\Delta \hat{Black}_{tg}$ x 2015	−0.04	−0.06	0.34	0.01	0.01	−0.08	0.13	−0.36
	(0.04)	(0.14)	(0.25)	(0.38)	(0.06)	(0.21)	(0.25)	(0.46)
$\Delta \hat{Black}_{tg}$ x 2016	−0.02	0.06	0.03	0.11	0.02	−0.01	−0.36	−0.34
	(0.04)	(0.12)	(0.21)	(0.39)	(0.06)	(0.18)	(0.50)	(0.46)
$\Delta \hat{Black}_{tg}$ x 2017	−0.06	−0.22	−0.11	0.10	−0.03	−0.41	−0.50	−0.08
	(0.05)	(0.15)	(0.23)	(0.48)	(0.06)	(0.25)	(0.38)	(0.46)
$\Delta \hat{Black}_{tg}$ x 2018	−0.10				−0.11			
	(0.06)				(0.08)			
No. Districts	1236	1236	1236	1236	1049	1049	1049	1049
Observations	749,375	408,714	408,714	408,714	279,708	152,542	152,542	152,542
R^2	0.95	0.95	0.95	0.88	0.96	0.94	0.95	0.88
Adjusted R^2	0.94	0.93	0.93	0.84	0.94	0.92	0.92	0.82

Notes: Table displays the difference-in-differences estimates of the effect of changes in the Black share of neighborhood schools (based on changes in school attendance boundaries) on house prices and mortgage volumes. IHS refers to the inverse hyperbolic sine transformation. Reported coefficients are β_t in 6. All regressions are weighted by census tract population in 2010. $\hat{Black}_{tg}$ represents the predicted change in the share of Black students for schools attended by students in grade g in census tract t between 2013 and 2015.

Table B7: Effect of Neighborhood School Assignment Changes on House Prices

	Small Districts				Small Districts, Excl. Multi-School Tracts			
	HPI	IHS(Total)	IHS(White)	IHS(URM)	HPI	IHS(Total)	IHS(White)	IHS(URM)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta \hat{Hispanic}_{tg}$ x 2008	-0.18^{*}				-0.20^{*}			
	(0.11)				(0.11)			
$\Delta \hat{Hispanic}_{tg}$ x 2009	0.06				0.07			
	(0.06)				(0.11)			
$\Delta \hat{Hispanic}_{tg}$ x 2010	-0.02				-0.03			
	(0.04)				(0.05)			
$\Delta \hat{Hispanic}_{tg}$ x 2011	-0.04				0.01			
	(0.02)				(0.04)			
$\Delta \hat{Hispanic}_{tg}$ x 2013	-0.12^{***}	0.14	0.10	0.40	-0.06	0.29	0.57	0.24
	(0.03)	(0.12)	(0.35)	(0.31)	(0.04)	(0.20)	(0.38)	(0.43)
$\Delta \hat{Hispanic}_{tg}$ x 2014	-0.18^{***}	-0.06	0.15	-0.31	-0.15^{***}	-0.23	0.15	-0.79^{*}
	(0.05)	(0.14)	(0.25)	(0.37)	(0.05)	(0.22)	(0.24)	(0.45)
$\Delta \hat{Hispanic}_{tg}$ x 2015	-0.27^{***}	-0.004	0.25	-0.13	-0.29^{***}	0.04	0.68	0.10
	(0.07)	(0.17)	(0.34)	(0.32)	(0.06)	(0.27)	(0.46)	(0.54)
$\Delta \hat{Hispanic}_{tg}$ x 2016	-0.42^{***}	0.01	0.32	-0.23	-0.39^{***}	-0.23	0.50	-0.21
	(0.10)	(0.21)	(0.31)	(0.44)	(0.08)	(0.26)	(0.41)	(0.61)
$\Delta \hat{Hispanic}_{tg}$ x 2017	-0.54^{***}	-0.18	-0.37	0.41	-0.52^{***}	-0.36	-0.17	0.37
	(0.13)	(0.25)	(0.33)	(0.44)	(0.11)	(0.36)	(0.48)	(0.68)
$\Delta \hat{Hispanic}_{tg}$ x 2018	-0.64^{***}				-0.63^{***}			
	(0.16)				(0.13)			
No. Districts	1236	1236	1236	1236	1049	1049	1049	1049
Observations	749,375	408,714	408,714	408,714	279,708	152,542	152,542	152,542
R^2	0.95	0.95	0.95	0.88	0.96	0.94	0.95	0.88
Adjusted R^2	0.94	0.93	0.93	0.84	0.94	0.92	0.92	0.82

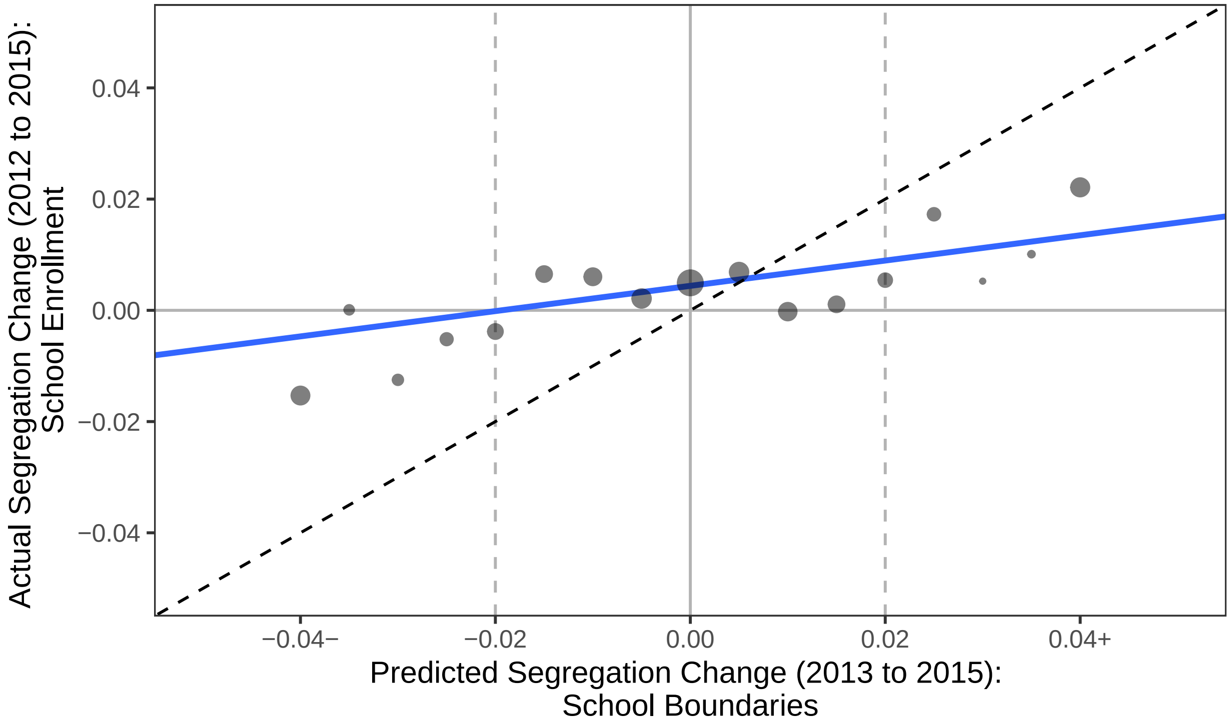
Notes: Table displays the difference-in-differences estimates of the effect of changes in the Hispanic share of neighborhood schools (based on changes in school attendance boundaries) on house prices and mortgage volumes. IHS refers to the inverse hyperbolic sine transformation. Reported coefficients are β_t in 6. All regressions are weighted by census tract population in 2010. $\widehat{Hispanic}_{tg}$ represents the predicted change in the share of Hispanic students for schools attended by students in grade g in census tract t between 2013 and 2015.

Table B8: Effect of Neighborhood School Assignment Changes on House Prices

	Small Districts				Small Districts, Excl. Multi-School Tracts			
	HPI	IHS(Total)	IHS(White)	IHS(URM)	HPI	IHS(Total)	IHS(White)	IHS(URM)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta \hat{Quality}_{tg}$ x 2008	0.01*				0.02***			
	(0.01)				(0.01)			
$\Delta \hat{Quality}_{tg}$ x 2009	0.004				0.01			
	(0.005)				(0.01)			
$\Delta \hat{Quality}_{tg}$ x 2010	0.002				0.01			
	(0.004)				(0.01)			
$\Delta \hat{Quality}_{tg}$ x 2011	0.001				0.005			
	(0.002)				(0.003)			
$\Delta \hat{Quality}_{tg}$ x 2013	0.001	−0.01	0.02	−0.01	0.001	−0.03	−0.01	−0.005
	(0.003)	(0.02)	(0.03)	(0.04)	(0.003)	(0.02)	(0.03)	(0.05)
$\Delta \hat{Quality}_{tg}$ x 2014	0.0003	0.03**	0.02	0.07*	−0.0002	−0.02	−0.09***	0.16***
	(0.005)	(0.02)	(0.02)	(0.04)	(0.01)	(0.03)	(0.03)	(0.06)
$\Delta \hat{Quality}_{tg}$ x 2015	0.005	0.02	0.02	0.03	0.004	−0.01	−0.03	0.07
	(0.01)	(0.02)	(0.02)	(0.04)	(0.01)	(0.02)	(0.03)	(0.06)
$\Delta \hat{Quality}_{tg}$ x 2016	0.01	0.05***	0.07***	0.09**	0.01	0.03	0.06	0.14**
	(0.01)	(0.02)	(0.02)	(0.04)	(0.01)	(0.02)	(0.04)	(0.06)
$\Delta \hat{Quality}_{tg}$ x 2017	0.01	0.06***	0.06***	0.10**	0.01	0.05**	0.03	0.15**
	(0.01)	(0.02)	(0.02)	(0.04)	(0.01)	(0.02)	(0.03)	(0.06)
$\Delta \hat{Quality}_{tg}$ x 2018	0.02*				0.02*			
	(0.01)				(0.01)			
No. Districts	1236	1236	1236	1236	1049	1049	1049	1049
Observations	749,375	408,714	408,714	408,714	279,708	152,542	152,542	152,542
R^2	0.95	0.95	0.95	0.88	0.96	0.94	0.95	0.88
Adjusted R^2	0.94	0.93	0.93	0.84	0.94	0.92	0.92	0.82

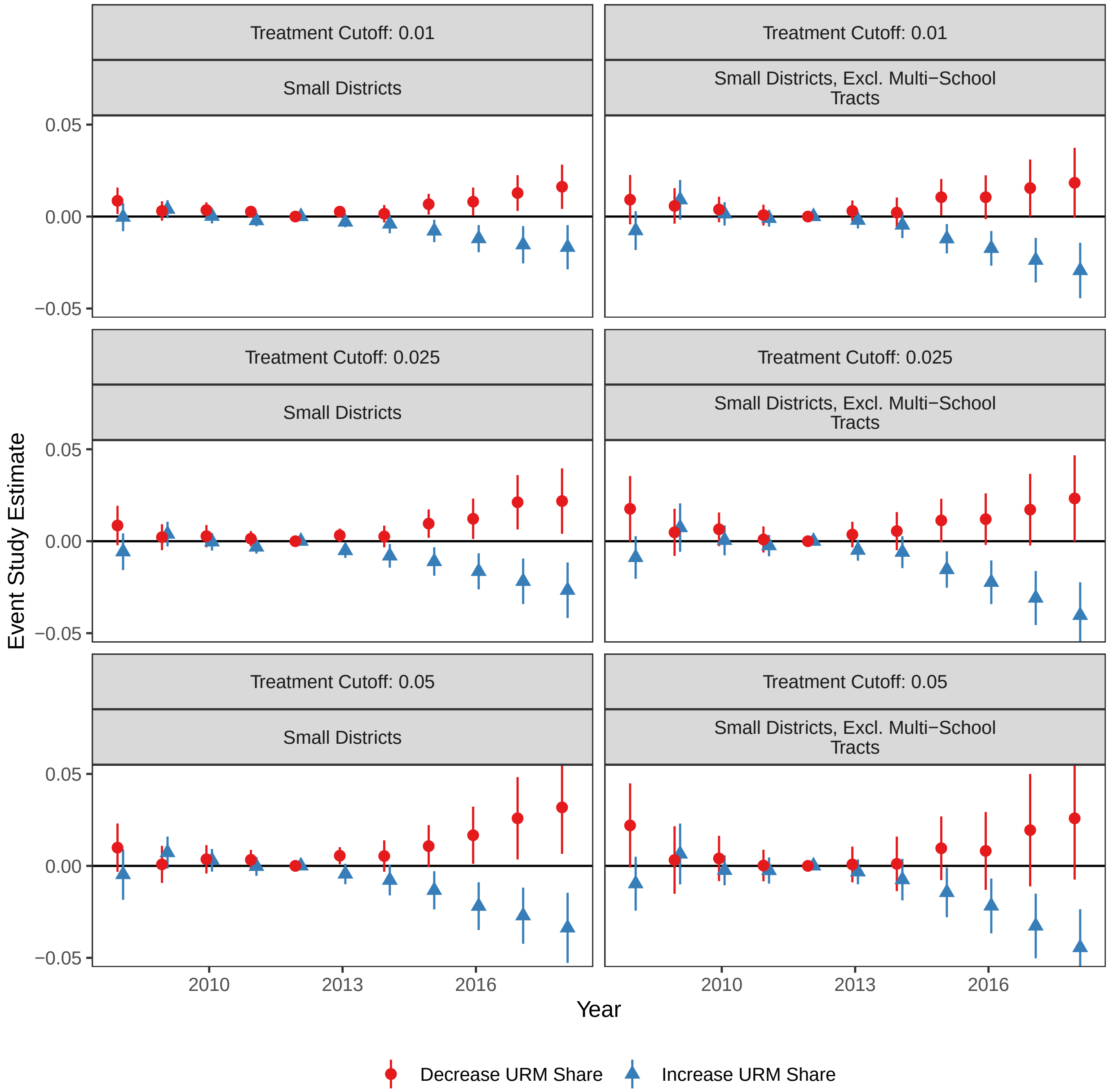
Notes: Table displays the difference-in-differences estimates of the effect of changes in the historical standardized test performance of neighborhood schools (based on changes in school attendance boundaries) on house prices and mortgage volumes. IHS refers to the inverse hyperbolic sine transformation. Reported coefficients are β_t in 6. All regressions are weighted by census tract population in 2010. $\hat{Quality}_{tg}$ represents the predicted change in the school quality for schools attended by students in grade g in census tract t, where each school's quality is measured based on pre-2013 test score performance from EdFacts, residualized against state-by-year-by-grade fixed effects and controls for enrollment and racial composition. I calculate school quality as the average school-level residuals, normalized to have mean 0 and standard deviation 1.

Figure B1: Predicted versus Actual Changes in Segregation



Notes: Figure displays the relationship between predicted changes in segregation between 2013 and 2015 (based on changes in school attendance boundaries) on actual changes in segregation between 2012 and 2015. Segregation is measured using Black-White dissimilarity, which is bounded between zero and one and represents the share of Black students that would need to be reallocated to equalize their share across schools. Displayed points represent conditional means in each half-percentage point bin of predicted segregation change. Dashed vertical lines are at -0.02 and 0.02 and dashed diagonal line is the 45 degree line. In the figure, predicted changes in segregation is winsorized to trim its distribution to 0.04 from above and -0.04 from below.

Figure B2: Decomposition of House Price Effects



Notes: Figure displays the difference-in-differences estimates of the effect of changes in the URM share of neighborhood schools (based on changes in school attendance boundaries) on house prices. Reported coefficients show the effect of increasing or decreasing URM share of neighborhood schools over time. Panels use different samples and different thresholds for identifying tracts where URM share increased or decreased. All regressions are weighted by census tract population in 2010.