Merging neighborhood schools to reduce socioeconomic segregation: Evidence from Charlotte, North Carolina*

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Abstract

Since the end of court-mandated desegregation, many U.S. school districts have re-segregated along racial and economic lines. This paper examines the impacts of a 2018 policy in Charlotte, North Carolina which merged student populations from neighboring schools with differing socioeconomic statuses (SES). I find that the policy significantly reduced economic segregation at merged schools. However, it also led to a sharp decline in enrollment as families, primarily low-SES ones, opted out of attending these schools. I also find that students from predominantly low-SES neighborhoods (who would have likely attended the low-SES school in absence of the merging) experienced declines in standardized test performance and increased short-term out of school suspension rates, whereas those from predominantly high-SES neighborhoods showed modest gains in test scores and no change in suspension likelihood. Teacher retention increased at the merged schools. Finally, neighborhoods that were previously zoned to majority low-SES schools experienced an increase in house prices after the policy was implemented.

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1 Introduction

The removal of court-ordered desegregation plans coincided with the re-segregation by race and socioeconomic status (SES) of many U.S. K-12 schools, which has persisted until today (Orfield and Frankenberg, 2014; Monarrez and Chien, 2021). In the 2018-19 school year, 40% of public K-12 students in the U.S. attended schools in which at least 75% of students were a single race or ethnicity, and 45% attended schools in which at least 75% of students were from the same economic background (National Center for Education Statistics, 2020). This is a concern because studies suggest that students at majority-black schools perform lower on exams than those at integrated schools (Card and Rothstein, 2007; Hanushek et al., 2009) and have worse behavioral outcomes (Guryan, 2004; Weiner et al., 2009; Johnson, 2011). Additionally, intergroup contact theory in social psychology suggests that exposure to socioeconomic and racial diversity reduces intergroup prejudice (Allport, 1954). An open question is what policies would effectively address modern-day segregation in schools; while we have evidence on the effectiveness of various historical school integration plans (Guryan, 2004; Jackson, 2009; Johnson, 2011; Billings et al., 2013), we lack evidence for modern efforts, for which the context differs in consequential ways. In particular, it is now illegal for many districts to use race when assigning students to schools, so recent plans tend to integrate using SES. Additionally, given historical pushback to busing students across town, recent efforts focus on integrating schools locally rather than maximizing integration across the district as a whole.

In this paper, I study the effects of a 2018 school pairing policy implemented in the Charlotte-Mecklenburg Schools (CMS) school district in North Carolina. The policy combined the student bodies of adjacent but socioeconomically-segregated elementary schools – one with a majority of low-SES students and the other with a majority of high-SES students – into one that was more socioeconomically integrated. Here, SES is defined using data from the National School Lunch Program (NSLP), where individuals classifed as economically disadvantaged by the NSLP are defined as low-SES in this paper, and those who are not classified as economically disadvantaged by the NSLP are defined as high-SES[1]. Given the increased size of the student body, students in the paired schools attend the formerly majority low-SES school for kindergarten, first, and second grade, and then attend the majority high-SES school for third through fifth grades. This policy represents one of several voluntary integration efforts currently being used by school districts.[2] It

[1]To be eligible for free lunch directly, one's household income must be no more than 1.3 times the federal income poverty line. To qualify for reduced-price lunch directly, one's household income must be no more than 1.85 times the federal poverty line. Schools can also qualify to provide free meals to all students without collecting household applications / income data via the Community Eligibility Provision (CEP). Through the CEP, if a certain percentage of students at a school are eligible for free school meals based on their participation in other means-tested federal benefits programs (for example, the Supplemental Nutrition Assistance Program (SNAP)), then all students at that school receive free meals. The goal is to ensure that all students in high-poverty areas have access to nutritious foods without incurring any financial or time costs. Within my data, I am able to distinguish whether a student within a CEP school is economically disadvantaged (i.e. whether they participate in federal benefits programs).

[2]Other examples include Howard County Public Schools in Maryland, which approved a redistricting plan in 2019 to balance the proportion of low-SES students across schools, and Minneapolis Public Schools in Minnesota, which

was effectively a pilot, only affecting two pairs of non-magnet elementary schools; however, it serves as a proof of concept for thousands of other public schools in the U.S. that could be paired under the same criteria.

I start by studying whether the policy was successful at achieving its primary goal of reducing economic segregation at merged schools. I find that economic segregation decreases at these schools; however, they also experienced large declines in enrollment, particularly amongst low-SES students, indicating that families responded endogenously to the policy by moving their children to different schools. As a result, I cannot study causal effects using average school-level outcomes. I then turn to student-level data to study the causal effects of pairing on students' educational outcomes. To address endogeneity concerns, I instrument for whether a student attended a merged school with whether they were zoned to attend a merged school prior to the policy. I find that students from low-SES neighborhoods who were zoned to attend and attended merged schools perform worse on standardized tests and are more likely to be suspended after the policy goes into effect. Conversely, students from high-SES neighborhoods who were zoned to attend and attended merged schools perform better on standardized tests and experience no change in their likelihood of being suspended. When I move to teacher-level data, I find that teacher retention increased at merged schools. Finally, I study the effects of the policy on neighborhoods more generally and find that house prices in neighborhoods previously zoned to majority low-SES merged schools increase in response to the policy.

First, to examine how the policy affected the socioeconomic composition and enrollment of students at paired schools, I use the synthetic differences-in-differences (SDID) method (Arkhangelsky et al., 2021). This method is similar to synthetic controls in that it creates hypothetical counterfactual schools using a weighted combination of other neighborhood K-5 schools in the district that are at least five miles from a merged school. It is similar to difference-in-differences in that the counterfactual school only needs to match the merged schools on outcome variables' trends, not levels, in the years prior to the policy's introduction.[3] The policy intended to make paired schools more socioeconomically integrated; however, a common concern was that high-SES students would drop out of the paired schools to avoid attending schools that have a higher concentration of low-SES students. Surprisingly, I find the opposite: the proportion of low-SES students in treated school pairs drops by eight percentage points relative to the counterfactual. This suggests that low-SES students were more likely to leave paired schools in response to the policy, which is what I find when studying effects on enrollment. In particular, I find that enrollment of high-SES students at each school pair decreases by an average of 103 students (a 17% decrease), and enrollment of low-SES students decreases by 198 students (39%). Students who formerly attended paired schools

redrew attendance boundaries to be less racially and economically segregated in 2020. The Bridges Collaborative, a school integration initative backed by The Century Foundation, was also created in 2020 to assist school districts across the country in their efforts to integrate schools.

[3] Unlike both synthetic controls and differences-in-differences, SDID allows for unequal weights on pre-policy time periods.

but moved when pairing went into effect primarily move to other public schools in the district, with students from formerly high-SES schools moving to schools that are on average 4.8 miles from their home, and those from formerly low-SES schools moving to schools that are an average of 3.4 miles from home. Enrollment at private schools within two miles of paired schools also increases by 81 students after pairing, suggesting that 13% of the decline in enrollment at paired schools can be attributed to students enrolling in private schools.

Next, I study the policy's effects on individual student outcomes by comparing the academic and behavioral outcomes of students in early, unaffected cohorts with those of late, affected cohorts in treated and control schools. To address potential endogenous responses to the policy, I instrument for whether a student is treated (i.e. attends a paired school) with whether they were zoned to a paired school based on their home Census block before the policy was announced. There are several reasons to believe that students previously zoned to majority high-SES paired schools experience different effects from those zoned to majority low-SES schools. One reason comes from the peer effects literature; in particular, the linear-in-means model of peer effects predicts that students are affected by the mean of their peers' attributes. According to this model, if the average student at paired schools comes from a high-SES school – which is the case in CMS – then students from low-SES schools will likely experience positive effects of pairing on their educational outcomes, while those from high-SES schools will experience no change in outcomes. Therefore, I allow for potential heterogeneous treatment effects based on the SES of the school to which a student was previously zoned. Contrary to predictions of the linear-in-means model and findings in the existing applied literature, I find that third- through fifth-grade students from low-SES treated neighborhoods who attend paired schools perform 0.18 standard deviations worse on standardized math exams, 0.15 standard deviations worse on reading exams, and are more likely to receieve a short-term, out of school suspension than those who attend control schools, suggesting that pairing had negative impacts on students from low-SES Census blocks. Conversely, third- through fifth-grade students from high-SES treated neighborhoods who attend paired schools perform 0.17 standard deviations better on math standardized exams, 0.12 standard deviations better on reading exams, and experience no change in the likelihood of receiving a short-term suspension.

Afterward, I study how teachers responded to the policy. The idea is that by altering the demographic composition and academic background of students at a school, the policy could make teaching in these schools more challenging, thereby inducing teachers to leave. Since the pairing policy only affected four of the 114 elementary schools in CMS, it would be easy in theory for teachers to transfer to other schools in the district to avoid it.[4] One hypothesis is that teachers from high-SES schools might anticipate that they will face more behavioral issues in the classroom, and leave these schools as a result. Alternatively, the fact that students from low-SES neighborhoods fare worse on educational outcomes as a result of pairing raises the possibility that teachers from

[4]Most instructional positions are posted at the district-level, after which teachers are allocated to specific schools if offered a position. Some positions, however, are posted at the school level. Once hired by CMS, teachers can move across schools.

low-SES schools may have left in response to the policy. In particular, if teachers from low-SES schools are more effective at teaching the students who were previously zoned to attend these schools compared to teachers from high-SES schools, the departure of low-SES teachers could disproportionately harm these students. Using an event study specification, I study whether year-over-year teacher attrition at treated schools changed after the policy was announced. I look within the school pair unit, rather than at individual schools, so as to not pick up mechanical moves that occurred as a result of the policy splitting schools by grades. Contrary to both of the hypotheses above, I find that teachers from majority low-SES schools are 15 percentage points (23%) more likely to stay at their school pair after pairing was announced, while teachers from majority high-SES schools experience no differential change in movement.

Finally, I study the policy's effects on the local housing market using data on house sales. Historically, many white families moved out of districts affected by desegregation orders – a phenomenon known as "white flight." If families also responded to pairing by "voting with their feet" (Tiebout, 1956), there should be a change in the volume of house sales in affected neighborhoods. Additionally, as the value of local public goods tends to be capitalized into house prices, the price of houses in affected neighborhoods should change if these neighborhoods become more or less desirable as a result of the policy's effects on their local public school. Note that in CMS, school funding is allocated at the district level, so changes in a specific neighborhood's property values do not directly impact the funding of individual schools within that neighborhood. Instead, property taxes collected countywide contribute to a general funding pool for CMS, which the district then allocates across schools based on factors like enrollment, student needs, and program requirements. To study whether the policy induced changes in house prices or the volume of house sales near affected borders, I compare properties that lie in the same neighborhood but on separate sides of affected attendance boundaries before and after pairing. I find that there is no statistically significant change the volume of home sales; however, house prices increased in neighborhoods that were previously zoned to low-SES treated schools. This suggests that pairing made neighborhoods formerly zoned to low-SES schools more desirable.

This paper makes three main contributions. To begin, it provides one of the first causal analyses on the effects of modern school integration plans. While there is a large literature studying the effects of the implementation and removal of historical desegregation plans (Guryan, 2004; Jackson, 2009; Johnson, 2011; Billings et al., 2013), there are no papers to my knowledge which study recent integration plans, which differ from historical plans in important ways – namely, in integrating on SES rather than race, and keeping students close to home rather than busing them long distances. Second, this paper contributes to a literature in applied microeconomics which highlights the importance of accounting for endogenous responses to school policy changes when formulating such policies. The majority of papers in this literature study endogenous sorting of families/students in response to school finance reforms (Chakrabarti and Roy, 2015; Dinerstein and Smith, 2021; Biasi, 2023); other papers look at the endogenous responses of teachers (Jepsen and Rivkin, 2009) and students (Gilraine et al., 2018) to a class size reduction program in California. This paper combines insights from the existing literature to study the sorting of both teachers and students in response to a school assignment policy change using the simulated instruments approach from Biasi (2023). Finally, this paper contributes to a large literature which studies homeowners' valuation of various local (dis)amenities Black (1999); Kane et al. (2006); Linden and Rockoff (2008); Boustan (2012) by contributing evidence on how the housing market responds to school integration plans.

The rest of the paper is organized as follows. Section 2 describes CMS' historical desegregation plan and the current landscape in the district. Section 3 describes the data. Section 4 describes the empirical strategy and results. Section 5 discusses external validity. Section 6 concludes.

2 Background

In 1971, the US Supreme Court case *Swann v. Charlotte-Mecklenburg Board of Education* ruled that mandatory busing aimed at racially integrating schools was legal, given that the existence of racially-segregated neighborhoods necessitated the use of busing to achieve integration. As a result, CMS used school pairing combined with mandatory busing from 1974 to 1992 to integrate its school system (Mickelson, 2001). It paired majority-black and majority-white schools within the district, combining the student bodies of both schools and splitting the students across schools by grade. Busing was used to transport students from home to their assigned schools. While the desegregation plan aimed to minimize the total amount of transportation needed, it placed the greatest burden upon students from black residential areas (Lord, 1975). In the 1973-74 academic year, 71% of black students needed to be transported to their assigned school, compared to 49% of white students.[5]

CMS was one of the most successful districts in achieving racial balance in schools during this period; however, it still did not achieve the maximum integration projected ex ante due to white flight: the decline in white public school enrollment in affected areas in response to the desegregation plan. Lord (1975) studies white flight in CMS and finds that white flight was higher in areas with higher family income, and in cases where students from white middle- or high-income areas were bused to distant schools in black residential areas (as opposed to when black students were bused into white neighborhoods). In 2001, following several years of legal proceedings and an appeal, a federal court declared that CMS had achieved unitary status in the case of *Swann v. Charlotte-Mecklenburg Board of Education*. This decision rendered CMS's racial integration plan illegal and required the district to end mandatory busing. According to the 1991 Supreme Court case *Board of Education of Oklahoma City Public Schools v. Dowell*, school districts could reach unitary status once they eliminated racial segregation "to the extent practicable." Once a school district achieves unitary status, it is released from judicial oversight. The alternative to

[5] These numbers come from historical pupils transportation records, made available by the the J. Murrey Atkins Library Special Collections and University Archives, University of North Carolina at Charlotte. See the original table on page 81 of the Citizens' Advisory Group document here.

a unitary system is a "dual" system, whereby black and white students attend separate schools. Existing studies suggest that the removal of race-based-busing in CMS led to a decrease in teacher quality, test scores, and high school graduation rates at schools that experienced an increase in black student enrollment (Jackson, 2009; Billings et al., 2013). In both 2006-07 and 2016-17, CMS was the most racially- and economically-segregated district in the state (Nordstrom, 2018).

In response to high rates of segregation by race, ethnicity, and wealth in the district, the Charlotte-Mecklenburg Board of Education began a student assignment review in 2015. The primary goal of the review was to identify how to improve economic diversity within schools without driving families out of the public school system. The school board voted on proposed changes in May 2017, and those that were approved went into effect in the 2018-19 academic year. Part of these approved changes included the pairing of four neighborhood elementary schools: Billingsville and Cotswold, and Dilworth and Sedgefield.[6] While historical school pairing in CMS largely involved pairing schools that were far apart from each other due to the existence of residential segregation by race, these pairs of schools were chosen because they contain schools with adjacent attendance zones. As such, students are not required to take long bus rides to get to their assigned school. In addition, these pairs were chosen based on the SES, rather than race, of students, given that the court case *Capacchione v. Charlotte-Mecklenburg Schools* bans CMS from using race in school assignment mechanisms. Therefore, each pair contains one school with a majority of low-SES students and one school with a majority of high-SES students. As race and SES are highly correlated in CMS, the two neighborhood school pairs end up containing one majority-black school and one majority-non-black school (see Figure 1). Furthermore, within each pair, the school with a majority of high-SES students has higher student enrollment. This was not an explicit goal when choosing pairs of schools; rather, schools with a majority of low-SES students tend to be under-enrolled within CMS, while those with a majority of high-SES students tend to be close to capacity or over-enrolled. That said, parents of high-SES students may be less opposed to a pairing for which the resulting student body is still majority high-SES.

In terms of the logistics of the school pairing plan, Billingsville and Cotswold Elementary Schools were paired to form Billingsville Cotswold Elementary School, in which kindergartners through second-graders attend the original Billingsville campus and third- through fifth-grade students attend the original Cotswold campus. Dilworth and Sedgefield Elementary Schools were paired to form Dilworth Elementary, in which kindergartners through second-graders attend the original Sedgefield campus, and third- through fifth-grade students attend the original Dilworth campus. Note that in every pairing, the school with a majority of low-income students was assigned lower grades (K-2). Figure 2 demonstrates what pairing would look like at Dilworth Elementary

[6] Neighborhood elementary schools serve students living within designated attendance zones, where all students residing in the zone are guaranteed a spot at that school. The alternative to attending a neighborhood elementary school in CMS is enrolling in a magnet school, which does not have attendance zones and admits students through a lottery system. Prior to the academic year 2010-11, Dilworth Elementary was an arts magnet school; however, in 2010-11, it was turned into a neighborhood school through a re-zoning plan.

if compliance with the policy was perfect – that is, if no students moved or transferred across schools in response to the policy. In 2016-17, only 20% of Dilworth's 803 students were low-SES. At Sedgefield, 87% of its 425 students were low-SES. Consequently, in the absence of transfers, the combined student body would have had 43% low-SES students. For Billingsville Cotswold Elementary, Billingsville had 312 students, 90% of whom were low-SES, and Cotswold had 906 students, 36% of whom were low-SES, in 2016-17. Thus, in absence of transfers, 50% of students would have been low-SES.

There are several reasons why pairing schools with contiguous attendance zones is feasible now but was not commonly used in the past. First, historical court-ordered school desegregation initiatives required maximizing integration across the district as a whole, while modern day integration efforts are voluntary; thus, while there are some schools that could have been paired contiguously in the past, doing so would not achieve the requirements of desegregation court orders. In particular, at least two of the schools that were paired in 2018 could have been paired by race in the 1970s, when CMS was required by courts to racially integrate its schools, as shown in Figure A2. Given the racial composition in each Census tract in 1970, there were likely many schools that could have been integrated via contiguous school pairing in CMS alone; however, this would leave schools outside of the center of the county segregated. Second, historical integration plans integrated schools on race rather than SES, and while race and SES are highly correlated in some parts of the country, there are other parts of the country with little racial diversity but significant economic diversity. Figure A3 shows how race versus income were distributed across the continental US in 1990, the first year for which there are shapefiles for all Census tracts in the US. While there is very little concentration of blacks outside of the South, and hence little room for integration on race outside of the South, there is significant variation in income at the Census tract level across the US. This suggests that if historical integration plans integrated on income rather than race, contiguous pairing may have been more feasible. Third, it is possible that gentrification in recent decades has led to the coexistence of poor and rich neighborhoods in close proximity within cities, making school pairing without the need for busing increasingly feasible. In CMS in particular, low-income households were concentrated in Charlotte in 1970 but have now become more dispersed across the county, as shown in Figure A4, supporting this hypothesis.

2.1 Options outside of one's neighborhood school

Since 2002, school choice has played a large role in student assignment to schools within the district. While all students are guaranteed a spot in their neighborhood school, they are also able to apply to one of the many magnet schools in the district via the school choice lottery. If accepted, students are guaranteed transportation to other schools in their transportation zone. Additionally, students are able to apply to transfer to other public schools in the district, although approval is never guaranteed. According to the district's website, the main factors considered when evaluating transfer requests are space availability, program compatibility, and district guidelines.

Transportation is only provided if the request is to a school choice program within one's home transportation zone. In the 2023-2024 academic year, there were 37 school choice programs across 71 schools in the district. For context, the district had 141,456 K-12 students enrolled across its 184 schools in 2023-2024.

Another option for students who do not want to enroll in their neighborhood school is private school. Mecklenburg County, the second largest county in North Carolina by population, has the most private schools and the second largest private school enrollment of all counties in the state, with a total of 103 schools and 20,857 students enrolled in private schools in the 2022-23 academic year (Cashwell and Dixon, 2023). Half of these schools are religious. Figure A5 shows the spatial distribution of private schools in the county by enrollment in 2022.

3 Data

The main data are from the North Carolina Education Research Data Center (NCERDC) and contain student- and teacher-level administrative data for all public schools in the state of North Carolina from 2010-2021. For reference, in the 2014-15 academic year, 79.4% of children residing in CMS boundaries attended CMS schools; 4.2% were homeschooled; 10.5% attended private schools; and 5.9% attended charter schools (Nelson and Lane, 2015). Thus, the NCERDC data contains the roughly 85% of students attending CMS or charter schools.

All elementary schools in CMS can be grouped into one of three categories based on whether they were paired in 2018-19: not paired, paired with a majority of low-SES students, or paired with a majority of high-SES students. Table 1 shows descriptive statistics of students in CMS elementary schools in 2016-17 by school pairing status. By definition, paired, low-SES schools have signficantly more economically disadvantaged students than paired, high-SES schools. Paired, low-SES schools also have statistically-significantly more economically disadvantaged students than non-paired schools, and paired, high-SES schools have statistically-significantly fewer economically disadvantaged students compared to non-paired CMS schools.

In terms of other demographic and academic characteristics, paired, low-SES schools have statistically-significantly more black and fewer white students than both not paired and paired, high-SES schools. Furthermore, third-through-fifth-grade proficiency rates in reading and math are markedly lower in paired low-SES schools compared to non-paired and paired high-SES schools. Similarly, the likelihood of a student receiving a short-term out-of-school suspension is significantly higher in paired low-SES schools than in both non-paired and paired high-SES schools. Paired high-SES schools, by contrast, enroll significantly more white students, fewer black students, and exhibit higher proficiency rates in reading and math than non-paired schools. Note that none of these differences is due to variations in funding across schools – consistent with the approach used in many countywide school districts, CMS allocates funding equally at the district level, so individual school funding does not fluctuate based on neighborhood characteristics.

Table 2 shows characteristics of teachers at CMS elementary schools in 2016-17 by the pairing status of the schools they taught at in that year. Paired, low-SES schools have significantly fewer white teachers than non-paired and paired, high-SES schools. While the proportion of male teachers at paired low- and high-SES schools does not differ statistically significantly, non-paired schools have a larger proportion of male teachers. Average total gross pay is lower in paired, low-SES schools than in non-paired and paired, high-SES schools; however, this is mechanical as in North Carolina, teacher pay is a fixed function of years of experience, National Board Certification status, and, in some cases, whether a teacher has an advanced degree.[7] There is no statistically significant difference in the proportion of teachers with master's degrees across the three groups of schools, nor in years of teaching experience, but teachers at paired, low-SES schools are significantly less likely to be National Board Certified than those at non-paired and paired, high-SES schools. This seems to be the primary reason for which teachers at paired, low-SES schools receive lower pay.

In addition to the NCERDC data, I have data from seven rounds of the Private School Universe Survey (PSS) which cover the following academic years: 2009-10, 2011-12, 2013-14, 2015-16, 2017-18, 2019-20, and 2021-22. The PSS is a biannual survey that collects various information on private elementary and secondary schools in the US; however, I only use information on geolocation and K-5 enrollment in this paper. While it aims to collect information from the full universe of private schools, participation in the PSS is voluntary and hence coverage is not complete. For reference, the City of Charlotte's Open Data Portal contains a dataset with the full geospatial inventory of K-12 schools in Mecklenburg County, which was published in 2022. In it, there are 100 private K-12 schools listed, whereas in the 2021-22 PSS data, there are 63 private schools surveyed in Mecklenburg County. However, the dataset from the City of Charlotte is only published at one point in time, and is missing enrollment data for many of the schools. Because I need cross-sectional data on private school enrollment from before and after CMS' pairing policy in 2018, the PSS is the most comprehensive data available.

For the housing market analysis, I use NC OneMap's 2014 NC Master Address Dataset to identify the population of residential properties in Charlotte. These data contain addresses, geo-coordinates, and land/property type (i.e. commerical, apartment building, single family residence, etc) classifications for all addresses in North Carolina in 2014. NC OneMap has an additional address/parcel dataset which is continuously-updated; however, in order to keep the housing stock fixed pre-policy, I use the 2014 master address dataset in analysis. To identify which houses to include in the sample, I use attendance boundary shapefiles from both the National Center for Education Statistics (NCES) School Attendance Boundary Survey (SABS) and Mecklenburg County GIS Open Mapping Data. I also use shapefiles of highways and bodies of water in Mecklenburg

[7] Prior to the 2014-2015 school year, teachers were paid more if they had a master's, six-year, or doctoral degree. If a teacher was being paid on an advanced-degree pay scale before 2014-15 *or* completed a master's, six-year, or doctoral degree "for which they completed at least one course prior to August 1, 2013" and for which they would have qualified for an advanced degree salary supplement according to the policy in effect in June 2013 (SBE policy TCP-A-006), they receive advanced degree pay.

County from NC OneMap to determine whether a school attendance boundary is bordered by a barrier, either man-made or natural. For characteristics of properties and for information on house sales, I use property tax and owner transfer data from CoreLogic, respectively. The property tax data contain the address, number of bedrooms, number of bathrooms, number of stories, square footage, year built, and lot size of all properties by year. The owner transfer data contain address and sale price (adjusted to 2010 US dollars) for all properties sold between 2000 and 2022. I merge these data with the master sample of properties on address.

Finally, I have Census data that I use to generate geospatial controls. Specifically, I have Census block group level data from the 2010 American Community Survey: 5-Year Data (2006-2010) and Census block group shapefiles that I downloaded from IPUMS National Historical Geographic Information System (NHGIS).

4 Empirical Analysis

To estimate the effects of school pairing, I study four main units of analysis: 1) schools, 2) students, 3) teachers, and 4) neighborhoods.

I will begin by introducing relevant notation. Student i of type $T \in \{L, H\}$ attends school s in pair p at time t, where $T = L$ indicates that student i is from a low-SES household, while $T = H$ indicates i is from a high-SES household.

4.1 School pair level effects

The primary goal of the pairing policy was to reduce economic segregation across treated schools. However, some anticipated that wealthy parents would withdraw their children from treated schools in response to the policy, enrolling them in other CMS public schools, charter schools, or private schools – a phenomenon I call "rich flight." If this occurred, the share of low-SES students in each pair would increase after pairing, as the total student population declined while the number of low-SES students remained stable. If not, the share should follow the counterfactual trend. Therefore, studying changes in the share of low-SES students at treated schools is first order to understanding whether the policy triggered behavioral responses.

Before I do so, Figure 4 plots raw data for the share of low-SES students in majority high- and low-SES schools before and after pairing. The solid lines denote the actual shares of low-SES students at paired schools. By design, low-SES paired schools had a majority of low-SES students and high-SES paired schools had a majority of high-SES students prior to the policy. After pairing, the share of low-SES students at majority low-SES paired schools dropped significantly and the share at majority high-SES schools increased slightly, indicating that pairing was successful at increasing socioeconomic integration at paired schools. After the 2017-18 school year, the transparent lines denote what enrollment at paired schools would have looked like if the number and socioeconomic composition of students by grade level were held fixed at 2017-18 levels and shares but were reallocated across schools according to the pairing rules in 2018-19. Therefore, the misalignment of the solid and transparent lines in 2018-19 indicates that there were likely behavioral responses to pairing.

Now, I use the synthetic difference-in-differences (SDID) method (Arkhangelsky et al., 2021) to study the policy's effects on schools. The natural unit of analysis here is school pair p, rather than school s within pair p, given that treatment occurred at the pair level. Note that for non-paired schools, s and p are equivalent units. The set up is similar to that of a basic two-way fixed effects regression: to study the causal effect of pairing on outcome of interest y for school pair p in year t using two-way fixed effects, I run the following specification:

$$y_{pt} = \mu + \alpha_p + \beta_t + treat_{pt}\tau + \epsilon_{pt}, \tag{1}$$

where α_p is a school pair fixed effect, β_t is a year fixed effect, and $treat_{pt}$ equals 1 if pair p was exposed to school pairing at time t and equals 0 otherwise. However, while the basic two-way fixed effects model minimizes the weighted least squares objective function $\min_{\tau,\mu,\alpha,\beta}\left\{\sum_{p=1}^{N}\sum_{t=1}^{T}(y_{pt} - \mu - \alpha_p - \beta_t - treat_{pt}\tau)^2\right\}$, the SDID method incorporates weights to balance the pre-exposure trends and minimize the following weighted least squares objective: $\min_{\tau,\mu,\alpha,\beta}\left\{\sum_{p=1}^{N}\sum_{t=1}^{T}(y_{pt} - \mu - \alpha_p - \beta_t - treat_{pt}\tau)^2\hat{\omega_p}^{sdid}\hat{\lambda_t}^{sdid}\right\}$, where $\hat{\omega_p}^{sdid}$ are school pair weights that align trends in the outcome for unexposed units with those of exposed units in the periods before exposure occurs, and $\hat{\lambda_t}^{sdid}$ are time weights that balance the pre- and post-exposure time periods of unexposed units. I use the placebo method standard error estimator from (Arkhangelsky et al., 2021) to calculate standard errors as recommended in the paper, given that the number of treated units is small. This entails computing the SDID estimator many times using non-treated units (without replacement), and then using these SDID estimates to compute a variance estimator. It is similar to permutation tests in randomization inference, except that treatment is not randomly assigned in this case.

The sample of potential control schools is restricted to neighborhood schools that 1) were open for all school years from 2011-12 - 2018-19 and 2020-21, and 2) are either paired in 2018 or are at least five miles from a paired school.[8] The former criterion is included to maintain a balanced sample as I use data from the respective years in my analysis, and the latter criterion is included to prevent violations of the no interference component of SUTVA – in particular, it is possible that students who would have attended treated schools in absence of pairing may have transferred to other nearby public schools in response to pairing, in which case outcomes at these nearby schools will be affected by the treatment status of paired schools. By restricting control schools to those that are located at least five miles from a paired school, I minimize the likelihood of this SUTVA violation, as students are less likely to transfer to schools that are far from their home.[9]

[8]The geographic distribution of potential control schools is shown in Figure A7.

[9]In 2016-17, 6% of students attending paired schools were traveling five miles or more to get to school.

I find that the share of low-SES students in treated pairs drops by eight percentage points in response to the policy, as shown in Figure 5. These results are not consistent with rich flight, and instead suggest that either low-SES students were more likely than high-SES ones to move out of paired schools in response to the policy ("poor flight"), or high-SES students were more likely than low-SES ones to move into these schools. I can study which of these forces seems to dominate by running an SDID analysis on enrollment at paired schools. Specifically, I compare K-5 enrollment at treated schools to K-5 enrollment at other neighborhood (non-magnet) elementary schools in CMS before and after pairing. I split enrollment into two separate outcome variables – enrollment of low-SES students and enrollment of high-SES students – to determine whether students of different socioeconomic backgrounds responded differently to the policy.

Within each of the two pairs, I find that the policy led to a 198 person decrease in K-5 enrollment of low-SES students, as shown in Figure 6, and a 103 person decrease in K-5 enrollment of high-SES students, as shown in Figure 7. Thus, while many high-SES students left treated schools in response to the policy as anticipated, the decline was larger for low-SES students.

4.1.1 Where do students who leave treated schools go?

Because I have data for all public schools in the state of North Carolina, I can directly identify where most students who attended paired schools in the 2017-18 academic year go to school in 2018-19, the first year pairing is in effect. Specifically, I look at the sample of students who were in kindergarten through fourth grade at paired schools in 2017-18 as these students should still be in elementary school (in particular, in grades one through five) in 2018-19. I split students based on whether they were attending majority high- versus low-SES schools in 2017-18 to allow for differential migration patterns by students from these schools. Then, in Table A1, I categorize the schools these students attended in 2018-19 by district and location.

Of the 1,258 students who attended grades K-4 at majority high-SES paired schools in 2017-18, 74% attend a paired school in 2018-19, 16% attend a non-paired CMS school, 1% attend a charter school in Mecklenburg County, 2% attend a North Carolina public school that is not in CMS or Mecklenburg County, and 7% drop out of the public school data (either because they moved out of state, enrolled in a private school, or started homeschooling, the most common of which is enrolling in private schools). For the students who attend a non-paired CMS school, their school is on average 4.82 miles from their home Census block; in comparison, students who attend paired schools live on average 1.75 miles from their school. Of the 617 K-4th grade students at majority low-SES paired schools in 2017-18, 47% attend a paired school in 2018-19, 45% attend a non-paired CMS school, 1% attend a charter school in Mecklenburg County, 4% attend a North Carolina public school that is not in CMS or Mecklenburg County, and 3% drop out of the public school data. Students who attend a non-paired CMS school live on average 3.39 miles from their school, while those who attend paired schools live on average 1.85 miles from their school.

To get at effects on private school enrollment more directly, I run a SDID analysis on private schools. I define the private school sample to contain the subset of private schools in Mecklenburg County that 1) appear in the PSS in all seven survey rounds from 2009-2010 to 2021-22, and 2) are either less than two or more than five miles from one of the treated schools, following the same rationale used to restrict the public neighborhood school sample.[10] I then compare K-5 enrollment at schools within two miles of treated schools to K-5 enrollment at schools that are at least five miles from treated schools before and after pairing. Results are robust to altering the distance threshold used to define the control group.

I find that pairing led to an average increase of 27 K-5 students in each of the three private schools located within two miles of paired schools, as shown in Figure A9, resulting in a total increase of 72 K-5 students. Reassuringly, this estimate aligns closely with the numbers above, which indicate that 106 K-4 students who were attending paired schools in 2017-18 left the North Carolina public school system. I expect the SDID estimate to underestimate the true effect on private school enrollment as the PSS does not capture the full universe of private schools, and the SDID sample only includes private schools surveyed in every PSS round from 2009-10 to 2021-22. Additionally, students may enroll in private schools that are more than two miles from their homes, and hence would not be captured in my SDID estimate. Nonetheless, this estimate suggests that 13% of lost enrollment at paired schools can be attributed to enrollment in nearby private schools.

4.2 Student-level effects

While pairing was intended to affect students who would have attended the four treated schools in absence of the policy, it seems to have also affected selection into these schools, thereby changing the recipients of treatment. For this reason, I cannot use school-level analyses to determine the causal effect of pairing on educational outcomes. Consequently, I turn to student-level data to study the causal effects of pairing.

4.2.1 Empirical strategy

To study the causal effects of pairing on students' educational outcomes, I run a triple differences specification comparing the academic and behavioral outcomes of early (unaffected) cohorts with late (affected) cohorts in treated versus control schools. Control schools are all other CMS schools that offer grades K-5. The sample of students includes all third- through fifth-grade students attending treated and control schools in the following academic years: 2014-15, 2015-16, 2016-17, 2018-19, and 2020-21. I define early cohorts to include students who were in grades 3-5 at in-sample schools in the 2014-15, 2015-16, and 2016-17 academic years, and late cohorts to include students who were in grades 3-5 at in-sample schools in 2018-19 and 2020-21. This allows me to compare students who attended treatment and control schools before 2018 to those who attended these schools starting in 2018. I focus on third- through fifth- graders because test scores, a key outcome

[10]The geographic distribution of in-sample private schools is shown in Figure A8.

of interest, are not collected until third grade. Additionally, I exclude the 2019-20 academic year from analysis because test scores and attendance data are not available for that year due to the COVID-19 pandemic.

To study exposure to the policy, I generate a dummy variable $treat_i$ which is equal to one if student i attends a paired school and is equal to zero if student i attends a control school. Because student composition in schools after 2018 is endogenous to the policy change, I instrument for actual treatment using a simulated treatment measure, as done in Gruber and Saez (2002), Hoxby and Weingarth (2005), and Biasi (2023), which fixes the pre-policy addresses of students and applies the school pairing rules to them in the post-policy school years (2018-19 and 2020-21). I use 2016-17 data on the 2010 Census block IDs associated with students' homes to identify what schools students would have been zoned to attend in 2018 in absence of school pairing.[11] In the pre-policy years, simulated treatment is defined using the contemporaneous Census blocks of students. Specifically, I define simulated treatment to be the proportion of one's truncated home Census block that overlaps with the treated schools' 2016-17 attendance zones. This measure is discrete but not binary; however, this is only due to data limitations. To determine which truncated block IDs overlap with treated and control schools, I take a shapefile of 2010 Census block IDs and collapse its polygons at the truncated block ID level. Then, I run an overlap analysis using school attendance boundary shapefiles from the 2015-16 NCES SABS. I identify which Census blocks overlap with treated and control schools, as well as determine, for each Census block that overlaps with an in-sample school attendance boundary, the proportion of the Census block that falls within each school boundary.

I allow for heterogeneous treatment effects based on whether a student was zoned to attend a school that had a majority of high-SES students versus one that had a majority of low-SES students prior to the policy. There are multiple reasons to believe these groups might experience different effects from pairing. The linear-in-means model of peer effects, which is the most commonly used model of peer effects in existing research, says that students are affected by the mean of their peers' attributes. In the paired schools, the mean student comes from a high-SES school as enrollment at these schools was roughly double the size of enrollment at low-SES schools. Students at high-SES schools that were paired had much higher average achievement on standardized test scores than those from low-SES schools. As such, this model predicts that the presence of high-achieving students will positively influence the behavior and academic outcomes of lower-achieving students. High-SES students, on the other hand, will experience neutral or slightly negative peer effects. The integration of lower-performing students could, in some cases, lead to a more challenging learning environment if teachers need to focus more on remedial instruction, potentially slowing the pace of instruction. However, because high-SES students make up the majority of the integrated student

[11] I use 2016-17 data rather than 2017-18 data because school pairing was announced in May 2017, so there may have been some preemptive moves in the 2017-18 school year. Additionally, student address data is not available after the 2016-17 academic year. Consequentially, only fourth and fifth graders are included in the analysis in the 2020-21 academic year as third grade students were not yet in the school system in 2016-17.

body, they are not particularly likely to experience negative peer effects in paired schools.

That said, if students are tracked by academic ability or by socioeconomic background in paired schools, the linear-in-means peer effects model predicts the opposite effect on students' outcomes. Tracking involves grouping students into different classes by characteristics such as previous academic performance. Thus, the linear-in-means model predicts that tracking on previous academic performance would likely result in learning gains for students who were already at the higher end of the test score distribution, while students at the lower end of the distribution would experience little to no improvements. Tracking on SES would have similar effects because most low-SES students come from formerly majority low-SES schools and hence have much lower average test scores than high-SES students.

The empirical specification is as follows. To start, I run first-stage equations using the simulated treatment variables:

$$\begin{aligned} y_{it} = \mu_0 &+ \mu_1 ZonedHighSES_{it} + \mu_2 SimTreat_i \times ZonedHighSES_{it} \\ &+ \mu_3 ZonedHighSES_{it} \times post_t + \mu_4 SimTreat_i \times post_t \\ &+ \mu_5 ZonedHighSES_{it} \times SimTreat_i \times post_t + \gamma_t + \mu_p + \mathbf{X}_{it} + \upsilon_{it}, \end{aligned} \tag{2}$$

where y_{it} is one of the following: $treat_i \times ZonedHighSES_{it}$, $treat_i \times post_t$, $ZonedHighSES_{it} \times treat_i \times post_t$. $t \in [2015, 2017] \cup [2019, 2021]$, $t \in \mathbb{Z}$, with 2015 representing the 2014-15 academic year. $treat_i = 1$ if student i attended a paired school and $= 0$ otherwise. $post_t = 1$ if $t \geq 2019$ and $= 0$ otherwise. γ_t are year fixed effects and μ_p are school pair fixed effects. $\mathbf{X}_{it}$ is a vector of Census tract level characteristics from the 2006-2010 5-year ACS – namely, the proportion of houssholds whose income was below 1.84 times the poverty line, which is approximately the cutoff to qualify for reduced-price lunch, and the proportion of housing units that were rented rather than owned – interacted with year dummies.

For $t \in [2015, 2017]$, $SimTreat_i \in [0, 1]$ is the proportion of student i's home Census block at time t that overlapped with paired schools' attendance zones. This measure is exogenous to the policy, as the policy was not approved until the end of the 2016-17 school year. Therefore, any moves prior to this would have been for reasons unrelated to the policy. However, if families responded to the policy by moving to different neighborhoods for those neighborhoods' schools, then defining $SimTreat_i$ using contemporaneous home Census blocks in $t \in [2019, 2021]$ would make the instrument endogenous. To maintain exogeneity, pre-policy data is used to define $SimTreat_i$ in the post-policy period. Specifically, for $t \in [2019, 2021]$, $SimTreat_i$ is the proportion of student i's 2017 home Census block that overlapped with paired schools' attendance zones.

$ZonedHighSES_{it} = 1$ if student i's home Census block at time t was primarily zoned to a majority high-SES school, and $= 0$ if it was zoned to a low-SES school, for $t \in [2015, 2017]$. For $t \in [2019, 2021]$, $ZonedHighSES_{it} = 1$ if student i's 2017 home Census block was primarily zoned to a majority high-SES school, and $= 0$ if it was zoned to a low-SES school. This variable is included in the empirical specification so that I can study whether students originally zoned to majority low- versus high-SES paired schools experienced differential treatment effects. Because the policy directly impacts the socioeconomic composition of students at paired schools, using contemporaneous data to define $ZonedHighSES_{it}$ in the post-policy years could miscategorize students. However, contemporaneous data is suitable for $t \in [2015, 2017]$ because there were no policy changes during that period. Both $SimTreat_i$ and $ZonedHighSES_{it}$ are missing for students who do not have geocoded address data.

After running the first-stage regressions, I plug the predicted values of $treat_i \times ZonedHighSES_{it}$, $treat_i \times post_t$, and $ZonedHighSES_{it} \times treat_i \times post_t$ into the following regression:

$$\begin{aligned} y_{it} = {} & \alpha_0 + \alpha_1 ZonedHighSES_{it} + \alpha_2 \widehat{treat_i \times ZonedHighSES_{it}} \\ & + \alpha_3 ZonedHighSES_{it} \times post_t + \alpha_4 \widehat{treat_i \times post_t} \\ & + \alpha_5 \widehat{ZonedHighSES_{it} \times treat_i \times post_t} + \gamma_t + \mu_p + \mathbf{X}_{it} + \epsilon_{it}. \end{aligned} \tag{3}$$

As mentioned, the inclusion of $ZonedHighSES_{it}$ terms allows me to test for heterogeneous treatment effects for students formerly zoned to majority low-SES schools versus those zoned to majority high-SES ones. In particular, the coefficient α_4 gives the treatment effect for students who were living in neighborhoods that were zoned to majority low-SES treated schools in 2016-17 and who attended treated schools post-pairing. $\alpha_4 + \alpha_5$ gives the treatment effect for students who were living in neighborhoods that were zoned to majority high-SES treated schools in 2016-17 and who attended treated schools post-pairing. If α_5 is statistically significantly different from 0, then there are heterogeneous treatment effects for compliers from majority low- versus high-SES schools.

For equation 3 to identify the causal effects of school pairing on students' educational outcomes, three identification assumptions must be satisfied. The first two are instrumental variables' identification assumptions, and the latter is a triple differences identification assumption. For all these assumptions, the focus is on the regression terms which represent treatment effects: $treat_i \times post_t$ and $ZonedHighSES_{it} \times treat_i \times post_t$. First, the instrumental variables' relevance assumption must be satisfied: the instruments must be highly correlated with their corresponding endogenous variables. I can test this both by running the first-stage regressions and looking at the resulting coefficients, as seen in Table 3, and by looking at the first stage Wald test statistics, shown at the bottom of Table 4. The first stage coefficient on $treat_i \times post_t$ is 0.36 and the first stage coefficient on $ZonedHighSES_{it} \times treat_i \times post_t$ is 0.98, indicating that the instruments strongly predict their respective endogenous variables. The Wald statistics for these variables are also well over the weak instruments' benchmark of 10. Second, the instrumental variables exclusion restriction must hold. As such, there cannot be time-varying qualities of Census blocks that influence students' educational outcomes other than through their effects on the schools these students attend. For example, if neighborhood characteristics in areas zoned to paired schools shift or other policy interventions occur in these areas at the same time the pairing policy goes into effect, then these neighborhood factors could affect students' academic performance regardless of what school they attend. Given the short time frame in which pairing occurred and given that the district did not couple pairing with other initiatives in neighborhoods zoned to treated schools, this violation seems implausible.

Thirdly, parallel trends must be satisfied – that is, in absence of the policy, students who were zoned to attend low-SES schools and who attended paired schools must have evolved similarly on outcomes of interest to those who were zoned to attend low-SES schools and who attended control schools for the triple differences specification to provide a causal estimate; the same is true for those zoned to attend high-SES schools. While this assumption is not directly testable, I assess its plausibility by checking whether students experience similar trends in outcomes of interest in the years leading up to the policy. Specifically, I run the same regression as above, defining 2011-12, 2012-13, and 2013-14 as pre years and 2014-15, 2015-16, and 2016-17 as post years. As shown from the coefficients for $treat_i \times post_t$ and $ZonedHighSES_{it} \times treat_i \times post_t$ in Table A2, there are no statistically-significantly different pre-trends for students zoned to high- or low-SES schools on the outcome variables of interest. Given the identification assumptions are all satisfied, I now move to the results.

4.2.2 Results

To begin, I run the OLS analog of equation 3 to study what happens to educational outcomes of all students attending treated schools. While these estimates are not causal, as they do not account for endogenous selection into and out of paired schools in response to the policy, they provide insight into how students overall fared at paired schools. The results are displayed in the odd columns of Table 4, and the point estimates that indicate what happened to student outcomes after pairing are in the rows $treat_i \times post_t$ and $ZonedHighSES_{it} \times treat_i \times post_t$. Specifically, $treat_i \times post_t$ indicates what happened to outcomes of students who attended paired schools and who were zoned to a neighborhood school that had a majority of low-SES students in 2016-17. Note that the socioeconomic composition of the school a student is zoned to serves as a proxy for the socioeconomic composition of the neighborhood they live in. Adding the point estimates for $treat_i \times post_t$ and $ZonedHighSES_{it} \times treat_i \times post_t$ together indicates what happened to outcomes of students who attended paired schools and who were zoned to a neighborhood school that had a majority of high-SES students in 2016-17. This estimate can be found at the bottom of Table 4 in the row that indicates the marginal treatment effect for students who were formerly zoned to high-SES schools.

The outcomes of interest include performance on end-of-year standardized tests in math and reading, disability (emotional or intellectual) status, and whether a student received a short-term out of school suspension during the academic year. Column 1 of Table 4 shows what happened to student performance on math standardized tests at paired schools after pairing. Students previously zoned to low-SES schools experienced a 0.077 standard deviation decrease on math exams, while those from high-SES schools saw a 0.13 standard deviation increase. Similarly, column 3 presents data on reading standardized tests, revealing a 0.11 standard deviation decrease in reading test scores for students from low-SES schools and a 0.06 standard deviation increase for those from high-SES schools. Regarding disability status, students zoned to high-SES schools were 3 percentage points less likely to be classified as having a disability (see column 5), whereas those from low-SES schools were 4.8 percentage points more likely to be classified as having a disability. In column 7, there was no statistically significant change in the likelihood of receiving a short-term out of school suspension for either group of students. Overall, these results suggest that students at treated schools who came from low-SES neighborhoods faced negative impacts on test scores due to pairing, while those from high-SES neighborhoods experienced slight improvements in test scores and lower rates of disability classification.

Next, I run the 2SLS analysis to identify the causal effects of treatment on students' educational outcomes for students who were previously zoned to attend and who attended paired schools. These results are displayed in the even columns of Table 4. After pairing, students formerly zoned to attend and attending majority low-SES paired schools performed 0.18 standard deviations worse on math standardized tests and 0.15 standard deviations worse on reading tests than those who were formerly zoned to low-SES schools and who were attending control schools, as shown by the coefficients on $treat_i \times post_t$. However, these estimates are not statistically significant. Students formerly zoned to attend and attending majority high-SES paired schools performed 0.17 standard deviations better on math exams and 0.12 standard deviations higher on reading exams than those who were formerly zoned to attend majority high-SES schools and who were attending control schools, as shown by the addition of the coefficients on $treat_i \times post_t$ and $ZonedHighSES_{it} \times treat_i \times post_t$.

These results contradict what the linear-in-means peer effects model predicts in absence of tracking, which I find no evidence of in the data. They also contradict what many existing empirical studies of peer effects have found (Hoxby, 2000; Jackson, 2009; Billings et al., 2013). However, my findings for low-SES students are consistent with the hypothesis of "teaching to the middle," which predicts that in classes with students of varying ability or knowledge, teachers teach to the median student. Because students at majority low-SES treated schools were performing worse on math and reading tests prior to pairing, and these schools had much lower enrollment than majority high-SES treated schools in the pre-period, combining these two groups of students into one school without tracking would result in the median student being from a high-SES school. If teachers teach at the level of knowledge of the median student in their class, students from low-SES schools will be taught above their level, while students from high-SES schools will be taught at their level. As such, students from majority low-SES schools, who were already behind academically compared to their new peers from high-SES schools, fall further behind after being placed in classes with them. Note that this is only a concern in the first few years of the policy's implementation as students will be coming into the same schools with differing previous educational experiences. Students who start elementary schools in paired schools will be entering with approximately the same knowledge, regardless of whether they would have attended a low- versus high-SES school in absence of the pairing policy. Unfortunately, because I need to observe students in the data before pairing was announced in order to calculate a simulated treatment measure for them, I am not able to look at effects on "fully-treated" students: students who started elementary school after pairing was in place.[12]

Finally, it is important to note that the pairing policy assigned students in grades 3-5 to attend school in the physical buildings that housed formerly high-SES schools. As such, students from low-SES schools may have experienced adverse effects from having to adjust to a new school that students from high-SES schools did not experience. This could partially explain the negative impacts on educational outcomes for students from low-SES schools. As school pairing necessitates the splitting of students across schools by grades, and hence requires all students to switch physical schools in the middle of elementary school, it would be useful to understand whether this is true for future policy implications. If one of the pairs in CMS had assigned 3rd-5th graders to a majority low-SES school and the other had assigned students to a majority high-SES school, I could try to identify so-called "disruption" effects; however, because this didn't occur, I can only speculate about their existence.

4.2.3 Understanding the local average treatment effect (LATE)

It is important to note that my 2SLS estimates represent the local average treatment effect (LATE) for compliers: children who attend paired schools if previously zoned to them and attend non-paired schools otherwise. To understand the LATE better, I characterize treated compliers relative to all treated students using the approach developed by Abadie (2003) and explained by Angrist and Pischke (2009). I describe this process in detail in Appendix B.

Table A3 shows characteristics of all students in the analysis sample who attended paired schools in 2018-19, 2020-21, and 2021-22, compared with those of students who attended paired schools and complied with their simulated treatment status in the same years – that is, they were zoned to attend a paired school prior to pairing and attended one after pairing. Treated compliers are much less likely to be white and much more likely to be low-SES than treated students as a whole. They also perform worse on math and reading exams than all treated students. As such, it is highly unlikely that the LATE and average treatment effect (ATE) are the same.

4.2.4 Comparing OLS and 2SLS to understand selection

There are two main reasons why the OLS and 2SLS estimates may differ – one is that compliers experience different treatment effects from non-compliers, and the other is that there is selection on unobservables that biases the OLS estimates.

Having run the OLS and 2SLS regressions, I can now compare the OLS coefficients in Table 4 with the 2SLS ones to try to understand what types of individuals selected into treated schools

[12]The district has also switched from pairing to a new integration technique, limiting the years of post-pairing data available for analysis.

after pairing. Specifically, I compare the coefficients on $treat_i \times post_t$ and $ZonedHighSES_{it} \times treat_i \times post_t$ in the OLS columns with those in the 2SLS columns of Table 4 for each outcome variable of interest to determine how compliers performed relative to all students at treated schools after pairing. I find that relative to compliers who were zoned to majority low-SES schools prior to pairing, the average student who attends treated schools and comes from a majority low-SES neighborhood performs slightly better on math tests and roughly the same on reading tests: the coefficient on $treat_i \times post_t$ when looking at performance on math standardized tests is -0.077 for all treated students and -0.182 for treatment compliers, while the coefficient for reading standardized tests is -0.105 for all treated students and -0.151 for treatment compliers. For behavioral outcomes, treated students as a whole saw a small but non-statistically signficant increase in the probability of receiving a short-term out of school suspension of 2.7 percentage points, compared to an increase of 16.3 percentage points for treatment compliers.

Looking at students zoned to majority high-SES schools prior to pairing, compliers perform better on both math and reading standardized tests than the average treatment school attendee after pairing: the coefficients on $treat_i \times post_t$ and $ZonedHighSES_{it} \times treat_i \times post_t$ combined when looking at performance on math standardized tests is 0.13 for all treated students and 0.17 for treatment compliers, while the coefficient for reading standardized tests is 0.06 for all treated students and 0.12 for treatment compliers. For behavioral outcomes, treated students as a whole saw a small but non-statistically signficant decrease in the probability of receiving a short-term out of school suspension of 1 percentage point; the estimate was the same for treatment compliers. These results suggest that students who choose to attend treated schools despite not living within the home attendance zones and who live in neighborhoods zoned to majority low-SES schools prior to pairing are positively selected, while those who select into treatment schools and were zoned to other majority high-SES schools prior to pairing are negatively selected.

4.3 Teacher-level effects

The previous section showed that test scores of students from low-SES neighborhoods declined at treated schools, while those of students from high-SES neighborhoods increased slightly. One possible explanation for this finding is that teachers at treated schools endogenously responded to the school policy change. If teachers from low-SES schools were more likely to leave after the policy was announced than those from high-SES schools, students formerly zoned to low-SES schools may have experienced negative effects on their educational outcomes for multiple reasons. For one, teachers from high-SES schools may be inexperienced at working with students from low-SES backgrounds, and as a result are less effective at teaching them. Additionally, if teachers did not change their curriculum or teaching styles after pairing went into effect, then teachers from high-SES paired schools will likely be more effective at conveying material to students from high-SES schools and vice versa.

To explore this, I now examine how the policy affected year-over-year teacher attrition at treated schools. Specifically, to determine whether teachers leave treated schools at a differential rate after the policy was announced in May 2017, I run the following event-study specification:

$$y_{it} = \sum_{\tau=-3}^{4} \beta_\tau \mathbb{1}\{2017 = t - \tau\} + \epsilon_{it}, \tag{4}$$

where y_{it} is equal to 1 if teacher i is teaching at the same school pair in school year t as they were at $t-1$ and equal to 0 otherwise. β_τ, $\tau \in [-3,-1] \cup [1,4] \cap \mathbb{Z}$, represents the proportion of teachers at $t = 2017+\tau$ who are in the same treated school pair as they were at $t-1$ relative to the proportion at $t = 2017$ (β_0). I look at movement out of school pairs rather than out of individual schools so as to not pick up mechanical moves as a result of the policy (i.e. if a third-grade teacher formerly taught at a school that is assigned grades K-2 after pairing, it is likely she will move to the other school in the pair so that she can continue teaching third grade – I do not want this to be classified as a move in my specification). The sample is restricted to individuals who were teaching at one of the four treated schools at time $t-1$. I also split teachers into two subsamples – those who taught at treated schools with a majority of low-SES students versus those who taught at treated schools with a majority of high-SES students prior to pairing – and run the event study separately for these two groups. This allows me to study whether teachers responded to the policy differentially based on the socioeconomic composition of the schools they initially selected into.

Since the policy assigned teachers to schools based on the grade they taught (K-2 or 3-5) starting in 2018-19, I limit my analysis to the 2018-19 school year and earlier. This allows me to focus on teachers' responses before grade-level assignments became a factor in choosing what school to teach at. Contrary to the intuition from above, I find that teachers from formerly low-SES schools are 15 percentage points more likely to remain at their school pair after the policy was announced, as seen in Figure 8. Additionally, teachers from formerly high-SES schools are no more likely to leave their school pair as a result of pairing. This suggests that effects on test scores are not driven by teachers responding to the policy by leaving treated schools to teach elsewhere.

Another potential explanation for the test score findings relates to the fit between teachers and the student populations they are accustomed to teaching. If teachers from high-SES schools are more effective at teaching high-SES students, and teachers from low-SES schools are better at teaching low-SES students, this mismatch could have affected outcomes post-pairing. Since more teachers at paired schools originally taught at high-SES schools (due to their higher enrollment prior to pairing), students from low-SES schools may have faced a learning environment less suited to their needs. Additionally, teacher retention rates were higher among those from high-SES schools (93% stayed in paired schools in 2018-19, compared with 81% from low-SES schools, as shown in Figure 8), further amplifying this imbalance. Consequently, this could explain the worse outcomes for students from low-SES schools in integrated classrooms.

4.4 Neighborhood effects

Beyond its direct effects on the school system, pairing may have had indirect effects on nearby neighborhoods. For example, neighborhoods that were previously zoned to low-SES treated schools are now presumably going to have higher quality neighborhood school options (proxying for school quality using average test scores) after pairing due to the influx of students from high-SES schools – who, on average, have higher test scores – at these schools. Given that the quality of the local public schools is accounted for in the price of homes, an improvement in school quality may lead to an increase in house prices. Additionally, if people are happier with their neighborhood schools, they may be less likely to move out of the neighborhood, resulting in a reduction in the volume of house sales in that neighborhood. The same logic would predict opposite effects for houses that were previously zoned to high-SES paired schools.

To determine whether the local housing market is affected by pairing, I first use the border discontinuity method developed in Black (1999) to identify the sample of houses to study in my analysis. Using data on the full stock of properties in Charlotte in 2014, I restrict my sample to houses that lie within 300 meters of the paired schools' attendance boundaries so as to only compare houses that lie within the same geographic area (this bandwidth was chosen arbitrarily – future iterations of analysis will choose the optimal regression discontinuity bandwidth according to Calonico et al. (2019) and also using empirical permutation tests). Houses on one side of the attendance boundary are zoned to schools that become paired, while houses on the other side – presumably similar in neighborhood characteristics due to their close proximity – are zoned to different schools. Therefore, conditional on house characteristics (i.e. number of bedrooms, number of bathrooms, acreage, living square footage, and year built) and boundary fixed effects, differences in house prices can be attributed to the perceived relative quality of the schools these houses are zoned to. As in Black (1999), in-sample properties must also be 1) classified as residential (specifically, duplex/triplex, single family residential, or townhouse); and 2) not separated from other properties by a main road, given that this inherently divides the properties into separate neighborhoods. Figure A10 displays the geospatial distribution of the in-sample properties. All properties within 300 meters of the highway are excluded from the sample; this results in 9% of potential properties being dropped. There are some boundaries which only have properties on one side; I also exclude these properties from the sample. The final sample contains 5,146 properties.

After finalizing the sample of houses to study, I use difference-in-differences to identify how much of the difference in cross-border house prices is due to the policy change rather than to pre-existing differences in schools. The first difference – comparing homes on either side of the boundary – captures pre-policy differences in the perceived quality of schools these homes are zoned to. The second difference – comparing home prices before and after the policy announcement – captures changes over time. By calculating the difference in these differences, I can isolate the causal impact of the school pairing policy on house prices. The difference-in-differences specification is as follows:

$$y_{hbt} = \alpha_0 + \alpha_1 treat_h + \alpha_2 post_t + \alpha_3 treat_h \times post_t + \mathbf{X}_h + \gamma_b + \epsilon_{hbt}, \tag{5}$$

where y_{hbt} is either sale price or probability of sale for house h along boundary b at time t; $treat_h$ equals 1 if house h lies within a treated attendance zone and equals 0 if not; $post_t$ equals 1 for t between April 2017 and March 2020 and equals 0 for t between April 2014 and March 2017; $\mathbf{X}_h$ is a vector of characteristics of house h – namely, the number of bedrooms and bathrooms, acreage, living square footage, and year built; and γ_b are boundary fixed effects. To generate boundary fixed effects, I combine the school attendance boundaries of paired schools into one joint boundary, dissolving the lines within the boundary that formerly separated the two attendance zones. Then, I split the new combined boundary into segments of equal length (currently, 300 meters).

To allow for heterogeneous treatment effects for houses zoned to majority high- versus low-SES elementary schools prior to pairing (as done in the student-level analysis), I run a triple-differences specification which includes a binary variable $HighSES_b$ that is equal to 1 if boundary b borders a previously majority high-SES paired school, and is equal to 0 if it borders a previously majority low-SES paired school:

$$\begin{aligned} y_{hbt} = {} & \alpha_0 + \alpha_1 treat_h + \alpha_2 HighSES_b + \alpha_3 post_t + \alpha_4 treat_h \times HighSES_b + \alpha_5 post_t \times HighSES_b \\ & + \alpha_6 treat_h \times post_t + \alpha_7 treat_h \times post_t \times HighSES_b + \mathbf{X}_h + \gamma_b + \epsilon_{hbt}. \end{aligned} \tag{6}$$

Columns 1-3 and 6 of Table 5 show the results from running the regression in equation 5 with and without controls for house characteristics and neighborhood fixed effects. I find that amongst houses that lie within 300 meters of affected attendance boundaries, those on the treated side of the boundary experience an increase in the price of homes (with point estimates ranging from 0.095 to 0.123 percent) and no change in the number of homes sold relative to those on the control side of the boundary after pairing. When I allow for the possibility of heterogeneous treatment effects, I find results in line with what I expected: the increase in house prices is driven exclusively by houses that were previously zoned to low-SES treated schools. As shown in column 4 of Table 5, houses that were previously zoned to low-SES treated schools sell for 18.7% more than those sold on the untreated side of the same boundary. These results indicate that pairing made living in neighborhoods formerly zoned to low-SES schools more desirable.

5 External Validity

While this policy initially affected only four schools in one US public school district, it has the potential for broader application nationwide. In particular, in 2018-19, 11% of public elementary school students attended schools that could be paired under the following criteria: both schools in the pair serve grades K-5 and are within two miles of each other, one school has at least 50% low-

SES students, the other has less than 50% low-SES students, and there is at least a 25 percentage point difference in the proportion of low-SES students between the two schools[13] (National Center for Education Statistics, 2022). If I make the criteria for pairing more stringent, requiring paired schools to have at least a 55 percentage point difference in the proportion of low-SES students to match the composition of paired schools in CMS[14], 3% of public elementary school students attended schools that could be paired in 2018-19. As shown in Figure A1, these schools are primarily located in cities.

In the 239 public school districts that have at least two potential pairs under the more stringent pairing criteria, 67% of elementary school students currently attend schools where at least 75% of the student body shares the same SES. Integrating these schools, assuming perfect compliance, would lower this figure to 57%, a decrease of 15 percent. If, however, enrollment patterns mirrored those observed in CMS after pairing – with paired schools experiencing a 39% decrease in the enrollment of low-SES students and a 17% decrease in the enrollment of high-SES students – this figure would drop to 61%, representing a 9 percent decrease. This suggests that many districts, particularly in urban areas, could adopt similar policies to reduce the concentration of low- and high-SES students across schools. Even with imperfect compliance, meaningful reductions in segregation could be achieved.

However, although pairing may be effective at reducing segregation, my results show that it may also widen educational disparities between low- and high-SES students in the short run. These short-term effects highlight the need for targeted interventions to support low-SES students during the transition. Nevertheless, school pairing could still serve as a viable strategy for reducing socioeconomic segregation in the long run, provided that such challenges are addressed. Ensuring that low-SES students benefit equally will be crucial for the long-term success of these policies. By proactively addressing these unintended consequences, districts can work toward achieving both socioeconomic integration and educational equity.

6 Conclusion

In this paper, I examine the effects of a recent school integration policy which aimed to reduce socioeconomic segregation across elementary schools by combining the student bodies of adjacent elementary schools – one with a majority of low-SES students and the other with a majority of high-SES ones – and redistributing the combined student body across the two schools by grade. I find that the policy successfully achieved its primary goal, as paired schools became more socioeco-

[13] Free and reduced-price lunch data are needed to identify the proportion of low-SES students at a school (i.e. the proportion marked eligible for free or reduced-price lunch); as such, schools must have reported statistics for free and reduced-price lunch to the National Center for Education Statistics (NCES) Common Core of Data (CCD) in the 2018-19 academic year to be included in this statistic. The two mile threshold is used as the schools that were paired in CMS lie within two miles of each other.

[14] In one of the CMS pairs, there was a 54 percentage point difference in the proportion of low-SES students prior to pairing, and in the other, there was a 67 percentage point difference.

nomically integrated. However, enrollment dropped significantly at paired schools, suggesting that some students responded to the policy by switching schools. Thus, to understand the causal effects of the policy on educational outcomes at affected schools, I use student-level data and instrument for whether a student attends a treated school, which is a potentially endogenous measure, with whether they were zoned to attend a treated school based on their pre-policy address.

I find that third- through fifth-grade students previously zoned to majority low-SES treated schools who attend treated schools after pairing experience declines in math and reading standardized test scores and are more likely to receive a short-term out of school suspension. In contrast, students previously zoned to majority high-SES treated schools who attend treated schools after pairing experience slight improvements in test scores and no change in short-term suspension rates. Effects on test scores cannot be explained by endogenous teacher movement out of paired schools; in fact, teachers are more likely to stay in paired schools after pairing than they were previously. Additionally, when I move outside the school system and look at the policy's effects on the neighborhoods surrounding paired schools, I find that houses which were previously zoned to low-SES paired schools experience an increase in house prices, conditional on sale.

The results of this paper have three main policy implications: first, school districts must consider both mechanical and potential behavioral responses when designing integration policies, as the specific details of these policies can lead to vastly different outcomes. For example, if high-SES students are only willing to attend integrated schools in which the majority of their peers remain similar to those they would have had in the absence of the integration policy, then a policy that deviates from this norm is likely to face higher non-compliance among high-SES students. These are the kinds of policy design considerations that are crucial to the success of integration efforts. Additionally, flexibility in policy design, along with ongoing evaluation, is necessary to adjust policies over time based on observed responses and outcomes without compromising the goal of greater socioeconomic diversity.

Second, parental responses to pairing suggest the need for more family and community involvement in developing and implementing similar policies. Online articles and school board meeting notes suggest that the policy's development mostly involved families from high-SES schools. This could be part of the reason that high-SES students are significantly less likely to leave paired schools. Moving forward, efforts to involve families and the community in policy design should put equal emphasis on engaging low- and high-SES families.

Thirdly, this paper finds that school pairing differentially impacted low- versus high-SES students, raising equity concerns. Moving forward, it is important to understand whether these differential impacts occur only during the policy's transition phase, or are long-lasting. If low-SES students face challenges only in the first few years due to previous educational experiences that may not have fully prepared them for the instruction level at integrated schools, additional resources such as remedial instruction could be targeted to support their transition and success. However, if instead, low-SES students are always worse off when attending schools with more high-SES students due to cultural, social, or structural factors that consistently disadvantage them in these environments, then integration efforts may need to be reconsidered or supplemented with broader reforms. These reforms might include addressing implicit biases in teacher expectations, creating inclusive school cultures, and ensuring equitable access to academic and extracurricular opportunities.

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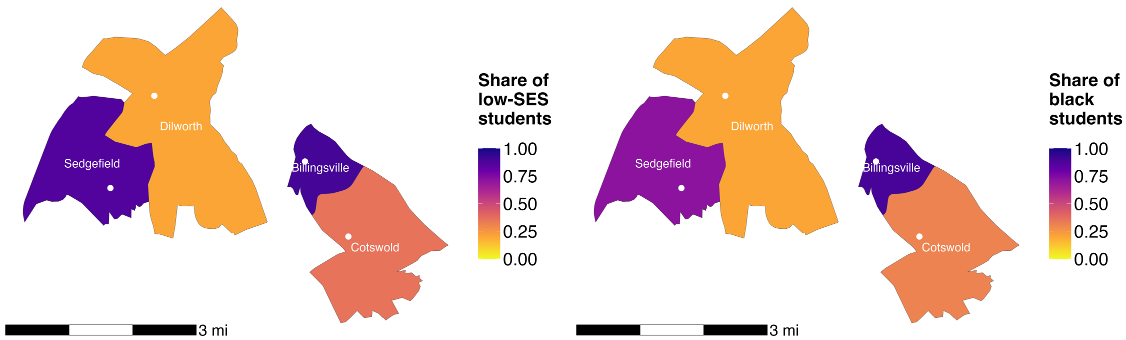
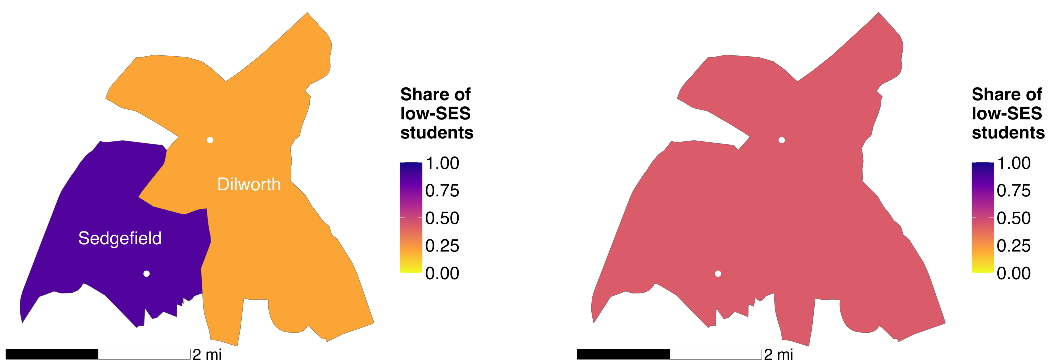
Figures and Tables

Figure 1: Proportion of low-SES and black students in paired schools, 2016-17



(a) Low-SES students (b) Black students

Figure 2: Example of pairing using 2016-17 data



(a) Unpaired (b) Paired

Table 1: CMS elementary school student summary statistics by school pairing status, 2016-17

	Not paired	Paired, low SES	Paired, high SES	P-value: All	P-value: Not paired vs paired low SES	P-value: Not paired vs paired high SES	P-value: Paired low SES vs high SES
Economically disadvantaged	0.47	0.86	0.30	0.000	0.000	0.000	0.000
	(0.50)	(0.35)	(0.46)				
White	0.27	0.02	0.62	0.000	0.000	0.000	0.000
	(0.44)	(0.15)	(0.48)				
Black	0.37	0.79	0.25	0.000	0.000	0.000	0.000
	(0.48)	(0.41)	(0.43)				
Male	0.51	0.55	0.50	0.084	0.093	0.736	0.079
	(0.50)	(0.50)	(0.50)				
Academically gifted	0.09	0.01	0.20	0.000	0.000	0.000	0.000
	(0.29)	(0.09)	(0.40)				
Has a disability	0.08	0.14	0.07	0.000	0.000	0.466	0.000
	(0.27)	(0.35)	(0.26)				
Proficient in reading (grades 3-5 only)	0.57	0.31	0.73	0.000	0.000	0.000	0.000
	(0.49)	(0.46)	(0.44)				
Proficient in math (grades 3-5 only)	0.65	0.46	0.78	0.000	0.000	0.000	0.000
	(0.48)	(0.50)	(0.41)				
Received an in-school suspension	0.09	0.16	0.09	0.000	0.000	0.771	0.000
	(0.29)	(0.37)	(0.28)				
Received a short-term out of school suspension	0.04	0.15	0.02	0.000	0.000	0.023	0.000
	(0.19)	(0.36)	(0.15)				
N	72246	745	1588				

Table 2: CMS elementary school teacher summary statistics by school pairing status, 2016-17

	Not paired	Paired, low SES	Paired, high SES	P-value: All	P-value: Not paired vs paired low SES	P-value: Not paired vs paired high SES	P-value: Paired low SES vs high SES
White	0.69	0.41	0.89	0.000	0.000	0.000	0.000
	(0.46)	(0.49)	(0.31)				
Male	0.18	0.08	0.10	0.004	0.048	0.043	0.927
	(0.38)	(0.28)	(0.30)				
Total gross pay ($)	43275	37846	44531	0.000	0.000	0.372	0.000
	(10890)	(14684)	(10581)				
Has master's degree	0.43	0.44	0.37	0.433	0.951	0.422	0.561
	(0.49)	(0.50)	(0.49)				
Years of teaching experience	12.5	13.2	13.6	0.318	0.798	0.350	0.929
	(9.10)	(8.07)	(9.44)				
National Board Certified	0.15	0.05	0.20	0.006	0.013	0.286	0.004
	(0.36)	(0.21)	(0.40)				
N	7555	88	139				

Table 3: First stage, student-level triple differences analysis

	$treat_i \times$ $ZonedHighSES_{it}$ (1)	$treat_i \times$ $post_t$ (2)	$ZonedHighSES_{it} \times$ $treat_i \times post_t$ (3)
$ZonedHighSES_{it}$	0.023	0.004	0.009*
	(0.014)	(0.003)	(0.005)
$SimTreat_i \times ZonedHighSES_{it}$	0.769***	-0.272***	-0.121***
	(0.068)	(0.039)	(0.017)
$ZonedHighSES_{it} \times post_t$	0.002	0.002	0.004
	(0.002)	(0.004)	(0.005)
$SimTreat_i \times post_t$	-0.118***	0.426***	-0.053***
	(0.024)	(0.159)	(0.009)
$ZonedHighSES_{it} \times SimTreat_i \times post_t$	0.015	0.499***	0.961***
	(0.063)	(0.081)	(0.104)
Observations	117,452	117,452	117,452
School pair FE	✓	✓	✓
Year FE	✓	✓	✓

Note: Standard errors are clustered at the school pair level. Regressions include year fixed effects, school pair fixed effects, and controls for Census tract level characteristics from the 2006-2010 5-year ACS (namely, the proportion of houesholds whose income was below 1.84 times the poverty line, which is the cutoff to qualify for reduced-price lunch, and the proportion of housing units that were rented rather than owned) interacted with year dummies. 2SLS regressions instrument for $treat_i$ with $SimTreat_i$, where $treat_i = 1$ if student i attended a paired school at time t and $= 0$ otherwise, and $SimTreat_i \in [0, 1]$ is the proportion of student i's contemporaneous home Census block that overlapped with paired schools' attendance zones for $t \in [2015, 2017]$, and is the proportion of student i's 2017 home Census block that overlapped with paired schools' attendance zones if $t \in (2019, 2021)$. $t \in [2015, 2017] \cup (2019, 2021)$, $t \in \mathbb{Z}$, with 2015 representing the 2014-15 academic year. $post_t = 1$ if $t \geq 2019$ and $= 0$ otherwise. For $t \in [2015, 2017]$, $ZonedHighSES_{it} = 1$ if student i's home Census block at time t was primarily zoned to a majority high-SES school, and $= 0$ if it was zoned to a low-SES school. For $t \in (2019, 2021)$, $ZonedHighSES_{it}$ is calculated using data from 2017: $ZonedHighSES_{it} = 1$ if student i's 2017 home Census block was primarily zoned to a majority high-SES school, and $= 0$ if it was zoned to a low-SES school.

Figure 3: Proportion of low-SES students in CMS elementary schools, 2016-17

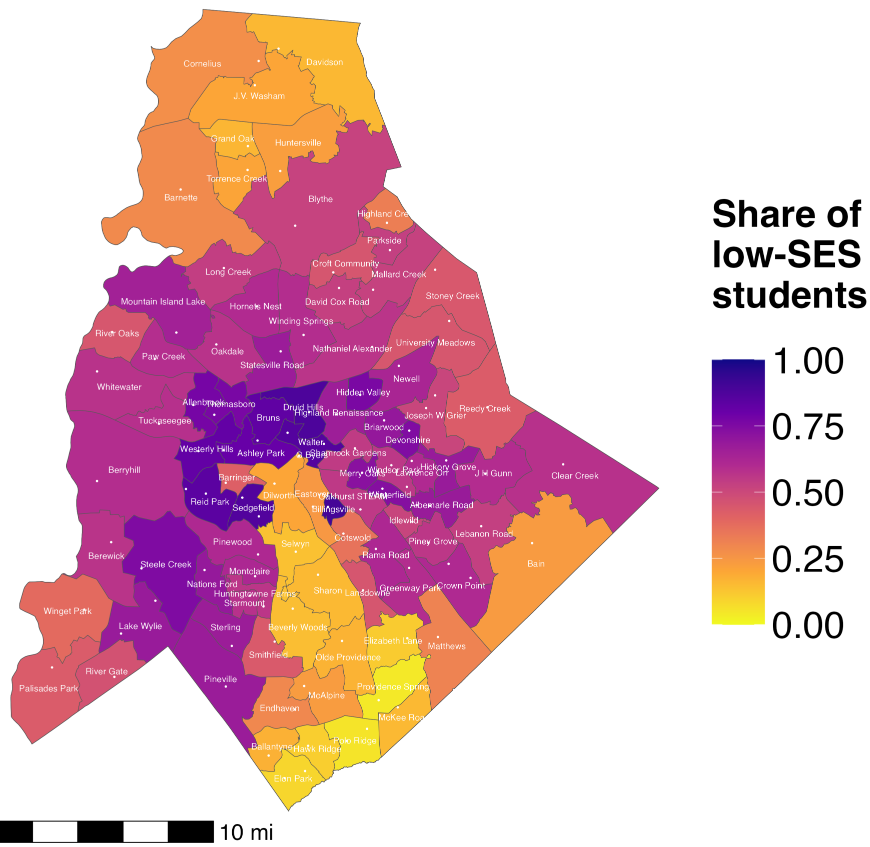
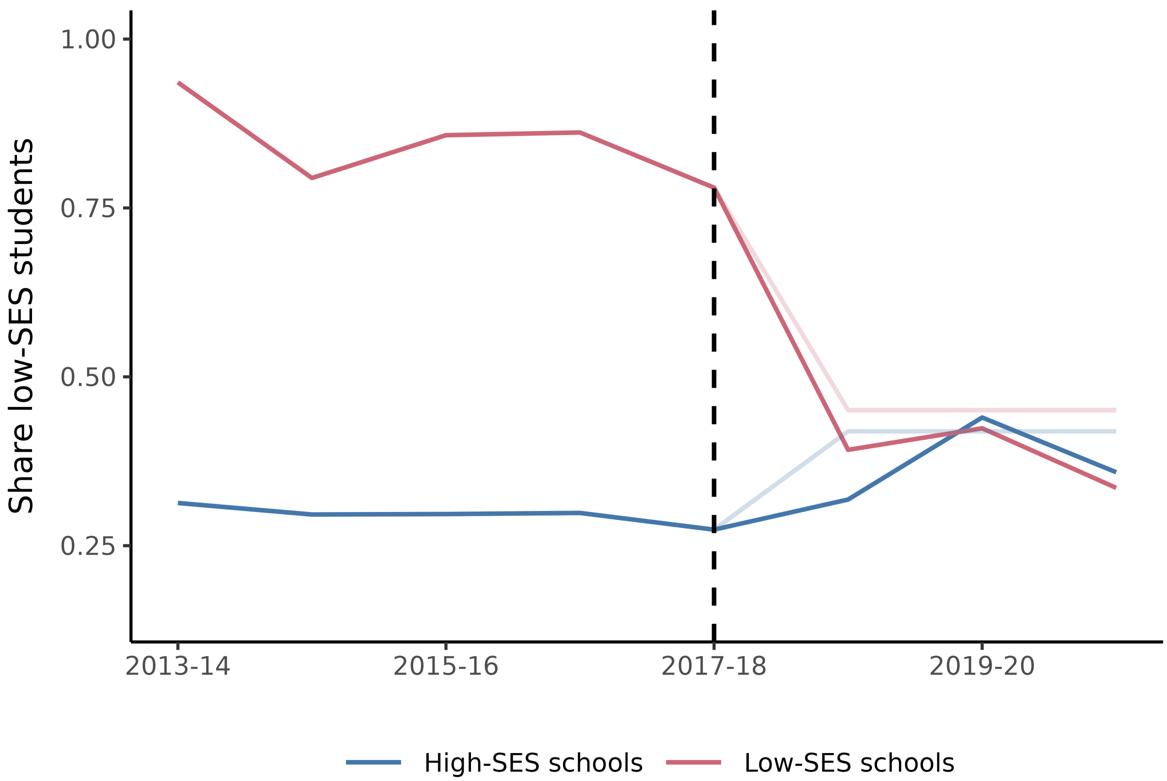
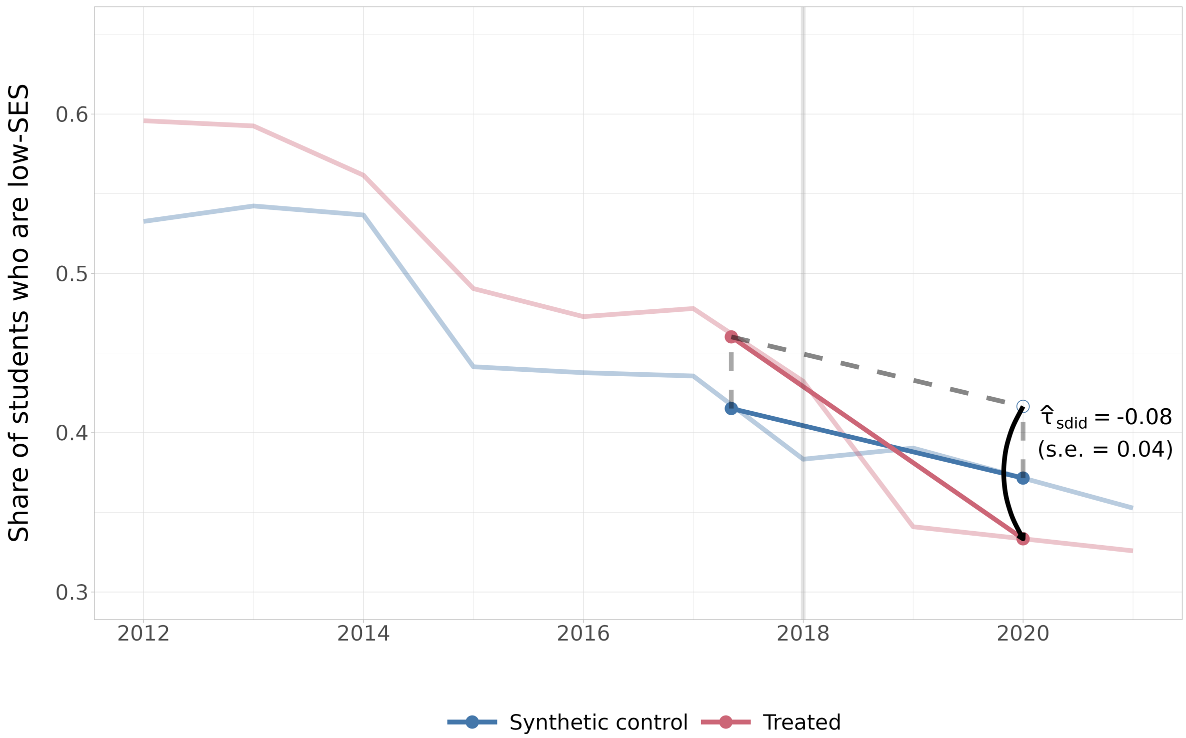


Figure 4: Share of low-SES students at treated schools after pairing under perfect compliance versus in reality



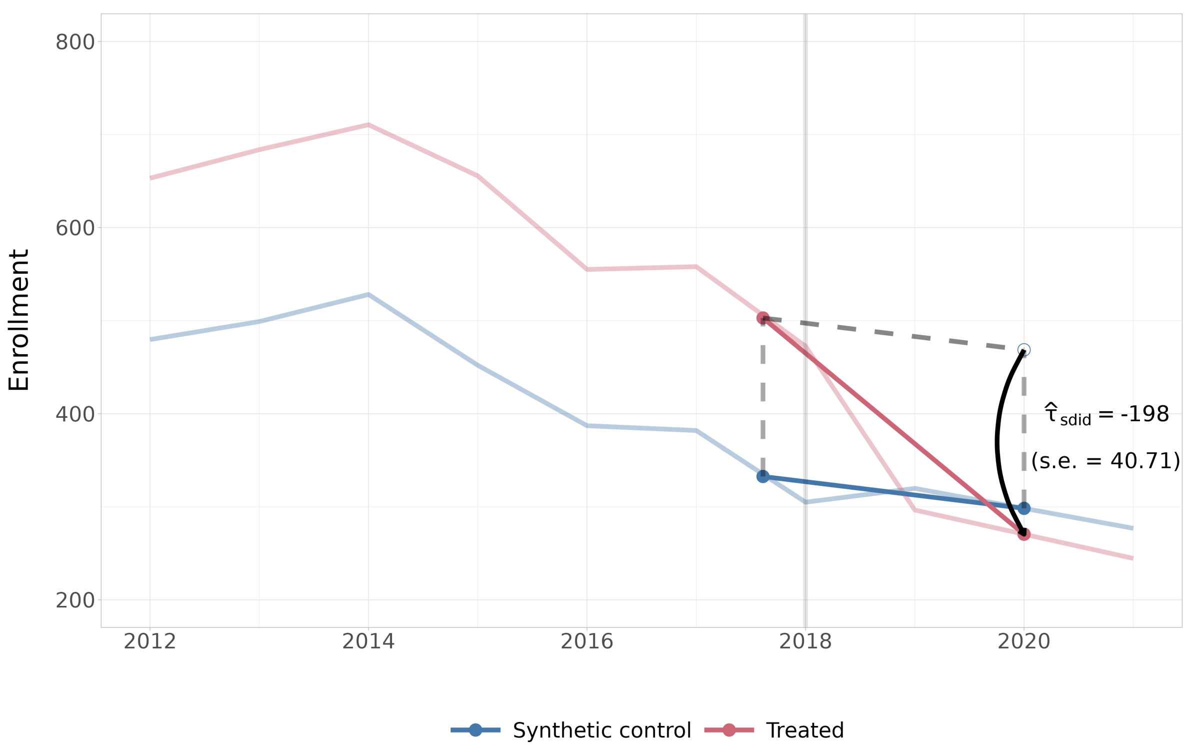
Note: Transparent lines indicate what enrollment at treated schools would have looked like if students were not allowed to change schools in response to pairing. Solid lines after 2017-18 indicate what enrollment actually looked like at treated schools after pairing.

Figure 5: Share of low-SES students in school pairs over time



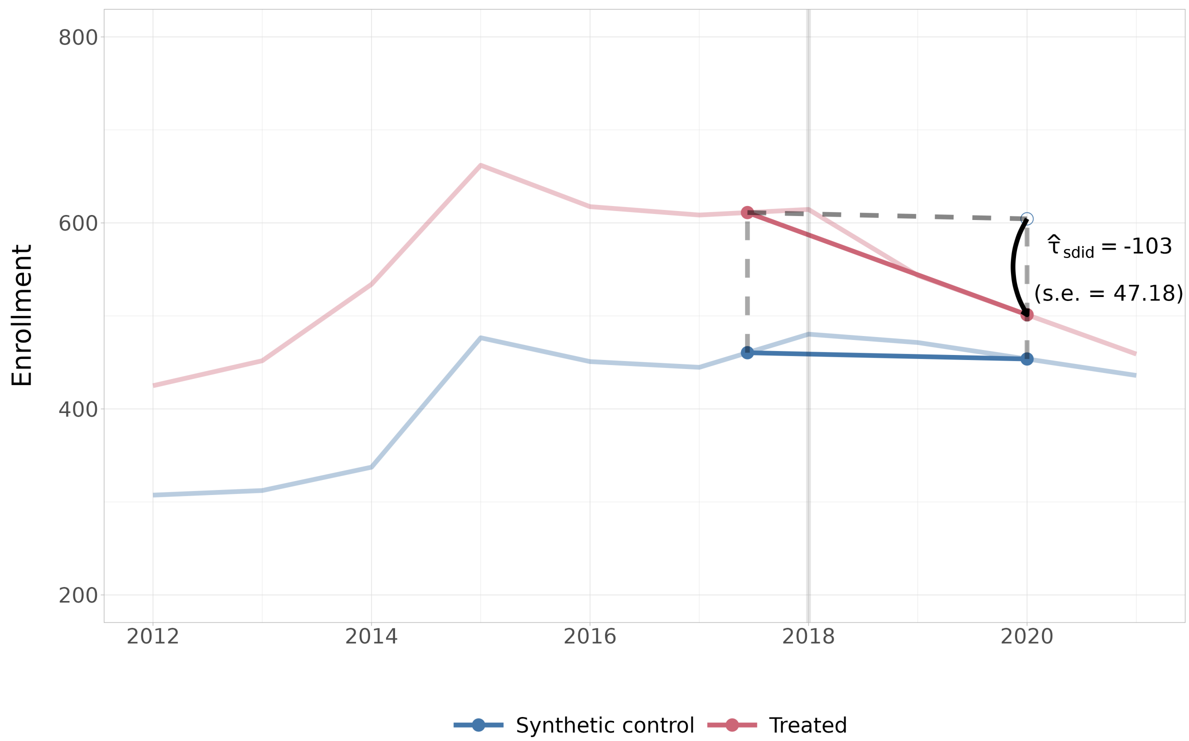
Note: Time weights are as follows: 2015 = 0.02, 2016 = 0.01, 2017 = 0.58, 2018 = 0.39.

Figure 6: Effects on public school enrollment of low-SES students



Note: Time weights are as follows: 2016 = 0.03, 2017 = 0.32, 2018 = 0.64.

Figure 7: Effects on public school enrollment of high-SES students



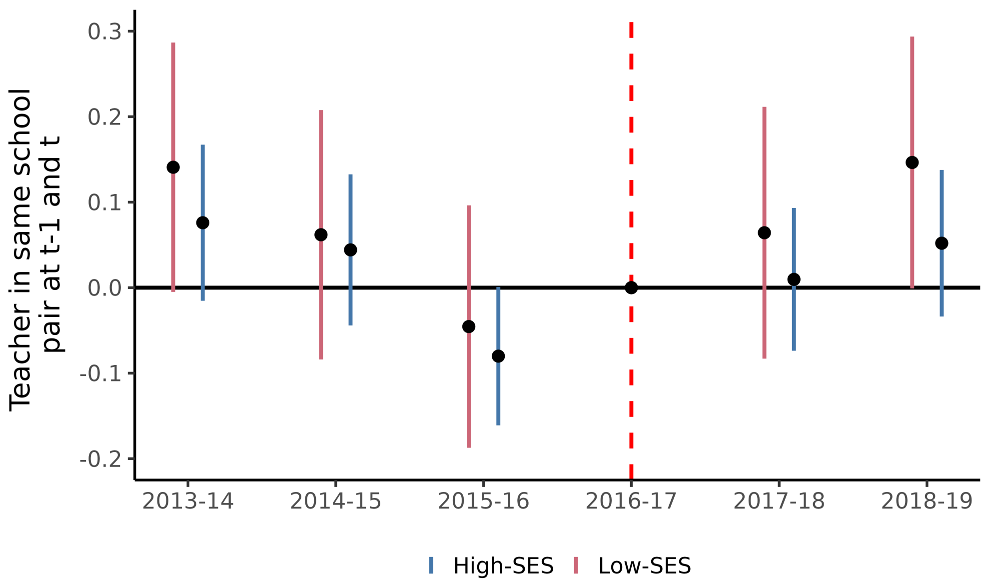
Note: Time weights are as follows: 2017 = 0.56, 2018 = 0.44.

Table 4: Student-level triple differences analysis

	Percentile on math exam			Percentile on reading exam			Has a disability			Received a short-term suspension		
	OLS	2SLS	Reduced form	OLS	2SLS	Reduced form	OLS	2SLS	Reduced form	OLS	2SLS	Reduced form
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$ZonedHighSES_{it}$	0.059*	0.048	0.063*	0.098***	0.083***	0.102***	-0.016	-0.017	-0.018*	-0.001	0.000	-0.002
	(0.032)	(0.033)	(0.032)	(0.030)	(0.030)	(0.030)	(0.010)	(0.010)	(0.010)	(0.004)	(0.004)	(0.004)
$treat_i \times ZonedHighSES_{it}$	0.486***	0.584***		0.596***	0.745***		-0.005	-0.001		-0.068***	-0.049	
	(0.138)	(0.173)		(0.178)	(0.207)		(0.017)	(0.026)		(0.025)	(0.038)	
$ZonedHighSES_{it} \times post_t$	0.081**	0.080**	0.082**	-0.014	-0.016	-0.014	-0.001	-0.001	-0.001	0.002	0.001	0.001
	(0.034)	(0.034)	(0.033)	(0.026)	(0.026)	(0.026)	(0.007)	(0.007)	(0.007)	(0.005)	(0.005)	(0.005)
$treat_i \times post_t$	-0.077**	-0.182		-0.105**	-0.151		0.038*	0.027		0.015	0.127	
	(0.034)	(0.166)		(0.045)	(0.158)		(0.020)	(0.049)		(0.036)	(0.088)	
$ZonedHighSES_{it} \times treat_i \times post_t$	0.205***	0.349**		0.169***	0.271*		-0.072***	-0.059		-0.007	-0.115	
	(0.041)	(0.164)		(0.024)	(0.159)		(0.021)	(0.048)		(0.030)	(0.083)	
$SimTreat_i \times ZonedHighSES_{it}$			0.460***			0.586***			-0.001			-0.058***
			(0.133)			(0.162)			(0.017)			(0.018)
$SimTreat_i \times post_t$			-0.163**			-0.164***			0.015			0.066***
			(0.065)			(0.057)			(0.022)			(0.023)
$ZonedHighSES_{it} \times SimTreat_i \times post_t$			0.257***			0.199***			-0.043*			-0.048***
			(0.070)			(0.057)			(0.024)			(0.018)
Observations	113,266	113,266	113,266	112,789	112,789	112,789	117,452	117,452	117,452	117,452	117,452	117,452
Mean of dep. variable, $treat_i = 1$, $post_t = 0$, $ZonedHighSES_{it} = 0$	-0.47	-0.47	-0.47	-0.52	-0.52	-0.52	0.15	0.15	0.15	0.13	0.13	0.13
Mean of dep. variable, $treat_i = 1$, $post_t = 0$, $ZonedHighSES_{it} = 1$	0.50	0.50	0.50	0.61	0.61	0.61	0.07	0.07	0.07	0.02	0.02	0.02
Marginal treatment effect, zoned to high SES	0.13	0.17		0.06	0.12		-0.03	-0.03		0.01	0.01	
Wald (1st stage), $treat_i \times ZonedHighSES_{it}$		279.23			276.10			231.28			231.28	
Wald (1st stage), $treat_i \times post_t$		378.09			381.30			378.21			378.21	
Wald (1st stage), $ZonedHighSES_{it} \times treat_i \times post_t$		1,010.73			990.03			1,061.91			1,061.91	
School pair FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note: Standard errors are clustered at the school pair level. Math and reading test scores are standardized to have a mean of zero and standard deviation of one in the baseline. Regressions include year fixed effects, school pair fixed effects, and controls for Census tract level characteristics from the 2006-2010 5-year ACS (namely, the proportion of households whose income was below 1.84 times the poverty line, which is the cutoff to qualify for reduced-price lunch, and the proportion of housing units that were rented rather than owned) interacted with year dummies. 2SLS regressions instrument for $treat_i$ with $SimTreat_i$, where $treat_i = 1$ if student i attended a paired school at time t and $= 0$ otherwise, and $SimTreat_i \in [0, 1]$ is the proportion of student i's contemporaneous home Census block that overlapped with paired schools' attendance zones for $t \in [2015, 2017]$, and is the proportion of student i's 2017 home Census block that overlapped with paired schools' attendance zones if $t \in (2019, 2021)$. $t \in [2015, 2017] \cup (2019, 2021)$, $t \in \mathbb{Z}$, with 2015 representing the 2014-15 academic year. $post_t = 1$ if $t \geq 2019$ and $= 0$ otherwise. For $t \in [2015, 2017]$, $ZonedHighSES_{it} = 1$ if student i's home Census block at time t was primarily zoned to a majority high-SES school, and $= 0$ if it was zoned to a low-SES school. For $t \in (2019, 2021)$, $ZonedHighSES_{it}$ is calculated using data from 2017: $ZonedHighSES_{it} = 1$ if student i's 2017 home Census block was primarily zoned to a majority high-SES school, and $= 0$ if it was zoned to a low-SES school.

Figure 8: Event study of annual teacher retention in treated schools after school pairing announcement, by pre-2018 majority SES of school



Note: The mean of the dependent variable in 2016-17 is 0.66 in low-SES schools and 0.88 in high-SES schools.

Table 5: Impact of school pairing on house prices and probability of sale

	Log sale price				Probability of sale	
	(1)	(2)	(3)	(4)	(5)	(6)
$treat_{hb}$	0.014	-0.005	-0.059	-0.215	0.024*	0.034
	(0.115)	(0.059)	(0.049)	(0.194)	(0.013)	(0.032)
$post_t$	0.101**	0.107***	0.128***	0.082*	-0.009	-0.022
	(0.042)	(0.032)	(0.026)	(0.046)	(0.009)	(0.021)
$treat_{hb} \times post_t$	0.123*	0.128***	0.095**	0.187*	-0.011	-0.039
	(0.064)	(0.044)	(0.041)	(0.103)	(0.018)	(0.041)
$ZonedHighSES_{hb}$				-0.187**		0.150***
				(0.075)		(0.035)
$ZonedHighSES_{hb} \times treat_{hb}$				0.193		-0.016
				(0.199)		(0.036)
$ZonedHighSES_{hb} \times post_t$				0.056		0.015
				(0.054)		(0.024)
$ZonedHighSES_{hb} \times treat_{hb} \times post_t$				-0.114		0.036
				(0.111)		(0.046)
House characteristics		✓	✓	✓	✓	✓
Neighborhood FE			✓	✓	✓	✓
N	2232	2030	2029	2029	8922	8922

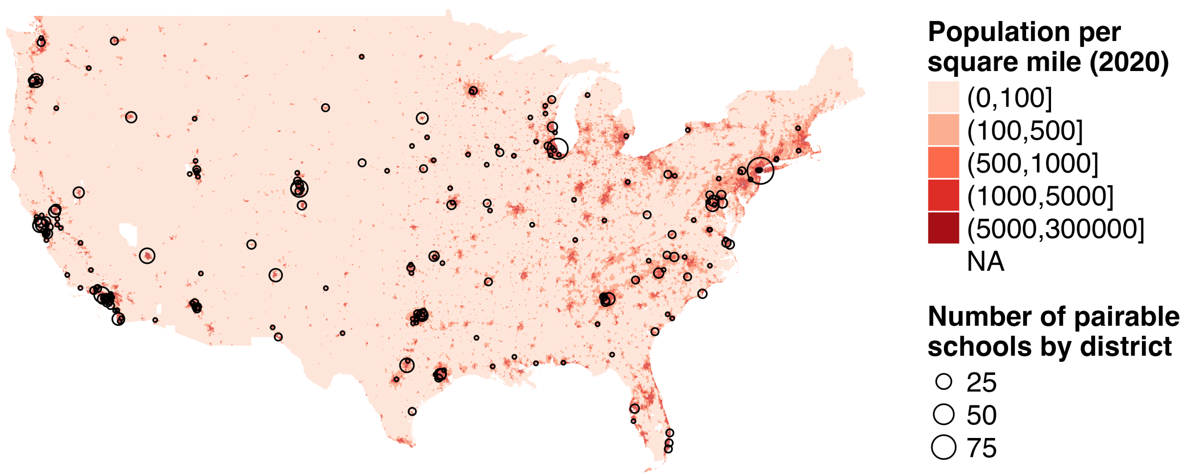
Note: Standard errors are clustered at the nearest boundary level. $treat_{hb}$ equals 1 if house h along boundary b is on the side of the boundary that lies within the treated attendance zone and equals 0 if it is on the side of the boundary that does not lie within the treated attendance zone. $post_t$ equals 1 for t between April 2017 and March 2020 and equals 0 for t between April 2014 and March 2017. $HighSES_{hb}$ is equal to 1 if house h lies within 300 meters of an attendance boundary b that borders a previously majority high-SES paired school, and equal to 0 if it borders a previously majority low-SES paired school. Regressions which control for house characteristics specifically control for the number of bedrooms and bathrooms in the house, its acreage and living square footage, and the year it was built. Neighborhood fixed effects represent fixed effects for the boundary b that house h is closest to.

Appendix

Table of Contents

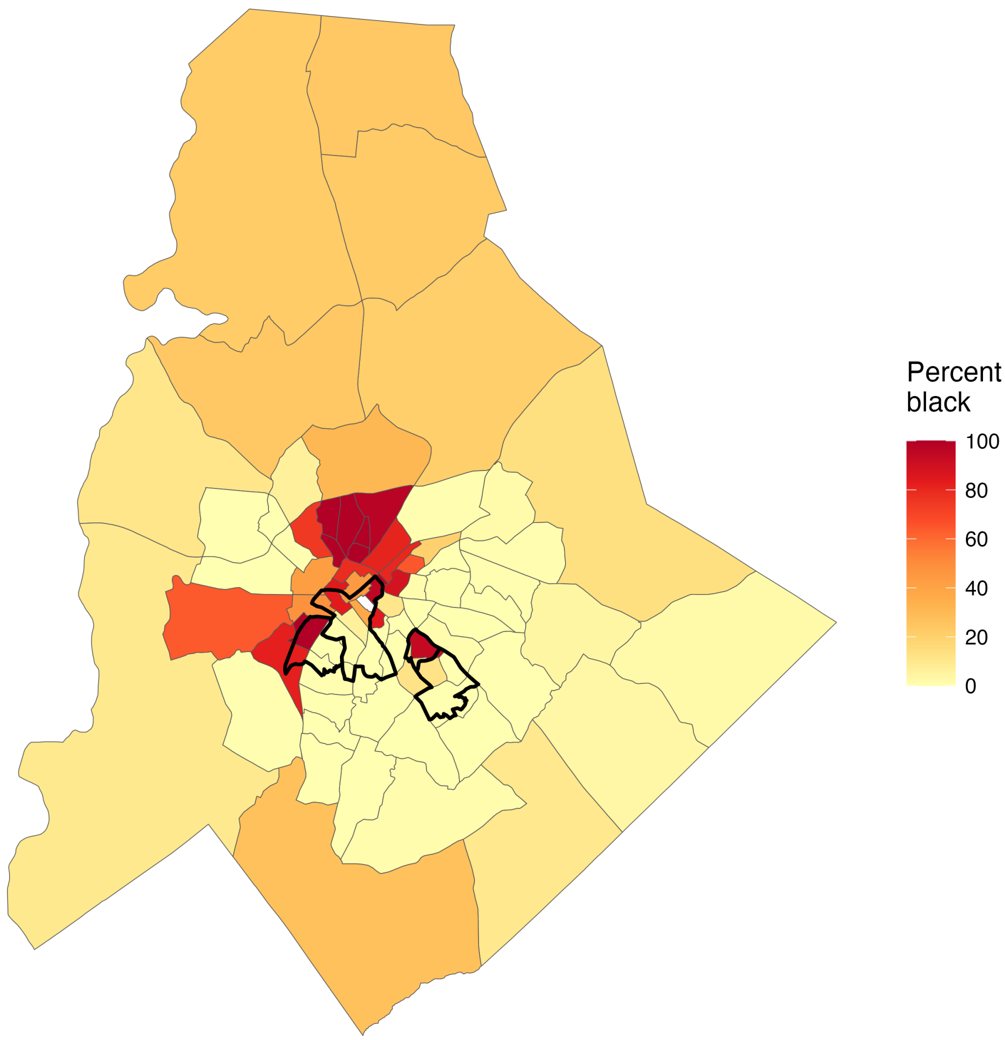
A Appendix Figures and Tables

Figure A1: Number of schools that could be paired under the same criteria used by CMS in 2018-19 by school district



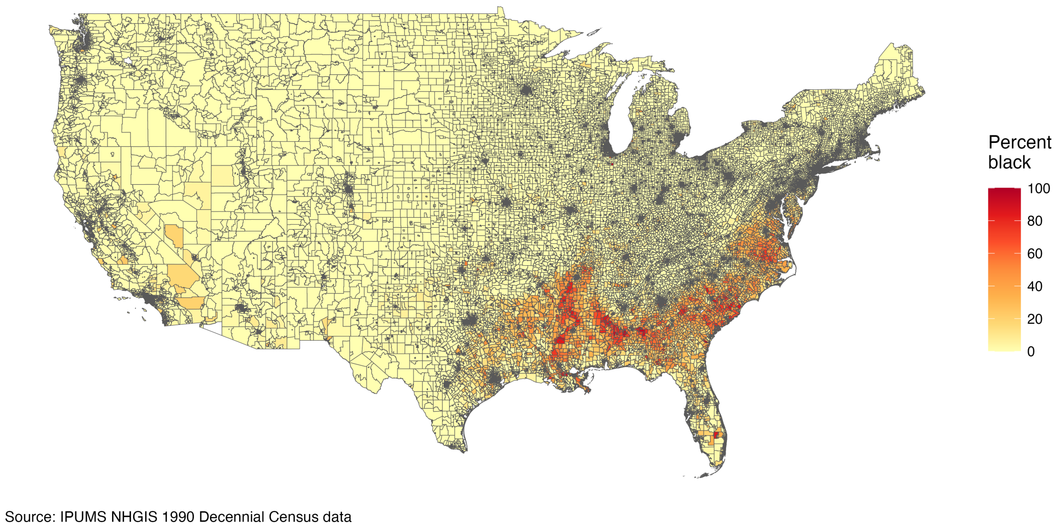
Source: NCES 2018-19 Common Core of Data and 2020 Census data.

Figure A2: Racial composition of Mecklenburg County Census tracts in 1970 overlayed with paired schools' attendance zones in 2015-16

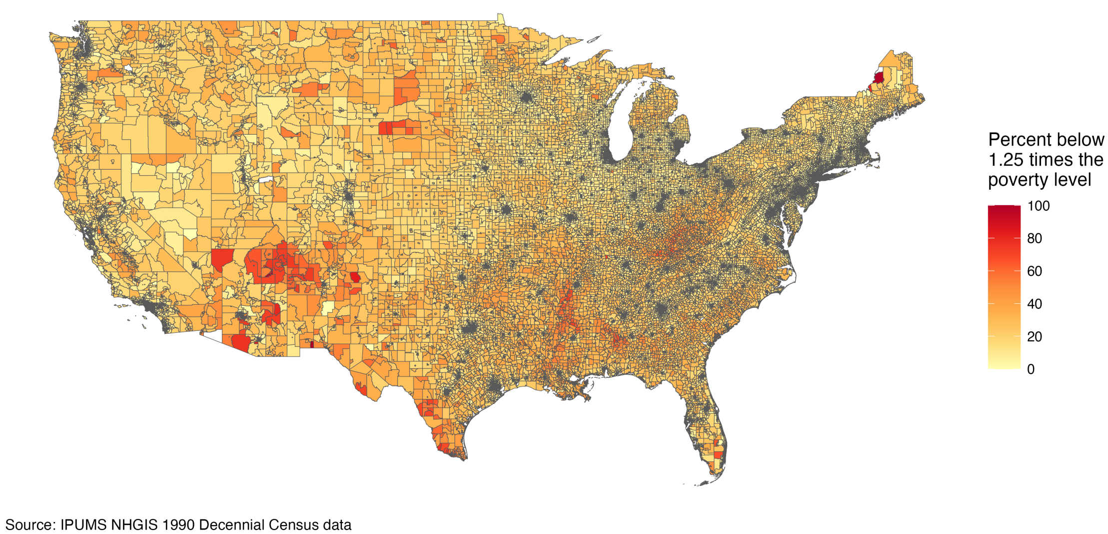


Source: IPUMS NHGIS 1970 Decennial Census data and 2015-16 NCES School Attendance Boundary Survey (SABS) data

Figure A3: Distribution of race versus income in 1990 across continental US by Census tract

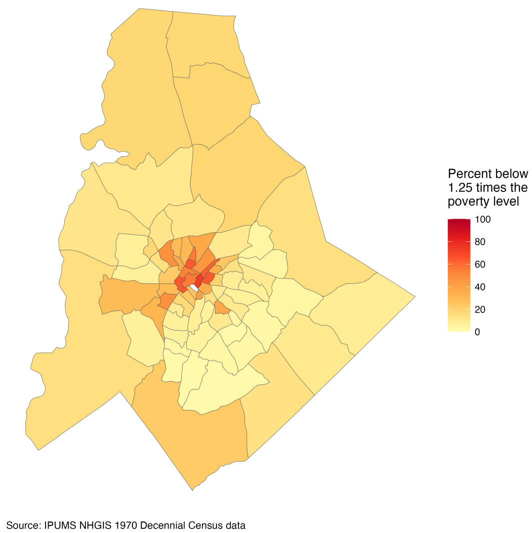


(a) Panel A: Race

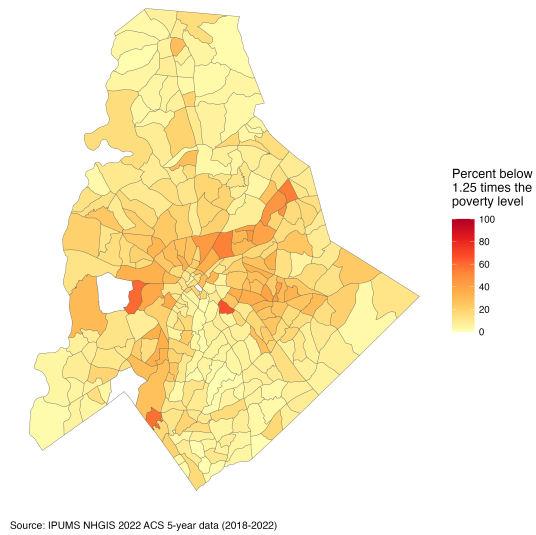


(b) Panel B: Income

Figure A4: Distribution of income in Mecklenburg County by Census tract, 1970 vs 2020



(a) Panel A: 1970



(b) Panel B: 2020

Table A1: Schools attended by students who were in K-4th grade at paired schools in 2017-18

School status	SES of school attended in 2017-18	Share	Average distance to school (miles)
Attends charter school in Mecklenburg County	High	0.01	
Attends non-CMS public school outside of Mecklenburg County	High	0.02	
Attends non-treated CMS school	High	0.16	4.82
Attends treated CMS school	High	0.74	1.75
Not in NC public school data	High	0.07	
Attends charter school in Mecklenburg County	Low	0.01	
Attends non-CMS public school outside of Mecklenburg County	Low	0.04	
Attends non-treated CMS school	Low	0.45	3.39
Attends treated CMS school	Low	0.47	1.85
Not in NC public school data	Low	0.03	

Figure A5: Distribution of private schools across Mecklenburg County in 2022

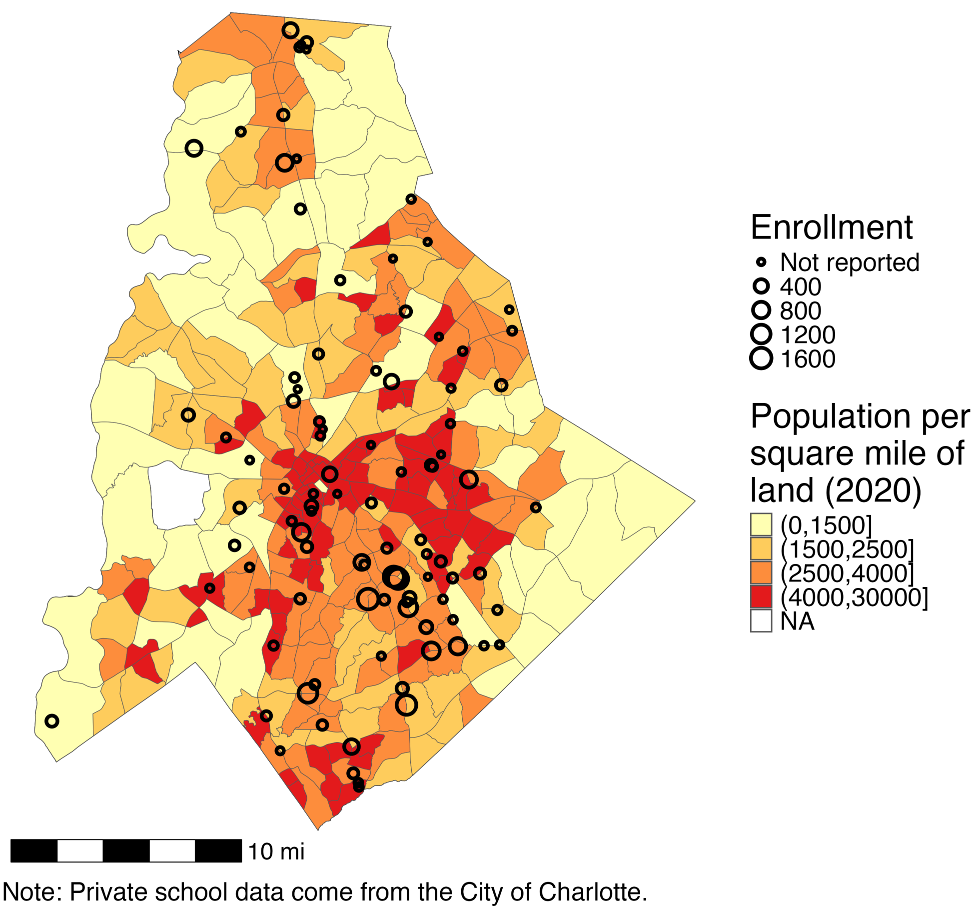


Figure A6: Overlap between 13-character Census blocks and treated schools

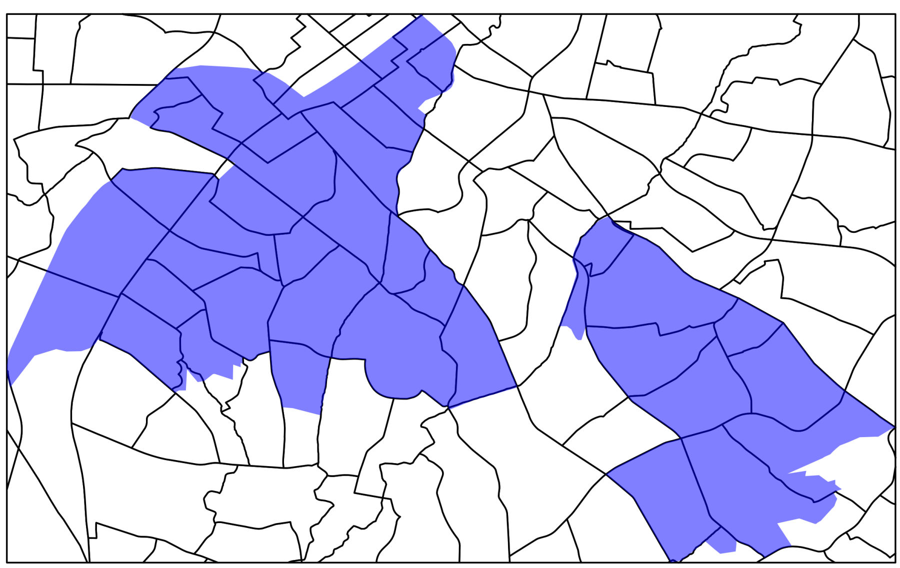


Figure A7: Geographic distribution of public schools included in synthetic difference-in-differences analysis

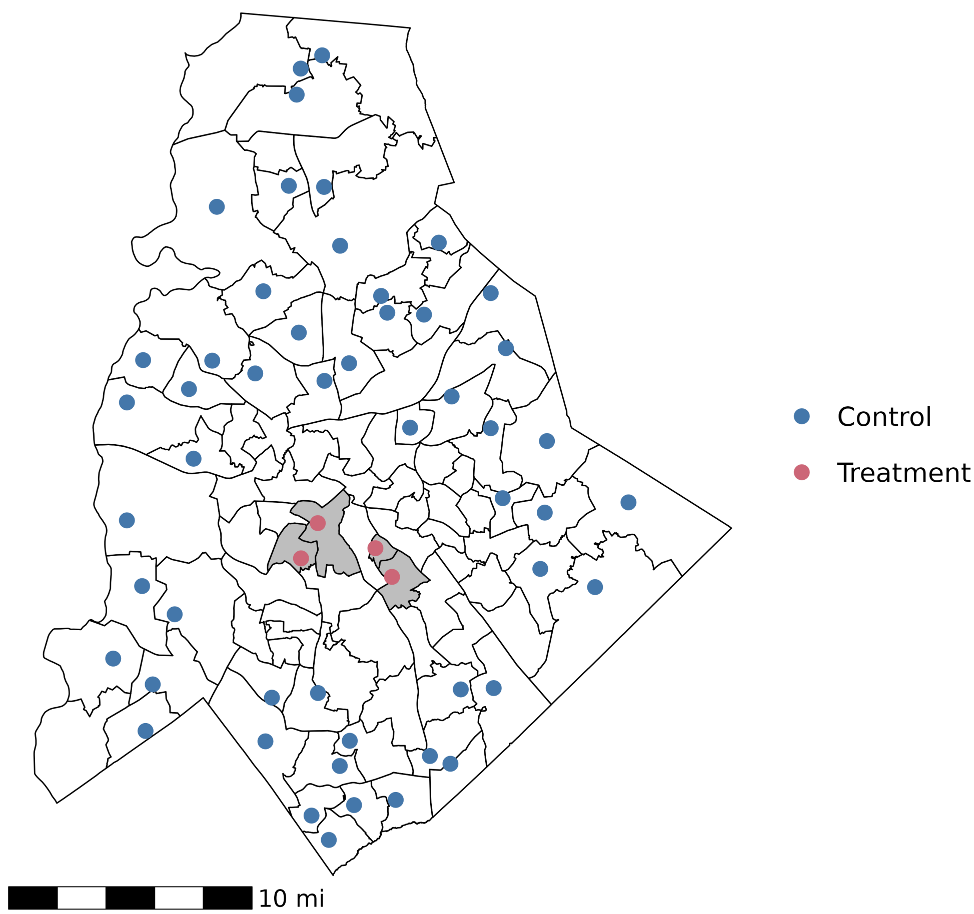


Figure A8: Geographic distribution of private schools included in synthetic difference-in-differences analysis

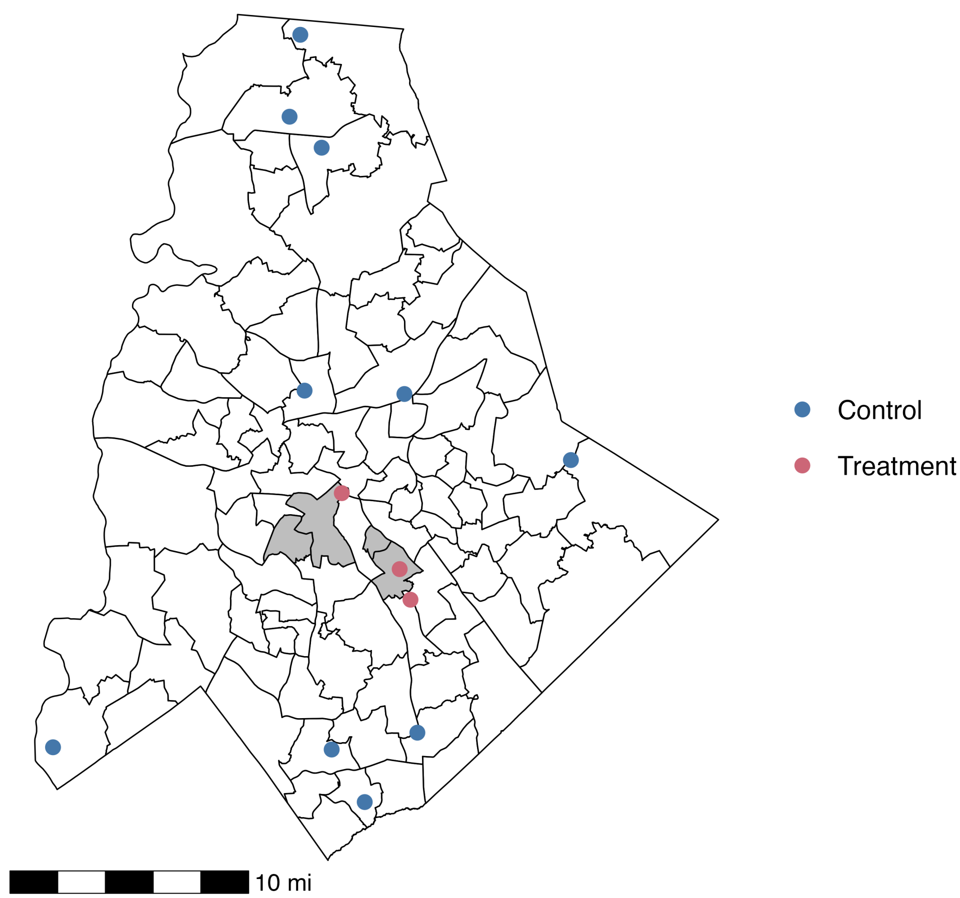
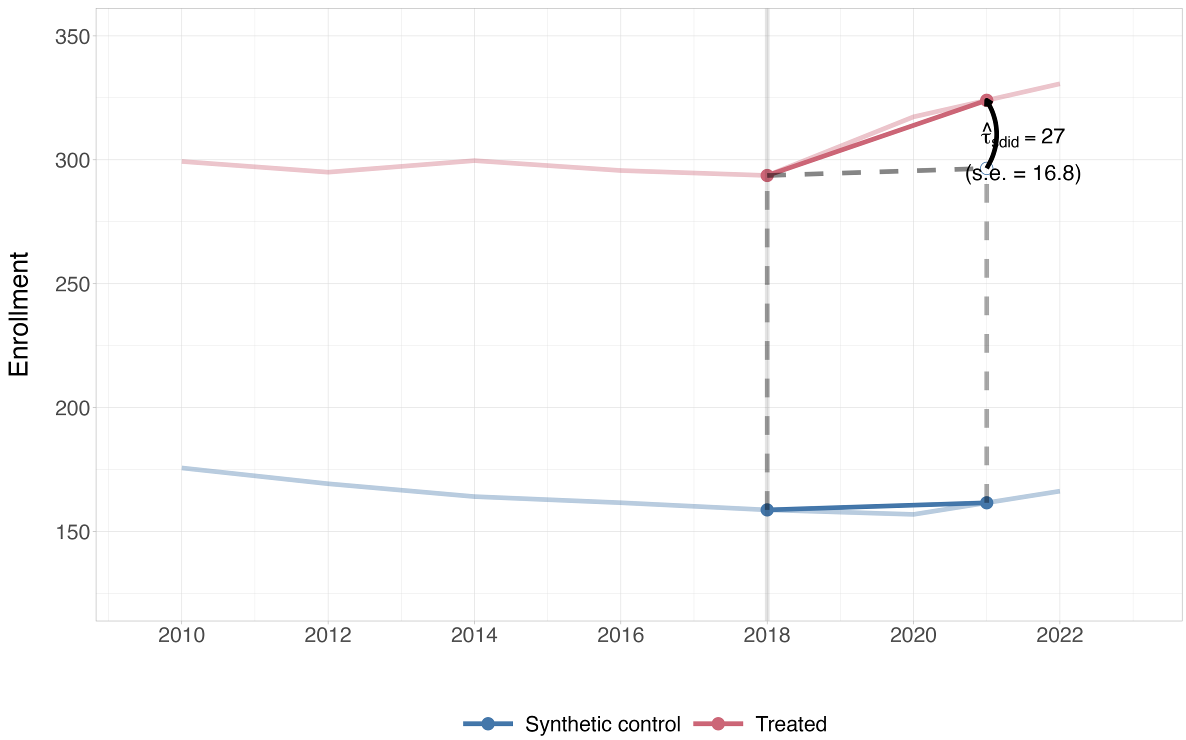


Figure A9: Effects on private school K-5 enrollment



Note: Time weights are as follows: 2018 = 1.

Table A2: Pre-trends, student-level triple differences analysis

	Percentile on math exam		Percentile on reading exam		Has a disability		Received a short-term suspension	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)	OLS (7)	2SLS (8)
$ZonedHighSES_{it}$	0.183***	0.179***	0.124**	0.116**	-0.025**	-0.025**	-0.022**	-0.021**
	(0.053)	(0.051)	(0.049)	(0.047)	(0.010)	(0.010)	(0.009)	(0.009)
$treat_i \times ZonedHighSES_{it}$	0.329***	0.353***	0.537***	0.590***	-0.060***	-0.025	-0.046*	-0.047*
	(0.099)	(0.084)	(0.136)	(0.168)	(0.011)	(0.043)	(0.023)	(0.027)
$ZonedHighSES_{it} \times post_t$	-0.099***	-0.100***	-0.019	-0.020	0.015**	0.014**	0.014**	0.014**
	(0.035)	(0.035)	(0.028)	(0.028)	(0.006)	(0.006)	(0.006)	(0.006)
$treat_i \times post_t$	-0.075	-0.085	-0.054	-0.074	-0.029***	0.024	0.009	0.022
	(0.095)	(0.145)	(0.077)	(0.092)	(0.007)	(0.070)	(0.009)	(0.025)
$ZonedHighSES_{it} \times treat_i \times post_t$	0.143	0.185	0.061	0.110	0.045***	-0.010	-0.022**	-0.037
	(0.093)	(0.146)	(0.061)	(0.079)	(0.012)	(0.069)	(0.009)	(0.026)
Observations	158,184	158,184	157,062	157,062	163,223	163,223	163,671	163,671
Mean of dep. variable, $treat_i = 1$, $post_t = 0$, $ZonedHighSES_{it} = 0$	-0.40	-0.40	-0.47	-0.47	0.17	0.17	0.14	0.14
Mean of dep. variable, $treat_i = 1$, $post_t = 0$, $ZonedHighSES_{it} = 1$	0.45	0.45	0.59	0.59	0.05	0.05	0.03	0.03
Marginal treatment effect, zoned to high SES	0.07	0.10	0.01	0.04	0.02	0.01	-0.01	-0.02
Wald (1st stage), $treat_i \times ZonedHighSES_{it}$		1,234.60		1,238.18		1,164.67		1,154.86
Wald (1st stage), $treat_i \times post_t$		340.68		334.76		308.99		304.74
Wald (1st stage), $ZonedHighSES_{it} \times treat_i \times post_t$		1,139.26		1,142.92		916.87		923.91
School pair FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: Standard errors are clustered at the school pair level. Math and reading test scores are standardized to have a mean of zero and standard deviation of one in the baseline. Regressions include year fixed effects, school pair fixed effects, and controls for Census tract level characteristics from the 2006-2010 5-year ACS (namely, the proportion of households whose income was below 1.84 times the poverty line, which is the cutoff to qualify for reduced-price lunch, and the proportion of housing units that were rented rather than owned) interacted with year dummies. $treat_i = 1$ if student i attended a paired school at time t and $= 0$ otherwise. $SimTreat_i \in [0, 1]$ is the proportion of student i's contemporaneous home Census block that overlapped with paired schools' attendance zones at time t. $t \in [2012, 2017]$, $t \in \mathbb{Z}$, with 2012 representing the 2011-12 academic year. $post_t = 1$ if $t \geq 2015$ and $= 0$ otherwise. $ZonedHighSES_{it} = 1$ if student i's home Census block at time t was primarily zoned to a majority high-SES school, and $= 0$ if it was zoned to a low-SES school.

Table A3: Characteristics of treated group and treated compliers

Student characteristics	All treated students	Compliers with SimTreat > 0	Relative likelihood
Black	0.344	0.594	1.724
White	0.506	0.090	0.178
Hispanic	0.099	0.315	3.175
Asian	0.029	0.086	2.981
Low-SES	0.360	0.634	1.762
Received short-term suspension	0.053	0.050	0.943
Percentile on math exam	60.205	43.282	0.719
Percentile on reading exam	56.980	41.703	0.732

Note: Relative likelihood is calculated as the ratio of the mean characteristic for compliers who attended paired schools to the mean characteristic for all paired school attendees for each characteristic. Sample is restricted to post-policy years: 2018-19, 2019-20, and 2020-21.

Table A4: Pre-trends, neighborhood analysis

	Log sale price				Probability of sale	
	(1)	(2)	(3)	(4)	(5)	(6)
$treat_{hb}$	-0.005	0.021	-0.047	-0.195	0.017	0.058**
	(0.142)	(0.079)	(0.053)	(0.202)	(0.012)	(0.025)
$post_t$	0.153*	0.249***	0.244***	0.297***	0.044***	0.060***
	(0.090)	(0.053)	(0.048)	(0.092)	(0.013)	(0.020)
$treat_{hb} \times post_t$	0.019	-0.031	-0.059	-0.021	0.003	-0.033
	(0.104)	(0.070)	(0.050)	(0.127)	(0.016)	(0.033)
$ZonedHighSES_{hb}$				-0.322***		0.093***
				(0.048)		(0.027)
$ZonedHighSES_{hb} \times treat_{hb}$				0.190		-0.052*
				(0.210)		(0.028)
$ZonedHighSES_{hb} \times post_t$				-0.061		-0.021
				(0.106)		(0.026)
$ZonedHighSES_{hb} \times treat_{hb} \times post_t$				-0.056		0.044
				(0.138)		(0.038)
House characteristics		✓	✓	✓	✓	✓
Neighborhood FE			✓	✓	✓	✓
N	2063	1866	1865	1865	8922	8922

Note: Standard errors are clustered at the nearest boundary level. $treat_{hb}$ equals 1 if house h along boundary b is on the side of the boundary that lies within the treated attendance zone and equals 0 if it is on the side of the boundary that does not lie within the treated attendance zone. $post_t$ equals 1 for t between April 2014 and March 2017 and equals 0 for t between April 2011 and March 2014. $HighSES_{hb}$ is equal to 1 if house h lies within 300 meters of an attendance boundary b that borders a previously majority high-SES paired school, and equal to 0 if it borders a previously majority low-SES paired school. Regressions which control for house characteristics specifically control for the number of bedrooms and bathrooms in the house, its acreage and living square footage, and the year it was built. Neighborhood fixed effects represent fixed effects for the boundary b that house h is closest to.

Figure A10: Sample of properties included in housing analysis

B Understanding the LATE

To characterize compliers, I use the approach developed by Abadie (2003) and explained by Angrist and Pischke (2009). This approach is for binary instruments but can be extended to instances in which the instrument is discrete but not binary, as is the case in this paper. Define Z_i to be my instrument, $SimTreat_i$, and D_i to be actual treatment status, $treat_i$. Compliers are individuals for whom $D_{1i} > D_{0i}$. In the case where Z is a binary variable, this means if $Z_i = 1$, $D_i = 1$, and if $Z_i = 0$, $D_i = 0$. In the case where Z is a discrete, non-binary variable, compliers are individuals for whom a higher value of Z_i increases the likelihood of treatment.
The mean of characteristic X for compliers is

$$E[X_i|D_{1i} > D_{0i}] = \frac{E[\kappa_i X_i]}{E[\kappa_i]}.$$

In the binary case, κ_i is defined as follows:

$$\kappa_i = 1 - \frac{D_i(1-Z_i)}{1-P(Z_i=1|X_i)} - \frac{(1-D_i)Z_i}{P(Z_i=1|X_i)}.$$

In the discrete, non-binary case, κ_i is defined as follows:

$$\kappa_i = 1 - \frac{D_i(1-Z_i)}{1-\hat{F}(Z_i|X_i)} - \frac{(1-D_i)Z_i}{\hat{F}(Z_i|X_i)},$$

where $\hat{F}(Z_i|X_i)$ is the empirical cumulative distribution function estimate of Z given X_i, based on the observed distribution of Z in the sample.[15]

[15]For each value of Z_i, $\hat{F}(Z_i|X_i)$ represents the proportion of observations with $Z \leq z$ given X_i, similar to the CDF in the continuous case but calculated discretely for each value in the discrete set.

C More Policy Background

There were no changes made to the middle school and high school assignments of students affected by the Billingsville and Cotswold pairing – they are still assigned to attend Alexander Graham Middle and Myers Park High after pairing. There were also no changes made to the middle and high school continuation schools for students in the modified Sedgefield Elementary attendance area – they continue to attend Sedgefield Middle and Myers Park High. However, beginning with students in the sixth grade in the 2019-2020 school year, the original Dilworth Elementary attendance area attends Sedgefield Middle instead of Alexander Graham for middle school. The change in the middle school home school area did not affect students in sixth grade prior to the 2019-2020 school year. There was no change in the high school assignment (Myers Park High).

In addition to the two school pairs I study in this paper, two other CMS schools were paired in the 2018-19 academic year: Nathaniel Alexander and Morehead STEM were paired to form Governors' Village STEM Academy (lower and upper campuses), with kindergartners through fourth-graders attending the original Nathaniel Alexander campus, and fifth- through eighth-graders attending the original Morehead STEM campus. Students in the Nathaniel Alexander home school attendance area would still attend Vance High School (now renamed Julius L Chambers High School), but Morehead STEM did not previously have an assigned neighborhood attendance zone associated with it as it was a full magnet school that offered kindergarten through eighth grade. Given that all of the other paired schools were neighborhood (non-magnet) schools that only served grades K-5, I abstract from studying the pairing of Nathaniel Alexander and Morehead STEM in this paper.