New Schools and New Classmates: The Disruption and Peer Group Effects of School Reassignment*

Darryl V. Hill
Unaffiliated Researcher
darryl_hill@mail.harvard.edu

Rodney P. Hughes
West Virginia University
rodney.hughes@mail.wvu.edu

Matthew A. Lenard
Harvard University
mlenard@g.harvard.edu

David D. Liebowitz
University of Oregon
daviddl@uoregon.edu

Lindsay C. Page
Brown University
lindsay_page@brown.edu

November 2022

Abstract

Policy makers periodically consider using student assignment policies to improve educational outcomes by altering the socio-economic and academic skill composition of schools. We exploit the quasi-random reassignment of students across schools in the Wake County Public School System to estimate the academic and behavioral effects of being reassigned to a different school and, separately, of shifts in peer characteristics. We rule out all but substantively small effects of transitioning to a different school as a result of reassignment on test scores, course grades and chronic absenteeism. In contrast, increasing the achievement levels of students' peers improves students' math and ELA test scores but harms their ELA course grades. Test score benefits accrue primarily to students from higher-income families, though students with lower family income or lower prior performance still benefit. Our results suggest that student assignment policies that relocate students to avoid the over-concentration of lower-achieving students or those from lower-income families can accomplish equity goals (despite important caveats), although these reassignments may reduce achievement for students from higher-income backgrounds.

Keywords: peer effects, student assignment, school integration, school mobility

***JEL* codes**: H75, I21, I24, I28, J24

*We thank the Wake County Public School System and the Center for Education Policy Research at Harvard University for facilitating data access. Jesse Dalton provided unflagging support for our remote access of the data. We thank Jason Cook, Joshua Goodman, Richard Murnane, Elizabeth Setren, Glen Waddell, several anonymous referees and seminar participants at AEFP, APPAM, and the University of Oregon for helpful feedback. Liebowitz received financial assistance from the 2011 HGSE Dean's Summer Fellowship. The views expressed in this paper do not reflect the views or opinions of the Wake County Public School System. All errors are our own.

1 Introduction

Recent scholarship and extensive associated media attention have shed light on growing rates of U.S. income and wealth inequality and declining rates of social mobility.[1] Simultaneously, differences in academic achievement between children from high- and low-income families remain large (Hasim et al., 2020; Hanushek et al., 2019; Reardon, 2011).[2] Policy makers regularly express interest in opportunities to reduce the strength of the relationship between students' socio-demographic characteristics and their educational outcomes. One such strategy involves changing children's within-school peer groups by reassigning students to attend school with peers of different socio-economic and academic skill backgrounds (e.g., Strauss, 2017; Belsha and Darville, 2020).

This strategy makes several assumptions about the ways in which students' peer groups influence their academic outcomes and about the consequences of changing schools. First, such a strategy assumes that lower-family-income or low-performing students might learn more effectively if exposed to classmates with socio-economic or academic-skill backgrounds different from their own. Second, this approach assumes that higher-family-income or higher-performing students either experience no harm from such school reassignments or that the harm is sufficiently minimal to justify the benefits to higher-need students. These first two assumptions implicitly argue that the structure by which peers influence each other is not identical among all students or, more formally, that the linear-in-means model of peer effects (Hoxby, 2000; Manski, 1993) does not hold. Third, such a strategy assumes that the potential benefits of changing the composition of one's peer group by switching schools dominate any potentially disruptive effects of adjusting to a new school.[3]

As a result, the implementation and comprehensive evaluation of student reassignment policies should be informed by the answers to two key questions: (1) what impact does changing schools have on students who are required to switch as a result of school reassignment; and (2) how do changes in peer composition that result from school reassignment policies affect students' outcomes?

In this paper, we exploit a quasi-random selection of students who were reassigned to different schools to inform these two key questions. We rely on administrative data from the 2005-06 to 2011-12 school years in the Wake County Public School System (WCPSS). During these years, WCPSS regularly reassigned a small share of its overall student body to attend different schools. These moves served both to address over-crowding in a rapidly growing metropolitan area and to limit the concentration of lower-family-income and academically struggling students in any

[1] Examples from the research literature include Chetty et al. (2017); Chetty et al. (2014); Piketty and Saez (2003); and Saez and Zucman (2016).

[2] While Hasim et al., Hanushek et al. and Reardon reach different conclusions about the long-term trends in achievement by SES, all conclude that the present-day 90-10 income percentile achievement gap is between 0.65 and 1.25 standard-deviation units for all subjects and grades, except that Hasim and co-authors estimate the 4th grade reading gap at 0.35 *SD* units.

[3] An additional assumption of this approach is that higher-income parents or parents of higher-achieving children will not send their children to private schools or move districts. An additional mechanism through which such an approach might improve student outcomes is through a redistribution of resources following the redistribution of students across schools. Exploration of these assumptions and mechanisms is beyond the scope of our analysis.

one school. The reassignment policy we consider relied on students' family-income and prior achievement to promote socio-demographic integration; accordingly, these are the dimensions of peer effects on which we focus.

Our identification strategy leverages the fact that, conditional on observable characteristics used to inform the assignment process, groups of students were selected arguably at random to attend different schools. Our critical assumption, which we justify with policy details and empirical tests, is that selection for assignment is *conditionally* ignorable. We employ instrumental variable approaches to estimate the effect of changing schools and, separately, the effect of shifts in peer composition driven by the district's practice of reassigning some students. For school movers, selection for reassignment, conditional on a set of baseline characteristics, serves as an instrument for switching to a different school without changing residence. For those not selected for reassignment, the policy-assigned change in peer composition serves as an instrument for the change in peer composition actually experienced.

Our analysis of peer effects builds most directly on Hoxby and Weingarth's (2005) study of the same (and the prior) school assignment policy in WCPSS. Our paper innovates beyond theirs in several important ways. First, as a result of our access to WCPSS administrative data associated with the school assignment process, we directly observe student-level school assignment and compliance with reassignment. Capitalizing on these data, we model selection for reassignment at the geographic level at which it occurred and address the endogenous bias resulting from students selectively complying with reassignment to different schools. To the best of our understanding, Hoxby and Weingarth did not have access to such student-level assignment data and, therefore, they assume a high level of compliance (see also Weingarth, 2005). In contrast, during the time period that we consider (one that does not overlap with their analytic window), almost one-half of students who are selected to switch schools do not comply. Second, relying on more recent insights from Angrist (2014), our approach to estimating peer effects relies only on the subset of students who experience exogenous changes in their peers' characteristics with no other contemporaneous changes in their educational experience. More specifically, our estimates of peer effects rely only on those students who are *not* selected for reassignment in a given year. Third, we examine outcomes beyond standardized test performance, including course grades and attendance.

To preview our results, we observe no substantively meaningful effects on test scores, absenteeism or course grades for students who change schools as a result of being reassigned, on average. We estimate null effects, on average, on test-score and attendance outcomes with relatively precise zeros. Domina et al. (2021) examine the effects of being selected for reassignment in Wake County between 2000 and 2010 in an event study framework. Despite only partially overlapping analytic windows and different identification strategies, our estimates of the average effect of being reassigned to a different school are comparable to theirs. An important advantage of our research design is our ability to model the behavior and outcomes of those who actually switch schools as a result of reassignment, rather than the intent-to-treat estimates of Domina and co-authors. Further, our data provides us information unexplored in Domina et al., permitting us to consider variation in effects by students' socio-demographic characteristics and prior achievement. We find suggestive evidence that switching schools due to reassignment negatively affects test-score outcomes for students with lower prior achievement.

Our central peer effects finding is that students' academic skills, as measured by standardized test scores, improve from having higher-achieving peers. A one-tenth of a standard deviation increase in students' peer-achievement level produces improvements in students' own test scores of 0.05 *SD*s [95% CI: 0.01, 0.08] in math and 0.03 *SD*s [95% CI: 0.01, 0.04] in English Language Arts (ELA). However, such an increase in peer achievement decreases students' course grades by 0.02 *SD* units, potentially through a mechanism of relative-rank comparisons. Similar to Denning et al. (2018), though in contrast to Murphy and Weinhardt (2020), we observe this phenomenon in ELA but not mathematics courses.

Test score benefits derived from higher-achieving peers are largest for students who do not qualify for free- or reduced-price school meals (FRPL) and are greatest in math (but not ELA) for higher-achieving students. However, FRPL-qualifying students and students with lower baseline achievement nevertheless benefit from higher-achieving peers. Thus, our estimates reject both the strictly linear-in-means model of peer effects as well as the Single-Crossing model (e.g., Bénabou, 1996; Epple et al., 1993). In math, students throughout the performance distribution experience course grade benefits from higher-achieving peers, whereas weaker-performing students experience the bulk of the negative effects on their ELA course grades.

Our findings contribute in two important ways to the understanding of peer effects and policies on school integration. First, we add to the broad body of causal literature on the impacts of changing the characteristics of one's peers. We find that, on the whole, students learn more when their peers are higher achieving. On the other hand, students receive worse course grades in ELA when they have higher-achieving peers, and these harms accrue primarily to low-achieving students. Our focus on changes in peers' family-income and prior-achievement levels underscores the policy-relevance of our study compared to others that emphasize changes in peer ethnoracial composition because, in the current legal climate, the use of students' race in K-12 student assignment policy is largely curtailed (see *Parents Involved v. Seattle*, 2007). Second, we find minimal evidence of negative effects from mandated school reassignment, on average, across multiple outcomes; however we do find suggestive evidence of negative effects of switching schools due to reassignment for low-achieving students.

Although understanding the consequences of school reassignment for movers and for stayers are critically intertwined research aims, their analytic approaches and associated assumptions are distinct. As a result, we structure our paper in an atypical manner. In Section 2, we motivate our study in the research literature and local policy context. We provide details of our data in Section 3. Then, in Section 4, we address the effects of the school assignment policy for those who are selected for reassignment and move to a different school as a result. Within this section, we provide details of our analytic strategy, test its assumptions and provide results. Next, in Section 5, we address the effects of the school assignment policy for those who are not selected to move but who may nevertheless have been affected through changes in their peers' demographic characteristics. We introduce a separate analytic approach, test its distinct assumptions and share our results. Finally, in Section 6, we integrate our results and conclude.

2 Background and Wake County Context

2.1 Peers' influence on learning

A rich research literature considers the effects peers have on their classmates' learning opportunities and outcomes. Sacerdote (2014) synthesizes the complex causal research base, concluding that peer effects in the elementary and secondary school contexts depend not only on the characteristics of one's peers but also on the characteristics of the individual and the interaction of the two. For the most part, well-identified studies have found smaller or no effects in linear-in-means specifications and larger effects in non-linear models (Sacerdote, 2011, 2014). Such non-linearities point to the potential to redistribute students across classrooms or schools in ways that would result in net learning gains. In some cases, high-SES neighborhood and school peers provide increased access to additional school resources, social capital and powerful networks for children (e.g., Bayer et al., 2008). As another explanation of the same phenomenon, high-ability peers might share knowledge, skills or learning and performance orientations with classmates and multiply the effects of in-class learning (Hoxby, 2000; Kimbrough et al., 2020; Patacchini et al., 2017).

Whereas the preceding results highlight the direct effect school peers have on each other, other peer effects models imply that school and classroom composition may affect the challenge of the teaching task. Burke and Sass (2013) find that grouping students with like-ability peers, whether of high- or low-ability, generates the greatest gains, implying that the complexities of the teaching task are simplified when students in a given class present with a smaller spread of starting abilities. Hoxby and Weingarth (2005) categorize non-linear peer effects structures with an evocative nomenclature. Through a decile-by-decile analysis of students' own achievement interacted with peer characteristics, they find evidence that students benefit from classrooms in which peer achievement is similar to their own. They also find that student test scores improve most in contexts with homogeneous levels of peer performance even if individuals themselves perform differently from this peer group. They term the first classroom composition the Boutique model of peer effects and the second the Focus model.

In addition to positive peer effects associated with exposure to high-skill or like-ability classmates, negative effects may occur when lower-family-income students are concentrated in schools and classrooms (Epple et al., 2002; Vigdor and Nechyba, 2007). Students who attend a predominantly low-income school are more likely to have highly mobile classmates who struggle with academics, attention, and behavior (Raudenbush et al., 2011). Xu et al. (2022) highlight one potential mechanism for these peer effects, documenting that low-achieving peers who have repeated a year have negative spillovers on peers' study habits.

Importantly, some of the ways the challenges of poverty manifest themselves are in the expression of anti-social behaviors that spill over into the school experiences of peers. Lower-family-income students are much more likely to have classroom peers who have experienced a higher frequency of childhood traumatic events and who are more likely to exhibit inappropriate classroom behavior (Duncan and Magnuson, 2011). Further, lower-family-income students who have traumatized children in their classrooms are also more likely to misbehave in class, as a product of the presence of traumatized children (Carrell and Hoekstra, 2010). These experiences carry far into the future and can manifest in worse labor market outcomes (Carrell et al., 2018). Such negative repercussions can also occur when classmates have had a parent who was arrested (Billings and Hoekstra, 2022). In fact, such peer influences are evident in the formation of criminal networks (Billings et al., 2019).

One frequent mechanism employed to understand the structure of peer effects is when students switch schools as a result of changing residences.[4] Hanushek and co-authors (2004) find in a Texas-based sample that moving homes and schools, independent of school quality, has a negative effect on both movers and their new peers, particularly for low-income students. In fact, whether as a result of foreclosure (Herbers et al., 2013), homelessness (Fantuzzo et al., 2012), natural disasters (Sacerdote, 2012), or the sale of their rental residence (Schwartz et al., 2017), students in these studies experience worse outcomes after moving. However, housing policies intended to ameliorate the neighborhood characteristics of program recipients have had mixed educational results (e.g., Sanbonmatsu et al., 2011; Schwartz, 2010), and the direction of their impact may depend on the age at which children move (e.g., Chetty et al., 2016). Of course, the observed changes in school context are likely compounded by other life and family structure changes simultaneous to or resulting from their residential move, so the independent causal effect of moving schools or changing peers is difficult to assess.

Another particularly common opportunity for studying the causal effect of switching schools is when schools close. Brummet (2014) finds that students who leave closed schools in Michigan experienced a short-term dip, with improved mid-term academic outcomes if they left a particularly low-performing school. However, Engberg and colleagues (2012) document persistent negative effects to being displaced as a result of school closures, though they find that these negative effects can be minimized when students move to higher-performing schools. Integrating the preceding findings, Bifulco and Schwegman (2020) conclude that an accountability-based school closure policy in New York had positive effects for high-performing students who avoided low-performing schools as a result but hurt low-performing students in shuttered schools. Again, school closures couple changes in schools and peers with community upheaval and distress (e.g., Ewing, 2020) such that isolating the causal effects of school switching and of peer effects presents a thorny challenge. Through this study, we seek to disentangle these phenomena.

2.2 Background on school assignment in Wake County

As of the 2018-19 school year, the Wake County Public School System (WCPSS) enrolled approximately 160,000 students and was the 15$^{\text{th}}$ largest U.S. school district. The district's efforts to use student assignment policy to promote diversity in schools have been widely publicized

[4] Generally, analysts consider three types of school moves: *structural*, *non-structural* and *policy-related* school changes. Structural moves involve between-grade-level changes and tend to cause declines in student test scores (e.g., Rockoff and Lockwood, 2010; Grigg, 2012; Schwerdt and West, 2013). However, as all students switch schools at these grade-level transition points and everyone experiences changes in peers, such structural moves are generally poor candidates to isolate the causal effects of peer changes. Non-structural moves tend to suffer from endogeneity challenges. As a result, policy-driven school changes such as within- or cross-district integration programs (e.g., Angrist and Lang, 2004; Mantil, 2021; Bergman, 2021) have dominated much of the K-12 peer effects literature.

and studied. During the years we examine (2005-06 to 2011-12), WCPSS sought to accomplish two distinct goals through its student assignment policy: (1) to ensure that no school served a student body made up of more than 40 percent economically disadvantaged students—defined operationally as whether the student received free- or reduced-price lunch (FRPL)—or more than 25 percent of students reading below grade level; and (2) to fill newly constructed schools and alleviate overcrowding in response to a greater than 50 percent growth in its student population between 2000-01 and 2011-12.[5] To do so, district administrators selected students residing within designated geographic areas (referred to as "nodes") for reassignment from their base (neighborhood) school to another existing or new school each year.

In Appendix B, we provide a broader discussion of the history of student school assignment in WCPSS. We refer readers to Carlson et al. (2019) and Parcel and Taylor (2015) for further details. Here, we highlight two features that are critical to our analytic approach. First, despite a common understanding among educational policy observers that the assignment policy in the first decade of the new millennium was intended to promote socio-economic integration in schools, the majority of relocated students were reassigned to respond to rapidly growing student populations, overcrowding, and the need to redistribute students to newly opened schools (Carlson et al., 2019; Hoxby and Weingarth, 2005; Parcel and Taylor, 2015). As one former school board member explained, the children were moved, "(. . .) from school to school because of population growth, and that is what it was. The busing was not intended primarily for diversity but just to fill in these schools" (Parcel and Taylor, 2015, p. 53). In accordance with Hoxby and Weingarth (2005) and Carlson et al. (2019), we present evidence below that only a small number of student reassignments demonstrably changed the achievement and family income levels of students' peers for students who were reassigned to a new school by virtue of the policy. This is important to contextualize the interpretation of our results in relation to settings in which most reassigned students experienced dramatic changes in their schooling environment (e.g., Angrist and Lang, 2004; Billings et al., 2014; Bergman, 2021). Given these contextual details, we interpret our main impacts of reassignment as the pure effect of switching schools, rather than the combined effect of switching schools and changing peer composition. However, for a small set of students who did not change schools, reassignment nevertheless did substantially change the composition of their classrooms, and it is from these students that we obtain the identifying variation for our peer effect estimates.

Second, the selection of any given geographic node for reassignment was, conditional on observable traits of the node, essentially random and not manipulable or anticipated by node residents. Each of the roughly 1,500 nodes represents a small geographic unit, sometimes as small as a city block, a housing development or an apartment complex that includes fewer than 150 students (see Figure 1 for the district's 2011-12 node map). As a result of the reassignment plan, geographically proximal and observationally similar nodes were treated differently. Students from the same geographic area but different assignment nodes, who had been assigned to

[5] The stated criteria for re-assignment from the WCPSS Office of Growth and Management were: under- and over-capacity at existing and new schools, expansion of year-round schools, school facility improvements, distance of students to schools, enrollment trends, percent of students from families qualifying for free- or reduced-price lunch and the reading scores for students in grades 3-8 (Hoxby and Weingarth, 2005).

attend the same school in one year, would be assigned to attend different schools the following year. Importantly, these decisions were to be made by the centralized WCPSS Office of Growth and Management, relying on data-based and public criteria to which we have access. Thus, in principle, these policy circumstances provide support for our contention that reassignment decisions were conditionally as-good-as random. However, in contrast to Hoxby and Weingarth's (2005) assumptions about reassignment compliance, we find that a large share of students (40 – 50 percent) did not comply with their reassignments during the years we study. According to Parcel and Taylor (2015), principals reported that many assigned students failed to appear from the first day, with some successfully appealing and others simply refusing to relocate from their original school (p. 53-54). We handle the presence of this non-compliance through the use of instrumental variables strategies to investigate the impacts of school reassignment and changes in peer composition driven by the WCPSS school assignment policy. This strategy permits us to extend Domina and colleagues' (2021) analysis by exploring estimands of critical policy import: what were the effects of *actually* switching schools in response to reassignment and of experiencing changes in peer composition resulting from reassignment, unbiased by endogeneous differences in who experiences these changes?

3 Data

We leverage student-level administrative records from the Wake County Public Schools to address our research questions. These data provide standard sociodemographic information, including student gender, race/ethnicity, and FRPL status. In addition, we observe, by year, each student's grade level, geographic node of residence, school assignment based on node of residence, actual school attended, and the most recent reassignment date for node of residence. In the main paper, we focus on results for middle-school students, as our identifying assumptions are not fully satisfied at the elementary level (see below). Nevertheless, we provide full results for 4^{th} and 5^{th} graders in Appendix C. We provide additional details on our data and sample construction in Appendix D.

We estimate the impacts of school reassignment and peer composition on student-level academic and attendance outcomes. Our academic outcomes include course grades in mathematics and English Language Arts (ELA) and scaled scores from the North Carolina End-of-Grade assessments in mathematics and ELA. Our attendance outcome is an indicator for chronic absence, equal to 1 if a student misses more than 10 percent of the academic year or 18 school days and zero otherwise.[6]

Our analyses focus on the period from 2005-06 to 2011-12 during which there was relative consistency in WCPSS in terms of assessment, accountability and the implementation of the district's school reassignment policy. Our starting analytic sample includes all 7^{th} and 8^{th} grade students attending Wake County schools in this period. For students in these grades, we can observe at least one prior year of standardized assessment performance. Additionally, in these grades, students typically do not move to a different school except in the case of school

[6]This is the official definition of chronic absenteeism used by the North Carolina Department of Public Instruction (NCDPI).

reassignment or a household move. In contrast, nearly all WCPSS students transition to a different school when they enter 6th grade.[7] In addition, we limit our data to begin with the 2005-06 school year because in that year, the NCDPI implemented new math standards which resulted in a significant revision to and rescaling of the state's EOG tests. These standards and associated achievement tests were used through the 2012-13 school year, when the state adopted the Common Core State Standards in both math and reading.[8] The 2012-13 school year was also when WCPSS formally implemented a new student assignment policy.[9]

During this seven-year period, the district selected 39,084 students across all grades to switch schools. Of these, 4,914 students feature in our sample of 7th and 8th grade students. In Table 1, we present descriptive statistics for students selected for reassignment to either newly opened or existing schools and for their non-selected, grade-level peers attending the same initial schools. In addition, we present information for the subset of students who complied with reassignment, labeled as "reassigned school switchers." Despite the political prominence of the district's reassignment policy, a surprisingly small share of all students (5.4 percent) was selected for reassignment across the years we examine.[10] As noted, compliance with reassignment is far from complete, with 54 percent of selected 7th and 8th graders transitioning to the school to which they were reassigned.

On average, students selected for reassignment are observationally different from their non-selected counterparts within the same initial school. Auxiliary regressions indicate that even when we condition on school-grade-year fixed effects, students selected for reassignment were more likely to be Black, Hispanic, or eligible for FRPL, and were less likely to be White. Selected students also had lower scores on prior mathematics and ELA assessments, on average. This is particularly true for students reassigned to existing schools, as compared to those reassigned to newly opened ones.

The students who actually complied with reassignment, and particularly those who moved to an existing school, are even more observationally different from non-selected students. In particular, compliers are less likely to be White, non-FRPL and higher-achieving than non-selected students and selected non-compliers. This likely reflects the fact that, among those selected for reassignment, comparatively advantaged students more successfully counter the reassignment process. For students who do not comply with reassignment, the large majority continue to attend the same school; only a very small share leave the school into which they

[7] Although reassignment also occurs in high school, we do not have standard measures of academic performance with which to compare students. This is because in high school in North Carolina, students take End-of-Course (EOC) exams rather than End-of-Grade (EOG) exams, and the timing of the EOC exams depends on whether and when students take certain courses.

[8] The state adopted its Accountability, Basics, and Local Control (ABCs) accountability system in 1996. This assessment and accountability model underwent cyclical changes (notably prior to the 2005-06 school year) after which the NC EOG assessments experienced only minor revisions until the state implemented a system aligned to the Common Core State Standards in 2012-13.

[9] Due to the endogeneity of this policy shift, we do not use the return to neighborhood school assignment as an additional mechanism to explore peer effects.

[10] Appendix Table A1 provides further evidence by grade and year on the students selected for reassignment. Though relatively small in scale, the reassignment process was distributed throughout the district. During the period we study, students were reassigned from between approximately one-quarter to one-half of schools (Appendix Table A2) and were reassigned to 20 to 40 percent of different schools throughout the district (Appendix Table A3) in any given year.

were zoned for a magnet school instead, for example.

As noted above, on average, the school switches made via the reassignment policy did not result in demonstrably different peer settings for those who were reassigned. We report in Figure 2 (and accompanying Appendix Table A4 and Table A5) the year-over-year difference in the proportion of reassigned students' school peers who received FRPL and who were assessed as below-grade-level in reading. These figures and tables compare the schools to which students were reassigned with their previous school by student characteristics and school year. If reassignment resulted in increased socioeconomic and achievement integration, we should find that FRPL-eligible students (or non-proficient readers) were reassigned to schools that have a smaller proportion of FRPL-eligible students (or non-proficient readers) than their originating school. We do not find this to be the case. We illustrate in Panel A that FRPL-eligible middle-school students are, across the seven years of our sample, reassigned to schools (in a given year t+1) that are approximately one percentage point *more* FRPL-eligible than their prior school (measured in t). Reassigned middle-school students who were not proficient in ELA experienced a one-and-a-half percentage point increase in the proficiency rates of their peers, on average, but proficient readers also experienced a small increase in the average proficiency rates of their peers (Panel B). On average, then, school switches did not much alter the peer characteristics of those who moved schools. It follows that the reassignments did not systematically result in more socioeconomic or academic integration.[11]

In sum, the settings to which students were reassigned do not differ markedly from the schools they were attending at the time of reassignment. We observe little change in academic or socioeconomic integration for the district overall as well as for the schools affected by node reassignment over the years we consider. These findings provide important context for our examination of the effects of switching schools and changing peer composition. Given the limited nature of the district's school reassignment effort and the lack of change in overall district integration on dimensions such as socioeconomic status or academic achievement, our analysis should not be viewed as providing an evaluation of a policy that comprehensively redistributes students to schools for socioeconomic or academic integration purposes, but rather the more constrained topic of changing selected students' assigned school or peer group.

[11] Appendix Figure A1 plots the dissimilarity index over these seven years for schools that were and were not affected by the reassignment process. We use the standard social science dissimilarity index calculated for school district j in time t: $\frac{1}{2}\sum_{i=1}^{n}\left|\frac{fr_{it}}{FR_{jt}} - \frac{nfr_{it}}{NFR_{jt}}\right|$, where fr_{it} is the number of FRPL students in school i at time t, FR_{jt} is the number of FRPL students in the district in time t, with similar notation for non-FRPL students in the second fraction. The dissimilarity index is interpretable as the proportion of individuals who would need to move to a different school for the school district's schools to be perfectly integrated, given the socio-economic composition of the district. We calculate the analogous statistic for the number of students scoring below Proficient in ELA. Across indices for both socio-economic and academic integration, the value of the dissimilarity index shows no decline for those schools that participated—either in raw terms or in comparison to those that did not participate.

4 The Effect of Changing Schools on Student Outcomes

4.1 Analytic strategy

Here, we detail our analytic process for estimating the impact of changing schools on student outcomes. As shown above, students who were selected for reassignment and who ultimately moved to a different school are observationally different from those who were not selected along several dimensions, including standardized test performance. It would not be surprising if such differences persisted in the years after selection for reassignment. Nevertheless, WCPSS's reassignment process does have an arguably random aspect to it, but only after conditioning on key observable characteristics that factor into the selection process. Therefore, our analytic approach assumes that selection for reassignment is conditionally ignorable.

The stated goals of the district's reassignment policy were (1) to reduce over-crowding; (2) to accommodate transportation logistics; and (3) to keep schools from serving an over-concentration of students who were from lower-family-income backgrounds or who exhibited low-levels of ELA proficiency (Parcel and Taylor, 2015; Weingarth, 2005). Selection for reassignment occurs at the level of the geographic node rather than the individual. More specifically, in a given school year and within a given node, students in certain grade-band levels (elementary, middle or high school) are selected. Thus, selection for reassignment is a grade-band-node-year level phenomenon, and we account for this in our modeling strategy.

We use an instrumental variables approach to estimate the causal effects of school reassignment on student outcomes. In our models, we refer to the year in which a student is selected as year t and the first year in which a student would attend a school to which she is reassigned as t+1. In each year in our panel, we treat node-grade-band selection for reassignment as an exogenous driver of school moves for students entering grades 7 and 8, conditional on node-grade-band measures of SES, ELA proficiency and location, and employ it as an instrument for school switching.

In each year, the specific node-grade-band measures on which we condition are the share of students who qualify for FRPL, students' average scores on their EOG ELA assessment, driving distance between the node centroid and the current school to which the node was assigned, and the count of schools that were newly opened and that first received students in year t+1 within 30 minutes driving distance of the node centroid.[12]

To model the causal effect of school moving on student outcomes, we use the following two-stage least squares (2SLS) setup:

$$M_{ings,t+1} = \alpha_{gs,t} + \beta_1 X_{ings,t} + \beta_2 N_{ngs,t} + \beta_3 Z_{ngs,t} + \nu_{ings,t} \tag{1}$$

[12] To be explicit, we assign the mean FRPL-status and ELA performance within each node-year block to all students in this block. We include linear and quadratic terms for our two core selection variables (FRPL and ELA score). Polynomials of these selection criteria remain significant up to the seventh order. However, they do not change the predictive strength of our instrument. In the interest of parsimony and to avoid over-fitting, we limit our results to a sparse first stage with only the linear and quadratic terms.

$$Y_{ings,t+1} = \alpha'_{gs,t} + \gamma_1 X_{ings,t} + \gamma_2 N_{ngs,t} + \gamma_3 \hat{M}_{ings,t+1} + \epsilon_{ings,t} \quad (2)$$

where for student i, residing in node n, attending grade g, in school s, in year t: $M_{ings,t+1}$ is an indicator for moving from school s to another school in year $t+1$, $\alpha_{gs,t}$ is a grade-school-year fixed effect that limits our analysis to variation in outcomes for students in the same grade g, within the same initial school s, and in the same year t. $Z_{ngs,t}$ is an indicator for being selected for school reassignment for students living in node n, attending grade g within school s, in year t. $X_{ings,t}$ represents student-level baseline characteristics, measured in the year that students are selected for reassignment, and $N_{ngs,t}$ represents group-level characteristics, measured at the node-grade-school-year level. In Equation 1, the causal effect of *selection* for reassignment on moving is represented by β_3.

In the second-stage model (Equation 2), we relate moving, as instrumented in the first-stage model, to outcomes measured in year $t+1$. The outcomes we consider, represented generically by $Y_{ings,t+1}$, are standardized test scores, course grades and school attendance. Parameter γ_3 represents the causal effect of node-reassignment-induced moving on student outcomes. We estimate results separately for elementary- and middle-school aged children.[13] We cluster standard errors at the node-year level as this is the level at which selection effectively occurs (Abadie et al., 2017).

Given the district's growth during the time period we consider, some students were reassigned to newly opened schools, whereas others were reassigned to existing schools. To account for this, we include two separate instruments in our first-stage equation Equation 1. The first instrument is an indicator for assignment to a newly opened school, and the second instrument is an indicator for assignment to an existing school. Although our instruments differentiate reassignment to a new versus an existing school, our outcome is whether a student moves to any school, as we are interested in the global effect of school switching as a result of reassignment.[14]

4.2 Assessing school-switching identification assumptions

The key identifying assumption in our analysis of school switching is the conditional ignorability assumption outlined above, which is critical to satisfying the exclusion restriction underlying instrumental variables estimation. For groups of students defined by school, grade, and node of residence in a given school year, we treat selection for reassignment as random, conditional on group level measures of FRPL status, ELA proficiency, driving distance from node n to school s, and the opening of new schools near node n. To assess whether our conditional ignorability

[13] This is because older and younger children may be affected differently by school reassignment (Chetty et al., 2016). For reasons detailed below, we feature the middle school results in the main text and reserve elementary results for Appendix C.

[14] In alternate specifications, we modify our analytic setup to include two first-stage equations that model moving to a new school and moving to an existing school separately. Then, the second-stage equation includes the two different instrumented terms for moving to new and existing schools. Using a *post-hoc* comparison test, we examine whether the effect of moving differs according to whether the school to which a student moved is new or existing. To note, while we interpret each of these effects — the effect of moving to a newly opened school and the effect of moving to an existing school — as causal, the comparison between these two effects is inherently descriptive. This is because the probability of being assigned to a newly opened or an existing school may differ according to student characteristics (observed and unobserved).

assumption is reasonable, we aggregate student-level data up to the grade-node-year level and examine whether node selection is predictive of various other sociodemographic, achievement and behavioral baseline measures, after conditioning on these group-level measures. In these regressions, we incorporate grade-school-year fixed effects to restrict comparison to groups of students who are in the same school and grade in year t but who differ in their selection for reassignment due to living in different nodes:

$$\bar{X}_{ngs,t} = \alpha_{gs,t} + \beta_1 Z^{\text{exist}}_{ngs,t} + \beta_2 Z^{\text{new}}_{ngs,t} + \gamma_1 \bar{X}^{\text{policy}}_{ngs,t} + \xi_{ngs,t} \tag{3}$$

To demonstrate the extent of potential bias in the absence of knowledge of the assignment process, we compare these estimates to naïve regressions predicting node characteristics from selection for reassignment alone (i.e., removing from the estimates the factors that led to a node being reassigned, $\bar{X}^{\text{policy}}_{ngs,t}$).

When conditioned on the characteristics considered in the assignment process, reassignment has limited predictive power on other node characteristics at the middle-school level. As we highlight in Table 2, naïve regressions indicate that within a grade-school-year cell, whether the district decided to select a group of students to switch schools (particularly to an existing school) was predictive of their residential node characteristics (Panel A). However, once we condition on elements of the reassignment process (Panel B), students reassigned to existing schools reside in nodes with minimal demographic differences from non-selected nodes. A comparison of the coefficients from the unconditional models to the conditional models—particularly for nodes reassigned to existing schools—reveals the value of our instrumental variables approach in removing bias from the estimates. After conditioning on the criteria used to inform reassignment, node-level selection is largely uncorrelated with node-level demographic, academic and behavioral measures. We do recognize that nodes selected to be reassigned to new schools had students with slightly fewer prior-year absences, even in our conditional model. Nevertheless, we judge failure to meet only one of our tests to be a relatively successful defense of the exclusion restriction assumption in the middle-school data.

At the elementary level, however, there is evidence that nodes with lower average test scores and with more FRPL-eligible, Black and Hispanic resident students were more likely to be reassigned. Conditioning on covariates of the assignment process reduces the strength of these relationships, but does not eliminate them (Appendix Table C5). While we are not aware of any particular institutional practices that led to this result for elementary schools, we do note that student-assignment officers reassigned lower-ELA-achieving and lower-family income nodes more frequently (see Appendix Table C1). While ELA scores and family-income correlate with other socio-demographic characteristics in a node, they do so imperfectly. This may be why that—even when we adjust for the assignment covariates—we still find that a node's selection for reassignment relates to its ethnoracial composition. We emphasize our middle-school results and report our elementary results in Appendix C due to concerns that the elementary-level estimates may still suffer some selection bias, and we acknowledge that the failure of our assumption check at the elementary level may warrant greater skepticism of our middle-school results.

Next, our ability to derive causal inferences from our analytic approach to estimating the effects of school changes rests on the assumption that selection for reassignment can only increase a student's likelihood of moving to a different school (i.e., that there are no defiers). We judge that this assumption is reasonable, given that in our sample, less than one percent of students who were not selected for reassignment ultimately moved to a school that was selected to receive reassigned students in the following year. Similarly, only five percent of students who were selected for reassignment moved to a school other than the one to which they were reassigned.

Finally, we consider how to interpret the causal effect of moving, γ_3. This effect could be driven both by disruption effects and by compositional effects, provided that peer and/or school characteristics change in the course of a school move. The descriptive evidence above suggests that the primary consequence of complying with reassignment to a new school is the disruption of switching to a different school, rather than how the characteristics of the reassigned school compare to those of a student's initial school. We now examine this more formally.

If a student does not experience compositional changes as a result of a school move, it means that the intended and actual composition of their reassigned school should be similar to the intended and actual composition the student would have experienced in her base school had she not been reassigned. Note that this does not mean that the reassignment process cannot change the composition of a school from one year to the next. Rather, it means that student composition should be similar in a student's current and prior school after reassignment has occurred; that is, in year $t+1$. We employ models of the form of Equation 1 above to examine this assumption. The right-hand side of the equation is as discussed above. We apply this model to leave-out mean and variance measures of students' grade-level peer groups along the following dimensions: prior academic achievement; race/ethnicity, and FRPL status.

Based on the estimates we present in Appendix Table A6, we conclude that, on average, moves due to reassignment did not lead students to experience substantially different school, teacher, or peer characteristics compared to what they would have experienced had they not been selected to switch schools. Our estimates of β_3 in these specifications are small and (in most cases) insignificant. As additional evidence in defense of our argument that schools were not providing systematically different learning environments to school switchers, we show that while school switchers experienced somewhat less experienced teachers, their teachers' prior-value-added was substantively identical to non-switchers at both the grade cohort (Appendix Table A7) and classroom (Appendix Table A8) levels.

Across the analyses discussed here, we judge our IV assumptions to be well met, particularly for our middle-school results. Further, given the lack of compositional changes that students experience, on average, as a result of moving, we reason that the effects of moving that we estimate in this context primarily represent disruption effects.

4.3 First-stage results

We assess the strength of our proposed instrumental variables for predicting variation in the endogenous measures of interest and find both to be strong instruments. We present results from fitting our first-stage IV models for school switching in Appendix Table A9.[15] Our proposed instruments of school reassignment to new or existing schools serve as strong predictors of school moving behavior. The regression-adjusted coefficients indicate that approximately half of students selected for reassignment to a new school comply and attend the newly-opened school to which they are assigned. The rates are slightly lower, but nevertheless high, for reassignment to an existing school. All F-statistics far exceed standard benchmarks, and are sufficiently above thresholds proposed by Lee and co-authors (2020) such that we conclude there is no need to adjust our t-ratio threshold for inference.

4.4 Impacts of school switching

We find no substantively meaningful effects on test scores, absenteeism or course grades overall for students who switch schools because of reassignment. We present these estimates in Table 3. We estimate our null effects with relatively precise zeros and our 95 percent confidence intervals rule out test score effects larger than a 0.06 *SD* decrease or an 0.02 *SD* increase. Similarly, our 95 percent confidence intervals exclude changes in the rates of chronic absenteeism greater than two percentage points in either direction. We observe significant, but substantively trivial, positive effects of switching schools on math course grades; the coefficient of 0.077 represents less than one-thirteenth of a GPA point. Minimally, this result suggests that changing schools should not harm students' grades. In Table A10 we observe no delayed effect of school switching on year $t + 2$ outcomes.[16]

For students with low prior performance, there is some evidence that changing school has worse effects. In Table 4, we explore the possibility of heterogeneity in the impacts of moving schools as a result of being reassigned.[17] Low-performing students experience test-score declines of 0.05 *SD* units. Though our divided sample produces less precise estimates, we test in auxiliary regressions (not presented here) whether there is a statistical difference of the effect of switching schools due to reassignment for top-quartile compared to bottom-quartile students, and we reject the null. There is no apparent heterogeneity of effects for absenteeism and modest sugges-

[15] We present results for two different samples: (1) all students for whom we observe achievement test scores and socio-demographic characteristics and (2) a somewhat smaller set of students for whom we observe course grade outcomes. Course grade outcomes are identified for students who had an course associated with their grade level in each subject ("LANGUAGE ARTS" for ELA and courses with titles including: "SEVENTH GRADE MATH", "EIGHTH GRADE MATH", "PRE-ALGEBRA", or "ALGEBRA I" for mathematics). Students who did not take any ELA or Mathematics courses or took an off-grade-level course ("MAGNET ADVANCED LANGUAGE ARTS", "GEOMETRY") are excluded from the course grades sample.

[16] We feature only the grade 7 students from Table 3 and consider their grade 8 outcomes so that we do not incorporate the separate effects of an additional school transition in 9^{th} grade. For completeness, we present estimates that separate out the second-stage predictors by moving to a new or existing school in Appendix Table A11. These results (as well as others in which we use a continuous measure of days absent) are consistent with our main results from Table 3. Students who move to new schools drive the positive course grade results, though the effects remain substantively small for them. As anticipated, our reduced-form intent-to-treatment estimates are even closer to zero (Appendix Table A12).

[17] We define prior-performance levels by dividing students into quartiles based on their prior-year ELA performance. Low-performing students are those in the bottom 25 percentiles of the distribution.

tive evidence that switching schools improves course grades for high-performing students. There is also no evident heterogeneity in the effects of school switching for students from lower- and higher-income families or for mid-level performers (Appendix Table A13 and Table A14). Additional heterogeneity tests show no meaningful or statistical differences by students' ethnoracial identities.

5 The Effect of School Composition Changes on Student Outcomes

5.1 Analytic strategy

Our second research question relates to the impact that the student assignment policy may have on students who are not selected for reassignment but who may nevertheless be affected because of changes in their schools' peer composition driven by the movement of reassigned students into or out of the school that they attend. To inform this question, we employ an approach similar to Hoxby and Weingarth (2005) but informed by more recent guidance from Angrist (2014). We include students in the estimation only in the year(s) in which they are not selected for reassignment. This is because, as noted above, outcomes in the year after a move could be a function of both peer composition and the disruption effects of changing schools.

Our goal is to assess peer composition effects on the same set of outcomes explored above but with a model of a substantially different structure. Here, we take a student-level fixed effects approach, such that we are relying on within-student variation over time. Of note, we subscript time differently than in our analysis of school switching. Specifically, in our peer effects analyses, we observe both treatment and outcome in each year for each student. Thus we subscript each year simply as year t. The general form of the model is as follows:

$$Y_{ings,t} = \alpha_i^{''} + \gamma_1 \bar{Y}^{\text{lag}}_{(i-1)gs,t} + \gamma_2 \bar{X}_{(i-1)gs,t} + \delta_{gt}^{''} + \epsilon_{igs,t} \tag{4}$$

where $\bar{Y}^{\text{lag}}_{(i-1)gs,t}$ represents the average prior (lagged) achievement of student i's school-grade-year level peers (with student i excluded from the calculation) and $\bar{X}_{(i-1)gs,t}$ represents the same with regard to other student-level characteristics, including our indicator of family income level: FRPL status.[18]

The model also includes grade-by-year fixed effects, $\delta_{gt}^{''}$, to net out yearly variation in the average performance of all students included in the analysis. Finally, the model includes student-level fixed effects ($\alpha_i^{''}$) such that we control for all time-invariant student-level characteristics (e.g., race/ethnicity, baseline achievement), and our estimation relies on variation in student-level peer composition across years in school. Note that, given this fixed effects structure, students who are observed in only one year will not contribute to the analysis. In Table A15, we

[18]In our primary models, we measure peer characteristics at the grade-cohort, rather than the classroom, level because at the middle-school level, peer groups are defined more by grade rather than class composition, as students typically move among different classroom peer groups throughout the school day. However, we test whether our results are sensitive to the definition of our endogenous peer characteristic predictor at the classroom level. These results are causally identified because while endogenous sorting occurs at the classroom level, we continue to define our instrument at the grade-cohort level which we argue is conditionally exogenous.

present results from fitting Equation 4 to our data and find that higher-performing peers have substantively meaningful positive effects on students' own test scores, but negative effects on their grades.

Of course, directly fitting Equation 4 to data does not return estimates of γ_1 and γ_2 that can be interpreted as the causal effects of a student's peers. This is due to the well-documented endogenous factors that relate both to peer composition and the outcomes of interest (e.g., Angrist, 2014). Therefore, we again use an instrumental variables strategy where we employ a pair of first-stage models to instrument for $\bar{Y}^{\text{lag}}_{(i-1)gs,t}$ and $\bar{X}_{(i-1)gs,t}$ using the values of these measures that are intended under the school reassignment strategy. These first-stage models are as follows:

$$\bar{Y}^{\text{lag}}_{(i-1)gs,t} = \alpha_i + \beta_1 \bar{Y}^{\text{lag,policy}}_{(i-1)gs,t} + \beta_2 \bar{X}^{\text{policy}}_{(i-1)gs,t} + \delta_{gt} + \nu_{igs,t} \tag{5}$$

$$\bar{X}_{(i-1)gs,t} = \alpha'_i + \phi_1 \bar{Y}^{\text{lag,policy}}_{(i-1)gs,t} + \phi_2 \bar{X}^{\text{policy}}_{(i-1)gs,t} + \delta'_{gt} + \varepsilon_{igs,t} \tag{6}$$

In short, we use information on student-level school (re)assignment to determine the grade-level peer composition the district intended for each student in each year of our panel. We include measures for the baseline achievement of student i's intended grade-level peer group, $\bar{Y}^{\text{lag,policy}}_{(i-1)gs,t}$, and sociodemographic characteristics of each student's intended peer group, $\bar{X}^{\text{policy}}_{(i-1)gs,t}$. Rates of compliance with reassignment policies may differ across contexts; thus, we view the most policy- and theory-relevant parameter of interest to be the effects of *actually* experiencing different peer group compositions.

To generate these peer characteristic measures, we use all students in all years of our data. Then, because we seek to estimate these peer-composition effects only for those students who are not also subject to potential disruption from being selected for reassignment, we drop observations for students in the year(s) in which they are selected for reassignment. This allows us to accomplish an important design feature that Angrist (2014) calls for in distinguishing between the subjects of a peer effects investigation and the peers who provide the mechanism for shifts in peer composition. As above, because grade 5 to grade 6 is a structural school transition for most students, we exclude grade 6 from our analysis.

Using this instrumental variables strategy, our estimates of γ_1 and γ_2 rely on year-over-year variation in peer composition experienced by individual students as a result of the district's reassignment strategy. With this analytic setup, we are relying on policy-induced changes in peer composition as an exogenous source of variation with which to identify the causal effects of shifts in peer composition on students' outcomes. If a given student i experiences no change in the predicted cohort, then that student will not contribute to the estimation of effects, given the student fixed-effects structure of the modeling strategy. We cluster standard errors at the level of the within-school, grade-level cohort predicted by the instrument, as this is the source of exogenous variation.

5.2 Assessing peer effects identification assumptions

Our analytic strategy to address our second research question relies on the prior strategy's assumption that selection for reassignment is conditionally exogenous. If this first condition is satisfied, a second assumption must also hold: for a given student who is not selected to move, the changes that the student experiences in peer composition should not be driven systematically by variation in that student's own characteristics over time. For example, it should not be the case that a given student's achievement in seventh grade is a predictor of her assigned peer group composition in eighth grade. If such an association exists, then it could be that the student's seventh grade achievement was a driver of both her subsequent achievement and the subsequent composition of her peers.

Note that here, we focus on time-varying measures associated with each student, given that our approach to this research question involves a student fixed-effect strategy which accounts for time-invariant student characteristics.

To test this assumption, we use a student fixed-effects model to estimate the relationship between characteristics of policy-assigned peer composition and lagged achievement and school attendance measures. Our model takes the following general form:

$$\bar{Y}^{\text{lag,policy}}_{(i-1)gs,t} = \alpha_i + \theta_1 ACHIEVE_{i,t-1} + \theta_2 ATTENDANCE_{i,t-1} + \delta_{gt} + \epsilon_{igs,t} \quad (7)$$

where for student i, $\bar{Y}^{\text{lag,policy}}_{(i-1)gs,t}$ represents a measure of the achievement of a student's assigned peers; α_i is a student-level fixed effect; and $ACHIEVE_{i,t-1}$ and $ATTENDANCE_{i,t-1}$ are measures for student i of achievement and attendance, respectively, in the year prior to when a reassignment would occur. We include grade-by-year fixed effects, δ_{gt}, to mirror the structure of the models expressed in Equation 4 through Equation 6.

We will consider our assumption to be supported if our estimates of θ_1 and θ_2 are close to zero and not statistically significant. We will interpret this to indicate that year-over-year changes in student i's individual characteristics are not predictive of year-over-year changes in assigned peer composition.

Using this general model structure, we consider two different specifications: one in which we include measures for student i based on the prior academic year ($t-1$ as expressed in Equation 7), and a second (for a subset of our sample) in which we include measures for student i based on two years prior ($t-2$). Using the $t-2$ measures may provide a more robust assessment, as the data from two years prior could plausibly be used to inform policy decisions, whereas the same is not true for the data from one-year prior. This is because the measures from one year prior would not be observable at the time that school assignment decisions are being made for the next academic year.

Finally, in addition to modeling policy-governed shifts in the composition of a given student's peers, we use this same model structure to examine the relationship between time-variant student characteristics and whether the student experienced any policy-induced shift in peer composition. That is, we replace the outcome in Equation 7 with $I^{\text{policy}}_{igs,t}$, where this indicator is equal to 1 if other students were assigned into or out of student i's school and grade-level in year t (and zero otherwise). We again expect that the estimates of θ_1 and θ_2 will be close to zero and will not be statistically significant.

In alignment with our tests of the exogeneity of school switching, our checks on the conditionally random nature of students' experienced change in peer characteristics reveal that our assumptions are best met for middle-school students. As we show in Table 5, middle-school students' measures of prior performance and attendance are minimally predictive of their assigned peers' performance and demographic characteristics; this is especially so for measures from two years prior. Importantly, students' own time-variant characteristics are unrelated to whether other students are reassigned into or out of their school. Due to the precision of our estimates, at times we reject the null, but the magnitude of the coefficients associated with measures from either one or two years prior are quite small. Thus, we present these assumption checks as evidence that our second exclusion restriction assumption is met.[19]

5.3 Considering non-linearities in peer effects

Our proposed models for assessing school composition effects, thus far, are structured to consider the linear relationship between individual student outcomes and the composition of their peers. Of course, as we think about the composition of any given student's peers, the performance of one's peers, on average, may matter less than the shape and spread of the distribution. For example, a school might be able to better serve its students if those students enter at a similar starting position. On the other hand, schools serving a more variable student population may have a comparatively harder task of meeting all students where they are. In this way, the study of peer effects in school contexts may be about how educational systems are able to respond to and serve a particular group of students in conjunction with the direct influence of peers on one another.

Indeed, much of the peer effects literature suggests non-linearities in these relationships. To explore such non-linearities in our analyses, we follow a structure analogous to that in Equation 4 through Equation 6, and model the effect of changes in the distribution of student i's peer group. Specifically, we consider the effects of changes in the standard deviation of lagged peer achievement, corresponding to Hoxby and Weingarth's (2005) "Focus" model, and the effects of changes in the share of students within 0.1 standard deviation units of student i's own lagged performance, corresponding to their "Boutique" model. One-tenth of a standard deviation is admittedly an arbitrary cutoff, although we find largely similar conclusions with a one-quarter standard deviation bandwidth. Our endogenous predictor is the *actual* standard deviation of peers' prior achievement leaving out student i, and our first-stage instrument is the *assigned* leave-student-out standard deviation of peers' prior achievement. The inclusion of the student fixed effect means that we leverage within-student variation. We also use the *assigned* share of students within one-tenth of a standard deviation of a student's own performance as an instrument in our "Boutique" models.

[19]As with our school-switching results, these assumptions are less well satisfied at the elementary school level (Appendix Table C9), further justifying our choice to de-emphasize these results.

5.4 First-stage results

Our assigned-peer-characteristic instruments (which in the context of our individual fixed effects capture deviations from mean peer characteristics) are powerful predictors of actual-peer-characteristics. We present the results of our first-stage models for our second research question in Appendix Table A16. We consider four different measures of peer characteristics: peer average achievement (as assessed by a composite measure of math and ELA test performance), share of peers who do *not* qualify for FRPL, share of Black cohort-mates and share of Hispanic cohort-mates.[20] In all cases, the changes in these measures that students could expect based on the student-school assignment policy are highly predictive of the changes in school composition that students actually experience. Again, F-statistics exceed standard benchmarks.[21]

5.5 Linear-in-means peer effects

We find evidence that students' skills improve when they attend school with higher-achieving peers. In Table 6, we present the main effects of changing peer characteristics on academic and behavioral outcomes. Panel A presents results for standardized test performance and attendance, and Panel B presents results for mathematics and ELA course grades. In Panel A, Column 1 of Table 6, we estimate that a one-tenth of a standard deviation increase in the achievement scores of peers results in a 0.05 *SD* unit increase in mathematics test scores. An analogous change in peer achievement increases a student's ELA test score by 0.03 *SD*s (Panel A, Column 3). We detect no effects on absenteeism.[22]

In contrast with our results for peer achievement levels, increases in the proportion of non-FRPL-eligible peers do not consistently lead to changes in student-test score outcomes, after accounting for changes in peers' achievement. A 10 percentage point increase in the proportion of non-FRPL-eligible peers results in an improvement in mathematics, but a decline in ELA, and both are statistically indistinguishable from zero.

Our estimates of the effects of higher-achieving peers are robust to the inclusion of adjustments for the percent of assigned Black and Hispanic peers (Panel A, Columns 2 and 4). As we discuss above, we focus on peer effect changes driven by the criteria of the reassignment policy: students' family-income and prior-achievement levels. However, we can still interpret other dimensions of γ_2 in Equation 4 causally because we instrument for changes in the ethnoracial composition

[20]The measure of peer achievement on which we focus here is the average performance on math and ELA assessments, standardized with mean 0, *SD* 1, as Math and ELA scores are so highly correlated. Results are nearly identical in magnitude if we consider math achievement measures and ELA achievement measures separately. We focus on the share of students who do not qualify for FRPL, so that the expected direction of effects for all of these measures on student outcomes will be the same, and we scale this measure such that a one unit difference represents a 10 percentage point change in the proportion of students who qualify for FRPL.

[21]The endogenous predictors for changes in peers' characteristics fall short of the 104.7 F-stat threshold from Lee et al. (2020). However the t-ratios Lee et al. propose given the size of our F-statistics in our test-score sample are 2.01, 2.11, 2.46 and 2.27 for prior achievement, non-FRPL, Black and Hispanic, respectively (2020, Table 3). All coefficient estimates we report as significant at the α-threshold of 0.05 are robust to these slightly higher t-ratios.

[22]Interestingly, our instrumental variable estimates are roughly similar to the OLS results in Appendix Table A15. We account for any family-income or achievement sorting across schools that is relatively constant via our student-fixed-effects analytic structure. Thus, in this context, there appears to be modest time-varying sorting of students across schools, independent of enrollment changes in response to the policy.

of students' peers using $\bar{X}^{\text{policy}}_{(i-1)gs,t}$. The coefficients on changes in Black and Hispanic peers are imprecise and statistically indistinguishable from zero. Although we are unable to rule out substantively meaningful effects, the signs of the coefficients are opposite across math and ELA. Without any substantive reason to believe that children of different ethnoracial backgrounds would affect their peers differently in different subjects, we interpret these opposite-directioned, imprecise results as generally supporting a null effects conclusion. These findings also align substantively with Hoxby and Weingarth's (2005) conclusions that once we account for assigned changes in peers' performance and family-income levels, changes in the proportion of racially or ethnically minoritized peers do not consistently predict increases or decreases in student achievement.

Next, we consider effects on course grades. We find mixed evidence that improvements in the achievement levels and increases in the average family-income levels of peers affect students' grades. In Panel B, we observe that a one-tenth-of-a-standard-deviation increase in average peer achievement increases course grades in math and decreases them in ELA. These course grade effects are equivalent to roughly a 0.01 and 0.02 *SD* change in math and ELA, respectively. There are equivalent effects from increases in the proportion of non-FRPL-eligible students on math grades, but not for ELA grades. We return in the discussion to an interpretation of the possible reason for these diverging effects across subject areas.

For many of our peer effect estimates, our confidence intervals are wide even when they exclude zero. The imprecision in our estimates is driven primarily by small variability in our endogenous peer-characteristic-change predictors.[23] Reassignment did not, on average, change most non-reassigned students' peer characteristics. The average absolute value for the change in peers' prior performance is 0.06 *SD*s and the average absolute change in non-FRPL peers is 2.1 percentage points. Thus, our identifying variation comes primarily from a smaller group of students whose peers' attributes did change. Given the imprecision, our results may be most appropriately interpreted directionally rather than as specific point estimates.

5.6 Non-linear and heterogeneous peer effects

We next present results from models that consider how the distribution of characteristics in students' peer groups affects their own performance in non-linear ways.

We find mixed evidence related to Hoxby and Weingarth's (2005) Focus model of schooling: a wider spread in the starting achievement levels within cohorts may decrease students' test scores and grades in ELA, but clearly benefits them in mathematics. In Table 7, we find that a one-tenth of a standard deviation unit increase in the standard deviation of peers' prior test scores improves students' own math test-score outcomes by 0.03 to 0.04 *SD*s. The same increase in the spread of prior performance increases students' mathematics course grades by even more. On

[23] Our relatively large standard errors are not primarily due to our IV framework as our OLS estimates are nearly as imprecise (see Appendix Table A15), nor are they driven by the student-fixed-effects approach as estimates that rely on a performance-change outcome measure and no student fixed effects produce equivalent standard errors. Out of concern that our results are influenced by model specification, we also present alternative ways of defining our instrument. Our results are robust to estimating each peer characteristic change as an instrument in separate regressions (Appendix Table A17). The magnitude of peers' effects on test scores and grades are slightly larger, but substantively identical, when we define peer groups at the classroom level (Appendix Table A18).

the other hand, changes in the spread of peers' prior performance has no effect on students' ELA test scores, grades or rates of chronic absenteeism. Of note, a one standard deviation change in the standard deviation of test scores is equivalent to 0.03 student-level standard deviation units. Thus, again, we urge caution in interpreting these coefficients as predictive of larger changes in the distribution of peers' prior achievement.

Our evidence is not consistent with the Boutique model. That is, having more peers just like oneself does not appear to improve learning outcomes. To explore evidence in support of the Boutique model on student outcomes, we estimate the effect of a change in the proportion of school peers who fall within 0.1 standard deviations of students' own performance. The coefficients that we present in Table 7 are scaled to pertain to a 10 percentage point increase in the proportion of peers within this bandwidth of each student's own performance. All associated coefficients are substantively small in magnitude and indistinguishable from zero.

Considering heterogeneity in effects, our results show that math and reading achievement test improvements from higher-skill peers accrue primarily to students who are already relatively advantaged: those who are the highest-performing and whose families have higher income (with one exception). In Table 8 and Table 9, we present the differential impacts of peer effects by students' family-income level. Appendix Table A19 and Table A20 present analogous results by prior-achievement levels for mathematics and ELA, respectively. We find that a one-tenth of a standard deviation improvement in average peer achievement increases non-FRPL-eligible students' test scores by 0.05 *SD*s in math (Table 8, Panel A, Column 3) and 0.03 *SD*s (Table 9, Panel A, Column 3) in ELA. The same improvements in peer academic skill levels result in roughly equivalent improvements for higher-achieving students in math (Appendix Table A19). While the benefits for these test-score outcomes are largest for non-FRPL-eligible and higher-achieving students, FRPL-eligible and lower-achieving students do benefit from stronger peers. Coefficients for peers' prior achievement effects on test-score outcomes are positive for FRPL students in math and ELA and for bottom-quartile students in math.

The divergent results for bottom-quartile students in ELA provide suggestive insights into the nature of learning across subjects. In contrast with the results we discuss in the previous paragraph, the lowest-performing students benefit the most from stronger peers on their ELA test scores (Table A20). Additionally, students throughout the prior performance distribution and across all levels of family income experience ELA course grade declines when higher-achieving students are assigned to their grade cohort (Panels B in Table 9 and Table A20). We interpret these differences in our conclusion.

Improvements in the average skill levels or family-income status of one's peers has roughly equivalent effects on math test scores for the middle of the performance distribution as it does for the overall sample (Appendix Table A21). However, middle-achievers experience smaller (if any) ELA test-score benefits and minimal meaningful effects on their grades. In supplemental heterogeneity analyses, we find that the strongest peer achievement effects (on both math and ELA test scores) are for Hispanic and White students, though coefficients are positive and of roughly equivalent magnitude across all ethnoracial groups. The negative peer achievement effects on ELA course grades are concentrated among Black and Hispanic students.

For completeness, we present in Appendix Table A22 through Table A25 non-linear-in-means estimates for sub-populations. The minimal variance in our predictors and the sub-setting of our sample cause our standard errors to grow so large in comparison to our point estimates in the Focus models (change in *SD* of cohort) as to render these results largely uninterpretable. However, our estimates of the Boutique model (percent of peers within 0.1 *SD*) for top-quartile and non-FRPL students have relatively small standard errors. In math, for top-quartile students having more students just like them depresses test-score performance by 0.10 SDs (Appendix Table A22, Panel A, Columns 7-8). The same is true for FRPL-eligible students (Appendix Table A24, Panel A, Columns 3-4). There is only one group for whom evidence supportive of the Boutique models exists: for non-FRPL students, having more students similar to them in prior performance improves both their math test scores and grades (Appendix Table A24, Panels A and B, Columns 7-8).[24]

6 Conclusion

When considering policy efforts that redistribute students across schools within a given school system, policy makers should attend to the potential effects of school reassignment both on students who are selected to move to a new school as well as those who do not change schools but may nevertheless experience changes in their school context due to the redistribution of students. In this study we contribute insights to the peer effects literature and to policy makers interested in real world applications of student assignment processes to yield potential benefits of peer effects. We represent our combined school switching and linear-in-means findings visually in Figure 3.

First, we find little evidence that school reassignment impedes student achievement or attendance for school switchers, on average. However, some evidence suggests that switching schools due to reassignment may lead to declines in performance among students who are already relatively low-achieving.

Second, we find that students achieve at higher levels when their peers are higher-achieving. In our data, the 90$^{\text{th}}$ percentile change in absolute value of peer prior achievement is 0.11 *SD*, and it is 4.9 percentage points for the absolute value of the change in non-FRPL peers. Thus, in Figure 3, we scale our estimated effects to both the mean absolute changes ("average" change) and round approximates of the 90$^{\text{th}}$ percentile of absolute change ("large" change) to provide reasonable estimates within the range of our data. To accomplish empirically moderate and substantively meaningful effects on student learning outcomes (Kraft, 2020), policy makers would need to change students' peers' prior performance by one-fifth to one-quarter of a standard deviation in prior performance and increase their non-FRPL-eligible peers by more than 10

[24]One concern with our non-linear peer effects estimates may be that changes in the *variance* of prior-peer-performance could drive changes in the *average* prior performance of students' peers and thereby violate the exclusion restriction assumption of our instrumental variables design. In fact, in our data, changes in the variance of peer-prior-performance are largely uncorrelated with changes in the mean of peers' prior performance. As a further test, we re-estimated all results from Table 7 and Appendix Table A22 – Table A25 and Table C11 including the exogenous instrument of students' *assigned* average-prior-peer performance in our first stage, as well as the the endogenous predictor of the average-prior-peer-performance that students *actually experienced* in our second stage. In all cases, our results were substantively identical to our main findings.

percentage points.

Beyond the evidence in Figure 3, we find that the benefits of higher-performing peers accrue differentially to different categories of students. Non-FRPL-eligible (and to a lesser degree, higher-achieving) students experience greater test-score benefits, though FRPL-eligible and lower-performing students do experience meaningful benefits. On the other hand, consistent with a theory of relative-rank grading effects, the introduction of higher-achieving students results in worse ELA grades. One possible (though speculative) explanation of this phenomenon is that mathematics grades depend more directly on objective mastery of the material, whereas ELA grades depend, in part, on comparisons with other students.

Finally, our findings imply that wide performance variation in a given grade may lead to improved achievement in math. On the other hand, it may depress achievement in English Language Arts. One possible explanation for these contrasting findings is that there are peer benefits of learning in mixed-ability classrooms, but there are subject-specific instructional challenges for teachers. In math, the positive peer effects may swamp the instructional challenges, but the opposite is true in ELA. This is largely speculative, however, as our data and research design do not permit us to test this theory. With the exception of non-FRPL-eligible students, students from other family-income or achievement backgrounds do not appear to benefit from more students with similar prior performance to them. In fact, the majority of the evidence suggests the opposite.

Given the outcomes to which we have access, our results suggest that increasing the overall proportion of high-achieving students in a school-grade cohort or classroom is likely to increase student achievement levels, though these benefits are largest for already-high-achieving students. In addition to standard cautions regarding the generalizability of these findings to other contexts, we also note the importance of longer-term outcomes, parental preferences and political feasibility in the complex policy-making process.

Our results suggest that student assignment policies that relocate higher-achieving students to optimize the average peer achievement level of lower-achieving students or those from lower-income families can accomplish equity goals. This is because such policies are unlikely to produce negative outcomes for more-advantaged school switchers and will produce benefits for comparatively disadvantaged students. However, the introduction of higher-performers may cause lower-achieving students to receive worse grades in courses for which grading includes more subjective components. Further, these reassignments may generate negative effects for higher-achieving or higher-family-income students who experience fewer advantaged peers. In sum, these findings suggest that policy makers interested in using student assignment policies to maximize student learning must carefully weigh different outcomes of interest, complementary policy and instructional practices, as well as equity principles.

References

Abadie, A., Athey, S., Imbens, G., and Wooldridge, J. (2017). When should you adjust standard errors for clustering? NBER Working Paper No. 24003, National Bureau of Economic Research, Cambridge, MA.

Angrist, J. D. (2014). The perils of peer effects. *Labour Economics*, 30:98–108.

Angrist, J. D. and Lang, K. (2004). Does school integration generate peer effects? Evidence from Boston's METCO program. *American Economic Review*, 94(5):1613–1634.

Bayer, P., Ross, S. L., and Topa, G. (2008). Place of work and place of residence: Informal hiring networks and labor market outcomes. *Journal of Political Economy*, 116(6):1150–1196.

Belsha, K. and Darville, S. (2020). A new national effort to promote school integration is underway. More than two dozen school districts want in. *Chalkbeat*, Oct 9:Online.

Bénabou, R. (1996). Heterogeneity, stratification, and growth: Macroeconomic implications of community structure and school finance. *American Economic Review*, 86(3):584–609.

Bergman, P. (2021). The risks and benefits of school integration for participating students: Evidence from a randomized desegregation program. Unpublished working paper.

Bifulco, R. and Schwegman, D. J. (2020). Who benefits from accountability-driven school closure? Evidence from New York City. *Journal of Policy Analysis and Management*, (3):96–130.

Billings, S. B., Deming, D. J., and Rockoff, J. (2014). School segregation, educational attainment, and crime: Evidence from the end of busing in Charlotte-Mecklenburg. *The Quarterly Journal of Economics*, 129(1):435–476.

Billings, S. B., Deming, D. J., and Ross, S. L. (2019). Partners in crime. *American Economic Journal: Applied Economics*, 11(1):126–150.

Billings, S. B. and Hoekstra, M. (2022). The effect of school and neighborhood peers on achievement, misbehavior, and adult crime. *Journal of Labor Economics*, Just Accep.

Brummet, Q. (2014). The effect of school closings on student achievement. *Journal of Public Economics*, 119:108–124.

Burke, M. A. and Sass, T. R. (2013). Classroom peer effects and student achievement. *Journal of Labor Economics*, 31(1):51–82.

Carlson, D., Bell, E., Lenard, M. A., Cowen, J. M., and McEachin, A. (2019). Socioeconomic-based school assignment policy and racial segregation levels: Evidence from the Wake County Public School System. *American Educational Research Journal*, 1(57):258–304.

Carrell, S. E., Hoekstra, M., and Kuka, E. (2018). The long-run effects of disruptive peers. *American Economic Review*, 108(11):3377–3415.

Carrell, S. E. and Hoekstra, M. L. (2010). Externalities in the classroom: How children exposed to domestic violence affect everyone's kids. *American Economic Journal: Applied Economics*, 2(1):211–228.

Chetty, R., Grusky, D., Hell, M., Hendren, N., Manduca, R., and Narang, J. (2017). The fading American dream: Trends in absolute income mobility since 1940. *Science*, 356(6336):398–406.

Chetty, R., Hendren, N., and Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment. *American Economic Review*, 106(4):855–902.

Chetty, R., Hendren, N., Kline, P., Saez, E., and Turner, N. (2014). Is the United States still a land of opportunity? Recent trends in intergenerational mobility. *American Economic Review*, 104(5):141–147.

Denning, J. T., Murphy, R. J., and Weinhardt, F. (2018). Class rank and long-run outcomes. IZA Working Paper No. 11808, IZA Institute of Labor Economics, Bonn, Germany.

Domina, T., Carlson, D., Carter III, J., Lenard, M., McEachin, A., and Perera, R. (2021). The kids on the bus: The academic consequences of diversity-driven school reassignments. *Journal of Policy Analysis and Management*, 4(40):1197–1229.

Duncan, G. and Magnuson, K. (2011). The nature and impact of early achievement skills, attention skills, and behavior problems. In Duncan, G. and Murnane, R. J., editors, *Whither Opportunity? Rising Inequality, Schools, and Children's Life Chances*, pages 47–70. Russell Sage Foundation, New York.

Engberg, J., Gill, B., Zamarro, G., and Zimmer, R. (2012). Closing schools in a shrinking district: Do student outcomes depend on which schools are closed? *Journal of Urban Economics*, 71(2):189–203.

Epple, D., Filimon, R., and Romer, T. (1993). Existence of voting and housing equilibrium in a system of communities with property taxes. *Regional Science and Urban Economics*, 23(5):585–610.

Epple, D., Newlon, E., and Romano, R. (2002). Ability tracking, school competition, and the distribution of educational benefits. *Journal of Public Economics*, 83(1):1–48.

Ewing, E. L. (2020). *Ghosts in the Schoolyard: Racism and School Closings in Chicago's South Side*. University of Chicago Press, Chicago, IL.

Fantuzzo, J. W., LeBoeuf, W. A., Chen, C.-C., Rouse, H. L., and Culhane, D. P. (2012). The unique and combined effects of homelessness and school mobility on the educational outcomes of young children. *Educational Researcher*, 41(9):393–402.

Flinspach, S. L. and Banks, K. E. (2005). Moving beyond race: Socioeconomic diversity as a race-neutral approach to desegregation in the Wake County Schools. In Boger, J. C. and Orfield, G., editors, *School resegregation: Must the south turn back?*, pages 261–280. University of North Carolina Press, Chapel Hill, NC.

Grigg, J. (2012). School enrollment changes and student achievement growth. *Sociology of Education*, 85(4):388–404.

Hanushek, E. A., Kain, J. F., and Rivkin, S. G. (2004). Disruption versus Tiebout improvement: the costs and benefits of switching schools. *Journal of Public Economics*, 88(9-10):1721–1746.

Hanushek, E. A., Peterson, P., Talpey, L., and Woessmann, L. (2019). The unwavering SES achievement gap: Trends in U.S. student performance. NBER Working Paper No. 25648, National Bureau of Economic Research, Cambridge, MA.

Hasim, S. A., Kane, T. J., Kelley-Kemple, T., Laski, M. E., and Staiger, D. O. (2020). Have income-based achievement gaps widened or narrowed? NBER Working Paper No. 27714, National Bureau of Economic Research, Cambridge, MA.

Herbers, J. E., Reynolds, A. J., and Chen, C.-C. (2013). School mobility and developmental outcomes in young adulthood. *Development and Psychopathology*, 25(2):501–15.

Hoxby, C. M. (2000). Peer effects in the classroom: Learning from gender and race variation. NBER Working Paper No. 7867, National Bureau of Economic Research, Cambridge, MA.

Hoxby, C. M. and Weingarth, G. (2005). Taking race out of the equation: School reassignment and structure of peer effects. Unpublished working paper.

Kimbrough, E. O., McGee, A. D., and Shigeoka, H. (2020). How do peers impact learning? An experimental investigation of peer-to-peer teaching and ability tracking. *Journal of Human Resources*, 0918-9770R.

Kraft, M. A. (2020). Interpreting effect sizes of education interventions. *Educational Researcher*, 49(4):241–253.

Lee, D. S., McCrary, J., Moreira, M. J., and Porter, J. (2020). Valid t-ratio Inference for IV. arxiv working paper:2010.05058, arXiv Econ Working Papers.

Macartney, H. and Singleton, J. (2017). School boards and student segregation. NBER Working Paper No. 23619, National Bureau of Economic Research, Cambridge, MA.

Manski, C. F. (1993). Identification of endogenous social effects: The reflection problem. *The Review of Economic Studies*, 60(3):531.

Mantil, A. (2021). Crossing district lines: The impact of urban–suburban desegregation programs on educational attainments:. *Educational Evaluation and Policy Analysis*, Online.

McDermott, K. A., Frankenberg, E., and Diem, S. (2015). The "post-racial" politics of race. *Educational Policy*, 29(3):504–554.

Murphy, R. and Weinhardt, F. (2020). Top of the class: The importance of ordinal rank. *The Review of Economic Studies*.

Parcel, T. L. and Taylor, A. J. (2015). *The End of Consensus: Diversity, Neighborhoods, and the Politics of Public School Assignments*. UNC Press, Chapel Hill, NC.

Parents Involved v. Seattle School District No. 1 (2007). 551 U.S. 701. Court case.

Patacchini, E., Rainone, E., and Zenou, Y. (2017). Heterogeneous peer effects in education. *Journal of Economic Behavior & Organization*, 134(February):190–227.

Piketty, T. and Saez, E. (2003). Income inequality in the United States, 1913-1998. *The Quarterly Journal of Economics*, 118(1):1–41.

Raudenbush, S. W., Marshall, J., and Art, E. (2011). Year-by-year cumulative impacts of attending a high-mobility elementary school on children's mathematics achievement in Chicago, 1995-2005. In Murnane, R. J. and Duncan, G., editors, *Whither Opportunity? Rising Inequality, Schools, and Children's Life Chances*, pages 359–375. Russell Sage Foundation, New York, NY.

Reardon, S. F. (2011). The widening academic achievement gap between the rich and the poor: New evidence and possible explanations. In Duncan, G. and Murnane, R. J., editors, *Whither Opportunity? Rising Inequality, Schools, and Children's Life Chances*, pages 91–116. Russell Sage Foundation, New York.

Rockoff, J. E. and Lockwood, B. B. (2010). Stuck in the middle: Impacts of grade configuration in public schools. *Journal of Public Economics*, 94(11-12):1051–1061.

Sacerdote, B. (2011). Peer effects in education: How might they work, how big are they and how much do we know thus far? In Hanushek, E., Machin, S., and Woessman, L., editors, *Handbook of the Economics of Education*, pages 249–277. Elsevier, San Diego, CA, vol. 3 edition.

Sacerdote, B. (2012). When the Saints go marching out: Long-term outcomes for student evacuees from Hurricanes Katrina and Rita. *American Economic Journal: Applied Economics*, 4(1):109–135.

Sacerdote, B. (2014). Experimental and quasi-experimental analysis of peer effects: Two steps forward? *Annual Review of Economics*, 6(1):253–272.

Saez, E. and Zucman, G. (2016). Wealth inequality in the United States since 1913: Evidence from capitalized income tax data. *The Quarterly Journal of Economics*, 131(2):519–578.

Sanbonmatsu, L., Katz, L., Ludwig, J., Gennetian, L., Duncan, G., Kessler, R., Adam, E., McDade, T., and Lindau, S. (2011). Moving to Opportunity for Fair Housing Demonstration Program: Final Impacts Evaluation. Technical report, Washington, DC.

Schwartz, A. E., Stiefel, L., and Cordes, S. A. (2017). Moving matters: The causal effect of moving schools on student performance. *Education Finance and Policy*, 12(4):419–446.

Schwartz, H. L. (2010). Housing Policy Is School Policy. Technical report, Washington, DC.

Schwerdt, G. and West, M. R. (2013). The impact of alternative grade configurations on student outcomes through middle and high school. *Journal of Public Economics*, 97:308–326.

Silberman, T. (2002). Wake County Schools: A Question of Balance. In Foundation, T. C., editor, *Divided We Fail: Coming Together through Public School Choice*, pages 141–163. The Century Foundation Press, Washington, DC.

Strauss, V. (2017). Nation's largest school district announcing effort to diversify segregated public schools. *Washington Post*, June 6:Online.

Vigdor, J. L. and Nechyba, T. S. (2007). Peer effects in North Carolina public schools. In Peterson, P. and Woessmann, L., editors, *Schools and the equal opportunity problem*. MIT Press, Cambridge, MA.

Wake County Public School System (2012). WCPSS Board of Education Magnet Presentation. Board presentation, Wake County, NC.

Weingarth, G. (2005). *Taking race out of the equation: The effect of changing classroom poverty concentrations on student achievement*. Bachelor of arts thesis, Harvard University.

Xu, D., Zhang, Q., and Zhou, X. (2022). The impact of low-ability peers on cognitive and non-cognitive outcomes: Random assignment evidence on the effects and operating channels. *Journal of Human Resources*, 57(2):555–596.

	Not selected	Reassigned to new school	Reassigned to existing school	Reassigned school switchers
Male	0.51	0.49	0.51	0.51
	(0.50)	(0.50)	(0.50)	(0.50)
Asian	0.05	0.08	0.04	0.07
	(0.22)	(0.28)	(0.21)	(0.25)
Black	0.21	0.18	0.29	0.29
	(0.41)	(0.38)	(0.45)	(0.45)
Hispanic	0.09	0.12	0.16	0.18
	(0.29)	(0.33)	(0.37)	(0.38)
White	0.59	0.56	0.47	0.42
	(0.49)	(0.50)	(0.50)	(0.49)
Free/Reduced Lunch elig	0.26	0.25	0.40	0.42
	(0.44)	(0.43)	(0.49)	(0.49)
Prior-year, NC EOG math	0.11	0.19	-0.11	-0.12
	(0.95)	(0.92)	(0.99)	(0.99)
Prior-year, NC EOG ELA	0.08	0.14	-0.14	-0.18
	(0.95)	(0.91)	(1.01)	(1.03)
Prior-year course grade, math	2.52	2.64	2.35	2.35
	(1.31)	(1.17)	(1.36)	(1.32)
Prior-year course grade, ELA	2.77	2.85	2.53	2.53
	(1.23)	(1.13)	(1.31)	(1.25)
Prior-year absences	7.36	7.37	8.56	8.65
	(7.06)	(6.47)	(8.07)	(7.97)
Prior-year chronic absence	0.07	0.06	0.10	0.11
	(0.26)	(0.24)	(0.30)	(0.31)
Observations	86698	1765	3149	2636

Notes: Each cell reports the sample average (standard deviation in parentheses).

Table 1: Main analytic sample ($7^{th}/8^{th}$ grade) student-level descriptive statistics, 2005/06 – 2011/12

Panel A. Middle grade-level node, without assignment covariates						
	% Black	% White	% Hisp	% Male	Prior Absence	Prior Math
	(1)	(2)	(3)	(4)	(5)	(6)
Reassigned to existing school	0.075***	-0.108***	0.046***	-0.011	0.490*	-0.120***
	(0.018)	(0.020)	(0.012)	(0.012)	(0.247)	(0.031)
Reassigned to new school	0.008	-0.018	0.006	-0.030	-0.853*	0.047
	(0.024)	(0.029)	(0.018)	(0.018)	(0.333)	(0.037)
Assignment covariates?						
Observations	8215	8215	8215	8215	8215	8215

Panel B. Middle grade-level node, with assignment covariates						
	(1)	(2)	(3)	(4)	(5)	(6)
Reassigned to existing school	0.006	-0.018	0.021*	-0.011	0.053	0.003
	(0.014)	(0.012)	(0.011)	(0.012)	(0.231)	(0.015)
Reassigned to new school	0.025	-0.040	0.013	-0.030	-0.785**	0.007
	(0.022)	(0.023)	(0.015)	(0.018)	(0.303)	(0.020)
Assignment covariates?	✓	✓	✓	✓	✓	✓
Observations	8215	8215	8215	8215	8215	8215

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors in parentheses. All models report estimates from Equation 3. Models fitted to data aggregated to the node-year level. All models include grade-band-school-year fixed effects. Assignment covariate models also include linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, and the number of students in each node-grade-school-year cell.

Table 2: Instrumental variable assumption checks for conditional randomization of reassignment

Panel A. Test scores and chronic absenteeism

	Math Test Score (1)	ELA Test Score (2)	Chronic Absence (3)
Switched schools	-0.021	-0.020	-0.002
	(0.019)	(0.019)	(0.011)
Observations	91612	91612	91612

Panel B. Course grades

	Math Course Grade (1)	ELA Course Grade (2)
Switched schools	0.077*	0.023
	(0.035)	(0.042)
Observations	85252	85252

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at node-year level in parentheses. All models report 2nd-stage estimates from Equation 2. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table 3: Instrumental variable estimates of effects of switching schools due to reassignment

Panel A. Test scores and chronic absenteeism						
	Bottom-quartile ELA students			Top-quartile ELA students		
	Math Test Score (1)	ELA Test Score (2)	Chronic Absence (3)	Math Test Score (4)	ELA Test Score (5)	Chronic Absence (6)
Switched schools	-0.047	-0.052	0.019	-0.021	-0.010	0.009
	(0.037)	(0.039)	(0.023)	(0.036)	(0.042)	(0.017)
Observations	24631	24631	24631	20667	20667	20667

Panel B. Course grades						
	Bottom-quartile ELA students			Top-quartile ELA students		
	Math Course Grade (1)	ELA Course Grade (2)		Math Course Grade (3)	ELA Course Grade (4)	
Switched schools	0.080	0.060		0.135*	-0.023	
	(0.069)	(0.069)		(0.054)	(0.050)	
Observations	22821	22821		18760	18760	

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors clustered at node-year level in parentheses. All models report 2nd-stage estimates from Equation 2. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table 4: Instrumental variable estimates of effects of school switching due to reassignment, by prior achievement

	Peers' prior perf.		% non-FRPL		% Black		% Hisp		Peers Re-assigned (0/1)?	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Prior math (t-1)	0.013***		-0.002		0.011		-0.002		0.010	
	(0.003)		(0.010)		(0.008)		(0.006)		(0.020)	
Prior ELA (t-1)	0.003*		-0.004		0.002		0.002		0.002	
	(0.001)		(0.007)		(0.006)		(0.003)		(0.010)	
Prior absences (t-1)	-0.000		-0.001		0.002***		-0.000		-0.001	
	(0.000)		(0.001)		(0.001)		(0.000)		(0.001)	
Prior math (t-2)		-0.003		0.005		-0.005		0.002		-0.012
		(0.003)		(0.011)		(0.009)		(0.007)		(0.026)
Prior ELA (t-2)		-0.000		0.019*		-0.006		-0.011**		0.005
		(0.002)		(0.008)		(0.007)		(0.004)		(0.010)
Prior absences (t-2)		-0.000		0.000		-0.001		-0.000		0.000
		(0.000)		(0.001)		(0.001)		(0.000)		(0.001)
Observations	59255	46272	59255	46272	59255	46272	59255	46272	59255	46272

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at school-grade-year level in parentheses. All models report estimates from Equation 7. All models include student and grade-year fixed effects.

Table 5: Instrumental variable assumption checks for conditional exogeneity of changes in peer composition

Panel A. Test scores and chronic absenteeism						
	Math Test Score		ELA Test Score		Chronic Absence	
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' prior test scores (0.1σ)	0.045**	0.041*	0.026**	0.032***	-0.002	0.001
	(0.016)	(0.017)	(0.009)	(0.009)	(0.005)	(0.005)
Pct. of non-FRPL peers (10 pp)	0.044	-0.020	-0.021	0.066	-0.007	0.041
	(0.048)	(0.087)	(0.035)	(0.051)	(0.016)	(0.026)
Pct. of Black peers (10 pp)		-0.069		0.100		0.092*
		(0.104)		(0.073)		(0.038)
Pct. of Hispanic peers (10 pp)		-0.132		0.171		0.035
		(0.162)		(0.094)		(0.047)
Observations	59255	59255	59255	59255	59255	59255

Panel B. Course grades				
	Math Course Grade		ELA Course Grade	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	0.015	0.013	-0.028	-0.015
	(0.055)	(0.054)	(0.033)	(0.031)
Pct. of non-FRPL peers (10 pp)	0.198	0.169	-0.006	0.179
	(0.192)	(0.258)	(0.116)	(0.169)
Pct. of Black peers (10 pp)		-0.058		0.259
		(0.261)		(0.245)
Pct. of Hispanic peers (10 pp)		-0.016		0.270
		(0.360)		(0.279)
Observations	53484	53484	53484	53484

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table 6: Linear-in-means instrumental variable estimates of changes in peer composition

Panel A. Test scores and chronic absenteeism												
	Math Test Score				ELA Test Score				Chronic Absence			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
0.1 *SD* prior test scores	0.029	0.041			-0.008	-0.020			0.006	0.006		
	(0.035)	(0.036)			(0.024)	(0.027)			(0.012)	(0.013)		
Peers w/in 0.1σ			0.014	0.014			0.029	0.029			-0.003	-0.004
of prior score (10 pp)			(0.019)	(0.019)			(0.024)	(0.025)			(0.011)	(0.011)
10 pp $\uparrow$ non-FRPL peers	0.083	-0.009	0.072	-0.012	-0.007	0.071	-0.004	0.072	-0.006	0.042	-0.008	0.042
	(0.053)	(0.090)	(0.050)	(0.088)	(0.041)	(0.052)	(0.037)	(0.052)	(0.018)	(0.027)	(0.016)	(0.027)
Peer race adjust?		✓		✓		✓		✓		✓		✓
Observations	59255	59255	59255	59255	59255	59255	59255	59255	59255	59255	59255	59255

Panel B. Course grades								
	Math Course Grade				ELA Course Grade			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.1 *SD* prior test scores	0.118	0.124			-0.034	-0.056		
	(0.121)	(0.121)			(0.082)	(0.085)		
Peers w/in 0.1σ			0.059	0.060			0.006	0.004
of prior score (10 pp)			(0.041)	(0.041)			(0.036)	(0.035)
10 pp $\uparrow$ non-FRPL peers	0.247	0.168	0.208	0.168	-0.036	0.179	-0.025	0.179
	(0.192)	(0.257)	(0.185)	(0.258)	(0.121)	(0.172)	(0.120)	(0.170)
Peer race adjust?		✓		✓		✓		✓
Observations	53484	53484	53484	53484	53484	53484	53484	53484

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table 7: Non-linear-in-means instrumental variable estimates of changes in peer composition

Panel A. Test scores				
	FRPL students		Non-FRPL students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	0.021	0.001	0.050**	0.048**
	(0.024)	(0.025)	(0.018)	(0.018)
Pct. of non-FRPL peers (10 pp)	0.076	-0.147	0.045	0.009
	(0.107)	(0.159)	(0.045)	(0.081)
Peer race adjust?		✓		✓
Observations	11119	11119	45671	45671

Panel B. Course grades				
	FRPL students		Non-FRPL students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	-0.016	-0.026	0.035	0.034
	(0.076)	(0.074)	(0.055)	(0.053)
Pct. of non-FRPL peers (10 pp)	0.044	-0.022	0.278	0.295
	(0.275)	(0.405)	(0.196)	(0.261)
Peer race adjust?		✓		✓
Observations	10049	10049	41210	41210

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table 8: Linear-in-means instrumental variable estimates of changes in peer composition by family-income level on Mathematics outcomes

Panel A. Test scores				
	FRPL students		Non-FRPL students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	0.011	0.027	0.029**	0.033***
	(0.020)	(0.0207)	(0.009)	(0.009)
Pct. of non-FRPL peers (10 pp)	-0.001	0.209	-0.012	0.046
	(0.086)	(0.120)	(0.038)	(0.057)
Peer race adjust?		✓		✓
Observations	11119	11119	45671	45671
Panel B. Course grades				
	FRPL students		Non-FRPL students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	-0.027	-0.006	-0.021	-0.007
	(0.060)	(0.058)	(0.029)	(0.026)
Pct. of non-FRPL peers (10 pp)	-0.114	0.094	-0.016	0.175
	(0.212)	(0.321)	(0.103)	(0.150)
Peer race adjust?		✓		✓
Observations	10049	10049	41210	41210

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table 9: Linear-in-means instrumental variable estimates of changes in peer composition by family-income level on English Language Arts outcomes

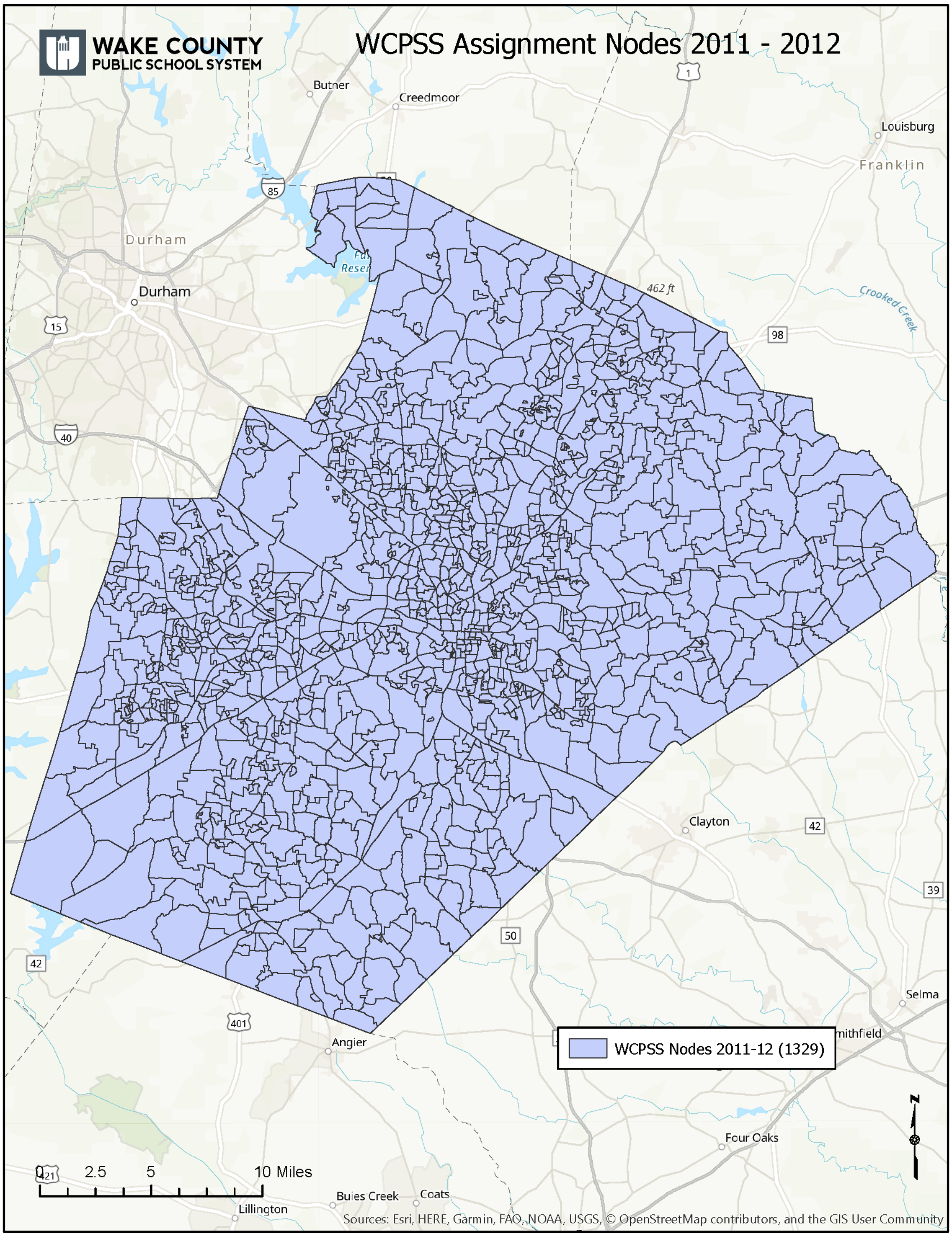
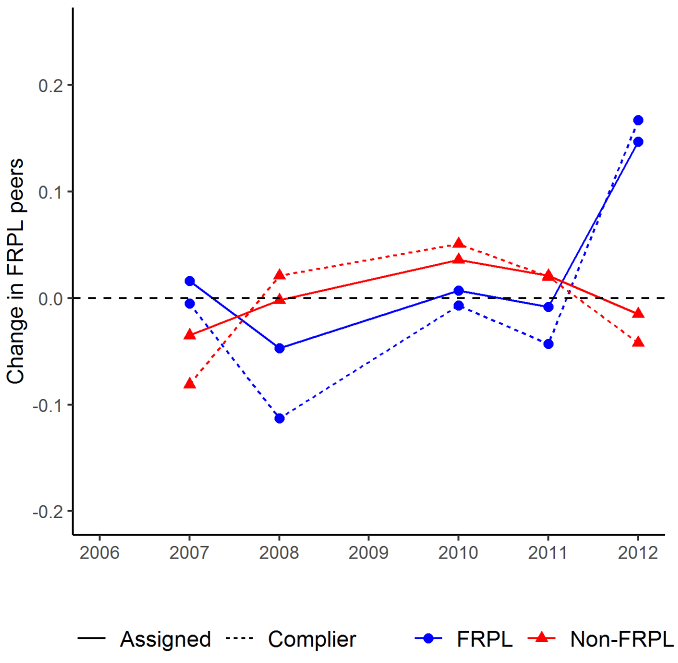
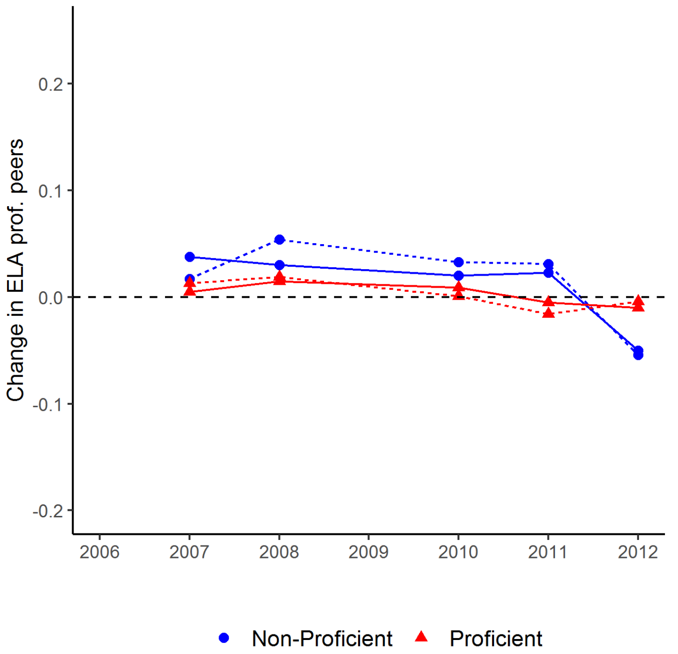


Figure 1: Wake County Public School System Node Map, 2011-12



(a) FRPL



(b) ELA Proficiency

Figure 2: Change in the proportion of peers receiving Free- or Reduced-Price Lunch (FRPL) and scoring Proficient or above in ELA for students reassigned to different schools

Notes: Values represent the average result of subtracting the proportion of students scoring at or above Level III (Proficient) or receiving FRPL (measured in time t) from the proportion of students scoring at or above Level III or receiving free- or reduced-price lunch (FRPL) in the new school of a student who has been selected for reassignment (measured in time t+1). All years represent spring of the academic year. 2008-09 school year excluded due to no middle school students being reassigned. If reassignment resulted in increased socio-economic integration, FRPL students should have negative values and non-FRPL students should have positive values. If reassignment resulted in increased academic integration, non-Proficient students should have positive values and Proficient students should have negative values. Annual means and *SD*s available in Appendix Table A4 and Table A5.

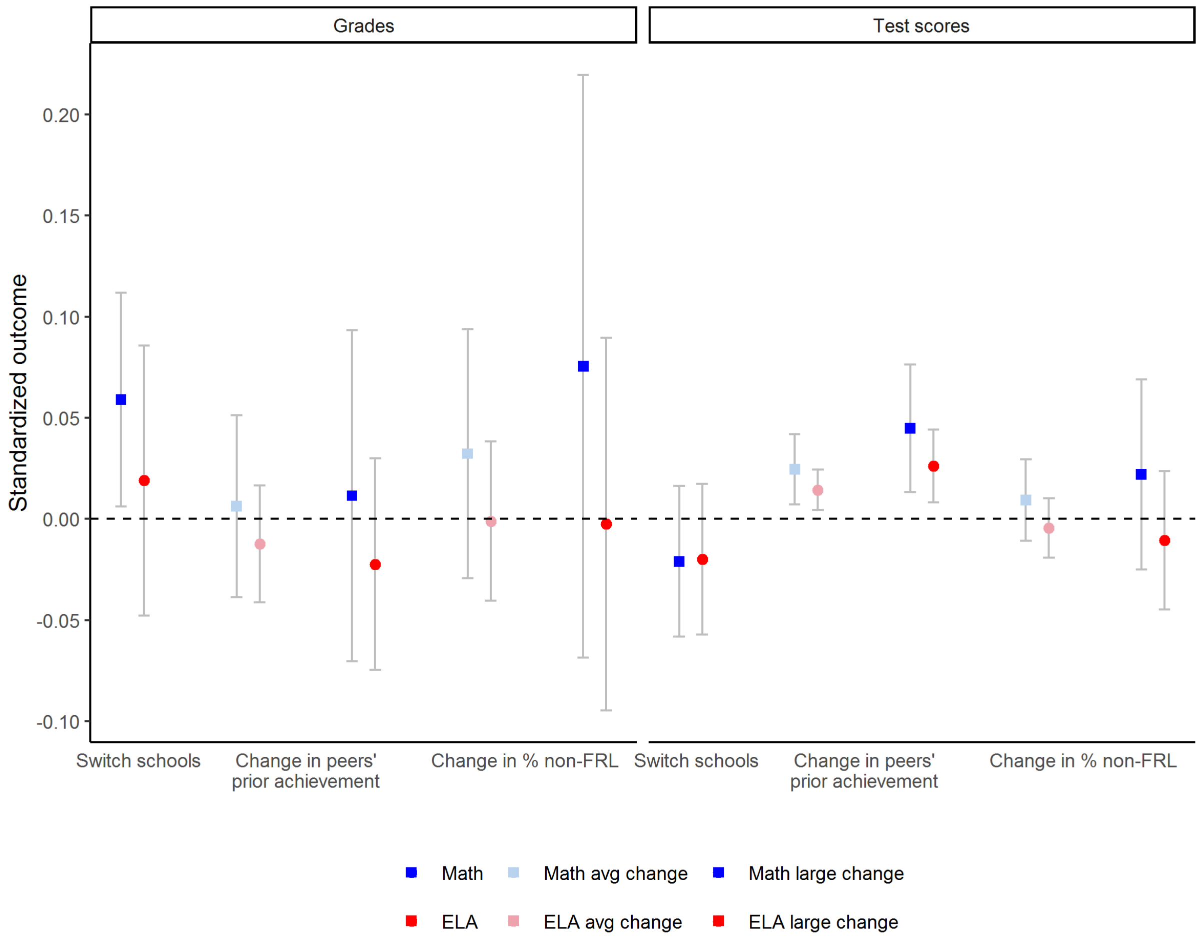


Figure 3: Combined estimates of school reassignment policies on mathematics and English Language Arts outcomes for school switching and at prototypical "average" and "large" changes in peer composition

Notes: Figure reports point estimates and 95% confidence intervals from Equation 2 and Equation 4 as reported in Table 3, Columns 1 and 2 and Table 6, Columns 1 and 3, scaled to "average" (0.055 *SD* prior performance and 2.1 p.p. non-FRPL) and "large" (0.1 *SD* prior performance and 5 p.p. non-FRPL) peer changes observed in data.

A Appendix Tables and Figures

Grade	2001-02	2002-03	2003-04	2004-05	2005-06	2006-07	2007-08	2008-09	2009-10	2010-11
1	311	234	726	465	910	1268	1141	710	272	408
2	379	215	737	443	833	1093	1047	649	233	433
3	353	170	608	355	678	987	798	591	173	360
4	370	185	621	**334**	**633**	**1000**	**720**	**596**	**189**	**377**
5	353	151	606	**359**	**689**	**889**	**746**	**525**	**174**	**341**
6	355	534	581	0	45	632	0	874	1071	214
7	357	557	633	0	**50**	**653**	0	**847**	**1122**	**191**
8	313	490	617	0	**39**	**619**	0	**773**	**989**	**203**
9	746	66	308	0	1267	0	0	412	967	233
10	647	46	267	0	1193	0	0	351	731	181
11	583	61	209	0	1032	0	0	347	657	182
12	8	48	201	0	828	0	0	259	564	139
Total Across Grades	4775	2757	6114	1956	8197	7141	4452	6934	7142	3262

Notes: Figures in bold reflect grade levels and years included in the initial analytic sample. Years represent year (t) in which reassignments were made. This contrasts with year $t+1$ in which we observe students in their new school or with new peers.

Table A1: Number of students reassigned, by grade and year

Grade	2001-02	2002-03	2003-04	2004-05	2005-06	2006-07	2007-08	2008-09	2009-10	2010-11
1	16	12	22	19	44	49	41	33	8	19
2	16	12	22	19	44	49	41	33	8	19
3	16	12	22	19	44	49	41	33	8	19
4	16	12	22	**19**	**44**	**49**	**41**	**33**	**8**	**19**
5	16	12	22	**19**	**44**	**49**	**41**	**33**	**8**	**19**
6	10	9	13	0	2	17	0	25	20	10
7	10	9	13	0	**2**	**17**	0	**25**	**20**	**10**
8	10	9	13	0	**2**	**17**	0	**25**	**20**	**10**
9	10	1	9	0	14	0	0	14	14	7
10	10	1	9	0	14	0	0	14	14	7
11	10	1	9	0	14	0	0	14	14	7
12	10	1	9	0	14	0	0	14	14	7
Total across grades	36	22	43	19	60	65	41	71	42	36
Schools in WCPSS	128	130	132	138	139	141	145	149	151	155

Notes: Figures in bold reflect grade levels and years included in the analytic sample. Years represent year (t) in which reassignments were made. This contrasts with year $t+1$ in which we observe students in their new school or with new peers.

Table A2: Number of "base" schools from which students are reassigned, by grade and year

Grade	2001-02	2002-03	2003-04	2004-05	2005-06	2006-07	2007-08	2008-09	2009-10	2010-11
1	15	10	24	12	32	34	30	27	9	13
2	15	10	24	12	32	34	30	27	9	13
3	15	10	24	12	32	34	30	27	9	13
4	15	10	24	**12**	**32**	**34**	**30**	**27**	**9**	**13**
5	15	10	24	**12**	**32**	**34**	**30**	**27**	**9**	**13**
6	9	8	13	0	3	13	0	21	15	11
7	9	8	13	0	**3**	**13**	0	**21**	**15**	**11**
8	9	8	13	0	**3**	**13**	0	**21**	**15**	**11**
9	7	1	6	0	13	0	0	14	13	8
10	7	1	6	0	13	0	0	14	13	8
11	7	1	6	0	13	0	0	14	13	8
12	7	1	6	0	13	0	0	14	13	8
Total across grades	31	19	42	12	48	46	30	62	37	32
Schools in WCPSS	128	130	132	138	139	141	145	149	151	155

Notes: Figures in bold reflect grade levels and years included in the analytic sample. Years represent year (t) in which reassignments were made. This contrasts with year $t+1$ in which we observe students in their new school or with new peers.

Table A3: Number of "selected" schools to which students are reassigned, by grade and year

Panel A. Assigned change in characteristics									
	All Selected					All Compliers			
	FRPL		Non-FRPL			FRPL		Non-FRPL	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Pooled	0.018	0.132	0.028	0.116	Pooled	0.015	0.129	0.009	0.110
2006	-	-	-	-	2006	-	-	-	-
2007	-0.063	0.061	-0.075	0.046	2007	-0.075	+	-0.077	0.007
2008	0.017	0.110	0.066	0.103	2008	0.017	0.117	0.058	0.101
2009	-	-	-	-	2009	-	-	-	-
2010	0.023	0.133	0.050	0.123	2010	0.022	0.120	0.041	0.115
2011	-0.026	0.120	0.011	0.097	2011	-0.042	0.111	0.002	0.088
2012	0.117	0.132	-0.080	0.194	2012	0.130	0.122	-0.123	0.189
Panel B. Actual change in characteristics									
	All Selected					All Compliers			
	FRPL		Non-FRPL			FRPL		Non-FRPL	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Pooled	0.009	0.148	0.016	0.092	Pooled	-0.004	0.165	0.021	0.112
2006	-	-	-	-	2006	-	-	-	-
2007	0.016	0.137	-0.035	0.048	2007	-0.005	+	-0.081	0.041
2008	-0.047	0.146	-0.002	0.087	2008	-0.113	0.216	0.021	0.133
2009	-	-	-	-	2009	-	-	-	-
2010	0.007	0.126	0.036	0.087	2010	-0.007	0.128	0.051	0.117
2011	-0.008	0.134	0.021	0.089	2011	-0.043	0.133	0.020	0.099
2012	0.147	0.169	-0.015	0.143	2012	0.167	0.172	-0.042	0.149

Notes: $^+$There was only one FRPL complier in 2006-07, so no standard deviation could be calculated. Values in mean column represent the average result of subtracting the proportion of students from families receiving FRPL in a student's prior school (measured in t) from the proportion of students from families receiving FRPL in the newly assigned school of a student who has been selected (and complied) for reassignment (measured in time t+1). All years represent spring of the academic year. 2005-06 and 2008-09 school years excluded due to no middle school students being reassigned for those years. If reassignment resulted in increased academic integration, FRPL students should have negative values and non-FRPL students should have positive values. SD represents within-year standard deviation.

Table A4: Difference in the proportion of assigned and actual changes in low-income students in the middle schools (Grades 7/8) to which students were reassigned compared to their previous school, by students' characteristics and school year (2005-06 to 2011-12)

Panel A. Assigned change in characteristics

	All Selected					All Compliers			
	Not Proficient		Proficient			Not Proficient		Proficient	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Pooled	0.014	0.085	0.001	0.075	Pooled	0.017	0.081	0.002	0.078
2006	-	-	-	-	2006	-	-	-	-
2007	0.020	0.015	0.016	0.018	2007	0.028	0.016	0.012	0.003
2008	-0.010	0.035	-0.011	0.036	2008	-0.021	0.025	-0.017	0.029
2009	-	-	-	-	2009	-	-	-	-
2010	0.020	0.097	0.005	0.093	2010	0.023	0.089	0.006	0.083
2011	0.026	0.070	0.004	0.078	2011	0.031	0.067	0.000	0.079
2012	-0.029	0.085	0.017	0.112	2012	-0.034	0.082	0.037	0.113

Panel B. Actual change in characteristics

	All Selected					All Compliers			
	Not Proficient		Proficient			Not Proficient		Proficient	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Pooled	0.014	0.101	0.004	0.069	Pooled	0.023	0.105	-0.006	0.082
2006	-	-	-	-	2006	-	-	-	-
2007	0.038	0.052	0.005	0.027	2007	0.017	0.023	0.013	0.025
2008	0.030	0.054	0.015	0.050	2008	0.054	0.058	0.019	0.068
2009	-	-	-	-	2009	-	-	-	-
2010	0.020	0.112	0.009	0.075	2010	0.033	0.116	0.001	0.092
2011	0.023	0.089	-0.005	0.072	2011	0.031	0.088	-0.016	0.079
2012	-0.050	0.104	-0.010	0.099	2012	-0.054	0.097	-0.004	0.097

Notes: Values in mean column represent the average result of subtracting the proportion of Proficient or above students in a student's prior school (measured in t) from the proportion of Proficient or above students in the newly assigned school of a student who has been selected (and complied) for reassignment (measured in time t+1). All years represent spring of the academic year. 2005-06 and 2008-09 school years excluded due to no middle school students being reassigned for those years. If reassignment resulted in increased academic integration, Non-Proficient students should have positive values and Proficient students should have negative values. SD represents within-year standard deviation.

Table A5: Difference in the proportion of assigned and actual changes in middle school students (Grades 7/8) scoring Proficient or above on the NC EOG ELA assessment in the schools to which students were reassigned compared to their previous school, by students' characteristics and school year (2005-06 to 2011-12)

	Avg. Prior Math (1)	SD Prior Math (2)	Avg. Non-FRPL (3)	% Black (4)	% Hisp (5)
Switched to new	0.031	-0.021***	0.020	-0.005	0.013**
	(0.026)	(0.003)	(0.011)	(0.007)	(0.005)
Switched to existing	0.023	0.023**	-0.023	0.021	0.006
	(0.037)	(0.008)	(0.016)	(0.012)	(0.006)
Observations	91612	91612	91612	91612	91612

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at node-year level in parentheses. Models present estimates from a modified version of Equation 1. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A6: Assumption check on actual changes in peer composition for school switchers

	(1) ELA teach exp	(2) Math teach exp	(3) Pct ELA novice	(4) Pct Math novice
Switched schools	-2.915***	-3.241***	0.096***	0.048***
	(0.363)	(0.320)	(0.009)	(0.007)
Observations	60464	68040	60464	68040
	(5) 1-yr lag math VA	(6) 2-yr lag math VA	(7) 1-yr lag ELA VA	(8) 2-yr lag ELA VA
Switched schools	-0.008**	0.000	-0.001	0.014***
	(0.003)	(0.004)	(0.001)	(0.002)
Observations	52310	39890	47032	35713

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at node-year level in parentheses. All models report 2nd-stage estimates from Equation 2. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics. Sample sizes differ from main models due to teachers with no prior VA measure, teachers missing employment history and teachers spanning grade levels. When we re-estimate models 5–8 and impute novice teachers' prior-VA to sample grade-by-year mean novice teacher VA, no estimates change by more than 0.005 *SD*. 1-year lag uses prior-year performance; 2-year lag pools two prior years, when available.

Table A7: Instrumental variable estimates of teacher characteristics for students who switch schools due reassignment, by grade cohort

	(1) Math teach exp	(2) Pct Novice	(3) 1-yr lag math VA	(4) 2-yr lag math VA
Switched schools	-4.043***	0.034**	-0.023***	-0.014
	(0.574)	(0.012)	(0.006)	(0.010)
Observations	52395	52582	33755	25489

Notes: *p < 0.05, **p < 0.01, ***p < 0.001. Robust standard errors clustered at node-year level in parentheses. All models report 2nd-stage estimates from Equation 2. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics. Sample sizes differ from main models due to teachers with no prior VA measure, teachers missing employment history and teachers spanning grade levels.

Table A8: Instrumental variable estimates of teacher characteristics for students who switch schools due reassignment, by classroom

	Endogenous predictor: School switch	
	Test-score sample	Course-grade sample
	(1)	(2)
Reassigned, new	0.536***	0.544***
	(0.032)	(0.032)
Reassigned, existing	0.357***	0.356***
	(0.017)	(0.017)
Observations	91612	85252
F-statistic	357.4	348.8

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors clustered at node-year level in parentheses. All models report first-stage results from Equation 1. F-test is Angrist-Pischke (2009) F-statistic of excluded instruments. First-stage models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A9: First-stage instrumental variable estimates for school switching

Panel A. Grade 8 next-year test scores and chronic absenteeism

	Math Test Score (1)	ELA Test Score (2)	Chronic Absence (3)
Switched schools	-0.021 (0.027)	-0.027 (0.028)	-0.013 (0.014)
Observations	43666	43666	43666

Panel B. Grade 8 next-year course grades

	Math Course Grade (1)	ELA Course Grade (2)
Switched schools	0.069 (0.050)	-0.104 (0.056)
Observations	38893	38893

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors clustered at the node-year level in parentheses. All models report 2nd-stage estimates from Equation 2. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A10: Within-school, year-after instrumental variable estimates of student reassignment outcomes, grade 8 only

Panel A. Test scores and chronic absenteeism

	Math Test Score (1)	ELA Test Score (2)	Chronic Absence (3)
Switched to new school	-0.028	-0.033	-0.006
	(0.022)	(0.023)	(0.013)
Switched to existing school	-0.013	-0.005	0.003
	(0.030)	(0.029)	(0.016)
Observations	91612	91612	91612

Panel B. Course grades

	Math Course Grade (1)	ELA Course Grade (2)
Switched to new school	0.123**	0.065
	(0.043)	(0.058)
Switched to existing school	0.013	-0.034
	(0.056)	(0.054)
Observations	85252	85252

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors clustered at the node-year level in parentheses. All models report 2nd-stage estimates from Equation 2. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A11: Instrumental variable estimates of effects of switching schools due to reassignment with two second-stage instruments

Panel A. Test scores and chronic absenteeism

	Math test score		ELA test score		Chronic absence	
	(1)	(2)	(3)	(4)	(5)	(6)
Reassigned to any school	-0.008		-0.007		-0.000	
	(0.008)		(0.008)		(0.005)	
Reassigned to new school		-0.015		-0.018		-0.003
		(0.012)		(0.012)		(0.007)
Reassigned to existing school		-0.004		-0.002		0.001
		(0.011)		(0.010)		(0.006)
Observations	91612	91612	91612	91612	91612	91612

Panel B. Course grades

	Math course grades		ELA course grades	
	(1)	(2)	(3)	(4)
Reassigned to any school	0.027		0.005	
	(0.016)		(0.017)	
Reassigned to new school		0.068**		0.036
		(0.024)		(0.032)
Reassigned to existing school		0.005		-0.012
		(0.020)		(0.019)
Observations	85252	85252	85252	85252

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors clustered at node-year level in parentheses. All models report reduced form regressing outcome on reassignnment. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A12: Reduced-form (ITT) estimates of effects of reassignment to different school

Panel A. Test scores and chronic absenteeism

	FRPL students			Non-FRPL students		
	Math Test Score (1)	ELA Test Score (2)	Chronic Absence (3)	Math Test Score (4)	ELA Test Score (5)	Chronic Absence (6)
Switched schools	0.008	-0.028	0.008	-0.036	-0.007	-0.003
	(0.037)	(0.036)	(0.023)	(0.022)	(0.023)	(0.010)
Observations	24361	24361	24361	67236	67236	67236

Panel B. Course grades

	FRPL students		Non-FRPL students	
	Math Course Grade (1)	ELA Course Grade (2)	Math Course Grade (3)	ELA Course Grade (4)
Switched schools	0.057	-0.013	0.085*	0.019
	(0.071)	(0.070)	(0.037)	(0.048)
Observations	22539	22539	62698	62698

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors clustered at the node-year level in parentheses. All models report 2nd-stage estimates from Equation 2. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A13: Instrumental variable estimates of effects of school switching due to reassignment, by family-income level

Panel A. Test scores and chronic absenteeism

	Math Test Score (1)	ELA Test Score (2)	Chronic Absence (3)
Switched schools	-0.012	-0.013	-0.016
	(0.026)	(0.027)	(0.014)
Observations	46292	46292	46292

Panel B. Course grades

	Math Course Grade (1)	ELA Course Grade (2)
Switched schools	0.051	-0.004
	(0.045)	(0.057)
Observations	43646	43646

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors clustered at the node-year level in parentheses. All models report 2nd-stage estimates from Equation 2. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A14: Instrumental variable estimates of effects of switching schools due to reassignment for middle two prior ELA achievement quartiles

Panel A. Test scores and chronic absenteeism						
	Math Test Score		ELA Test Score		Chronic Absence	
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' prior test scores (0.1σ)	0.051***	0.054***	0.034***	0.037***	0.002	0.004
	(0.013)	(0.013)	(0.006)	(0.006)	(0.003)	(0.003)
Pct. of non-FRPL peers (10 pp)	-0.058	-0.021	-0.014	0.033	0.015	0.043***
	(0.034)	(0.049)	(0.016)	(0.020)	(0.008)	(0.010)
Pct. of Black peers (10 pp)		0.062		0.055*		0.046***
		(0.051)		(0.023)		(0.013)
Pct. of Hispanic peers (10 pp)		0.053		0.103**		0.044**
		(0.079)		(0.035)		(0.017)
Observations	59255	59255	59255	59255	59255	59255

Panel B. Course grades				
	Math Course Grade		ELA Course Grade	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	-0.022	-0.013	-0.054*	-0.044
	(0.042)	(0.042)	(0.027)	(0.027)
Pct. of non-FRPL peers (10 pp)	0.045	0.144	0.012	0.129
	(0.109)	(0.167)	(0.085)	(0.098)
Pct. of Black peers (10 pp)		0.082		0.096
		(0.161)		(0.145)
Pct. of Hispanic peers (10 pp)		0.264		0.309
		(0.228)		(0.167)
Observations	53484	53484	53484	53484

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report endogenous OLS estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A15: Linear-in-means OLS estimates of endogenous changes in peer composition

Panel A. Test-score sample				
	Endogenous predictor:			
	Prior test (1)	% Non-FRPL (2)	% Black (3)	% Hisp (4)
Assigned peers' avg. prior test scores	0.558*** (0.061)			
Non-FRPL assigned peers (10 pp)		0.375*** (0.068)		
Black assigned peers (10 pp)			0.143*** (0.042)	
Hispanic assigned peers (10 pp)				0.196*** (0.042)
Observations	59255	59255	59255	59255
F-statistic	84.25	56.53	25.13	35.61
Panel B. Course-grade sample				
	Prior test (1)	% Non-FRPL (2)	% Black (3)	% Hisp (4)
Assigned peers' avg. prior test scores	0.577*** (0.063)			
Non-FPRL assigned peers (10 pp)		0.340*** (0.062)		
Black assigned peers (10 pp)			0.126** (0.041)	
Hispanic assigned peers (10 pp)				0.178*** (0.044)
Observations	53484	53484	53484	53484
F-statistic	99.73	47.51	22.03	30.39

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. Models report first-stage results from Equation 5 and Equation 6. All models regress endogenous variable on all four instruments; for clarity of exposition, we present only the coefficient on the instrument related to the endogenous predictor. F-statistic is Sanderson-Windmeijer (2016) multivariate F-test of excluded instruments for the instrument presented in that column. Models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A16: First-stage instrumental variable estimates for peer effects

Panel A. Test scores and chronic absenteeism

	Math Test Score			ELA Test Score			Chronic Absence		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Peers' prior test scores (0.1σ)	0.051**			0.023*			-0.003		
	(0.017)			(0.010)			(0.006)		
Pct. of non-FRPL peers (10 pp)		0.072			-0.004			-0.008	
		(0.050)			(0.037)			(0.016)	
Pct. of Black peers (10 pp)			-0.085			0.020			0.056
			(0.071)			(0.055)			(0.030)
Pct. of Hispanic peers (10 pp)			-0.145			0.084			-0.005
			(0.112)			(0.076)			(0.037)
Observations	59255	59255	59255	59255	59255	59255	59255	59255	59255

Panel B. Course grades

	Math Course Grade			ELA Course Grade		
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' prior test scores (0.1σ)	0.043			-0.029		
	(0.053)			(0.037)		
Pct. of non-FRPL peers (10 pp)		0.208			-0.025	
		(0.185)			(0.120)	
Pct. of Black peers (10 pp)			-0.205			0.128
			(0.222)			(0.182)
Pct. of Hispanic peers (10 pp)			-0.177			0.124
			(0.294)			(0.222)
Observations	53484	53484	53484	53484	53484	53484

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A17: Linear-in-means instrumental variable estimates of changes in peer composition with instruments estimated separately

Panel A. Test scores and chronic absenteeism						
	Math Test Score		ELA Test Score		Chronic Absence	
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' prior test scores (0.1σ)	0.056**	0.052*	0.034**	0.041***	-0.001	0.003
	(0.019)	(0.020)	(0.011)	(0.011)	(0.005)	(0.006)
Pct. of non-FRPL peers (10 pp)	0.046	-0.016	-0.016	0.093	-0.017	0.070
	(0.075)	(0.112)	(0.062)	(0.075)	(0.023)	(0.037)
Pct. of Black peers (10 pp)		-0.049		0.144		0.154*
		(0.158)		(0.125)		(0.066)
Pct. of Hispanic peers (10 pp)		-0.136		0.141		0.043
		(0.174)		(0.117)		(0.054)
Observations	55736	55736	55736	55736	55736	55736

Panel B. Course grades				
	Math Course Grade		ELA Course Grade	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	0.026	0.023	-0.032	-0.018
	(0.069)	(0.070)	(0.040)	(0.039)
Pct. of non-FRPL peers (10 p.p.)	0.254	0.194	-0.008	0.244
	(0.260)	(0.302)	(0.150)	(0.209)
Pct. of Black peers (10 p.p.)		-0.093		0.373
		(0.434)		(0.370)
Pct. of Hispanic peers (10 p.p.)		-0.052		0.249
		(0.431)		(0.319)
Observations	52936	52936	52936	52936

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A18: Linear-in-means instrumental variable estimates of changes in *classroom* peer composition

Panel A. Test scores				
	Bottom-quartile ELA students		Top-quartile ELA students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	0.041	0.037	0.046	0.046
	(0.028)	(0.026)	(0.025)	(0.025)
Pct. of non-FRPL peers (10 pp)	0.289*	0.206	0.106*	0.085
	(0.125)	(0.139)	(0.058)	(0.113)
Peer race adjust?		✓		✓
Observations	9654	9654	9666	9666
Panel B. Course grades				
	Bottom-quartile ELA students		Top-quartile ELA students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	0.049	0.020	0.026	0.045
	(0.078)	(0.077)	(0.041)	(0.043)
Pct. of non-FRPL peers (10 pp)	0.190	-0.084	0.313*	0.532*
	(0.292)	(0.367)	(0.195)	(0.243)
Peer race adjust?		✓		✓
Observations	8641	8641	8180	8180

Notes: $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A19: Linear-in-means instrumental variable estimates of changes in peer composition by prior ELA achievement on Mathematics outcomes

Panel A. Test scores				
	Bottom-quartile ELA students		Top-quartile ELA students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	0.026	0.041*	-0.000	0.003
	(0.023)	(0.021)	(0.019)	(0.018)
Pct. of non-FRPL peers (10 pp)	0.004	0.201	0.055	0.116
	(0.109)	(0.118)	(0.055)	(0.085)
Peer race adjust?		✓		✓
Observations	9654	9654	9666	9666
Panel B. Course grades				
	Bottom-quartile ELA students		Top-quartile ELA students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	-0.062	-0.063	-0.090	0.007
	(0.065)	(0.064)	(0.027)	(0.026)
Pct. of non-FRPL peers (10 pp)	-0.112	-0.136	-0.030	0.174
	(0.249)	(0.325)	(0.106)	(0.148)
Peer race adjust?		✓		✓
Observations	8641	8641	8180	8180

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A20: Linear-in-means instrumental variable estimates of changes in peer composition by prior ELA achievement on English Language Arts outcomes

Panel A. Test scores and chronic absenteeism

	Math Test Score		ELA Test Score		Chronic Absence	
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' prior test scores (0.1σ)	0.039*	0.032	0.010	0.015	-0.004	-0.001
	(0.018)	(0.020)	(0.011)	(0.011)	(0.007)	(0.007)
Pct. of non-FRPL peers (10 pp)	-0.031	-0.132	-0.012	0.077	-0.031	0.029
	(0.064)	(0.116)	(0.047)	(0.072)	(0.028)	(0.035)
Pct. of Black peers (10 pp)		-0.086		0.142		0.092
		(0.156)		(0.107)		(0.058)
Pct. of Hispanic peers (10 pp)		-0.252		0.106		0.074
		(0.197)		(0.128)		(0.062)
Observations	22361	22361	22361	22361	22361	22361

Panel B. Course grades

	Math Course Grade		ELA Course Grade	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	-0.002	-0.003	-0.016	0.007
	(0.061)	(0.0608)	(0.037)	(0.036)
Pct. of non-FRPL peers (10 pp)	0.222	0.244	-0.107	0.243
	(0.224)	(0.309)	(0.142)	(0.220)
Pct. of Black peers (10 pp)		0.150		0.537
		(0.315)		(0.329)
Pct. of Hispanic peers (10 pp)		-0.181		0.400
		(0.406)		(0.352)
Observations	20621	20621	20621	20621

Notes: $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A21: Linear-in-means outcomes for middle two quartiles

Panel A. Test scores								
	Bottom-quartile ELA students				Top-quartile ELA students			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.1 *SD* prior test scores	0.026	0.048			0.020	0.009		
	(0.053)	(0.060)			(0.027)	(0.029)		
Peers w/in 0.1σ			-0.120	-0.126			-0.103*	-0.112*
of prior score (10 pp)			(0.084)	(0.084)			(0.050)	(0.051)
10 pp ↑ non-FRPL peers	0.333**	0.236	0.330**	0.237	0.139*	0.081	0.123*	0.077
	(0.124)	(0.124)	(0.122)	(0.124)	(0.057)	(0.084)	(0.052)	(0.084)
Peer race adjust?		✓		✓		✓		✓
Observations	9654	9654	9654	9654	9666	9666	9666	9666
Panel B. Course grades								
	Bottom-quartile ELA students				Top-quartile ELA students			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.1 *SD* prior test scores	0.151	0.103			0.079	0.037		
	(0.110)	(0.123)			(0.055)	(0.064)		
Peers w/in 0.1σ			-0.120	-0.060			0.067	0.069
of prior score (10 pp)			(0.171)	(0.175)			(0.091)	(0.091)
10 pp ↑ non-FRPL peers	0.251	-0.068	0.240	-0.067	0.365**	0.507**	0.328**	0.511**
	(0.225)	(0.235)	(0.222)	(0.236)	(0.131)	(0.185)	(0.124)	(0.186)
Peer race adjust?		✓		✓		✓		✓
Observations	8641	8641	8641	8641	8180	8180	8180	8180

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A22: Non-linear-in-means instrumental variable estimates of changes in peer composition by prior achievement on Mathematics outcomes

Panel A. Test scores								
	Bottom-quartile ELA students				Top-quartile ELA students			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.1 *SD* prior test scores	-0.004	-0.018			0.024	0.004		
	(0.050)	(0.055)			(0.034)	(0.036)		
Peers w/in 0.1σ			-0.071	-0.079			0.067	0.063
of prior score (10 pp)			(0.090)	(0.090)			(0.059)	(0.059)
10 pp $\uparrow$ non-FRPL peers	0.029	0.233	0.030	0.234	0.069	0.116	0.058	0.117
	(0.105)	(0.124)	(0.103)	(0.124)	(0.066)	(0.093)	(0.060)	(0.093)
Peer race adjust?		✓		✓		✓		✓
Observations	9654	9654	9654	9654	9666	9666	9666	9666
Panel B. Course grades								
	Bottom-quartile ELA students				Top-quartile ELA students			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.1 *SD* prior test scores	-0.016	-0.004			0.046	0.011		
	(0.104)	(0.115)			(0.040)	(0.046)		
Peers w/in 0.1σ			0.020	0.011			-0.058	-0.052
of prior score (10 pp)			(0.157)	(0.158)			(0.070)	(0.070)
10 pp $\uparrow$ non-FRPL peers	-0.177	-0.185	-0.176	-0.185	-0.010	0.170	-0.037	0.169
	(0.200)	(0.228)	(0.198)	(0.228)	(0.092)	(0.140)	(0.086)	(0.140)
Peer race adjust?		✓		✓		✓		✓
Observations	8641	8641	8641	8641	8180	8180	8180	8180

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A23: Non-linear-in-means instrumental variable estimates of changes in peer composition by prior achievement on English Language Arts outcomes

	FRPL students				Non-FRPL students			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Test scores								
0.1 *SD* prior test scores	0.082	0.082			0.016	0.027		
	(0.050)	(0.054)			(0.017)	(0.018)		
Peers w/in 0.1σ			-0.124*	-0.112*			0.049*	0.048*
of prior score (10 pp)			(0.057)	(0.057)			(0.020)	(0.020)
Peer FRPL instrument?	✓	✓	✓	✓	✓	✓	✓	✓
Peer race adjust?		✓		✓		✓		✓
Observations	11119	11119	11119	11119	45671	45671	45671	45671
Panel B. Course grades								
	FRPL students				Non-FRPL students			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.1 *SD* prior test scores	0.027	0.008			0.141***	0.156***		
	(0.104)	(0.106)			(0.040)	(0.044)		
Peers w/in 0.1σ			-0.003	0.020			0.092*	0.091*
of prior score (10 pp)			(0.120)	(0.124)			(0.041)	(0.041)
Peer FRPL instrument?	✓	✓	✓	✓	✓	✓	✓	✓
Peer race adjust?		✓		✓		✓		✓
Observations	10049	10049	10049	10049	41210	41210	41210	41210

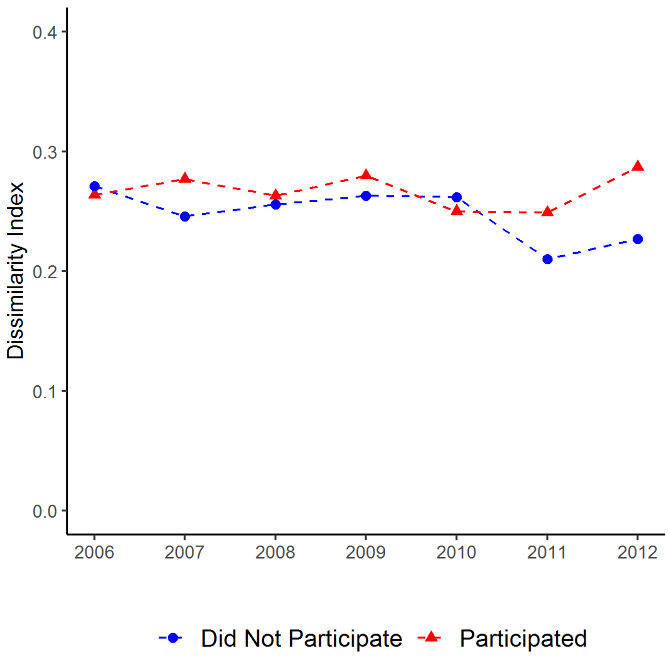
Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A24: Non-linear-in-means instrumental variable estimates of changes in peer composition by family-income level on Mathematics outcomes

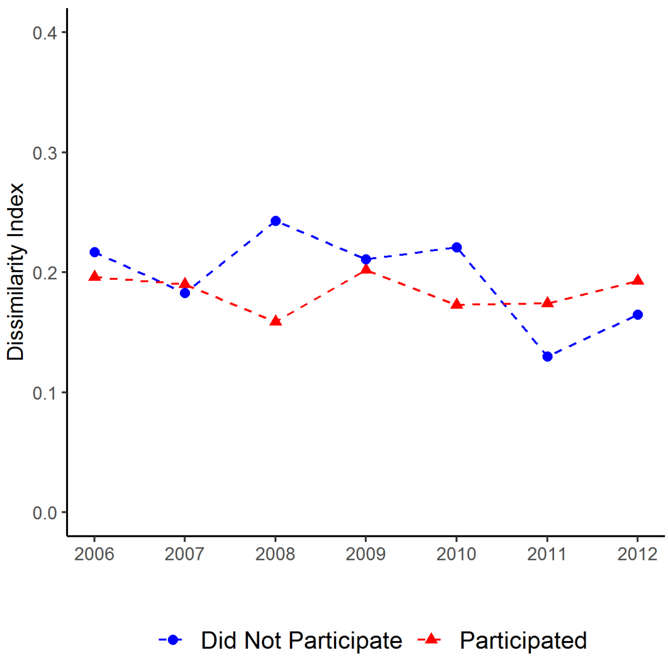
Panel A. Test scores								
	FRPL students				Non-FRPL students			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.1 *SD* prior test scores	-0.118*	-0.140*			0.019	0.012		
	(0.053)	(0.060)			(0.017)	(0.018)		
Peers w/in 0.1σ			-0.013	-0.012			0.035	0.036
of prior score (10 pp)			(0.062)	(0.062)			(0.023)	(0.023)
Peer FRPL instrument?	✓	✓	✓	✓	✓	✓	✓	✓
Peer race adjust?		✓		✓		✓		✓
Observations	11119	11119	11119	11119	45671	45671	45671	45671
Panel B. Course grades								
	FRPL students				Non-FRPL students			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
0.1 *SD* prior test scores	-0.256*	-0.235*			0.007	-0.020		
	(0.103)	(0.105)			(0.029)	(0.032)		
Peers w/in 0.1σ			0.055	0.018			0.004	0.007
of prior score (10 pp)			(0.113)	(0.115)			(0.036)	(0.036)
Peer FRPL instrument?	✓	✓	✓	✓	✓	✓	✓	✓
Peer race adjust?		✓		✓		✓		✓
Observations	10049	10049	10049	10049	41210	41210	41210	41210

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table A25: Non-linear-in-means instrumental variable estimates of changes in peer composition by family-income level on English Language Arts outcomes



(a) FRPL



(b) ELA Proficiency

Figure A1: Dissimilarity indices for Free- and Reduced-Price Lunch (FRPL) participation and English Language Arts (ELA) proficiency in Wake County, by whether schools ever participated in node reassignment (2006-2012)

Notes: Figures plot the dissimilarity index, as defined in the text in footnote #9, for schools that sent or received any nodes for reassignment in any year in our sample, and those that did not. All years represent spring of the academic year.

B History of Student Assignment in Wake County

Unlike most school districts in the South, Wake County was never the subject of a court desegregation order. In the 1970s, the Raleigh City Schools came under informal pressure from the Department of Health, Education and Welfare (HEW) to implement integration measures (McDermott et al., 2015). In 1975, the Boards of Education for the Raleigh City Schools and the Wake County Schools voted to merge effective July 1, 1976. This merger was approved by the boards and ratified by the North Carolina state legislature despite a non-binding community referendum held a few years earlier in which the voters in both districts voted against the proposed merger by more than a two-to-one margin (Wake County Public School System, 2012). Both Boards of Education found that the white flight from older, urban sections of Raleigh coupled with evidence of the beginnings of urban decay and increased residential segregation were major concerns for the community (Wake County Public School System, 2012). In addition, the danger of racial polarization, the lack of equity in educational programs, and the looming threat of civil rights action and court-mandated desegregation also served as catalysts for the merger in the face of public opposition (Wake County Public School System, 2012). According to Flinspach and Banks (2005), the Wake County commissioners, a number of school leaders, and the Raleigh business community believed that a single county-wide school system would improve the economic viability of greater Raleigh, and most especially improve areas near downtown. As a result, the approximately 20,000 Raleigh City students and the approximately 33,000 Wake County students joined to form the Wake County Public School System (WCPSS). This merger began a long history of the county-wide system's commitment to maintain diversity in its schools.

For much of the history of the district's student assignment policy, district leaders focused on maintaining and promoting diversity in its schools using student race as a factor in school assignment. They aimed to have the racial and ethnic demographic composition of the individual schools be roughly reflective of the district as a whole (Flinspach and Banks, 2005; Wake County Public School System, 2012). Specifically, WCPSS mandated that the share of Black students in all schools be fixed between 15 and 45 percent, a figure centered on the county-wide share of Black students in the mid-1970s of 30 percent (Flinspach and Banks, 2005; Parcel and Taylor, 2015).

One of the primary ways that this goal was accomplished came through the busing of Black students who were concentrated in downtown Raleigh to suburban Wake County, where schools were formerly all-white, coupled with more limited busing of white students to Black neighborhoods in urban centers (Flinspach and Banks, 2005). In addition, the district was divided into geographic nodes (See Figure 1 for school year 2011-12 node map) which represented neighborhoods (a geographic area, apartment complex, housing development, etc.) in the county, and children from each of these nodes were assigned to schools as a group in a way that aimed to limit racial segregation. Another instrument for accomplishing diversity within schools was the creation of a magnet program, which drew white students from suburban Wake County to the city center. These schools brought specialized programming such as attractive music and arts electives, more choices in foreign languages, and other features that might draw students from the suburbs into the city (Parcel and Taylor, 2015; Silberman, 2002).[25] Additional aims of the magnet program were to ensure that school facilities within the older and remote parts of Wake County were not underutilized and that students who lived in these areas had ready access to innovative educational programming.

Following shifting political and legal dynamics, Wake County gradually eliminated the use of race as a primary factor in determining student assignment in a series of policy shifts, and instead considered students' SES and reading levels. Throughout the 1990s, community members periodically advocated for a shift away from the race-based busing scheme to neighborhood schools (Flinspach and Banks, 2005; Parcel and Taylor, 2015). Following the *Dowell* and *Freeman* cases, the Fourth Circuit Court of Appeals ruled in two cases (*Eisenberg v. Montgomery County Public Schools* 197 F.3d 123 (1999) and *Tuttle v. Arlington County*, 195 F.3d 698 (1999)) that race could not be a determinative factor in judging students' applications to magnet schools. As North Carolina schools are controlled by Fourth Circuit Court decisions, Wake County ended the use of race in magnet school assignments in 1999 (McDermott et al., 2015).

Moreover, in the nearby Charlotte-Mecklenburg Schools, a federal district court ruled against race-based assignments in 1999 (Flinspach and Banks, 2005). Recall that Wake was never under formal court order; thus, this decision did not strictly preclude WCPSS from using race as a factor in student assignment. However, out of a prescient concern that the courts might soon restrict voluntary race-conscious student assignment policies, Wake County district leaders decided to further redesign their entire student assignment process to focus on balancing schools based on socio-economic diversity and academic achievement and avoid legal challenges of their own. Consequently, beginning in the 2000-01 school year, student SES rather than race was used as a factor in determining school assignment, with student eligibility for free or reduced-price lunch (FRPL) as a proxy for SES. In addition, the district sought to ensure that no school served an overwhelming concentration of students who scored below grade level on the state's reading assessment.[26] Consequently, the district implemented a school assignment strategy through which it selected geographic nodes of students for school reassignment and busing with the stated goal that no school served a student body made up of more than 40 percent FRPL students and more than 25 percent students reading below grade level. This policy guided the district for the next ten years.

Under the version of the district's assignment plan that we examine, system administrators sought to reach socio-economic integration targets by reassigning all students of the same grade level who lived in the same node as a group to a school with a different socio-economic and reading proficiency make-up than the average poverty rate and reading proficiency levels of their node (Flinspach and Banks, 2005; Parcel and Taylor, 2015). The decision about which nodes to reassign was reached by combining information on the distance students would need to travel to the nearest socioeconomically-different (but under capacity) or new school with feedback from the community and school board. District administrators would notify families

[25] The percentage of magnet schools over our period of study typically represents one-quarter to one-third of total schools.

[26] This was defined as below Level III on the NC End of Grade ELA test. The cut score for this reading level was consistent in our years of study from 2005/06 to 2006/07 and then from 2007/08 to 2011/12.

of plans to reassign a node to a different school approximately one year in advance, the Board of Education would vote on new assignment patterns in December, and final notification would occur to families in the winter. Families would then have between January and the summer to accept a transfer or go through an appeal process to remain in their base school (Parcel and Taylor, 2015).

Importantly, the student assignment policy was not limited to the need to balance racial and SES composition across schools. In practice, many decisions about student assignment related to the dramatic population growth experienced by the county and subsequent patterns in school overcrowding and new school construction. According to Census Data, between 2000 and 2010, Wake County's population grew by 42 percent. Indeed, student enrollment in Wake County schools increased from nearly 98,000 students at the start of the 2000-01 school year to nearly 150,000 by 2012—an increase of 53 percent—creating the need for several new schools and reassignments as a result. As Parcel and Taylor note, "[the] accelerated and uneven growth [of the housing stock with] little regard for the geographic placement of existing public schools (...) forced the school board to undertake yearly efforts to reassign children to different, but not always brand-new schools" (Parcel and Taylor, 2015, p. 51). In addition to the creation of new schools, the district also implemented "year-round" schools, intended to alleviate overcrowding (Parcel and Taylor, 2015). In this model, cohorts of students within a school were scheduled to alternate the timing of their vacations so as to maximize classroom usage. Assignments to year-round schools, as with assignments to newly-built schools were centrally determined.

One additional explanation as to why the reassignment process did not produce integrative changes in school environments was parent advocacy. Parents in higher-income communities sometimes advocated against the reassignment of students from lower-income families into their schools. Parcel and Taylor (2015) provide such an example: "Residents of the town [of Garner] just south of Raleigh complained repeatedly to policy makers that their schools had become a target for annual transfers of low-income children so as to reduce the proportion of students receiving FRPL in parts of central Raleigh. With more than 50 percent of their children formally classified as low-income, some Garner schools in 2007 had a much greater proportion of such students than did the town's aggregate population." (p. 55-56). In response to declining compliance and a conservative majority elected to the school board in October 2009, WCPSS began to consider changes to its student assignment policy in early-2010. Throughout 2010 and much of 2011, debate and controversy ensued. A WCPSS Board committee developed and approved a 16-zone neighborhood school assignment policy, but its implementation was halted in October 2010. After the resignation of a long-time WCPSS educator from the superintendent's position in February 2010, the WCPSS Board appointed a new superintendent in December 2010 and charged him with developing a new student-assignment system.

Finally, in October 2011, the Wake County School Board approved a student assignment policy to go into effect for the 2012-13 school year which eliminated the diversity requirement (Parcel and Taylor, 2015, p. 86).[27] This policy provided base assignment choices that included priorities

[27] New evidence suggests that school board political affiliation in North Carolina reliably predicts student racial and socio-economic segregation across schools (Macartney and Singleton, 2017) implying that the nature of the

for school proximity and balance in academic achievement levels. Students living in historically low-performing neighborhoods were guaranteed a regional choice in a "high-performing" school, as measured by teachers' effectiveness and preparation and school growth, proficiency, and graduation rates. In addition, rising kindergarten, sixth grade, and ninth grade students were required to choose their school from a menu of choices within their region. These students would be guaranteed a feeder pattern from 2012-2013 and beyond based on the school choice they made during the 2011-2012 school year.

Wake County assignment policies may continue to evolve over political cycles.

C Elementary School Results

C.1 Elementary descriptives

In Appendix Table C1, we present descriptive statistics on our elementary school sample. As with the middle-school sample, White and higher-achieving students are less likely to be selected for (and comply with) reassignments; whereas, Black, Hispanic and FRPL-eligible students are more likely. The same patterns holds for prior achievement. Overall, 61 percent of 4^{th} and 5^{th} graders selected to switch schools comply. Elementary school students experienced, in some years, relatively substantial changes in their peers composition (Figure C1 and Table C2 and Table C3); though even in these cases, they were not changes that resulted in more socio-economic or academic integration (Figure C2).

C.2 School switching

Two of our core assumptions for research question 1 are not satisfied for elementary students. The characteristics of the schools attended by students who switched schools due to reassignment changed from what they would have otherwise been (Table C4) and, more importantly, even when we condition on the the assignment covariates, there are observable differences in average characteristics between nodes that are selected for reassignment (either to a new or existing school) and those that are not (Table C5). In particular, selected nodes have higher proportions of Black and Hispanic resident students, lower proportions of White students and have lower mean prior math performance. These facts justify our choice to emphasize our middle-school results. Nevertheless, our instrument is a strong predictor of switching schools (Table C6, Model 1) and changes in peer composition (Table C6, Models 2–5) and we present these results for completeness here.

At the elementary school level, we see minimal evidence that switching to a new school as a result of reassignment produces substantively meaningful effects on students' test scores or rates of chronic absenteeism, either for all students or by prior performance and family income level (Appendix Table C7 and Table C8). With the previous caveats about unmet instrumental variable assumptions for these grades, these estimates are consistent with our middle school results.

C.3 Peer effects

As with our assumptions for our first research question, the assumptions outlined above for our second research question on peer effects are less strongly justified at the elementary school level. In particular, students' own prior performance is positively related to changes in their assigned peers' prior performance (Table C9).

We present elementary results of peer effects in Appendix Table C10 – Table C12 with strong caveats about unmet instrumental variable assumptions for these estimates. In all cases our estimates are either substantively similar but smaller in magnitude than our middle-school results or are inconsistently signed. In no cases are they consistently directionally opposite to our middle-school results.

	Not selected	Reassigned to new school	Reassigned to existing school	Reassigned school switchers
Male	0.50	0.50	0.50	0.50
	(0.50)	(0.50)	(0.50)	(0.50)
Asian	0.06	0.08	0.05	0.06
	(0.23)	(0.26)	(0.22)	(0.24)
Black	0.21	0.27	0.29	0.33
	(0.41)	(0.44)	(0.45)	(0.47)
Hispanic	0.12	0.13	0.15	0.19
	(0.32)	(0.34)	(0.36)	(0.39)
White	0.56	0.48	0.46	0.38
	(0.50)	(0.50)	(0.50)	(0.49)
Free/Reduced Lunch elig	0.30	0.33	0.41	0.47
	(0.46)	(0.47)	(0.49)	(0.50)
Prior-year, NC EOG math	0.10	0.01	-0.08	-0.18
	(0.96)	(0.95)	(0.98)	(0.95)
Prior-year, NC EOG ELA	0.08	-0.04	-0.10	-0.23
	(0.96)	(0.97)	(1.00)	(0.98)
Prior-year absences	6.39	6.36	6.75	6.83
	(5.42)	(5.50)	(5.78)	(5.76)
Prior-year chronic absence	0.04	0.04	0.05	0.05
	(0.20)	(0.20)	(0.22)	(0.23)
Observations	97581	3160	3242	3935

Notes: Each cell reports the sample average (standard deviation in parentheses).

Table C1: Elementary ($4^{th}/5^{th}$) analytic sample student-level descriptive statistics, 2005/06 – 2011/12

Panel A. Assigned change in characteristics

	All Selected					All Compliers			
	FRPL		Non-FRPL			FRPL		Non-FRPL	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Pooled	0.057	0.197	0.047	0.134	Pooled	0.067	0.194	0.045	0.134
2006	0.085	0.184	0.032	0.131	2006	0.095	0.169	0.037	0.132
2007	0.027	0.153	0.026	0.145	2007	0.037	0.148	0.028	0.146
2008	0.042	0.218	0.084	0.149	2008	0.037	0.233	0.078	0.149
2009	-0.025	0.182	0.039	0.123	2009	-0.045	0.163	0.035	0.120
2010	0.004	0.100	0.042	0.081	2010	0.002	0.083	0.026	0.078
2011	0.212	0.233	-0.018	0.082	2011	0.232	0.242	-0.016	0.063
2012	0.178	0.212	0.070	0.187	2012	0.212	0.188	0.137	0.187

Panel B. Actual change in characteristics

	All Selected					All Compliers			
	FRPL		Non-FRPL			FRPL		Non-FRPL	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Pooled	0.031	0.178	0.009	0.108	Pooled	0.041	0.193	-0.004	0.129
2006	0.024	0.126	0.026	0.098	2006	0.025	0.124	0.033	0.138
2007	0.015	0.136	0.003	0.115	2007	0.030	0.133	0.001	0.142
2008	-0.005	0.185	0.010	0.103	2008	-0.023	0.213	0.001	0.106
2009	-0.054	0.153	-0.014	0.118	2009	-0.128	0.124	-0.059	0.127
2010	0.007	0.118	0.019	0.072	2010	0.003	0.114	0.031	0.088
2011	0.114	0.190	0.006	0.090	2011	0.138	0.209	-0.020	0.070
2012	0.165	0.205	0.096	0.177	2012	0.236	0.175	0.197	0.172

Notes: Values in mean column represent the average result of subtracting the proportion of students from families receiving FRPL in a student's prior school (measured in t) from the proportion of students from families receiving FRPL in the newly assigned school of a student who has been selected (and complied) for reassignment (measured in time t+1). All years represent spring of the academic year. If reassignment resulted in increased academic integration, FRPL students should have negative values and non-FRPL students should have positive values. SD represents within-year standard deviation.

Table C2: Difference in the proportion of assigned and actual changes in low-income students in the elementary schools (Grades 4/5) to which students were reassigned compared to their previous school, by students' characteristics and school year (2005-06 to 2011-12)

Panel A. Assigned change in characteristics

	All Selected					All Compliers			
	Not Proficient		Proficient			Not Proficient		Proficient	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Pooled	-0.063	0.135	-0.040	0.098	Pooled	-0.063	0.123	-0.039	0.095
2006	-0.060	0.065	-0.048	0.055	2006	-0.077	0.055	-0.062	0.044
2007	0.007	0.092	0.000	0.082	2007	-0.002	0.100	-0.003	0.085
2008	0.000	0.082	-0.016	0.066	2008	0.007	0.077	-0.015	0.069
2009	-0.173	0.117	-0.161	0.083	2009	-0.151	0.109	-0.145	0.073
2010	0.034	0.078	0.009	0.079	2010	0.038	0.051	0.029	0.064
2011	-0.045	0.119	-0.001	0.069	2011	-0.053	0.114	0.008	0.062
2012	-0.097	0.145	-0.053	0.132	2012	-0.126	0.122	-0.081	0.123

Panel B. Actual change in characteristics

	All Selected					All Compliers			
	Not Proficient		Proficient			Not Proficient		Proficient	
	Mean	SD	Mean	SD		Mean	SD	Mean	SD
Pooled	-0.065	0.141	-0.021	0.106	Pooled	-0.075	0.128	-0.033	0.105
2006	-0.037	0.084	-0.019	0.078	2006	-0.080	0.073	-0.056	0.080
2007	0.035	0.068	0.022	0.071	2007	0.020	0.069	0.019	0.077
2008	0.015	0.067	0.014	0.059	2008	0.015	0.067	0.015	0.063
2009	-0.179	0.118	-0.147	0.098	2009	-0.139	0.105	-0.126	0.093
2010	0.012	0.100	0.007	0.087	2010	-0.007	0.088	-0.014	0.089
2011	-0.048	0.146	-0.007	0.133	2011	-0.107	0.127	-0.120	0.127
2012	-0.099	0.157	-0.049	0.157	2012	-0.140	0.150	-0.117	0.147

Notes: Values in mean column represent the average result of subtracting the proportion of Proficient or above students in a student's prior school (measured in t) from the proportion of Proficient or above students in the newly assigned school of a student who has been selected (and complied) for reassignment (measured in time t+1). All years represent spring of the academic year. If reassignment resulted in increased academic integration, Non-Proficient students should have positive values and Proficient students should have negative values. SD represents within-year standard deviation.

Table C3: Difference in the proportion of assigned and actual changes in elementary school students (Grades 4/5) scoring Proficient or above on the NC EOG ELA assessment in the schools to which students were reassigned compared to their previous school, by students' characteristics and school year (2005-06 to 2011-12)

	Avg. Prior Math (1)	SD Prior Math (2)	Avg. Non-FRPL (3)	% Black (4)	% Hisp (5)
Switched to new	-0.055**	-0.027***	-0.043***	0.042***	0.021***
	(0.018)	(0.007)	(0.012)	(0.012)	(0.005)
Switched to existing	-0.058	0.024**	-0.034	0.035**	0.012
	(0.032)	(0.008)	(0.017)	(0.013)	(0.007)
Observations	103983	103983	103983	103983	103983

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at node-year level in parentheses. Models present estimates from a modified version of Equation 1. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table C4: Assumption check on actual changes in peer composition for elementary school switchers

Panel A. Elementary grade-level node, without assignment covariates						
	% Black	% White	% Hisp	% Male	Prior Absence	Prior Math
	(1)	(2)	(3)	(4)	(5)	(6)
Reassigned to existing school	0.105***	-0.122***	0.018	-0.004	0.175	-0.047***
	(0.018)	(0.020)	(0.011)	(0.010)	(0.130)	(0.011)
Reassigned to new school	0.040*	-0.089***	0.035*	-0.012	-0.194	-0.042**
	(0.018)	(0.022)	(0.014)	(0.012)	(0.137)	(0.013)
Assignment covariates?						
Observations	8365	8365	8365	8365	8365	8365

Panel B. Elementary grade-level node, with assignment covariates						
	(1)	(2)	(3)	(4)	(5)	(6)
Reassigned to existing school	0.049***	-0.043***	-0.014	-0.007	-0.012	-0.009
	(0.013)	(0.012)	(0.011)	(0.010)	(0.123)	(0.005)
Reassigned to new school	0.026	-0.070***	0.030**	-0.014	-0.256	-0.016*
	(0.015)	(0.015)	(0.011)	(0.012)	(0.134)	(0.007)
Assignment covariates?	✓	✓	✓	✓	✓	✓
Observations	8365	8365	8365	8365	8365	8365

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors in parentheses. All models report estimates from Equation 3. Models fitted to data aggregated to the node-year level. All models include grade-band-school-year fixed effects. Assignment covariate models also include linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, and the number of students in each node-grade-school-year cell.

Table C5: Instrumental variable assumption checks for conditional randomization of reassignment for elementary grade-level nodes

	Endogenous predictor:				
	School switch	Prior test	% Non-FRPL	% Black	% Hisp
	(1)	(2)	(3)	(4)	(5)
Reassigned, new	0.543***				
	(0.014)				
Reassigned, existing	0.392***				
	(0.014)				
Assigned peers' prior scores		0.597***			
		(0.028)			
Non-FRPL assign. peers (10 pp)			0.341***		
			(0.033)		
Black assigned peers (10 pp)				0.191***	
				(0.027)	
Hispanic assigned peers (10 pp)					0.264***
					(0.028)
Observations	103983	62225	62225	62225	62225
F-statistic	1098.9	477.9	182.4	98.72	156.8

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at node-year level in parentheses. All models report first-stage results from Equation 1. F-test is Angrist-Pischke (2009) F-statistic of excluded instruments. First-stage models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table C6: First-stage instrumental variable estimates for elementary students

	Math Test Score	ELA Test Score	Chronic Absence
	(1)	(2)	(3)
Switched schools	-0.025	-0.011	0.004
	(0.018)	(0.016)	(0.007)
Observations	103983	103983	103983

Notes: $*p < 0.05$, $**p < 0.01$, $***p < 0.001$. Robust standard errors clustered at the node-year level in parentheses. All models report 2nd-stage estimates from Equation 2. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table C7: Instrumental variable estimates of effects of switching schools due to reassignment, grades 4-5

Panel A. Grades 4/5, by prior ELA achievement

	Bottom-quartile ELA students			Top-quartile ELA students		
	Math Test Score (1)	ELA Test Score (2)	Chronic Absence (3)	Math Test Score (4)	ELA Test Score (5)	Chronic Absence (6)
Switched schools	-0.026	-0.009	0.006	-0.052	0.000	0.017
	(0.029)	(0.028)	(0.011)	(0.038)	(0.040)	(0.016)
Observations	27458	27458	27458	23426	23426	23426

Panel B. Grades 4/5, by family-income level

	FRPL students			Non-FRPL students		
	Math Test Score (1)	ELA Test Score (2)	Chronic Absence (3)	Math Test Score (4)	ELA Test Score (5)	Chronic Absence (6)
Switched schools	-0.046	-0.033	0.010	-0.018	0.002	0.002
	(0.025)	(0.024)	(0.011)	(0.023)	(0.022)	(0.008)
Observations	31219	31219	31219	72759	72759	72759

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at the node-year level in parentheses. All models report 2nd-stage estimates from Equation 2. All models include grade-school-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table C8: Instrumental variable estimates of effects of school switching due to reassignment, by prior achievement and family-income level (Grades 4/5)

	Peers' prior perf. (1)	% non-FRPL (2)	% Black (3)	% Hisp (4)	Peers Re-assigned (0/1)? (5)
Prior math (t-1)	0.023***	-0.001	0.000	0.006	0.014
	(0.003)	(0.011)	(0.009)	(0.007)	(0.018)
Prior ELA (t-1)	0.005***	0.003	0.003	0.000	0.004
	(0.002)	(0.008)	(0.007)	(0.004)	(0.010)
Prior absences (t-1)	0.000	0.001	-0.001	0.000	-0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	62225	62225	62225	62225	62225

Notes: *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$. Robust standard errors clustered at school-grade-year level in parentheses. All models report estimates from Equation 7. All models include student and grade-year fixed effects. In contrast with Table 5, we are unable to observe the twice-lagged (t-2) prior outcomes for students' peers because twice-lagged outcomes for 4th graders reflect 2nd-grade performance and 2nd-graders do not sit for the EOG tests.

Table C9: Instrumental variable assumption checks for conditional exogeneity of changes in peer composition for elementary students

	Math Test Score		ELA Test Score		Chronic Absence	
	(1)	(2)	(3)	(4)	(5)	(6)
Peers' prior test scores (0.1σ)	0.012*	0.012*	-0.001	-0.002	0.003	0.004
	(0.008)	(0.008)	(0.007)	(0.007)	(0.002)	(0.002)
Pct. of non-FRPL peers (10 pp)	-0.000	-0.015	0.031	0.005	-0.009	-0.003
	(0.030)	(0.036)	(0.023)	(0.028)	(0.009)	(0.009)
Pct. of Black peers (10 pp)		-0.019		-0.054		0.026
		(0.050)		(0.043)		(0.017)
Pct. of Hispanic peers (10 pp)		-0.037		-0.049		0.004
		(0.053)		(0.045)		(0.018)
Observations	62225	62225	62225	62225	62225	62225

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table C10: Linear-in-means instrumental variable estimates of changes in peer composition (Grades 4/5)

	Math Test Score				ELA Test Score				Chronic Absence			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
0.1 *SD* prior test scores	-0.003	-0.003			-0.014	-0.012			-0.005	-0.006		
	(0.018)	(0.018)			(0.014)	(0.014)			(0.005)	(0.005)		
Peers w/in 0.1σ			-0.003	-0.003			0.010	0.009			-0.003	-0.003
of prior score (10 pp)			(0.014)	(0.014)			(0.015)	(0.015)			(0.006)	(0.006)
10 pp $\uparrow$ non-FRPL peers	0.008	-0.008	0.009	-0.007	0.027	0.001	0.031	0.004	-0.009	-0.001	-0.007	-0.000
	(0.029)	(0.035)	(0.028)	(0.035)	(0.025)	(0.028)	(0.024)	(0.028)	(0.010)	(0.009)	(0.009)	(0.009)
Peer race adjust?		✓		✓		✓		✓		✓		✓
Observations	62225	62225	62225	62225	62225	62225	62225	62225	62225	62225	62225	62225

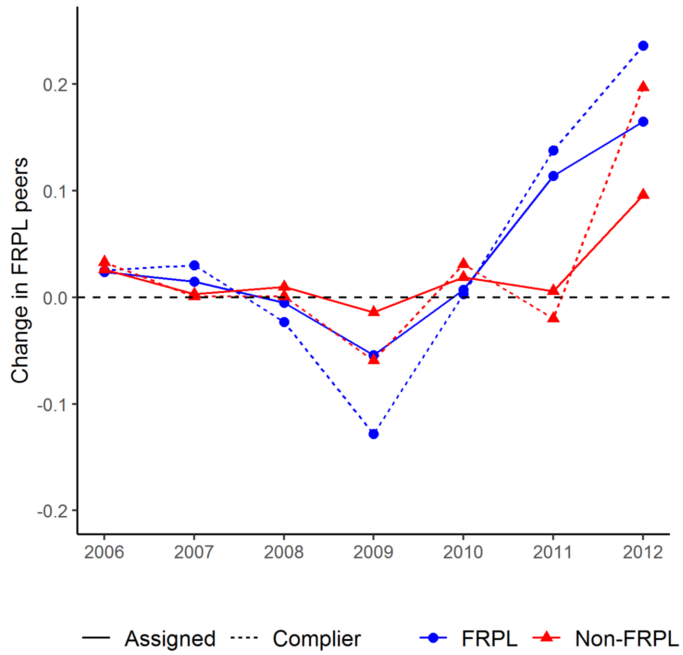
Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table C11: Non-linear-in-means instrumental variable estimates of changes in peer composition (Grades 4/5)

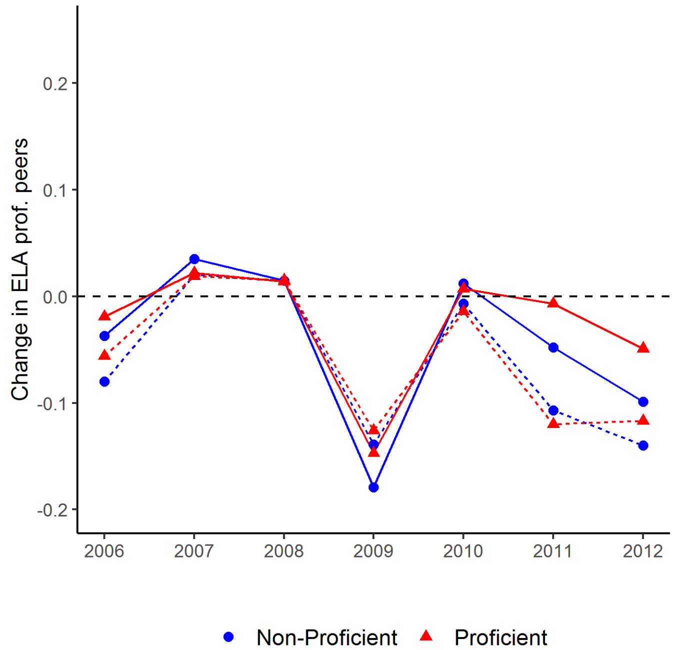
Panel A. Grades 4/5 peer effects on Math outcomes, by prior ELA achievement				
	Bottom-quartile ELA students		Top-quartile ELA students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	0.016	0.008	0.000	0.006
	(0.016)	(0.016)	(0.011)	(0.011)
Pct. of non-FRPL peers (10 pp)	-0.032	-0.125	-0.008	0.026
	(0.055)	(0.065)	(0.045)	(0.052)
Peer race adjust?		✓		✓
Observations	9854	9854	10389	10389
Panel B. Grades 4/5 peer effects on ELA outcomes, by prior ELA achievement				
	Bottom-quartile ELA students		Top-quartile ELA students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	0.006	-0.005	-0.002	-0.002
	(0.017)	(0.016)	(0.012)	(0.012)
Pct. of non-FRPL peers (10 pp)	0.113	-0.001	-0.031	-0.054
	(0.065)	(0.068)	(0.046)	(0.056)
Peer race adjust?		✓		✓
Observations	9854	9854	10389	10389
Panel C. Grades 4/5 peer effects on Math outcomes, by family income				
	FRPL students		Non-FRPL students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	0.027	0.018	0.005	0.008
	(0.014)	(0.013)	(0.008)	(0.008)
Pct. of non-FRPL peers (10 pp)	0.026	-0.015	-0.005	-0.000
	(0.049)	(0.054)	(0.032)	(0.038)
Peer race adjust?		✓		✓
Observations	13342	13342	46297	46297
Panel D. Grades 4/5 peer effects on ELA outcomes, by family income				
	FRPL students		Non-FRPL students	
	(1)	(2)	(3)	(4)
Peers' prior test scores (0.1σ)	-0.000	-0.007	-0.002	-0.002
	(0.015)	(0.014)	(0.007)	(0.007)
Pct. of non-FRPL peers (10 pp)	0.058	0.005	0.027	0.009
	(0.050)	(0.054)	(0.025)	(0.029)
Peer race adjust?		✓		✓
Observations	13342	13342	46297	46297

Notes: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors clustered at grade-school-year level in parentheses. All models report 2nd-stage estimates from Equation 4. All models include student fixed effects, grade-year fixed effects, linear and quadratic terms for node-grade-school-year % FRPL and average prior-year ELA test score, average distance to newly-opened schools serving the same grade level in the same year, number of newly-opened schools within 30 minutes' driving time, the number of students in each node-grade-school-year cell, node-grade-school-year characteristics (including average prior-year math score, average prior-year absences, % male, % Black, % Hispanic, and % Asian), individual student-level characteristics (including prior-year scores in math and ELA, prior-year absences, and indicators for FRPL, male, Black, Hispanic, and Asian), and indicators for missing node-grade-school-year characteristics or individual-level characteristics.

Table C12: Linear-in-means instrumental variable estimates of changes in peer composition by prior ELA achievement and family-income levels (Grades 4/5)



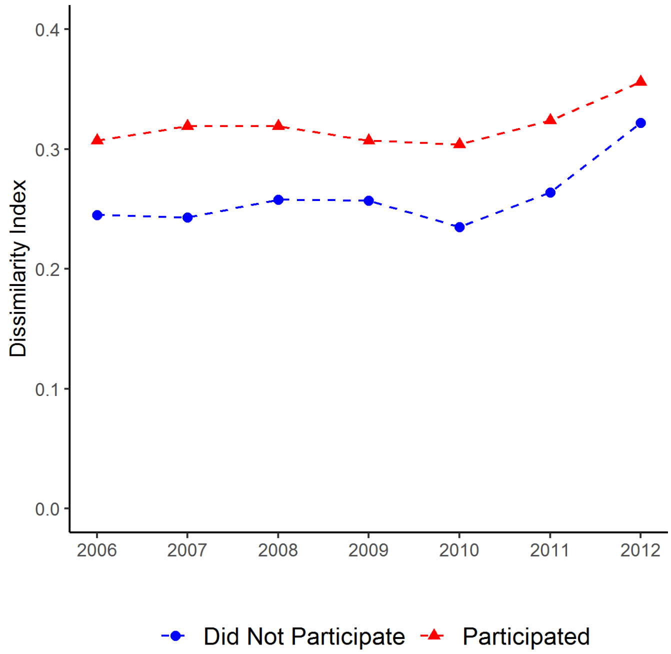
(a) FRPL



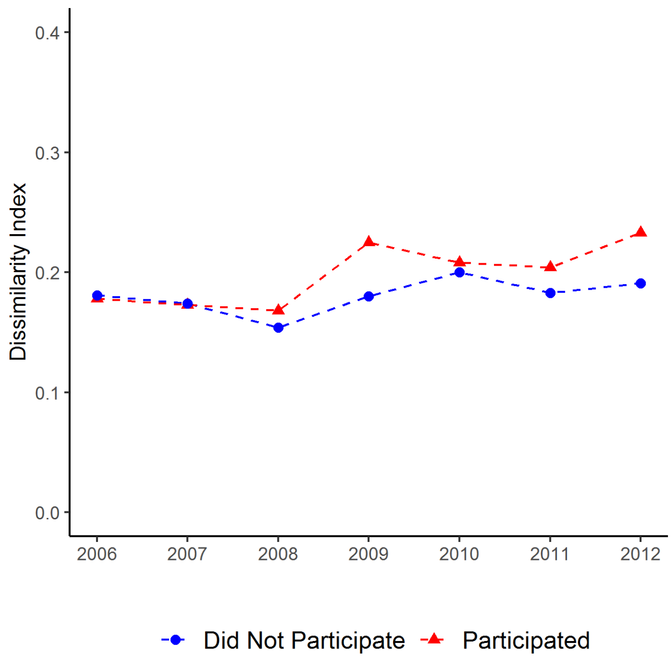
(b) ELA Profiency

Figure C1: Change in the proportion of peers receiving Free- or Reduced-Price Lunch (FRPL) and scoring Proficient or above in ELA for *elementary* students reassigned to different schools

Notes: Values represent the average result of subtracting the proportion of students scoring at or above Level III (Proficient) or receiving FRPL (measured in time t) from the proportion of students scoring at or above Level III or receiving free- or reduced-price lunch (FRPL) in the new school of a student who has been selected for reassignment (measured in time t+1). All years represent spring of the academic year. If reassignment resulted in increased socio-economic integration, FRPL students should have negative values and non-FRPL students should have positive values. If reassignment resulted in increased academic integration, non-Proficient students should have positive values and Proficient students should have negative values. Annual means and *SD*s available in Appendix Table C2 and Table C3.



(a) FRPL



(b) ELA Proficiency

Figure C2: Dissimilarity indices for Free- and Reduced-Price Lunch (FRPL) participation and English Language Arts (ELA) proficiency in Wake County, by whether *elementary* schools ever participated in node reassignment (2006-2012)

Notes: Figures plot the dissimilarity index, as defined in the text in footnote #9, for schools that sent or received any nodes for reassignment in any year in our sample, and those that did not. All years represent spring of the academic year.

D Data Description

Variable definition details. We identify the longitude and latitude coordinates of each node's centroid. With this information, we calculate distance and driving time measures between node pairs and between each node and the school to which the node is assigned (or reassigned) in a given year. In alignment with standards defined by the North Carolina Department of Public Instruction (NCDPI), we define chronic absenteeism as missing more than 10 percent of the academic year, or 18 school days.

Sample restrictions. Administrative data from WCPSS for school years 2005-06 through 2011-12 included 149,086 student-year observations for students in grades 4 and 5 and 142,998 student-year observations for students in grades 7 and 8. Key sample restrictions to generate the sample for IV analyses include (1) students have information about their reassignment status; (2) students did not attend a magnet school in the prior year; (3) students were enrolled in the district in the prior year; (4) students had non-missing values of current-year end-of-grade mathematics and ELA test scores and current-year absences; (5) students did not reside in a node that was newly created in the current year; (6) students were not missing characteristics of their node-school-grade band peers from year t; (7) students were not missing characteristics of their actual or assigned peers in year $t+1$; and (8) students were not the only student in their prior school-grade level-year cells in year $t+1$.

We excluded 774 4^{th} and 5^{th} grade student-year observations and 1,021 7^{th} and 8^{th} grade student-year observations that did not have populated information regarding their reassignment status. We then excluded 22,411 4^{th} and 5^{th} grade student-year observations and 31,718 7^{th} and 8^{th} grade student-year observations for students who attended magnet schools in the prior year. Students who attend a magnet school instead of their neighborhood school fall outside of the (re)assignment policies, even though they are "compliant" with the district's overall approach for school attendance. Finally, we excluded 2,345 4^{th} and 5^{th} grade student-year observations and 2,727 7^{th} and 8^{th} grade student-year observations for students who had enrolled in WCPSS previously but not in the immediate prior year, and we excluded 11,416 4^{th} and 5^{th} grade student-year observations and 9,615 7^{th} and 8^{th} grade student-year observations for students who were new to WCPSS in a given year. At this point, our viable sample included 112,140 student-year observations in grades 4 and 5 and 97,917 student-year observations in grades 7 and 8.

We next excluded student-year observations that were missing key current-year outcomes. We identified 8,091 student-year observations that were missing an end-of-grade math or ELA test score or the number of absences, and we identified a larger group of 14,794 middle-school student-year observations that were missing one of those outcomes or an end-of-course grade in math or ELA. We chose to create two branches of the middle-school sample. We use one sample to estimate effects on test score and absence outcomes, and we use a slightly smaller sample with non-missing course grades to study those additional outcomes. Excluding the 8,091 student-year observations missing test scores or absences, our sample for those outcomes included 107,425 student-year observations in grades 4 and 5 and 94,541 student-year observations in grades 7 and 8. With the further restriction to exclude observations missing test scores, absences, or course grades, our more-focused course-grades sample included 87,838 student-year observations in grades 7 and 8.

We excluded 3,412 student-year observations from the larger sample and 3,340 student-year observations from the more-focused course-grades sample for students who resided in a node in its first year of existence. We excluded 1,003 observations from the larger sample and 955 observations from the course-grades sample for students who were missing characteristics of their node-school-grade band peers from year t. We excluded 1,928 student-year observations from the larger sample and 1,710 observations from the course-grades sample for students who were missing characteristics of their actual or assigned peers in year $t+1$. Finally, we excluded 28 student-year observations from the larger sample and 23 student-year observations from the course-grades sample for students who were in singleton prior school-grade level-year cells in year $t+1$. In total, these restrictions yielded samples of 103,983 student-year observations in grades 4 and 5 and 91,612 student-year observations in grades 7 and 8 for the test scores and absences samples, and 85,252 student-year observations in grades 7 and 8 for the course-grades samples. These samples are featured in Table 1, Table 3, Table A6, Table A9, Table A11, Table A13 and Table A14. We feature slightly smaller samples in Table A7, Table A8 and Table A10 because we cannot estimate teacher effects for all students and Table A10 restricts to only students who are first treated in 7^{th} grade.

Models of the effects of changes in peer characteristics rely on students who are not selected to switch schools and the inclusion of student- and grade-by-year-level fixed effects for identification. From the featured samples, we excluded 14,309 student-year observations in grades 4 and 5, 9,464 student-year observations in grades 7 and 8 for the test scores and absences samples, and 8,737 student-year observations in grades 7 and 8 for the course-grades samples for students who switched into a new school for year $t+1$. Finally, we excluded 27,449 singleton student-year observations in grades 4 and 5, 22,893 singleton student-year observations in grades 7 and 8 for the test scores and absences samples, and 23,031 singleton student-year observations in grades 7 and 8 for the course-grades samples. The large number of exclusions at this stage is a result of the student- and grade-by-year-level fixed effects structure in which we drop observations if there is no within-student variation in reassigned peer characteristics. These restrictions yielded 62,225 student-year observations in grades 4 and 5, 59,255 student-year observations in grades 7 and 8 for the test scores and absences samples, and 53,484 student-year observations in grades 7 and 8 for the course-grades samples. These samples are featured in Table 5 through Table 9, Table A15, Table A16, Table A17, and Table A18.

Definition of selection. Throughout our analyses, we use a consistent definition of selection for reassignment. Students are considered to be selected for reassignment to attend a new school in year $t+1$ if all the students in their grade level who lived in their node and attended their school in year t were reassigned. (We treat students as reassigned in year $t+1$, while decisions are made based on enrollment patterns and student characteristics visible to WCPSS administrators in year t.) In particular, if a student deviated from their school assignment and attended a school s_2 in year t instead of their assigned school s_1, and students at s_1 were reassigned, we do not treat the student at s_2 as reassigned but we do leave them in our analysis sample.

Definition of assigned school. We consider a student's assigned school to be the school to which the student is assigned to attend by the district in a given year. In particular, if a student attended a magnet school in year t, we continue to consider the student's assigned school to be the same magnet school in year $t+1$. We make the assumption that if students attend magnet schools available to them under the district's available options, the district will expect those students to continue to attend the magnet schools in the future, and that those students' characteristics do not enter into the district's projections of changes in future school composition resulting from specific node and neighborhood school reassignments. As a result, in our analyses, the characteristics of students who attended magnet schools in the prior year do not enter into node-grade-school-year characteristics for our IV assumption checks or enter into policy-assigned peer characteristics for students assigned to attend neighborhood schools that we use as instruments for actual peer characteristics.

Imputation of missing baseline characteristics. For any student-year observations missing prior-year test scores, prior-year course grades, prior-year absences, or node-school-grade-year characteristics, we impute the missing values to be 0 and include a dummy variable for the respective variable to indicate that the original value of that variable was missing.