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New schools and new classmates: The disruption and peer group effects of school reassignment[☆]

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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.

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 (Hanushek et al., 2019; Hasim et al., 2020; 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., Belsha & Darville, 2020; Strauss, 2017).

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

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[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.

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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 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

[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.

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 & 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 & 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 & 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 & 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 et al. (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 et al. (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 15th largest U.S. school district. The district's efforts to use student assignment policy to promote diversity in schools have been widely publicized and studied. During the years we examine (2005–06 to 2011–12), WCPSS sought to accomplish two distinct goals through its

[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., Grigg, 2012; Rockoff & Lockwood, 2010; Schwerdt & 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 & Lang, 2004; Bergman, 2021; Mantil, 2021) have dominated much of the K-12 peer effects literature.

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 & Weingarth, 2005; Parcel & 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 & 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 & Lang, 2004; Bergman, 2021; Billings et al., 2014). 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 Fig. 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 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 et al. (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 4th and 5th 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 7th and 8th 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 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

[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 & Weingarth, 2005).

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

[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.

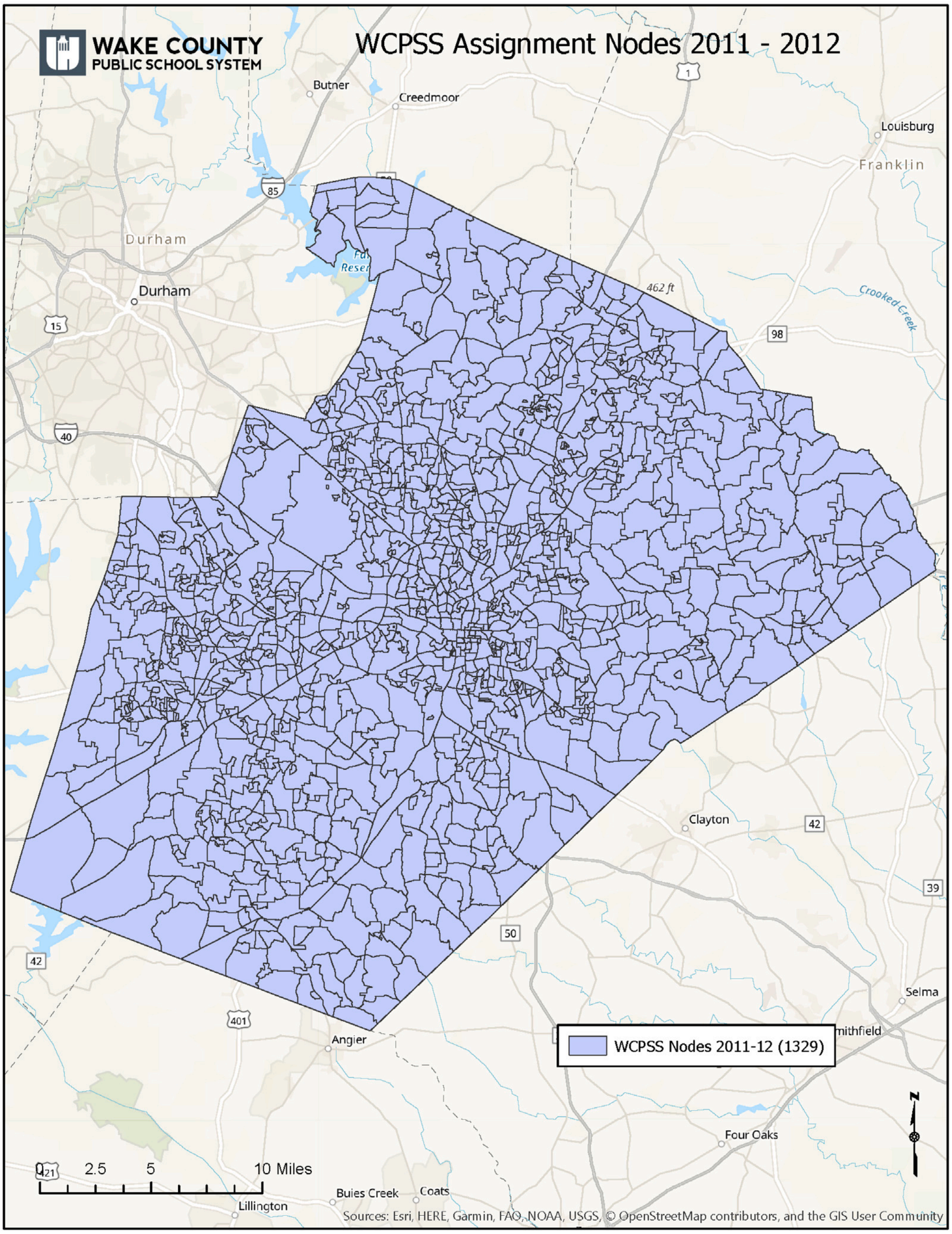

Fig. 1. Wake County Public School System Node Map, 2011–12.

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.

[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.

Table 1
Main analytic sample (7th/8th grade) student-level descriptive statistics, 2005/06–2011/12.

	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).

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 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 Fig. 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

[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.

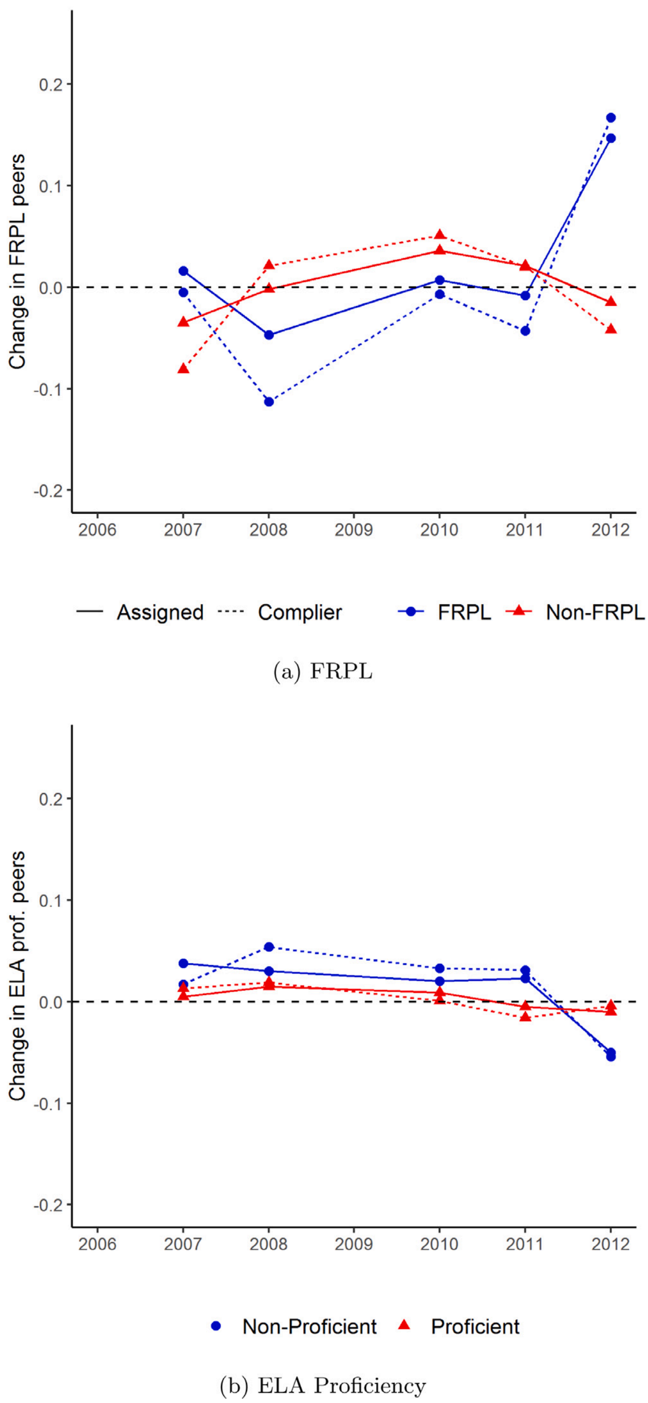

Fig. 2. Change in the proportion of peers receiving Free- or Reduced-Price Lunch (FRPL) and scoring Proficient or above in ELA for students reassigned to different schools.

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 *SDs* available in Appendix Table A4 and Table A5.

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.

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 & 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 min 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:

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

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

[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.

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 Eq. (1), the causal effect of *selection* for reassignment on moving is represented by β_3.

In the second-stage model (Eq. (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 Eq. (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 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.

[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).

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

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 Eq. (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.

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 Eq. (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 et al. (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]

[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

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

Panel A. Test scores and chronic absenteeism			
	Math Test Score (1)	ELA Test Score (2)	Chronic Absence (3)
Switched schools	−0.021 (0.019)	−0.020 (0.019)	−0.002 (0.011)
Observations	91612	91612	91612
Panel B. Course grades			
	Math Course Grade (1)	ELA Course Grade (2)	
Switched schools	0.077* (0.035)	0.023 (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 Eq. (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.

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 suggestive 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, δ_{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 (α_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 present results from fitting Eq. (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 Eq. (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

an additional school transition in 9th 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.

[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.

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

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 (0.037)	−0.052 (0.039)	0.019 (0.023)	−0.021 (0.036)	−0.010 (0.042)	0.009 (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 (0.069)	0.060 (0.069)	0.135* (0.054)	−0.023 (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 Eq. (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.

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:

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

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 Eq. (4) through Eq. (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 Eq. (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

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

	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 Eq. (7). All models include student and grade-year fixed effects.

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 Eq. (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 Eq. (4) through Eq. (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.

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]

[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.

[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

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)
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 Eq. (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.

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 Eq. (4) causally because we instrument for changes in the ethnoracial composition 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

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 (Lee et al., 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.

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 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 Tables 8 and 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 Fig. 3.

[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).

[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 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.

Table 7
Non-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 of prior score (10 pp)			0.014	0.014			0.029	0.029			−0.003	−0.004
			(0.019)	(0.019)			(0.024)	(0.025)			(0.011)	(0.011)
10 pp ↑ 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 of prior score (10 pp)			0.059	0.060			0.006	0.004
			(0.041)	(0.041)			(0.036)	(0.035)
10 pp ↑ 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 Eq. (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)	0.021	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 Eq. (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.

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 90th 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 Fig. 3, we scale our estimated effects to both the mean

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

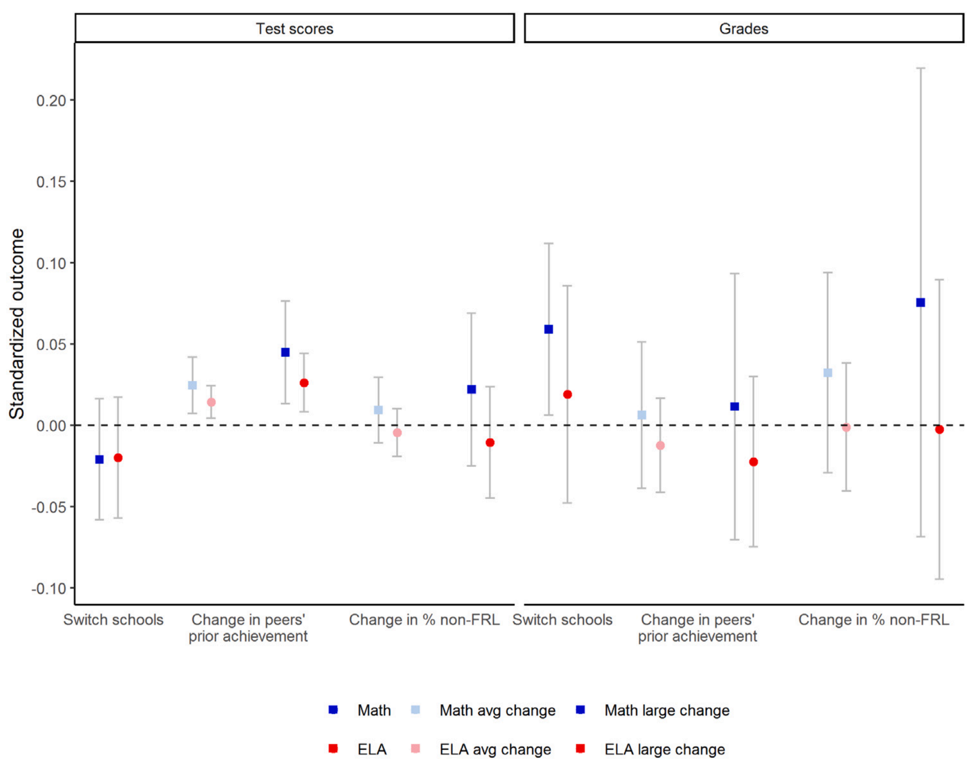

Fig. 3. Combined estimates of school reassignment policies on mathematics and English Language Arts outcomes for school switching and at prototypical "average" and "large" changes in peer composition.
Notes: Figure reports point estimates and 95% confidence intervals from Eqs. (2) and (4) as reported 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.

absolute changes ("average" change) and round approximates of the 90th 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 percentage points.

Beyond the evidence in Fig. 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.

CRediT authorship contribution statement

Darryl V. Hill: Conceptualization, Project administration, Resources, Writing – review & editing. **Rodney P. Hughes:** Conceptualization, Data curation, Formal analysis, Methodology, Project administration, Resources, Software, Validation, Visualization, Writing – original draft, Writing – review & editing. **Matthew A. Lenard:** Conceptualization, Project administration, Resources, Writing – review & editing. **David D. Liebowitz:** Conceptualization, Formal analysis, Funding acquisition, Methodology, Project administration, Resources, Validation, Visualization, Writing – original draft, Writing – review & editing. **Lindsay C. Page:** Conceptualization, Formal analysis, Methodology, Project administration, Resources, Validation, Visualization, Writing – original draft, Writing – review & editing.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.econedurev.2022.102316.

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