# Early Algebra Affects Peer Composition 

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#### Abstract

Although existing research suggests that students benefit on a range of outcomes when they enroll in early algebra classes, policy efforts that accelerate algebra enrollment for large numbers of students often have negative effects. Explanations for this apparent contradiction often emphasize the potential role of teacher and peer effects, which could create positive effects for individual students placed into early algebra that would not translate to larger-scale policies. We use detailed data from Oregon that contain information on the teachers and peers to whom students are exposed in order to investigate these explanations. Our regression discontinuity analyses replicate key findings from prior studies, indicating that placement in eighth-grade algebra boosts student achievement in math and English language arts. We then demonstrate that eighth-grade algebra placement positively affects the achievement level of students' classmates, as well as the years of experience and value added of students' math teachers. The effects on peer composition that we observe are large enough to plausibly explain the majority of the effects of eighth-grade algebra on student test scores.


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## 1 Introduction

Middle school course assignments represent a crucial branching point in many students' academic careers. Successfully completing algebra and related mathematics courses in middle school can put students on a trajectory to take calculus in high school and progress to more advanced mathematics and science postsecondary coursework (Adelman 1999; Attewell \& Domina 2008; Long, Conger, \& Iatorola 2012). Educators and educational policymakers have thus long viewed enrolling more eighth graders in algebra and other advanced mathematics courses as a promising strategy to improve access to STEM fields, particularly for students from low-income and minoritized backgrounds. Between 1990 and 2013, the proportion of eighth-grade students who enrolled in algebra or a more advanced mathematics course more than doubled to $46 \%$. More recently, however, the trend has begun to reverse. In 2022, just $36 \%$ of eighth graders enrolled in algebra or a more advanced mathematics course. At the local level, middle school mathematics course enrollment trends are even more volatile, as schools and districts experiment with a wide range of approaches to curricular acceleration and course placement (Domina, McEachin, Penner, \& Penner 2015; Domina, Hanselman, Hwang, \& McEachin 2016; Rickles 2011).

This volatility in middle school mathematics placements may reflect puzzling ambiguities in the research literature regarding the consequences of curricular acceleration for students' academic outcomes. Several studies, including studies that take advantage of randomized control trials or discontinuities in school placement policies, provide strong evidence to suggest that enrolling in eighth-grade algebra yields improvements in an individual student's math achievement as well as in other academic domains (Dougherty et al. 2017; Heppen et al. 2012; McEachin et al. 2020). But other studies suggest that largescale policies that increase the proportion of eighth graders enrolled in algebra or other advanced courses lead to broad test score declines (Clotfelter, Ladd \& Vigdor 2015; Domina et al. 2015).

If individual students benefit academically from enrolling in eighth-grade algebra courses, why might broad-based efforts to enroll more eighth graders in algebra have unintended negative consequences? Key differences between the effects of an individual-level treatment and a systemic change may help reconcile this puzzle. When an individual student takes an eighth-grade algebra course
rather than a lower-level mathematics course, that course placement likely affects at least three distinct aspects of students' educational experiences: (1) it increases students' exposure to the relatively advanced mathematics course content during their eighth-grade year, (2) it increases the chances that students have access to an experienced and effective mathematics instructor, and (3) it exposes students to a relatively high-achieving set of peers in their mathematics and other classes. It is not clear, however, that each of these mechanisms continues to operate in the same way when systemic efforts yield broad-based increases in eighth-grade algebra course-taking. Such curricular intensification policies explicitly aim to change the content of the mathematics instruction that students are exposed to in their eighth-grade year. However, since the pool of available teachers and the composition of students' peers is relatively fixed, increasing the share of students enrolled in eighth-grade algebra likely weakens the relationship between algebra courses and the characteristics of teachers and classroom peers.

In this paper, we build upon an approach developed by McEachin, Domina, and Penner (2020) to algorithmically identify schools in which students' probability of enrolling in eighth-grade algebra increases discontinuously at a threshold on their seventh-grade mathematics tests, using data describing students enrolled in Oregon public schools. We replicate McEachin et al,'s regression discontinuity findings regarding the effects of eighth-grade algebra on student academic outcomes, showing that eighthgrade algebra enrollment has a positive effect on student test scores in both math and English language arts (ELA). We further demonstrate that eighth-grade algebra enrollment substantially decreases the number of days students are absent in their eighth-grade year but has no effect on student suspensions.

We then investigate the extent to which teacher and peer exposure account for eighth-grade algebra's academic consequences. We find that being placed into advanced algebra has a significant impact on the education level, experience, and value added of students' eighth-grade math teacher. However, for ELA we see no such effects, leading us to conclude that while teacher characteristics might account for the math test score effects we observe, they are unlikely to account for the ELA test score
effects. ${ }^{1}$ By contrast, we find effects on peer test scores in both ELA and math classrooms as well as the peers with whom students take classes throughout the school day more broadly. Eighth-grade algebra course enrollment leads students to be exposed to higher-achieving peers in mathematics classrooms and reduces the number of their mathematics classroom peers who have experienced school suspensions. Because the mathematics courses students take can shape their schedule more broadly, we also examine the mean achievement of classmates that students share three or more classes with, finding that students in eighth-grade algebra courses move through their day with higher-achieving peers. Further supporting the idea that peer effects are a key driver of effects on ELA test scores, we find that effects of eighthgrade math placements on ELA scores are more pronounced in schools where there is a large difference in the prior year's test scores of peers with whom students take three or more classes.

The magnitude of the peer effects that we observe are large enough that, based on prior estimates of peer effects on achievement (Sacerdote 2011), they could plausibly account for the majority of eighthgrade algebra's observed effects on student achievement. We thus conclude that the changes in peer context that accompany being placed into eighth-grade algebra are crucial to understanding how advanced course-taking creates positive effects and why policies pushing all students into early algebra have failed to deliver their intended effects.

## 2 Early Algebra as a Collective Effects Problem

Penner, Domina, Penner, and Conley (2015) conceptualize the apparent contradiction between demonstrated positive individual-level effects of early algebra assignment and negative effects of broader curricular acceleration policies as a "collective effects" problem. Although it is tempting to view the provision of treatment at a small-scale to individuals as fundamentally the same treatment when offered at scale to the population more broadly, Penner et al. argue that these should be conceptualized as

[^0]fundamentally different treatments whose effects will only converge in specific circumstances. ${ }^{2}$ In the context of eighth-grade algebra, when an individual student moves from a grade-level course to an accelerated course, the move often results in a change in course curriculum, instructor, and classmates. Many of the most rigorous studies of the effects of eighth-grade algebra are designed to estimate this sort of individual-level or partial equilibrium treatment effect. For example, Heppen et al. (2012) report the results of a randomized controlled trial that offered access to online eighth-grade algebra courses to highachieving students in small, rural middle schools in which only grade-level mathematics courses were otherwise available. For these treated students, access to eighth-grade algebra represented access to highly trained teachers, a select set of high-achieving peers, and a virtual education setting, as well as accelerated mathematics curricula. While this study's RCT design likely yields unbiased estimates of the effects of this online eighth-grade algebra treatment, it is not clear that this design yields useful inferences regarding the consequences of broader curricular acceleration efforts.

Other well-identified studies in this literature use a regression discontinuity approach, identifying the effects of assignment to an eighth-grade algebra class for students who score just above a seventhgrade math test score threshold (Dougherty, Goodman, Hill, Litke, \& Page 2017; McEachin, Domina, \& Penner 2020). In this paper, we replicate and extend this research design, seeking to identify the ways eighth-grade algebra assignment affects students' educational experiences as well as their academic outcomes. Building on prior research suggesting that schools often assign relatively experienced and credentialed teachers, and relatively high-achieving students, to accelerated classes (Kalogrides, Loeb, \& Beteille 2013; Grissom, Kalogrides, \& Loeb 2015; Oakes 2005), we hypothesize that eighth-grade algebra

[^1]assignment provides students access to higher value added and to more experienced teachers and higher achieving peers compared to a lower-level course.

While our research strategy does not allow a formal mediation analysis, our analyses investigate whether eighth-grade algebra assignment improves student access to highly qualified teachers and highachieving classroom peers. Prior work has established strong causal links between teacher value added and a range of student outcomes (Chetty et al. 2014a, Chetty et al 2014b, Hanushek \& Rivkin 2010; Harris 2011; Jackson, Rockoff, \& Staiger 2013; Koedel, Mihaly, \& Rockoff 2015.) The evidence on the effects of classroom peers is relatively mixed, a reality that likely reflects limitations in our conceptualization of the attributes of peers that influence students (Fruehwirth 2014), student-level and contextual variation in susceptibility to peer effects, nonlinearities in the relationship between peer exposure and youth outcomes (Sacerdote 2014), and methodological challenges to the unbiased estimation of peer effects (Angrist 2014). Nonetheless, the most relevant existing evidence suggests that relatively high-achieving students benefit academically from exposure to relatively high- achieving classroom peers (Burke \& Sass 2013; Vardardottir 2013) and do worse when they share classrooms with peers experiencing behavioral problems or other emotional challenges (Carrell \& Hoekstra 2010; Figlio 2007; Hwang \& Domina 2021; Fletcher 2010).

Tracing the effect of eighth-grade algebra enrollment on students' teachers and peers may help to reconcile the apparent contradiction between studies demonstrating positive effects of enrolling in eighthgrade algebra at the individual level and negative effects of increasing eighth-grade algebra enrollment at the school or district level. Assuming that the number of teachers available to cover courses in a school is relatively fixed and that schools prefer to minimize the number of different courses their teachers teach, increasing the number of eighth-grade algebra classes a school offers likely requires reassigning teachers who are accustomed to teaching lower-level courses to higher-level courses. In many cases, this shift will tend to attenuate the association between course level and instructional quality.

A similar dynamic likely occurs regarding classroom peer composition in the face of broad curricular intensification. Since schools tend to assign relatively high-achieving students to accelerated
classes, an increase in the number of accelerated eighth-grade courses a school offers will tend to reduce the average level of student achievement and increase achievement heterogeneity in accelerated courses. As a result of these attendant changes in teachers and peers, broad-based efforts to increase eighth-grade algebra enrollment may not yield the positive effects of eighth-grade algebra courses observed in studies focusing on partial equilibrium effects, as teachers who have limited experience teaching accelerated content work to meet the instructional needs of classrooms with uneven levels of prior knowledge (Dougherty et al. 2006; McEachin et al. 2020).

The effects of math course assignments on students' teacher and peer exposure may not be limited to mathematics classrooms. Both scheduling constraints and teacher conceptions about the nature of student ability often lead students to move through their school days with a fairly consistent set of peers, so that the students who are in the same eighth-grade math classroom are likely to also be together in their English, social studies, and science courses. As a result, students who enroll in eighth-grade algebra may be exposed to more experienced or higher value-added teachers and higher-achieving peers in several of their academic classes, potentially driving the effects of eighth-grade algebra on student achievement and explaining why many studies find effects of eighth-grade algebra enrollment on student ELA achievement (McEachin et al. 2020). Just as the expansion of eighth-grade algebra is likely to weaken the correlation between algebra enrollment and the characteristics of students' math teachers and math classroom peers, it may also weaken the correlation between algebra enrollment and the characteristics of students' teachers and peers in the other courses they take.

## 3 Data

Our analyses use data provided by the Oregon Department of Education (ODE) covering all students in Oregon public schools from academic years 2013-14 to 2016-17. Our analytic sample includes eighthgrade students enrolled in a mathematics course during 2014/2015 to 2016/2017 school years for whom seventh-grade test scores from the prior year are available. The data provided by ODE include students' seventh-and eighth-grade Oregon Statewide Assessment scores for math and ELA, student-level
demographics, school and district identifiers, and course enrollment information. Course enrollment data includes subject area, course title, enrollment date, and unique teacher identifier. Student assessments for math and ELA are required for grades $3-8$. We restrict the sample to students in schools that enrolled 50 or more eighth graders in the focal students' eighth-grade year for whom seventh-grade test scores are available. ${ }^{3}$ Table 1 provides a summary of these data. Statewide, roughly $31 \%$ of eighth-grade students took an advanced algebra course in these years.

Our primary analyses use a regression discontinuity design, which we describe in detail below. We first assess the effects of eighth-grade algebra enrollment on student academic outcomes, as measured by their eighth-grade math and ELA test scores and number of school absences in the eighth-grade year. We then consider the effects of eighth-grade algebra on the years of experience a student's teacher had, and on their teacher's value added. Finally, we consider the effects of eighth-grade algebra enrollment on the achievement and disciplinary record for three sets of students' classroom peers: 1) peers in a student's eighth-grade math classroom, 2) peers in a student's eighth-grade ELA classroom, and 3) peers who are in three or more classes with a student.

## 4 Regression Discontinuity Search

Unlike a typical RD design, we do not know in advance the existence and location of advanced algebra placement thresholds for each school. Therefore, to determine whether there is a placement policy and what the threshold is for each school, we perform a search for threshold-based placement policies across all Oregon public schools. To do so, we first run a series of linear regressions separately for each school-by-year combination with at least 50 students:

[^2]\[

$$
\begin{equation*}
\text { Algebra }_{i t}=\beta_{0}+\beta_{1} 1\left[X_{i, t-1} \geq c\right]+\beta_{2}\left(X_{i, t-1}-c\right)+\beta_{3}\left(X_{i, t-1}-c\right) * 1\left[X_{i, t-1} \geq c\right]+\epsilon_{i t} \tag{1}
\end{equation*}
$$

\]

where Algebra $_{i t}$ is an indicator for whether a student takes advanced algebra in eighth grade, $X_{i, t-1}$ is the student's seventh-grade math test score, and $\epsilon_{i t}$ is an error term. We run a separate regression for each potential cutoff, $c$, that takes as values each possible unstandardized test score in a given year's test, and include all observations within one standard deviation of the cutoff $c$. For each school-by-year combination, we identify the cutoff $c$ that maximizes the R -squared as a potential threshold for advanced algebra placement.

Although every school-by-year combination will have a cutoff identified, we do not think that all schools used a threshold in students' prior achievement to inform algebra assignment decisions. To identify cutoffs that represent true thresholds, we use a bootstrap procedure where we run 1000 replications for the chosen cutoff and keep only school-by-year combinations in which the $99 \%$ bootstrap confidence interval does not include 0 (cf., Pan 2015; McEachin, Domina, \& Penner 2020). ${ }^{4}$

This approach has two advantages over limiting the sample to sites with explicitly articulated thresholdbased placement policies, as might be done in a more traditional regression discontinuity analysis (c.f. Dougherty et al. 2017): First, it identifies schools that place students into eighth-grade algebra classes in a discontinuous fashion based on prior year test scores, even if those schools have not publicly articulated this placement policy. As such, this search procedure allows our analysis to uncover placement policies that are either formally or informally based on test score cutoffs as well as settings in which the number of seats in eighth-grade algebra courses is fixed, and educators place the highest scoring students into eighth-grade algebra until the courses are full. Second, it focuses on the implementation of thresholdbased course placement, rather than the intention to implement such a policy. In districts where a

[^3]threshold-based placement policy exists there are school-by-school and year-to-year deviations from the policy in practice, as educators and other stakeholders bring additional considerations to their course placement practice. Our automated search accounts for these deviations as it flags schools as using discontinuous placement only in the years in which they implemented the placement policy in a manner that generated observable discontinuities in placement at a test score threshold. ${ }^{5}$

Figure 1 shows the geographic distribution of schools identified as having placement policies, showing that our analysis sample includes students and schools across the state. That said, as is evident from Table 1, schools identified as using thresholds are relatively higher performing, enroll larger proportions of White students, and enroll fewer proportions of Hispanic students than the state as a whole.

Figure 2 shows the location of the cutoff thresholds in our sample. There are cutoffs in multiple places across the distribution, but the largest spike occurs around 0.5 standard deviations above the mean, which is slightly below the threshold for a student to be categorized as "exceeding" proficiency standards (which ranges from 0.60 to 0.75 standard deviations depending on the year). In Appendix B we show that analyses restricting to just thresholds close to known proficiency thresholds produce qualitatively similar results to those presented below.

## 5 Analytic Methodology

Taking the thresholds identified in the relevant schools and years identified by the RD search procedure above, we next use a fuzzy regression discontinuity design to estimate the effect of placement into algebra in eighth grade. Because the seventh-grade math test changed during the period of our study, we first create a standardized running variable and cutoff using a mean 0 , standard deviation 1 standardized test score. This allows us to pool across all years and estimate regressions such as the following:

$$
\begin{equation*}
Y_{i s t}=\beta_{0}+\beta_{1} 1\left[X_{i t} \geq c\right]+\beta_{2}\left(X_{i t}-c\right)+\beta_{3}\left(X_{i t}-c\right) * 1\left[X_{i t} \geq c\right]+\gamma_{s t}+\epsilon_{i t} \tag{2}
\end{equation*}
$$

[^4]where $Y_{i s t}$ is an outcome of interest, $X_{i t}$ is the student's standardized seventh-grade math test score, and $c$ is the threshold that is identified in the RD search procedure. Because the identification of an average treatment effect is complicated by the presence of multiple cutoffs, we include school-by-year fixed effects, $\gamma_{s t}$, following the approach of Fort et al. (2022). $\epsilon_{i t}$ is an error term, and as with other RD studies the key assumption to the validity of this specification is that there are no unobserved determinants of outcomes that vary discontinuously across the threshold. All standard errors are robust to heteroskedasticity.

This fuzzy RD approach makes the following identifying assumptions: (1) selection into eighthgrade math courses is strongly influenced by student test scores, such that students who score above the threshold are considerably more likely to enroll in eighth-grade algebra; (2) students and teachers are not able to influence which students fall on either side of the placement threshold; (3) students on both sides of the threshold are comparable to one another, such that student potential outcomes vary continuously at the placement threshold. If these assumptions hold, this RD model provides highly internally valid estimates of the local average treatment effect (LATE) of eighth-grade algebra for students near the threshold in each of these schools (Imbens \& Lemiuex, 2007; Lee \& Lemiuex, 2010; McCrary, 2008).

In Table 2, we report the results of several analyses designed to investigate the extent to which the schools where we identified threshold-based eighth-grade mathematics placement practices meet RD analyses' core identifying assumptions. Panel A reports the results of these models for the full sample of schools identified by the search procedure.

The results reported in Table 2's first column reveal that students who are just above their school's placement threshold are, on average, 48.5 percentage points more likely to enroll in eighth-grade algebra than students just below the placement threshold. ${ }^{6}$ Note that this represents a "fuzzy" regression

[^5]discontinuity design, where the probability jumps discontinuously but does not approach 1 . This is to be expected as other factors such as grades can also influence placement into advanced algebra.

Column 2 of Table 2 reports the results of a test of the density of the running variable (cf. McCrary 2008), by using the number of students scoring a given test score value as the outcome in our placebo specification. These analyses reveal no significant discontinuities in the density of students at schools' placement thresholds. While not sufficient to prove that no manipulation of the running variable is occurring, these results are consistent with the conclusion that manipulation is not leading to an unexpected number of students above or below the threshold. Direct manipulation of student test scores is unlikely given the difficulty of precisely altering test scores in order to ensure placement into advanced algebra. In addition, while it is possible that schools could try to pick a seventh-grade math test score for their cutoff in response to the set of students near a threshold to "boost" some students over, this also seems unlikely.

The remaining columns of Table 2 report the results of a series of placebo regression discontinuity analyses that use students' prior achievement and demographics as dependent variables. Differences in student characteristics at RD thresholds may indicate endogenous placement of students at the margin of either side of the cutoff or may be the result of a choice by school officials regarding where to locate the placement threshold. Potentially problematically, students just above eighth-grade algebra placement thresholds have higher sixth-grade math and ELA test scores than their peers just below the threshold.

To address this concern, we trim the sample by discarding schools where the placebo analysis
significantly predicts prior achievement (using a mean standardized score of sixth-grade math, sixth-grade ELA, and seventh-grade ELA). We refer to our final sample as the "trimmed sample." Table 3 shows the results of this process. Starting from a total sample of 300-400 schools and $25,000-30,000$ students per year, only roughly 20-25 schools per year and 2,500 students per year are identified as having thresholds by the bootstrap procedure. Our trimmed sample excludes a few more schools, so that our final trimmed sample used for analyses below contains 5,562 students in 49 schools. Similar to McEachin et al. (2020)
we find less evidence of contamination around the threshold in placebo analyses estimated on the trimmed sample, reported in Panel B of Table 2, than in placebo analyses estimated on the full sample and reported in Panel A of Table 2.

## 6 Results

Figure 3 illustrates the discontinuity in eighth-grade algebra course enrollment rates that occurs at the thresholds identified by our search procedure in both our full sample and our trimmed sample. Consistent with the analyses reported in Table 2 above, this illustration shows that student eighth-grade algebra placement rates increase by more than 40 percentage points at the placement threshold.

Our analyses exploit these discontinuities to examine the effects of being assigned into advanced algebra on student academic outcomes and on the teachers and peers to which they are exposed. In the results that follow, we primarily focus on the results using the trimmed sample but note that results using the full sample of schools, reported in Appendix C, are qualitatively similar.

### 6.1 Effects on Student Academic Outcomes

Figure 4 presents averages of our focal student academic outcomes-eighth-grade math and ELA end-ofgrade test scores, as well as school absences in the eighth-grade year-at binned values representing percentiles of seventh-grade math achievement. Previewing our results below, we see modest estimated discontinuities on each of these variables at the algebra course placement threshold. We see these discontinuities as evidence of an effect of eighth-grade algebra course placement on student achievement.

Table 4 presents the results of a more formal reduced form (intent-to-treat) regression discontinuity analysis on these student academic outcomes for the trimmed sample and 1 standard deviation bandwidth. We see evidence of a substantial, significant increase in eighth-grade math test scores of 0.12 standard deviations and a smaller significant increase in eighth-grade ELA test scores of 0.065 standard deviations for students who score above the threshold cutoff. We also present reduced form results for eighth-grade
absences and suspensions, showing a decrease of nearly one day absent from school (which represents more than a tenth of a standard deviation in the trimmed sample).

These results generally align with those in McEachin et al. (2020), who use a similar methodological approach to study placement into advanced math in California. In general, the test scores effects that we report here are larger in magnitude here than in California. McEachin et al. (2020) estimate an ITT effect of 0.031 in mathematics and an ITT effect that ranges from 0.012 to 0.020 in ELA; our estimated test scores are three to four times as large. Test score timing may be a partial explanation for this difference. McEachin et al. (2020) only have access to mathematics test scores measured in the spring of tenth gradea full two years after exposure to eighth-grade algebra-and do not have access to the more proximal eighth-grade test scores that are the outcomes in our analyses. If, as seems likely, the achievement effects of eighth-grade algebra fade as students progress through high school, it is entirely likely that effects on test scores are particularly pronounced in eighth grade and fade out slightly as students progress through high school. We note, however, that McEachin et al. estimate the spillover effects of eighth-grade algebra on ELA achievement in eighth, ninth, tenth, and eleventh grade and find no evidence to suggest that the effects of eighth-grade algebra fade out in this domain.

### 6.2 Teacher Characteristics and Placement into Advanced Math

While the effects of eighth-grade algebra on mathematics achievement and student attendance are consistent with the theory of action motivating math course acceleration efforts, the spillover effects on ELA achievement that we and McEachin et al. identify are somewhat more surprising. Why might access to an early algebra course improve a students' ELA achievement? As we argued above, we suspect that assignment to an eighth-grade algebra class changes several aspects of a students' educational experience, including the characteristics of the teachers and the peers to which the student is exposed. In addition to accounting for these ELA effects, these mechanisms may help to explain why the student-level effects of eighth-grade algebra assignment so often do not scale in policy settings that aim to provide access to
accelerated algebra for larger numbers of students. To explore these mechanisms, we next present results related to teacher and peer outcomes.

Figure 5 provides a visual representation of the relationship between seventh-grade math test scores and several attributes of the teachers that students are exposed to in the eighth-grade math and ELA courses, including their experience and test score value-added measures. In general, these relationships are noisy. While there is some evidence in the graphs of discontinuities in math teacher characteristics at the eighthgrade algebra course placement threshold, the graphs representing ELA teacher attributes do not provide evidence to suggest that eighth-grade math course placement changes students' ELA teachers.

Table 5 presents estimates from our more formal regression discontinuity model for the effect of eighthgrade algebra placement on years of experience and value-added measures for a students' eighth-grade math and ELA teachers. Students who score above the threshold have significantly more experienced eighth-grade math teachers, with teacher experience increasing by an average of 1.2 years at the placement threshold. We also estimate that scoring above the threshold results in students being exposed to higher value-added math teachers, showing that the value added of students' eighth-grade math teacher increases by 0.09 standard deviations if they score above the threshold cutoff, and they are significantly more likely to be exposed to a math teacher in the top quartile of value added. We thus conclude that placement into advanced math significantly changes the characteristics of the eighth-grade math teachers to which students are exposed in terms of value added and experience. Based on prior research linking a standard deviation increase in teacher value added to an 0.1 to 0.3 standard deviation increase in achievement (Hanushek \& Rivkin 2010; Harris 2011; Jackson, Rockoff, \& Staiger 2013), we believe that it is plausible that teacher value added explains as much as $10 \%$ of the effect of eighth-grade algebra on student math achievement.

Unlike math, we do not see any significant differences in eighth-grade ELA teacher characteristics between students who score above or below the threshold cutoff, and the magnitude of the differences we observe is small regardless of whether the results examine teacher experience or value added. Thus, these
results suggest that the observed spillover effects on students' eighth-grade ELA test scores are not caused by placement into classrooms with more highly qualified ELA teachers.

### 6.3 Advanced Math Placement and Changes in Peer Composition

We next examine how placement into advanced math changes the composition of peers to which a student is exposed. Figure 6 provides a set of scatter plots for measures of peer composition, and Table 6 provides corresponding regression coefficient estimates. These plots show noticeable discontinuities at the threshold; these differences are especially pronounced for peers in the student's eighth-grade math classroom but are also present for peer composition measures indexing other courses. Table 6 presents these results more formally-we examine not just the peers in a student's eighth-grade (1) math and (2) ELA classes, but also (3) "common" peers who share three or more courses with the student.

We first look at composition of eighth-grade math and ELA classes. We see large increases in the mean seventh-grade math score of a student's eighth-grade math class of 0.55 standard deviations if students score above the threshold cutoff. The differences that we observe in eighth-grade ELA classes are substantially more modest: the mean seventh-grade ELA score within a student's eighth-grade ELA class increases by 0.04 standard deviations. Although these differences remain statistically significant, the magnitude of the point estimates suggests that the peer composition in ELA courses per se is unlikely to be driving the ELA test score effects we observe for students who are placed into algebra in eighth grade. Given the lack of differences in the characteristics of the ELA teachers that students are exposed to, the small significant differences in ELA peers suggests that the characteristics of the student's specific eighth-grade ELA course are unlikely to be the driving force for the improvement that we observe in eighth-grade ELA test scores.

Next, we look at the prior test scores of the group of peers that students share three or more classes with. When we look instead at peers who share three or more courses with a student (i.e., those who they are
sharing a substantial portion of their day with), we see significant and substantial increases in both mean seventh-grade math ( 0.199 standard deviations higher) and ELA ( 0.149 standard deviations higher). These results underscore the important role that math course assignments can play in shaping students' schedules and the peers with whom they take classes.

Although there is no estimate in the peer effects literature that corresponds to the causal effect of the average achievement of classmates with whom middle school students share three or more classes, existing peer effects estimates suggest that peer achievement has measurable effects on student achievement. Sacerdote (2011) provides a comprehensive review of peer effects estimates (for a succinct summary of these estimates, see Table 4.2 on p. 261-262). Some of the larger plausible estimates of peer effects in the literature are sufficiently large that peer effects could account for the entirety of the effects of eighth-grade algebra placement on student achievement that we observe. Our estimates reported above indicate that placement into eighth-grade algebra leads to a 0.149 standard deviation increase in the mean ELA achievement among peers who are in three or more of students' classes, and a 0.199 standard deviation increase in their mean math scores. If a standard deviation change in the mean achievement of peers who are in three or more middle school classes boosts student ELA achievement by 0.433 standard deviation, such a peer effect would, in and of itself, be sufficient to explain the causal link between eighth-grade algebra placement and ELA achievement that we identify $(0.149 * 0.433=0.065)$. A similar calculation indicates that a peer effect of 0.613 in math would be sufficient to account for the entirety of the math achievement effects that we observe $(0.199 * 0.613=0.122$.) While these peer effects are large relative to the magnitude of well-estimated peer effects in the existing literature, they are roughly in in line with Whitmore's (2005) estimates of peer effects (0.6) using the Tennessee STAR data.

Further, even more conservative estimates of peer effects are sufficiently large to suggest that peer effects could account for a substantial proportion of the effect of eighth-grade algebra placement on student achievement. For example, peer effects of .25 (in line with those estimated by Hoxby and Weingarth 2005) would be sufficient to explain over half of the effect we observe in ELA and $40 \%$ of the effect that
we observe in math achievement. Peer effects around .10 (in line with those estimated by Vigdor and Nechyba 2007) would explain $20 \%$ of the achievement effect that we observe in ELA and $15 \%$ of the achievement effect we observe in mathematics. Thus, although we are unable to formally test the extent to which peer exposure mediates the effects of eighth-grade algebra on student academic outcomes-and we acknowledge that available estimates of peer effects in education vary considerably with study design and context-we believe that our analyses provide evidence that eighth-grade algebra affects students' academic outcomes at least in part by changing the academic characteristics of the classroom peers to which they are exposed.

### 6.4 School-level Correlates of Eighth Grade Algebra Effects

In our final set of analyses, we take advantage of the fact that we have identified eighth-grade algebra placement policies in 49 different school-by-year contexts to consider the school-level factors that correlate with larger estimated eighth-grade algebra effects. In Figure 7, we present a series of scatterplots examining the correlates of school-by-year estimates. The y-axis on these scatterplots is the school-by-year-specific regression discontinuity estimate of the effects of eighth-grade algebra placement; the x -axis represents a series of school-by-year-specific observable factors including: two measures of the extent to which a school's eighth-grade algebra placement regime restricts the course to relatively high-performing students (the location of the placement cutoff and the proportion of eighth graders enrolled in advanced math), a measure of teacher value added in advanced math courses as compared to grade-level math courses, a measure of teacher experience in advanced math courses as compared to grade-level math courses, a measure of the difference between the average peer math achievement for students in eighthgrade algebra classes compared to students in less advanced math courses, and a measure of the difference between the average math achievement of peers in three more classes for students who take eighth-grade algebra compared to students in less advanced math courses. The results in Figure 7 show only a few clear correlates of the effect of advanced algebra on eighth-grade math scores. The most pronounced relationship is with the proportion of students in advanced algebra. This indicates that the
positive effects of eighth-grade algebra on math scores are more pronounced in schools with relatively few students taking algebra in eighth grade, so that the effects of eighth-grade algebra are less pronounced when it is widely available.

In Figure 8, we replicate these analyses to examine the correlates of school-specific estimates of the effects of eighth-grade algebra placement on ELA test scores. The scatterplots in the first row of this figure show no relationship between the restrictiveness of eighth-grade algebra placement and the course's effects on ELA test scores. The scatterplots in the second row suggest that eighth-grade algebra placements have somewhat larger effects on ELA achievement in schools where placement in the course is associated with exposure to more experienced and effective ELA teachers, although consistent with the analyses presented in Tables 5 and 6, these scatterplots indicate that most schools are clustered close to zero on these ELA teacher indicators. Notably, however, the scatterplot reported in the bottom right panel shows a clear positive relationship between the prior test score of the average peer that a student shares three or more classes with and the estimated ELA effect. Although these results are descriptive, they nonetheless again suggest that peer effects play an important in explaining the spillover effects of advanced math course-taking on ELA test scores.

## 7 Conclusion

Previous research on the effects of early algebra has found mixed evidence regarding its effects on student achievement, with studies focused on the effects of individual student placements generally returning positive effects that studies of large-scale curricular intensification efforts typically fail to replicate. Although prior research has posited that a large scale rollout was a fundamentally different treatment than the assignment of individual students to advanced courses, the mechanisms underlying these divergent results have not been well established. In this paper, we attempt to shed light on why large-scale curricular intensification efforts often fail to achieve the sorts of improvements in student outcomes that one might expect by investigating the effects of advanced algebra on the characteristics of the teachers
and the mean achievement of the classroom peers to which students are exposed. Using data from Oregon, we first conduct a search procedure to identify advanced algebra placement policies across all Oregon public schools. This allows us to use a regression discontinuity design to replicate previous analyses using similar designs, suggesting that eighth-grade algebra has positive effects on student achievement in math and ELA test scores as well as on student attendance. We then expand on the existing literature by examining a rich set of teacher and peer outcomes for students who are placed into eighth-grade algebra. We find that being placed into eighth-grade algebra has a significant effect on the experience and value added of students' eighth-grade math teacher, but not their ELA teachers. We thus conclude that while math teacher value added and experience may be an important mechanism through which eighth-grade algebra placements influence outcomes that are closely related to their math course experiences, these teacher characteristics are unlikely to explain the effects of eighth-grade algebra on student ELA achievement.

By contrast, we find significant changes in peer composition in math courses, ELA courses, and the peers students are exposed to in three or more classes. Although we find statistically significant differences across all of these different measures of peer composition, the magnitude of the effects is particularly large for mathematics course peers and peers who share three or more classes. We show that these peer composition effects are sufficiently large that they could plausibly account for a substantial portion of the mathematics and ELA achievement effects that we observe. Given that the peer composition effects we observe are likely to occur when individual students are placed into advanced courses, but not when policies shift the course-taking of an entire school, our peer composition findings provide important insights into why prior research examining broader curricular intensification efforts finds such markedly different results from prior research investigating the effects of eighth-grade algebra course placement at the student level.

In highlighting the substantial effects that being placed into eighth-grade algebra has on peer composition, and to a lesser extent, on teacher characteristics, our study has important implications for policy. At a
minimum, our findings suggest that policies aimed at boosting student outcomes through assigning students to more advanced coursework need to pay particular attention to the teachers in the classroom and the distribution of peers across classes. A less optimistic, and perhaps more realistic, reading of the implications of our findings and prior research is that we should not expect curricular intensification policies to operate at scale. Our findings indicate that when students are assigned to advanced courses, they gain access to more experienced and higher value-added teachers as well as higher-achieving peers. Since these educational resources are relatively fixed in a given school environment, our findings suggest that advanced courses assignments allow students to secure advantages at the expense of other students. As such, we believe that efforts to boost student achievement through advanced algebra placement policies and similar curricular efforts are unlikely to succeed because they fundamentally misunderstand an important causal mechanism.

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Figures
Figure 1: Location of Schools and Districts in Analysis Sample


Source: Geographic regions depict Oregon school district boundaries in 2017. Composite school district boundaries are from Institute of Education Sciences, National Center for Education Statistics. Shaded regions and designated schools are those identified for the full analysis sample from Oregon Department of Education administrative records.

Figure 2: Location of Discontinuities across Oregon Schools


Source: Oregon Department of Education administrative records. Distribution of standardized seventhgrade math test score cut points for the full analysis sample identified from the algorithm search and bootstrap test.

Figure 3: First-Stage Regression, Standardized Running Variable

Panel A - Full Sample


Panel B - Trimmed Sample


Source: Oregon Department of Education administrative records. Figures represent the pooled first-stage results for standardized seventh-grade math scores across academic years 2014-2017 for all students in the full sample and in the trimmed sample. Dots represent averages for a percentile of the running variable.

Figure 4: Reduced-Form Results for Student Academic Outcomes




Source: Oregon Department of Education administrative records. Figures represent the pooled reducedform results for standardized seventh-grade math scores across academic years 2014-2017 for all students in the trimmed sample. Dots represent averages for a percentile of the running variable.

Figure 5: Reduced-Form Estimates for Teacher Attributes





Source: Oregon Department of Education administrative records. Figures represent the reduced-form results for standardized seventh-grade math scores across academic years 2014-2017 against student- and teacher-related outcomes for the trimmed analysis sample. Dots represent averages for a percentile of the running variable.

Figure 6: Reduced-Form Results for Peer Composition Outcomes


Source: Oregon Department of Education administrative records. Figures represent the reduced-form results for standardized seventh-grade math scores across academic years 2014-2017 against peer-related outcomes for the trimmed analysis sample. Dots represent averages for a percentile of the running variable.

Figure 7: School-Level Correlates of Advanced Algebra Effects on Math Scores


Source: Oregon Department of Education administrative records. Each dot represents an estimated effect for a school*year combination, where the size of dots is weighted by the number of students in that school*year.

Figure 8: School-Level Correlates of Advanced Algebra Effects on ELA Scores


Source: Oregon Department of Education administrative records. Each dot represents an estimated effect for a school*year combination, where the size of dots is weighted by the number of students in that school*year.

Tables
Table 1: Descriptive Statistics

|  | All Students |  |  | Students in RD Sample |  |  | Trimmed RD Sample |  |  |
| :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | Mean | SD | N | Mean | SD | N | Mean | SD | N |
| Panel A: Student-Level Variables |  |  |  |  |  |  |  |  |  |
| $8^{\text {th }}$-Grade Advanced Math | 0.3128 | 0.4637 | 73,542 | 0.4072 | 0.4913 | 7,905 | 0.4144 | 0.4927 | 5,562 |
| $8^{\text {th }}$-Grade ELA | 0.0178 | 0.9614 | 70,434 | 0.2798 | 0.8023 | 7,508 | 0.2555 | 0.8055 | 5,232 |
| 8th-Grade Math | 0.018 | 0.9607 | 70,084 | 0.2884 | 0.7369 | 7,499 | 0.2776 | 0.7489 | 5,229 |
| Absences in $8^{\text {th }}$ Grade | 10.21 | 10.12 | 72,762 | 8.95 | 8.33 | 7,862 | 8.91 | 8.04 | 5,528 |
| $7^{\text {th }}$-Grade Math Score | 0.0616 | 0.9337 | 73,542 | 0.394 | 0.5682 | 7,905 | 0.3822 | 0.5693 | 5,562 |
| 7th-Grade ELA Score | 0.0106 | 0.9551 | 73,180 | 0.3001 | 0.7726 | 7,881 | 0.2854 | 0.7755 | 5,545 |
| Female Student | 0.5123 | 0.4999 | 73,542 | 0.5131 | 0.4999 | 7,905 | 0.5093 | 0.5 | 5,562 |
| Hispanic Student | 0.2411 | 0.4278 | 73,542 | 0.1767 | 0.3815 | 7,905 | 0.1819 | 0.3858 | 5,562 |
| White Student | 0.6236 | 0.4845 | 73,542 | 0.6963 | 0.4599 | 7,905 | 0.6996 | 0.4585 | 5,562 |
| Other Race Student | 0.1352 | 0.342 | 73,542 | 0.127 | 0.333 | 7,905 | 0.1185 | 0.3232 | 5,562 |
| Panel B: Teacher-Level Variables |  |  |  |  |  |  |  |  |  |
| $8^{\text {th }}$-Grade Math Teacher VAM | 0.1003 | 0.5091 | 68,417 | 0.0461 | 0.4714 | 7,634 | 0.0483 | 0.4542 | 5,306 |
| 8th-Grade ELA Teacher VAM | 0.3334 | 1.616 | 67,783 | 0.4707 | 1.4605 | 7,651 | 0.2789 | 1.189 | 5,356 |
| Experience of $8^{\text {th }}-$ Grade ELA Teacher | 11.63 | 8.27 | 70,595 | 12.43 | 8.49 | 7,223 | 12.8 | 8.83 | 5,271 |
| Experience of $8^{\text {th }}$ Grade Math Teacher | 12.36 | 8.61 | 73,208 | 12.62 | 8.51 | 7,885 | 12.97 | 8.61 | 5,553 |
| Panel C: Peer Composition Variables |  |  |  |  |  |  |  |  |  |
| Average $7^{\text {th }}$-Grade Math Score of $8^{\text {th }}-$ Grade Math Classroom | $0.0155$ | 0.7261 | 73,541 | 0.1998 | 0.6856 | 7,905 | 0.1904 | 0.6936 | 5,562 |
| Average $7^{\text {th }}$-Grade ELA Score of $8^{\text {th }}$ Grade ELA Classroom | $0.0159$ | 0.5449 | 71,033 | 0.1371 | 0.4829 | 7,230 | 0.1351 | 0.4727 | 5,278 |
| Average $7^{\text {th }}$-Grade Math Score of Common Students | 0.0301 | 0.606 | 70,331 | 0.1937 | 0.5662 | 7,579 | 0.1798 | 0.5569 | 5,398 |
| Average $7^{\text {th }}$-Grade ELA Score of Common Students | 0.0282 | 0.5657 | 70,328 | 0.1783 | 0.5317 | 7,578 | 0.1647 | 0.5201 | 5,397 |

Source: Oregon Department of Education administrative records. Descriptive statistics are provided for all students in $7^{\text {th }}$ grade from 2014-2017, those identified in the full analysis sample, and those identified in the trimmed analysis sample.

Table 2: RD Search Summary

| Panel A: Number of Schools |  |  |  |
| :--- | :--- | :--- | :--- |
|  | $\mathbf{2 0 1 4}$ | $\mathbf{2 0 1 5}$ | $\mathbf{2 0 1 6}$ |
| Schools with an $8^{\text {th }}$-grade math course and $7^{\text {th }}$-grade test score | 446 | 360 | 345 |
| Schools with $>50$ students per school-by-year | 187 | 163 | 171 |
| Schools with significant positive betas after algorithm search | 61 | 56 | 71 |
| Schools that pass bootstrap using 1 SD bandwidth | 21 | 23 | 24 |
| Schools that pass placebo check | 18 | 15 | 16 |

Source: Oregon Department of Education administrative records. Number of schools within each academic year at each stage of identifying the final analysis sample. Schools that pass bootstrap using 1sd bandwidth represent the full sample. Schools that pass placebo check represent the trimmed sample.

| Panel B: Number of Students |  |  |  |
| :--- | :--- | :--- | :--- |
|  | $\mathbf{2 0 1 4}$ | $\mathbf{2 0 1 5}$ | $\mathbf{2 0 1 6}$ |
| Students with an $8^{\text {th }}$-grade math course and $7^{\text {th }}$-grade test score | 33,021 | 25,538 | 26,388 |
| Students with $>50$ students per school-by-year | 29,009 | 21,589 | 22,944 |
| Students with significant positive betas after algorithm search | 9,640 | 7,776 | 9,910 |
| Students that pass bootstrap using 1 SD bandwidth | 2,539 | 2,649 | 2,717 |
| Students that pass placebo check | 2,028 | 1,606 | 1,928 |

Source: Oregon Department of Education administrative records. Number of students within each academic year at each stage of identifying the final analysis sample. Students that pass bootstrap using 1sd bandwidth represent the full sample. Students that pass placebo check represent the trimmed sample.

Table 3: Placebo Checks
Panel A: Full sample

|  | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 |
| :--- | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | First <br> Stage | Density <br> Test | $6^{\text {th }}$-Grade <br> Math <br> Score | $6^{\text {th }}$-Grade <br> ELA <br> Score | $7^{\text {th }}$-Grade <br> ELA <br> Score | Female <br> Student | White <br> Student | Hispanic <br> Student | Non-Native <br> English <br> Speaker |
| [SCORE $>=$ cut] $]$ | $0.485^{* * *}$ | 0.003 | $0.111^{* * *}$ | $0.071^{* *}$ | $0.065^{* * *}$ | 0.010 | -0.019 | $0.027^{*}$ | 0.007 |
|  | $(0.017)$ | $(0.067)$ | $(0.021)$ | $(0.029)$ | $(0.026)$ | $(0.022)$ | $(0.019)$ | $(0.015)$ | $(0.014)$ |
|  |  |  |  |  |  |  |  |  |  |
| Observations | 7,905 | 7,905 | 7,555 | 7,557 | 7,881 | 7,905 | 7,905 | 7,905 | 7,905 |
| N School*Year | 67 | 67 | 67 | 67 | 67 | 67 | 67 | 67 | 67 |

Panel B: Trimmed sample

|  | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 |
| :--- | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: | :---: |
|  | First <br> Stage | Density <br> Test | $6^{\text {th }}-$ Grade <br> Math <br> Score | $6^{\text {th }}-$ Grade <br> ELA <br> Score | $7^{\text {th }}$-Grade <br> ELA <br> Score | Female <br> Student | White <br> Student | Hispanic <br> Student | Non-Native <br> English <br> Speaker |
| $1[$ SCORE $>=$ cut] | $0.506^{* * *}$ | 0.040 | $0.070^{* * *}$ | 0.024 | 0.034 | 0.035 | -0.010 | 0.024 | -0.008 |
|  | $(0.020)$ | $(0.083)$ | $(0.024)$ | $(0.035)$ | $(0.032)$ | $(0.026)$ | $(0.023)$ | $(0.019)$ | $(0.016)$ |
| Observations | 5,562 | 5,562 | 5,310 | 5,313 | 5,545 | 5,562 | 5,562 | 5,562 | 5,562 |
| N School*Year | 49 | 49 | 49 | 49 | 49 | 49 | 49 | 49 | 49 |

Source: Oregon Department of Education administrative records. Point estimates come from specifications such as those shown in Equation (2). Model includes school-year fixed effects. Robust standard errors in parentheses. * indicates significance at the 0.10 level, ${ }^{* *}$ at the 0.05 level, and ${ }^{* * *}$ at the 0.01 level.

Table 4: Effects of Placement into Advanced Algebra on Student Outcomes, Trimmed Sample

|  | $8^{\text {th }}$-Grade ELA Score | $8^{\text {th }}$-Grade Math Score | $8^{\text {th }}$-Grade Absences (days) |
| :--- | :---: | :---: | :---: |
| $1[$ SCORE $>=$ cut] (1 SD) | $0.065^{*}$ | $0.122^{* * *}$ | $-0.967^{* *}$ |
|  | $(0.034)$ | $(0.024)$ | $(0.404)$ |
| Observations | 5,232 | 5,229 | 5,528 |

Source: Oregon Department of Education administrative records. Point estimates come from specifications such as those shown in Equation (2). Model includes school-year fixed effects. Robust standard errors in parentheses. * indicates significance at the 0.10 level, ${ }^{* *}$ at the 0.05 level, and $* * *$ at the 0.01 level.

Table 5: Effects of Placement into Advanced Algebra on Teacher Characteristics, Trimmed Sample

|  | $8^{\text {th }}$-Grade Math Tch VAM | $8^{\text {th }}$-Grade ELA <br> Teacher VAM | $8^{\text {th }}$-Grade ELA Teacher Experience (years) | $\begin{gathered} 8^{\text {th }} \text {-Grade Math } \\ \text { Teacher } \\ \text { Experience (years) } \end{gathered}$ |
| :---: | :---: | :---: | :---: | :---: |
| 1 [SCORE > ${ }^{\text {cut] }}$ ( 1 SD ) | $\begin{gathered} 0.090^{* * *} \\ (0.015) \end{gathered}$ | $\begin{gathered} -0.001 \\ (0.026) \end{gathered}$ | $\begin{gathered} -0.051 \\ (0.384) \\ \hline \end{gathered}$ | $1.226^{* * *}$ $(0.320)$ |
| Observations | 5,306 | 5,356 | 5,271 | 5,553 |

Source: Oregon Department of Education administrative records. Point estimates come from specifications such as those shown in Equation (2). Model includes school-year fixed effects. Robust standard errors in parentheses. * indicates significance at the 0.10 level, ${ }^{* *}$ at the 0.05 level, and ${ }^{* * *}$ at the 0.01 level.

Table 6: Effects of Placement into Advanced Algebra on Peer Characteristics, Trimmed Sample
$\left.\begin{array}{lccc}\hline & \begin{array}{c}\text { Mean } 7^{\text {th }} \text {-grade } \\ \text { Math Score of } 8^{\text {th }}- \\ \text { Grade Math Class }\end{array} & \begin{array}{c}\text { Mean 7 } 7^{\text {th }} \text {-Grade } \\ \text { ELA Score of } 8^{\text {th }}- \\ \text { Grade ELA class }\end{array} & \begin{array}{c}\text { Mean } 7^{\text {th }} \text {-grade Math } \\ \text { Score Any Students } \\ \text { in } 3 \text { or More of } 8^{\text {th }}- \\ \text { Grade Classes }\end{array}\end{array} \begin{array}{c}\text { Mean } 7^{\text {th }} \text {-grade ELA } \\ \text { Score Any Students in } \\ 3 \text { or More of } 8^{\text {th }} \text {-Grade } \\ \text { Classes }\end{array}\right]$

Source: Oregon Department of Education administrative records. Point estimates come from specifications such as those shown in Equation (2). Model includes school-year fixed effects. Robust standard errors in parentheses. * indicates significance at the 0.10 level, ${ }^{* *}$ at the 0.05 level, and ${ }^{* * *}$ at the 0.01 level.

## Appendices

## Appendix A: Supplementary Results

Figure A.1: Example Thresholds


Source: Oregon Department of Education administrative records. Each panel represents a single school in a given academic year and the distance of the seventh-grade standardized math score from the identified cut point. Dots represent average values for a given level of the seventh-grade test score.

Figure A.2: First-Stage Regression by Year, Unstandardized Running Variable.
Panel A: First-Stage Regression for 2014, Unstandardized Running Variable


Source: Oregon Department of Education administrative records. Figures represent the first-stage results for raw seventh-grade math scores in academic year 2014 for all students in the full sample.

Panel B: First-Stage Regression for 2015, Unstandardized Running Variable


Source: Oregon Department of Education administrative records. Figures represent the first-stage results for raw seventh-grade math scores in academic year 2015 for all students in the full sample.

Panel C: First-Stage Regression for 2016, Unstandardized Running Variable


Source: Oregon Department of Education administrative records. Figures represent the first-stage results for raw seventh-grade math scores in academic year 2016 for all students in the full sample.

## Appendix B: Results for Cutoffs at Proficiency Thresholds

Table B.1: Effects of Placement into Advanced Algebra on Student Outcomes
$\left.\begin{array}{lcccc}\hline & \begin{array}{c}\text { First Stage } \\ \text { (Placement into Adv } \\ \text { Math) }\end{array} & \begin{array}{c}8^{\text {th }} \text {-Grade ELA } \\ \text { Score }\end{array} & 8^{\text {th }} \text {-Grade Math } \\ \text { Score }\end{array} \quad \begin{array}{c}8^{\text {th }} \text {-Grade } \\ \text { Absences (days) }\end{array}\right]$

Source: Oregon Department of Education administrative records. Point estimates come from specifications such as those shown in Equation (2). Results are listed for schools identified with a cutoff in a range of $\pm 2$ raw points in 2014 and $\pm 15$ raw points in 2015-2016 from known proficiency levels established by the Oregon Department of Education. This is a subset of schools within the trimmed sample. Model includes school-year fixed effects. Robust standard errors in parentheses. * indicates significance at the 0.10 level, ${ }^{* *}$ at the 0.05 level, and ${ }^{* * *}$ at the 0.01 level.

Table B.2: Effects of Placement into Advanced Algebra on Teacher Characteristics

|  | $8^{\text {th }}$-Grade Math Teacher Experience (years) | $8^{\text {th }}$-Grade ELA <br> Teacher <br> Experience (years) | $8^{\text {th }}$-Grade Math <br> Teacher VAM | $8^{\text {th }}$-Grade ELA <br> Teacher VAM |
| :---: | :---: | :---: | :---: | :---: |
| 1[SCORE > = cut] (1 SD) | $\begin{gathered} \hline 1.354 * * * \\ (0.417) \end{gathered}$ | $\begin{gathered} 0.461 \\ (0.594) \end{gathered}$ | $\begin{gathered} \hline 0.123 * * * \\ (0.026) \end{gathered}$ | $\begin{gathered} 0.022 \\ (0.047) \end{gathered}$ |
| Observations | 2,743 | 2,461 | 2,560 | 2,615 |

Source: Oregon Department of Education administrative records. Point estimates come from specifications such as those shown in Equation (2). Results are listed for schools identified with a cutoff in a range of $\pm 2$ raw points in 2014 and $\pm 15$ raw points in 2015-2016 from known proficiency levels established by the Oregon Department of Education. This is a subset of schools within the trimmed sample. Model includes school-year fixed effects. Robust standard errors in parentheses. * indicates significance at the 0.10 level, ** at the 0.05 level, and $* * *$ at the 0.01 level.

Table B.3: Effects of Placement into Advanced Algebra on Peer Characteristics

|  | Mean $7^{\text {th }}$-Grade Math Score of $8^{\text {th }}$ Grade Math Class | Mean $7^{\text {th }}$-Grade ELA Score of $8^{\text {th }}$-Grade ELA Class | Mean $7^{\text {th }}$-Grade Math Score Any Students in 3 or More of $8^{\text {th }}$ Grade Classes | Mean $7^{\text {th }}$-Grade ELA Score Any Students in 3 or More of $8^{\text {th }}$-Grade Classes |
| :---: | :---: | :---: | :---: | :---: |
| $1[\mathrm{SCORE}>=\mathrm{cut}](1 \mathrm{SD})$ | $\begin{gathered} 0.588 * * * \\ (0.037) \end{gathered}$ | $\begin{gathered} 0.066 * * * \\ (0.024) \end{gathered}$ | $\begin{gathered} 0.235 * * * \\ (0.031) \end{gathered}$ | $\begin{gathered} \hline 0.170 * * * \\ (0.029) \end{gathered}$ |
| Observations | 2,744 | 2,462 | 2,650 | 2,649 |

Source: Oregon Department of Education administrative records. Point estimates come from specifications such as those shown in Equation (2). Results are listed for schools identified with a cutoff in a range of $\pm 2$ raw points in 2014 and $\pm 15$ raw points in 2015-2016 from known proficiency levels established by the Oregon Department of Education. This is a subset of schools within the trimmed sample. Model includes school-year fixed effects. Robust standard errors in parentheses. * indicates significance at the 0.10 level, ${ }^{* *}$ at the 0.05 level, and ${ }^{* * *}$ at the 0.01 level.

## Appendix C: Results for Full "Non-Trimmed" Sample, by Bandwidth

Table C.1: Effects of Placement into Advanced Algebra on Student-Related Outcomes

|  | $8^{\text {th }}$-Grade ELA Score | $8^{\text {th }}$-Grade Math Score | $8^{\text {th }}$-Grade Absences (days) |
| :---: | :---: | :---: | :---: |
| 1[SCORE >= cut] (1 SD) | 0.064** | 0.113*** | -0.834** |
|  | -0.028 | -0.02 | -0.339 |
| Observations | 7,508 | 7,499 | 7,862 |
| $1[$ SCORE $>=$ cut] $(0.5 \mathrm{SD})$ | 0.158*** | 0.156*** | -0.963** |
|  | -0.039 | -0.028 | -0.486 |
| Observations | 4,121 | 4,114 | 4,303 |
| 1[SCORE >= cut] (2 SD) | 0.100*** | 0.162*** | -0.629** |
|  | -0.023 | -0.016 | -0.27 |
| Observations | 10,272 | 10,255 | 10,727 |

Source: Oregon Department of Education administrative records. Point estimates come from specifications such as those shown in Equation (2). Results are listed for various bandwidths for the running variable for the full analysis sample. Model includes school-year fixed effects. Robust standard errors in parentheses. * indicates significance at the 0.10 level, ${ }^{* *}$ at the 0.05 level, and ${ }^{* * *}$ at the 0.01 level.

Table C.2: Effects of Placement into Advanced Algebra on Teacher-Related Outcomes

|  | $8^{\text {th }}$-Grade Math <br> Teacher <br> Experience <br> (years) | $8^{\text {th }}$-Grade ELA <br> Teacher <br> Experience <br> (years) | $8^{\text {th }}$-Grade Math <br> Teacher VAM | $8^{\text {th }}$-Grade <br> ELA Teacher <br> VAM |
| :--- | :---: | :---: | :---: | :---: |
| 1[SCORE $>=$ cut] (1 SD) | $0.880^{* *}$ | -0.12 | $0.088^{* * *}$ | 0.002 |
|  | -0.265 | -0.313 | -0.013 | -0.032 |
| Observations | 7,885 | 7,223 | 7,634 | 7,651 |
| [SCORE $>=$ cut] (0.5 SD) | $1.142^{* * *}$ | 0.136 | $0.090^{* * *}$ | -0.045 |
|  | -0.379 | -0.429 | -0.019 | -0.044 |
| Observations | 4,314 | 3,938 | 4,197 | 4,210 |
| [SCORE $>=$ cut] (2 SD) | $0.959^{* * *}$ | -0.133 | $0.104^{* * *}$ | 0.027 |
|  | -0.212 | -0.248 | -0.01 | -0.026 |
| Observations |  |  |  | 10,369 |

Source: Oregon Department of Education administrative records. Point estimates come from specifications such as those shown in Equation (2). Results are listed for various bandwidths for the running variable for the full analysis sample. Model includes school-year fixed effects. Robust standard errors in parentheses. * indicates significance at the 0.10 level, ${ }^{* *}$ at the 0.05 level, and ${ }^{* * *}$ at the 0.01 level.

Table C.3: Effects of Placement into Advanced Algebra on Peer-Related Outcomes

|  | Mean $7^{\text {th }}-$ <br> Grade Math <br> Score of $8^{\text {th }}$ - <br> Grade Math <br> Class | Mean $7^{\text {th }}-$ Grade ELA Score of $8^{\text {th }}-$ Grade ELA Class | Mean $7^{\text {th }}$ - <br> Grade Math Score Any Students in 3 or More of $8^{\text {th }}$ Grade Classes | Mean $7^{\text {th }}-$ <br> Grade ELA <br> Score Any <br> Students in 3 <br> or More of <br> 8th-Grade <br> Classes |
| :---: | :---: | :---: | :---: | :---: |
| 1[SCORE > = cut] (1 SD) | $\begin{gathered} \hline 0.519^{* * *} \\ -0.021 \end{gathered}$ | $\begin{gathered} \hline 0.057 * * * \\ -0.015 \end{gathered}$ | $\begin{gathered} \hline 0.190 * * * \\ -0.019 \end{gathered}$ | $\begin{gathered} \hline 0.143 * * * \\ -0.018 \end{gathered}$ |
| Observations | 7,905 | 7,230 | 7,579 | 7,578 |
| $1[$ SCORE $>=$ cut] ( 0.5 SD ) | $\begin{gathered} 0.532 * * * \\ -0.03 \end{gathered}$ | $\begin{gathered} \hline 0.062 * * * \\ -0.021 \end{gathered}$ | $\begin{gathered} \hline 0.197 * * * \\ -0.028 \end{gathered}$ | $\begin{gathered} \hline 0.143 * * * \\ -0.026 \end{gathered}$ |
| Observations | 4,329 | 3,943 | 4,149 | 4,149 |
| 1[SCORE >= cut] (2 SD) | $\begin{gathered} \hline 0.586 * * * \\ -0.016 \end{gathered}$ | $\begin{gathered} \hline 0.077 * * * \\ -0.013 \end{gathered}$ | $\begin{gathered} \hline 0.223 * * * \\ -0.015 \end{gathered}$ | $\begin{gathered} \hline 0.168 * * * \\ -0.014 \end{gathered}$ |
| Observations | 10,805 | 9,923 | 10,356 | 10,354 |

Source: Oregon Department of Education administrative records. Point estimates come from specifications such as those shown in Equation (2). Results are listed for various bandwidths for the running variable for the full analysis sample. Model includes school-year fixed effects. Robust standard errors in parentheses. * indicates significance at the 0.10 level, ${ }^{* *}$ at the 0.05 level, and ${ }^{* * *}$ at the 0.01 level.


[^0]:    ${ }^{1}$ Although it is, of course, possible that having a more experienced and effective mathematics teacher affects students' ELA test scores, this seems unlikely to account for the magnitude of effects that we observe.

[^1]:    ${ }^{2}$ Of particular relevance for the current context, positional goods (e.g., goods that have value because they are scarce) will have different effects when provided to a small number of students then when provided at scale. Although this can be conceptualized as a violation of the stable unit treatment value assumption (SUTVA) for causal inference with regards to estimating a population-level effect (i.e., the value derived from receiving a positional good is expressly defined by whether others do or do not receive it), we argue that it is more fruitful to conceptualize individual and population level effects as separate and that it can be illuminating to understand the reasons for differences between these two effects.

[^2]:    ${ }^{3}$ One important implication of the restriction to students who have a seventh-grade test score in the prior year in the state of Oregon is that it excludes many students who were in special education in their seventh-grade year, since these students often do not take the end-of-grade assessments. We do not condition on whether students have switched schools.

[^3]:    ${ }^{4}$ In some school*year combinations, the bootstrap procedure does not fully run all 1000 replications due to imbalanced data, such as a school having all students in advanced math. We remove these school*year combinations that run fewer than 950 replications. We also restrict to school*year combinations that produce a positive beta from the bootstrap procedure.

[^4]:    ${ }^{5}$ For examples of thresholds identified by this search process, see Appendix Figure A.1.

[^5]:    ${ }^{6}$ For plots of the first-stage relationship between advanced algebra course-taking and test score cutoffs, see Appendix Figure A.2.

