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Intensive Math Instruction and Educational Attainment: Long-Run Impacts of Double-Dose Algebra

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Kalena Cortes
Texas A&M University

Joshua Goodman
Harvard Kennedy School

Takako Nomi
St. Louis University

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Kalena Cortes
Texas A&M University
kcortes@bushschool.tamu.edu

Joshua Goodman†
Harvard University
joshua_goodman@hks.harvard.edu

Takako Nomi
St. Louis University
tnomi@slu.edu

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Abstract

Success or failure in freshman math has long been thought to have a strong impact on subsequent high school outcomes. We study an intensive math instruction policy in which students scoring below average on an 8th grade exam were assigned in 9th grade to an algebra course that doubled instructional time, altered peer composition and emphasized problem solving skills. Using a regression discontinuity design, we show positive and substantial long-run impacts of double-dose algebra on standardized test scores, high school graduation rates and college enrollment rates. The attainment effects were larger than the test score effects would predict, highlighting the importance of evaluating educational interventions on longer-run outcomes. Perhaps because the intervention focused on verbal exposition of mathematical concepts, the intervention's impact was generated largely by students with below average reading skills, highlighting the importance of targeting interventions towards appropriately skilled students. This is the first evidence we know of demonstrating the long-run impacts of such intensive math instruction.

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†Corresponding author.

1. Introduction

The high school graduation rate for American students has declined since the 1970s to about 75 percent, with black and Hispanic graduation rates hovering around 65 percent (Heckman and LaFontaine 2010). Poor academic preparation of students entering high school is often cited as a major source of such high dropout rates. Results from the 2011 National Assessment of Educational Progress suggest that only 35 percent of students enter high school with math skills considered proficient. Black and Hispanic students' proficiency rates are an even lower 13 and 20 percent respectively.[1] These low academic skills may explain observed high failure rates in 9th grade coursework, particularly in algebra (Herlihy 2007, Horwitz and Snipes 2008).

Such high failure rates are particularly worrying because of their close association with dropout rates in later grades. Early course failures prevent students from progressing to more advanced coursework and from earning the credits needed to graduate (Allensworth and Easton 2007). In the Chicago Public Schools (CPS), the focus of this study, roughly half of high school freshmen fail at least one course, with the highest failure rates in math courses (Allensworth and Easton 2005). Concern about this fact and the apparent failure of remediating students before entering high school led CPS to implement a double-dose algebra policy starting with students entering high school in the fall of 2003. Under this policy, students scoring below the national median on an 8th grade math test were subsequently assigned to two periods of freshman algebra rather than the usual one period. CPS hoped that this doubling of instructional time, along with an increased emphasis on problem solving skills and increased instructional support for teachers, would improve algebra passing rates in the short-run and high school graduation rates in the long-run.

[1] Similarly large skill gaps by income are also apparent. Students poor enough to qualify for free lunch under the National School Lunch Program have a proficiency rate of 17 percent, compared to a 47 percent proficiency rate among students who do not qualify for such subsidies. See "The Nations Report Card: Mathematics 2011" published by the National Center for Education Statistics.

To analyze the effect of the double-dose policy, we employ a regression discontinuity design comparing students just above and below the threshold for assignment to additional instructional time. Using longitudinal data that tracks students from 8th grade through college, we show positive and substantial long-run impacts of double-dose algebra on standardized exam scores, high school graduation rates and college enrollment rates. The attainment effects were larger than the test score effects would predict, highlighting the importance of evaluating educational interventions on longer-run outcomes. Perhaps because the intervention focused on verbal exposition of mathematical concepts, the intervention's impact was generated largely by students with below average reading skills, highlighting the importance of targeting interventions towards appropriately skilled students. This is the first evidence we know of demonstrating long-run impacts of such intensive math instruction.

Our work contributes to three strands of the research literature. First, given that the intervention studied here doubled the amount of time students were exposed to 9th grade algebra, our study adds to the literature on the importance of instructional time to student achievement. Some education reformers have pushed U.S. schools to lengthen school days and years, noting that students in many academically successful nations, particularly in Asia, spend substantially more time in school than do American students. Proponents of this view point to evidence on summer learning loss (Cooper et al. 1996), the impact of snow days (Marcotte and Hemelt 2008), the association between charter school effectiveness and instructional time (Dobbie et al. 2011, Hoxby and Murarka 2009), and other such patterns linking student achievement to hours spent learning (Lavy 2010, Fitzpatrick et al. 2011). Another set of studies suggests this evidence is weaker than it first appears, with Fryer Jr. and Levitt (2004) observing little differential summer learning loss, Goodman (2012a) showing little impact of snow days on achievement, Angrist et al. (2011) showing little relation between instructional time and charter school effectiveness, and Checkoway et al. (2011) showing little effect of an intervention that substantially increased schools' instructional times. The emerging consensus from this literature is that increasing instructional time is no guarantee of better student outcomes if such time is not well spent. Our results are consistent with heterogeneous impacts of increased instructional time by math and reading skills.

Second, our work adds to the literature concerning the short-run impact of curricular interventions, particularly for students struggling in mathematics. Recent years have seen three main curriculum approaches tried by American schools. Remediation, which diverts students into basic courses prior to taking regular courses, has generally had little discernible impact on student achievement, particularly at the college level where it has most often been studied (Jacob and Lefgren 2004, Lavy and Schlosser 2005, Calcagno and Long 2008, Bettinger and Long 2009, Martorell and McFarlin Jr 2011, Boatman and Long 2010, Scott-Clayton and Rodriguez 2012). Algebra "for all", which pushes students to take algebra courses in earlier grades than they otherwise would have, actually harms student achievement by forcing students into subjects for which they are not sufficiently prepared (Clotfelter et al. 2012, Allensworth, Nomi, Montgomery, and Lee 2009). Double-dosing, which places students in regular courses but supplements those courses with additional instructional time, has generated short-run gains in some settings and no gains in others (Nomi and Allensworth 2009, Nomi and Allensworth 2010, Roland G. Fryer 2011, Taylor 2012, Dougherty 2012). Perhaps because of perceived effectiveness at raising short-run achievement levels, the double-dose strategy has become increasingly common, with half of large urban districts reporting it as their most common form of support for struggling students.[2]

Third, and perhaps most important, we contribute to the literature on the long-run impacts of curriculum on student outcomes. Nearly all such research points to a close association between coursework completed in high school and later outcomes

[2] See "Urban Indicator: High School Reform Survey, School Year 2006-2007", by the Council of Great City Schools, 2009.

such as college enrollment and labor market earnings (Altonji 1995, Levine and Zimmerman 1995, Rose and Betts 2004, Attewell and Domina 2008, Long et al. 2009, Long et al. 2012). Most such papers attempt to deal with the bias generated by selection into coursework by controlling for a rich set of covariates, either through OLS or propensity score matching. However, such methods leave open the possibility that the remaining unobservables are still important factors. The few papers that use quasi-experimental methods to convincingly eliminate such selection bias also, however, find strong associations between completed coursework and long-run outcomes, suggesting that such selection bias is not generating the central findings (Joensen and Nielsen 2009, Goodman 2012b). This paper is one of the better identified links between high school coursework and educational attainment.

The structure of our paper is as follows. In section 2, we describe in detail the double-dose algebra policy. In section 3, we describe the data and offer descriptive statistics about students in our sample. In section 4, we explain the regression discontinuity underlying our identification strategy. In sections 5 and 6, we describe the impact of double-dosing on students' educational experiences, grades, test scores and educational attainment. In section 7, we discuss robustness, heterogeneity and spillovers into other subjects. In section 8, we conclude. We now turn to a description of the double-dose algebra policy itself.

2. Implementing Double-Dose Algebra

Since the late 1990s, Chicago Public Schools (CPS) have been at the forefront of curriculum reform designed to increase the rigor of student coursework and prepare students for college entrance. Starting with students entering high school in the fall of 1997, CPS raised its graduation requirements to align with the New Basics Curriculum.[3]

[3] The new basics curriculum was a minimum curriculum recommended by the National Commission of Excellence in Education in 1983, which consists of four years of English, three years of each mathematics, science, and social studies,

CPS eliminated lower-level and remedial courses so that all first-time freshmen would enroll in algebra in 9th grade, geometry in 10th grade and algebra II or trigonometry in 11th grade. Soon after these reforms, CPS officials realized that students were unable to master the new college-prep curriculum. Passing rates in 9th grade algebra were quite low, largely because students entered high school with such poor math skills (Roderick and Camburn 1999).

In response to these low passing rates in 9th grade algebra, CPS launched the double-dose algebra policy for students entering high school in the fall of 2003. Instead of reinstating the traditional remedial courses from previous years, CPS required enrollment in two periods of algebra coursework for all first-time 9th graders testing below the national median on the math portion of the 8th grade Iowa Tests of Basic Skills (ITBS).[4] Such students enrolled for two math credits, a full-year regular algebra class plus a full-year algebra support class.[5] Our analysis focuses on the first two cohorts of students because the test score-based assignment rule was not followed closely after the second year. We will refer to these as the 2003 and 2004 cohorts.

Prior to the double-dose policy, algebra curricula had varied considerably across CPS high schools due to the fairly decentralized nature of the district. Conversely, CPS offered teachers of double-dose algebra two specific curricula called Agile Mind and Cognitive Tutor, stand-alone lesson plans they could use, and thrice annual professional development workshops where teachers were given suggestions about

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and one-half year of computer science. The CPS requirements are actually slightly higher than the New Basics Curriculum, which includes two years of a foreign language and specific courses in mathematics (i.e., algebra, geometry, advanced algebra, and trigonometry).

[4] All CPS high schools were subject to the double-dose algebra policy, including 60 neighborhood schools, 11 magnet schools, and 6 vocational schools (Nomi and Allensworth 2009).

[5] Double-dose algebra students received 90 minutes of math class time every day for a full academic year. The first math course, regular algebra, consisted mostly of class lectures. The second math course, algebra with support or algebra problem solving, focused on building math skills that students lacked. Extended instructional time allowed flexibility in instructional activities for double-dose teachers. For example, the teachers covered materials in a different order than the textbook and used various instructional activities, such as working in small groups, asking probing and open-ended questions, and using board work (Wenzel et al. 2005, Starkel and Price 2006).

how to use the extra instructional time.[6] Though it is difficult to know precisely what occurred in these extra classes, Nomi and Allensworth (2012) analyzed survey data to learn more about the classroom learning environment. They found that students assigned to double-dose algebra reported much more frequently: writing sentences to explain how they solved a math problem; explaining how they solved a problem to the class; writing math problems for other students to solve; discussing possible solutions with other students; and applying math to situations in life outside of school. The additional time thus focused on building verbal and analytical skills may have conferred benefits in subjects other than math.

In order to provide coherent instruction to students, CPS also strongly advised schools to schedule their algebra support courses in three specific ways. First, double-dose algebra students should have the same teacher for their two periods of algebra. Second, the two algebra periods should be offered consecutively. Third, double-dose students should take their algebra support class with the same students who are in their regular algebra class. Most CPS schools followed these recommendations in the initial year (Nomi and Allensworth 2009). For the 2003 cohort, 80 percent of double-dose students had the same teacher for both courses, 72 percent took the two courses consecutively, and rates of overlap between the two classes' rosters exceeded 90 percent. By 2004, schools began to object to the scheduling difficulties of assigning the same teacher to both periods so CPS removed that recommendation. For the 2004 cohort, only 54 percent of double-dose students had the same teacher for both courses

[6] The district made the new double-dose curricula and professional development available only to teachers teaching double-dose algebra courses, but there was a possibility of spillover effects for teachers in regular algebra. However, the professional development was geared towards helping teachers structure two periods of algebra instruction. Moreover, based on CPS officials and staff members' observations of double-dose classrooms, they found that even teachers who taught both single-period and double-dose algebra tended to differentiate their instruction between the two types of classes. Specifically, teachers tended to use new practices with the double-period class, but continued to use traditional methods with the single-period class. Teachers said that they did not feel they needed to change methods with the advanced students (i.e., non double-dose students), and that they were hesitant to try new practices that may be more time-consuming with just a single period. The double period of algebra allowed these teachers to feel like they had the time to try new practices (e.g., cooperative groups).

and only 48 percent took the two courses consecutively. Overlap between the rosters remained, however, close to 90 percent. Near the end of our analysis, we also explore whether the program's impacts vary by cohort in part because of this variation in implementation.

The treatment under consideration here thus had multiple components. Assignment to double-dose algebra doubled the amount of instructional time and exposed students to the curricula and activities discussed above. As we will show, the recommendation that students take the two classes with the same set of peers caused tracking by skill to increase, thus reducing classroom heterogeneity. All of these factors were likely to, if anything, improve student outcomes (Duflo et al. 2011). We will also show, however, that the increased tracking by skill placed double-dosed students among substantially lower skilled peers than non-double-dosed students. This factor is likely to, if anything, hurt student outcomes. Our estimates will, therefore, capture the net impact of all of these components.

3. Data and Descriptive Statistics

We use longitudinal data from CPS that tracks students from 8th grade through college enrollment. These data include demographic information, detailed high school transcripts, numerous standardized test scores, and graduation and college enrollment information. Our main sample consists of students entering 9th grade for the first time in the fall of 2003 and 2004. We include only students who have valid 8th grade math scores and who enroll in freshman algebra. We include only high schools in which at least one classroom of students was assigned to double-dose algebra. For binary outcomes, students who leave the CPS school system for any reason are coded as zeroes. CPS attempts to track students' reasons for leaving. In our sample, students who leave CPS are about evenly divided between those who are known dropouts, those who leave for other schools (private schools or public schools outside of Chicago), and those whose reasons for leaving are unknown.

The summary statistics of the analytic sample are shown in Table 1. Column (1) includes the entire sample and column (2) includes only students within 10 percentiles of the double-dose threshold, our main analytic sample. Columns (3) and (4) separate that sample by cohort. As seen in panel (A), about 90 percent of CPS students are black or Hispanic, with 20 percent in special education. Because more than 90 percent of CPS students are low income as indicated by participation in the federal subsidized lunch program, we use more informative socioeconomic and poverty measures constructed for each student's residential block group from the 2000 Census and standardized within the full sample.[7] We also observe each student's 8th grade reading percentile.

The first row of panel (B) shows our instrument, each student's 8th grade score on the math portion of the Iowa Test of Basic Skills (ITBS), which all CPS 8th graders are required to take. The mean CPS 8th grade student scores are between the 45th and 46th percentiles on this nationally normed exam. As shown in column (1), 55 percent of CPS students score below the 50th percentile and thus should be assigned to double-dose algebra, though the transcript data reveal that only 44 percent are actually enrolled in this class, suggesting imperfect compliance with the rule. As a result, the average CPS freshman in our sample takes 1.4 math courses freshman year.

The transcript data also allow for detailed exploration of the treatment itself. We construct variables, shown in panel (B), showing the extent to which schools were complying with CPS' guidelines for implementing double-dose algebra. The average student attended a school in which 62 percent of double-dosed students had their two

[7] Our data are linked to the 2000 U.S. Census at the block level corresponding with each student's home address. Indicators of students' socioeconomic status and concentration of poverty were derived from the census data about the economic conditions in students' residential block groups. More specifically, the socioeconomic status variable is based on the percentage of employed persons 16 years (or older) who are managers and executives and the mean level of education among people over 18. The concentration of poverty variable is based on the percentage of males over 18 who are employed one or more weeks during the year and the percentage of families above the poverty line.

algebra courses during consecutive periods, in which 66 percent of double-dosed students had the same teacher for both courses, and in which 92 percent of double-dosed students' regular algebra classmates were themselves double-dosed. Consistent with schools' complaints about the difficulty of scheduling double-dose algebra for consecutive periods and with the same teacher, columns (3) and (4) show that compliance with those guidelines was substantially lower in 2004 than in 2003.

We focus on two primary sets of outcomes. First, in panel (C), we explore whether double-dosing helps student's academic achievement by constructing a variety of variables measuring grades, coursework and standardized test scores. The grades and coursework variables reveal that only 62 percent of the full sample pass algebra (i.e., receiving a D or higher), while even fewer pass higher level courses such as geometry and trigonometry. We also use a variety of test scores standardized by cohort to measure students' mathematical knowledge, including the PLAN exam, which all CPS students take in September of both their second and third years in high school, and the ACT exam, which all CPS students take in April of their third year and is commonly used in the Midwest for college applications.

Second, in panel (D), we explore whether double-dosing improves educational attainment by constructing measures of high school graduation and college enrollment. Students are coded as high school graduates if they received a regular CPS diploma within four or five years of starting high school. About 50 percent of CPS students in our sample graduate high school within four years, with another 5 percent graduating in their fifth year. CPS has matched its data on high school graduates with the National Student Clearinghouse (NSC) data on college enrollment, allowing us to observe initial college enrollment for any CPS student with a high school diploma.[8] We construct

[8] The NCS collects information on students' postsecondary education, which includes semester-by-semester college enrollment and attainment. NCS covers 91 percent of colleges (more than 2,800 postsecondary institutions) in the United States. CPS graduates mostly enroll in local colleges that participate in NCS. We therefore have excellent college enrollment records for about 95 percent of CPS graduates.

indicators for enrollment in college by October 1 of the fifth year after starting high school. Only 29 percent of the full sample both graduate from a CPS high school and enroll in college within this time frame, more than half of whom enroll in two-year colleges. We cannot explore college completion rates because students in our sample have not yet had sufficient time to graduate from four-year colleges and because NSC has poor graduation data from two-year colleges in this sample.

4. Empirical Strategy

Comparison of the outcomes of students who are and are not assigned to double-dose algebra would likely yield biased estimates of the policy's impacts given potentially large differences in unobserved characteristics between the two groups of students. To eliminate this potential bias, we exploit the fact that students scoring below the 50th percentile on the 8th grade ITBS math test were required to enroll in double-dose algebra. This rule allows us to identify the impact of double-dose algebra using a regression discontinuity design applied to the two treated cohorts. We use the assignment rule as an exogenous source of variation in the probability that a given student will be double-dosed.

We implement the regression discontinuity approach using the regressions below:

$$Y_{it} = \alpha_0 + \alpha_1 \cdot lowscore_{it} + \alpha_2 \cdot math8_{it} + \alpha_3 \cdot lowscore_{it} \cdot math8_{it} + \varepsilon_{it} \quad (1)$$

$$DoubleDose_{it} = \gamma_0 + \gamma_1 \cdot lowscore_{it} + \gamma_2 \cdot math8_{it} + \gamma_3 \cdot lowscore_{it} \cdot math8_{it} + \eta_{it} \quad (2)$$

$$Y_{it} = \beta_0 + \beta_1 \cdot DoubleDose_{it} + \beta_2 \cdot math8_{it} + \beta_3 \cdot lowscore_{it} \cdot math8_{it} + \mu_{it} \quad (3)$$

where for student *i* in cohort *t*, *lowscore* indicates an 8th grade math score below the 50th percentile, *math8* is each student's 8th grade math score re-centered around the 50th percentile cutoff, *DoubleDose* is an indicator for assignment to the extra algebra period, and Y represents an outcome of interest. The *lowscore* coefficient (α_1) from equation (1) estimates the discontinuity of interest by comparing the outcomes of students just below and just above the double-dose threshold. This reduced form equation produces an intent-to-treat (ITT) estimate because of imperfect compliance with the assignment rule. The *lowscore* coefficient (γ_1) from the first stage equation (2) measures the difference in double-dose rates between students just below and just above the threshold.

We focus our subsequent discussion on estimates of the *DoubleDose* coefficient (β_1) from equation (3), in which *DoubleDose* has been instrumented with *lowscore*. This approach estimates a treatment-on-the-treated effect (TOT), namely the impact of double-dose algebra on those students treated as a result of the assignment rule. The validity of these TOT estimates depends in part on the assumption that assignment to the treatment or control group affects only compliers, those whose participation is affected by the assignment rule. This assumption would be violated if, for example, the signal of a low 8th grade math score had stigmatizing or other effects on never-takers, those who would not enroll in double-dose algebra regardless of the assignment rule. We do not, however, think this is a substantial concern in this context.

Our preferred specification will use local linear regression weighted by an edge kernel. We use the procedure detailed by Imbens and Kalyanaraman (2012) to compute the optimal bandwidth to use for the edge kernel. Unfortunately, this procedure generally suggests bandwidths on the order of 2 percentiles, too narrow to generate estimates. As a result, we show results for bandwidths of 3, 6 and 10 percentiles and show that our central results are robust to the choice of bandwidth. We cluster standard errors by 8th grade math score to account for the coarse nature of the forcing variable (Lee and Card, 2008).

Before moving onto our main analysis, we perform two checks of the validity of the regressions discontinuity strategy. First, as suggested by McCrary (2008), we check that the density of 8th grade math scores is smooth around the cutoff. Figure 1 provides clear visual evidence of this smoothness, suggesting little scope for manipulation of such scores by students or teachers and little impact of the threshold on attrition from the sample prior to high school. Second, in Table 2 we check that student covariates vary smoothly around the threshold. There is no clear evidence of discontinuity in the distribution of covariates concerning race and ethnicity, special education status, Census block poverty and socioeconomics measures, or date of birth. Gender does, however, vary discontinuously at the cutoff, with those just below the cutoff 4-6 percentage points less likely to be female than those just above the cutoff. Because female students have higher reading scores than male students, this also results in a roughly 1 percentile discontinuity in 8th grade readings scores. We find no evidence that this results from differential selection by gender into 9th grade attendance and believe that these discontinuities are unrelated to double-dose algebra or other CPS policies. We show that our central results are robust to inclusion of such covariates, which have relatively little relation to the outcomes of interest once we have controlled for 8th grade math scores.

5. The Treatment

We first explore the treatment itself to learn more about how the double-dose algebra policy changed students' freshman year experiences. Before turning to regression results, we look at visual evidence. The darkest dots in panel (A) of Figure 2 plot the proportion of students double-dosed for each 8th grade math percentile, showing a large but fuzzy discontinuity. If compliance with the double-dose rule were perfect, we would expect to see 100% double-dose enrollment rates to the left of the

50th percentile threshold. Instead, panel (A) shows imperfect compliance, with assignment rates reaching a maximum of about 80 percent for students in the 20-40th percentiles. Students in the lowest percentiles have lower double-dose rates because they are more likely to be supported through other, special education programs.[9] Some students above the threshold are double-dosed, perhaps because teachers thought they would benefit from the course or because schools cannot perfectly divide students into appropriately sized classes by the assignment rule. Separate plots for each treated cohort reveal that compliance with the assignment rule was higher in 2003 than in 2004, particularly to the left of the threshold. For this reason, we later explore whether our TOT estimates vary by cohort. Panel (B) shows the most apparent consequence of the assignment rule, namely that double-dose eligible students are assigned to substantially more instructional time in freshman algebra.

Table 3 shows the first-stage results corresponding to panel (A) of Figure 2. Here, panels (A)-(C) use bandwidths of 3, 6 and 10 and no controls, while panel (D) controls for a wide variety of covariates. The estimates in column (1), which includes both treated cohorts, suggest that students just below the eligibility threshold were about 39 percentage points more likely to be double-dosed than students just above the threshold. Consistent with panel (A) of Figure 2, columns (2) and (3) show that this discontinuity was much larger in 2003 (51 percentage points) than in 2004 (27 percentage points). These estimates are highly statistically significant, with an F-test of the excluded instrument yielding values upwards of 200 in the full sample. Inclusion of controls has nearly no impact on these first-stage coefficients.

Table 4 explores the impact of assignment to double-dose algebra on the freshman course enrollment pattern. All of the coefficients come from regressions in which assignment to double-dose algebra has been instrumented by eligibility. As such,

[9] Our central results are unchanged if special education students are excluded from the regression discontinuity analysis, in part because such students tend to be far below the eligibility threshold.

these are treatment-on-the-treated (TOT) estimates of the impact of double-dosing on those actually double-dosed. Column (1) shows that being double-dosed increased the number of yearlong freshman math courses taken by nearly one, as would be expected from the double-dose strategy.[10] The policy thus doubled instructional time in math. Adding an additional math course barely increased, however, the total number of courses students took during their freshman year. Columns (2)-(4) show that the total number of yearlong courses taken during 9th grade increased by only 0.14 because the additional math course came at the expense of a small number of core academic courses and much larger number of elective courses such as fine arts.

Columns (5) and (6) highlight channels other than instructional time by measuring characteristics of students' regular (i.e., not support) algebra courses, the reduced form of which is shown in Figure 3. The increased skill tracking implied by CPS' guidelines meant that double-dosed students took algebra classes with peers whose 8th grade math scores were substantially lower than the peers of non-double-dosed students. The estimates in column (5) imply that double-dosing lowered the mean peer skill of double-dosed students by over 19 percentiles. Column (6) suggests that double-dosed students near the threshold were, however, in more homogeneous classrooms than their non-double-dosed peers, with the standard deviation of math skill roughly 3 percentiles lower. The double-dose policy thus doubled instructional time in math by replacing other coursework, increased homogeneity of algebra classrooms and lowered peer skill levels. None of these aspects of the treatment varied substantially by cohort. We now turn to analysis of the overall impact of these various channels on coursework, test scores and educational attainment.

[10] These coefficients would have a value exactly equal to one if not for students who drop out of high school before completing the second semester of double-dose algebra.

6. Grades, Test Scores and Educational Attainment

A visual preview of the reduced form (ITT) version of our results is available in Figures 4, 5, 6 and 7. Using a bandwidth of 10 percentiles on either side of the threshold, each of these figures plots the mean of the given outcome by 8th grade math score, as well as fitted straight lines predicted by our specifications using a bandwidth of 10 and no controls.[11] Figure 5 suggests a substantial but noisy impact of double-dosing on algebra passing rates but a very clear impact on the fraction of students receiving at least a B. Figure 6 suggests little impact of double-dosing on math scores the fall after freshman year ended, but a clear impact on such scores a full year after that. Figures 7 and 8 suggest impacts on both high school graduation and college enrollment rates, particularly with respect to two-year colleges. We now turn to regression analysis to measure more precisely the magnitudes and statistical significance of these discontinuities.

Table 5 explores the impact of the double-dose policy on math course passing rates and grades. Double-dosing increased the proportion of students earning at least a B in freshman algebra by roughly 12 percentage points, a near doubling from a base of 14 percentage points. Passing rates for freshman algebra increased by 5-7 percentage points, though the magnitude and statistical significance of that increase are somewhat dependent on the chosen specification.[12] It is unfortunately impossible to know whether these higher freshman grades reflect actual performance in class or the fact that teachers tend to grade on a curve, so that students just below the eligibility threshold compared much more favorably to their classmates than those just above that threshold.

Double-dosed students were no more likely to pass geometry but were, however, substantially more likely to pass trigonometry, a course typically taken in the third year

[11] Corresponding to panels (C) in the tables that follow.
[12] Some critics of double-dose algebra feared that increased instructional time could backfire by discouraging students from attending school at all. We see no evidence of differential attendance rates at the threshold, suggesting this concern was ultimately unfounded.

of high school. As a result, double-dosed students earned about 0.2 more math credits by the end of high school, not including the extra credit from the double-dose class itself. As a whole, these results imply that the double-dose policy greatly improved freshman algebra grades for the upper end of the double-dosed distribution but had a less clear impact on passing rates. There is, however, some evidence of improved passing rates and credits earned in later math courses, suggesting the possibility of longer-run benefits beyond freshman year. Though coursework and grades matter for students' academic trajectories, the subjective nature of course grading motivates us to turn to standardized achievement measures as an alternative measure of the impact of double-dosing on math skill.

Table 6 explores the impact of double-dosing on mathematics test scores as measured by the PLAN exams taken in October of a student's second and third years and the ACT exam taken in April of each student's third year, all of which test a variety of algebra and geometry concepts. Column (1) suggests unclear impacts on the first PLAN exam. Panel (D) suggests a statistically significant impact of 0.09 standard deviations but the estimate is quite sensitive to specification. It is worth noting that, consistent with prior evidence (Nomi and Allensworth, 2009), the first cohort saw gains on this exam that are at least marginally significant in most specifications. Much clearer is double-dosing's impact on later test scores. Double-dosing raises overall math scores in the fall of 11th grade by about 0.2 standard deviations, an effect size that remains statistically significant but drops by a third to a half by the time students take the ACT in the spring of 11th grade.

One potential concern is that differential selection into test-taking might be driving these effects if, for example, assignment to double-dose increase dropout rates among low-scoring students. We check this by replicating these regressions but using as outcomes indicators for having valid test scores. We see little systematic evidence of differential test-taking rates across the threshold. Nonetheless, to account for potential selection, we impute to students missing a given test score a value of -0.25 standard deviations. This corresponds to the 40th percentile of the skill distribution, the lowest skill level in any of the specifications we use. Columns (4)-(6) show results including those imputed values in the regressions. The magnitude of the impacts are slightly diminished but the overall story here is unchanged, suggesting that selection is not responsible for the positive test score impacts observed. Table 6 thus suggests that double-dosed students experienced achievement gains that persisted at least two years after the end of double-dose classes.

Table 7 explores the impact of double-dosing on educational attainment. Double-dosing improved by 4-6 percentage points the proportion of students earning 24 credits within their first four years of high school, the minimum number of credits required to earn a diploma. Similarly, the proportion of students graduating from high school in four years increases by 6-9 percentage points depending on the specification, while the proportion finishing in five years increases by an even larger 8-11 percentage points. Given that slightly more than half of students graduate within four or five years, these impacts represent a roughly 15 percent improvement in graduation rates.

Double-dosing also dramatically improved college enrollment outcomes. Double-dosed students are 6-8 percentage points more likely to ever enroll in college within five years of starting high school, a roughly 25 percent increase over the base college enrollment rate of 29 percent. Nearly all of this increase came from enrollment in two-year community colleges. Given the relatively low academic skills and high poverty rates of CPS students at the double-dose threshold, it is unsurprising that double-dosing improved college enrollment rates at relatively inexpensive and non-selective two-year postsecondary institutions. Though we cannot observe college completion, for reasons discussed previously, we can observe the number of semesters that students have enrolled in college within up to eight years of starting high school. Three of the four listed specification suggest that double-dosing increased college enrollment by about 0.5 semesters on average, a more than 25 percent improvement. All of these results point to substantial impacts of double-dose algebra on important indicators of educational attainment.

7. Robustness, Heterogeneity, Spillovers and Implementation

Our primary results suggest double-dose algebra improved students' math achievement, high school graduation rates and college enrollment rates. We turn now to questions of the robustness of these results, heterogeneity of the policy's impacts, and spillovers into subjects other than math. Table 8 shows robustness checks for the central results of the previous tables. Panel (A) shows the robustness of our results to other specifications. The top row replicates panel (C) from the previous tables, which employs local linear regression using an edge kernel of bandwidth 10. The second row runs the same regression with a uniform kernel (i.e., unweighted). The third row replicates the second but allows for cubic polynomials on either side of the threshold. The fourth row replicates the third but expands the bandwidth to 20 percentiles. None of these choices has any meaningful impact on our coefficient estimates. Panel (B) then provides placebo tests, with the first row presenting reduced form estimates of the main effects from prior tables. The second row then replicates the first but uses the untreated prior two cohorts. The third and fourth rows replicate the first but assume that the assignment threshold is at the 45th or 55th percentile. Nearly all of the estimated impacts vanish when using the untreated cohorts or the wrong threshold, suggesting that our estimates are not generated by spurious features of the data or unaccounted for factors associated with the assignment threshold. Our central results appear only at thresholds and for cohorts where we expect them.

Table 9 explores whether the impacts of double-dosing varied by the academic skill of the double-dosed student. Our primary regression discontinuity results estimate a local average treatment effect (LATE) for students near the given threshold, namely the 50th percentile of math skill. For comparison, we replicated some of our previous estimates in panel (A). Such students vary, however, in their reading skills as measured by their 8th grade ITBS reading scores. We exploit this fact in panel (B), where we divide students into those who scored above ("good readers") and below ("poor readers") the 45th percentile on the 8th grade ITBS reading test, which represents the median reading skill of students at the double-dose assignment threshold. We then interact double-dosing (and its instruments) with indicators for those two categories. The results are striking. For all of the outcomes shown, double-dosing had larger positive effects on poor readers than on good readers, differences which are all at least marginally statistically significant. For example, double-dosing raised poor readers' algebra passing rates by 10 percentage points but good readers' rates by only 1 percentage point. The impact of double-dosing on high school graduation and college enrollment is driven almost entirely by students in the lower part of the reading distribution. Similar hetereogeneity analysis by gender and race yields little evidence of differential impacts along these dimensions.

To explore whether double-dosing's impact varied by math skill, we implement in panel (C) a difference-in-difference specification using all students in the untreated 2001 and 2002 cohorts as a control for all students in the treated 2003 and 2004 cohorts. The first stage equation is given by,

$$DoubleDose_{it} = \gamma_0 + \gamma_1 \cdot lowscore_{it} \cdot after_{it} + \gamma_2 \cdot after_{it} + \gamma_3 \cdot lowscore_{it} + \eta_{it} \qquad (4)$$

which is used to instrument for double-dosing in the following equation,

$$Y_{it} = \beta_0 + \beta_1 \cdot DoubleDose_{it} + \beta_2 \cdot after_{it} + \beta_3 \cdot lowscore_{it} + \mu_{it} \qquad (5).$$

By controlling for differences between low- and high-scoring students in the pre-treatment cohorts and for overall differences between cohorts, we can thus estimate how the difference in outcomes between low- and high-scoring students changed at the time double-dose algebra was introduced. This approach estimates an average treatment effect (ATE) of double-dose algebra for all students double-dosed because of the policy, students who are on average lower skilled than students near the threshold itself. These estimates will be unbiased under the assumption that no other factors changed differentially between low- and high-scoring students over time. Given other policy changes occurring in CPS during this period, including other curricular reforms, this assumption is likely to be violated. We nonetheless present these estimates as suggestive evidence of the impact of double-dose algebra on the entire pool of treated students.

The results in panel (B) suggest that, across all double-dosed students, double-dosing did improve algebra passing rates, high school graduation rates, and two-year college enrollment rates, but had no discernible impact on ACT test scores. The magnitude of the impacts on high school graduation and college enrollment are about half those estimated by the regression discontinuity, suggesting that long-run impacts of double-dose algebra were substantially stronger for students near the threshold than those far from it. Together, panels (B) and (C) suggest that double-dose algebra had modest long-run impacts on the average double-dosed student but had large positive impacts on double-dosed students with relatively high math skills but low reading skills. That the majority of the positive long-run impact of double-dosing came through its effect on low skilled readers may be due to the intervention's focus on reading and writing skills in the context of learning algebra.

The increased focus on algebra at the cost of other coursework may potentially have affected achievement in other academic subjects, which we explore in Table 10. We find some evidence for slightly reduced GPA in freshman courses other than math, though the statistical significance of that result is sensitive to inclusion of student covariates. Conversely, double-dosed students had higher GPAs in non-math courses after freshman year, a result that is not sensitive to specification. Double-dosed students scored about 0.2 standard deviations higher on the verbal portion of their ACTs and were substantially more likely to pass chemistry classes usually taken in 10th or 11th grade. If anything, the skills gained in double-dose algebra generally seem to have helped, not hindered, students in other subjects and subsequent years.

Finally, Table 11 explores how the impact of double-dose algebra varied by the extent to which schools adhered to CPS' recommendation that schools schedule the two periods consecutively, with the same teacher and with the same students in each class. We construct a compliance measure that represents a school-level average of the fraction of double-dosed students with the same teacher for both algebra periods, the fraction with the two periods consecutive, and the fraction of peers in algebra who were also double-dosed. The measure thus takes a value of one in schools with perfect compliance and less than one otherwise, though we re-center the measure around the average compliance level. We then interact that re-centered measure with the instrument and the endogenous regressor in panel (A) to produce two estimates. The main coefficient estimates the impact of double-dose algebra on students in a school with an average compliance level. The interaction coefficient measures the extent to which the treatment effects varies in schools with higher compliance rates. We see no clear evidence of differential effectiveness in schools with higher compliance rates. A similar analysis in panel (B), in which we add interactions by treatment cohort, reveals little differential effect of double-dose algebra between the 2003 and 2004 cohorts. Given that, as shown in Table 1, schools in 2004 were much less likely to adhere to the implementation guidelines, these panels together tell a consistent story that those guidelines were not particularly important to the policy's success.

8. Conclusion

The double-dose strategy has become an increasingly popular way to aid students struggling in mathematics. Today, nearly half of large urban districts report doubled math instruction as the most common form of support for students with lower skills (Council of Great City Schools 2009). The central concern of urban school districts is that algebra may be a gateway for later academic success, so that early high school failure in math may have large effects on subsequent academic achievement and graduation rates. As the current policy environment calls for "algebra for all" in 9th grade or earlier grades, providing an effective and proactive intervention is particularly critical for those who lack foundational mathematical skills. A successful early intervention may have the greatest chance of having long-term effects on students' academic outcomes.

We provide evidence of positive and substantial long-run impacts of one particular form of intensive math instruction on standardized exam scores, high school graduation rates and college enrollment rates. We show that this intensive math instruction was quite successful for students with average math skills but relatively low reading skills, and modestly successful in the long run for the average treated student. This highlights the importance of carefully targeting such interventions to students most likely to benefit from them. Also, like other recent studies, we find that the test score impacts of this policy dramatically understate its long-run benefits as measured by educational attainment (Deming, 2009; Chetty et al., 2011). In our sample, OLS suggests that a 0.2 standard deviation increase in fall grade 11 math scores is associated with a 2 percentage point increase in college enrollment rates. We observe college enrollment effects three times that size, highlighting the fact that long-run analyses of such interventions may yield very different conclusions than short-run analyses.

Finally, our findings that the policy's effectiveness is not associated with the adherence to the implementation guidelines encouraged by CPS suggests that these impacts could be replicated in other urban school districts across the United States. Districts looking to adopt the double-dose strategy could likely reap its benefits without needing to radically restructure their school days, a welcome fact given the need to boost math performance in an environment with substantial resource constraints.

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Figure 1: Distribution of 8th Grade Math Scores

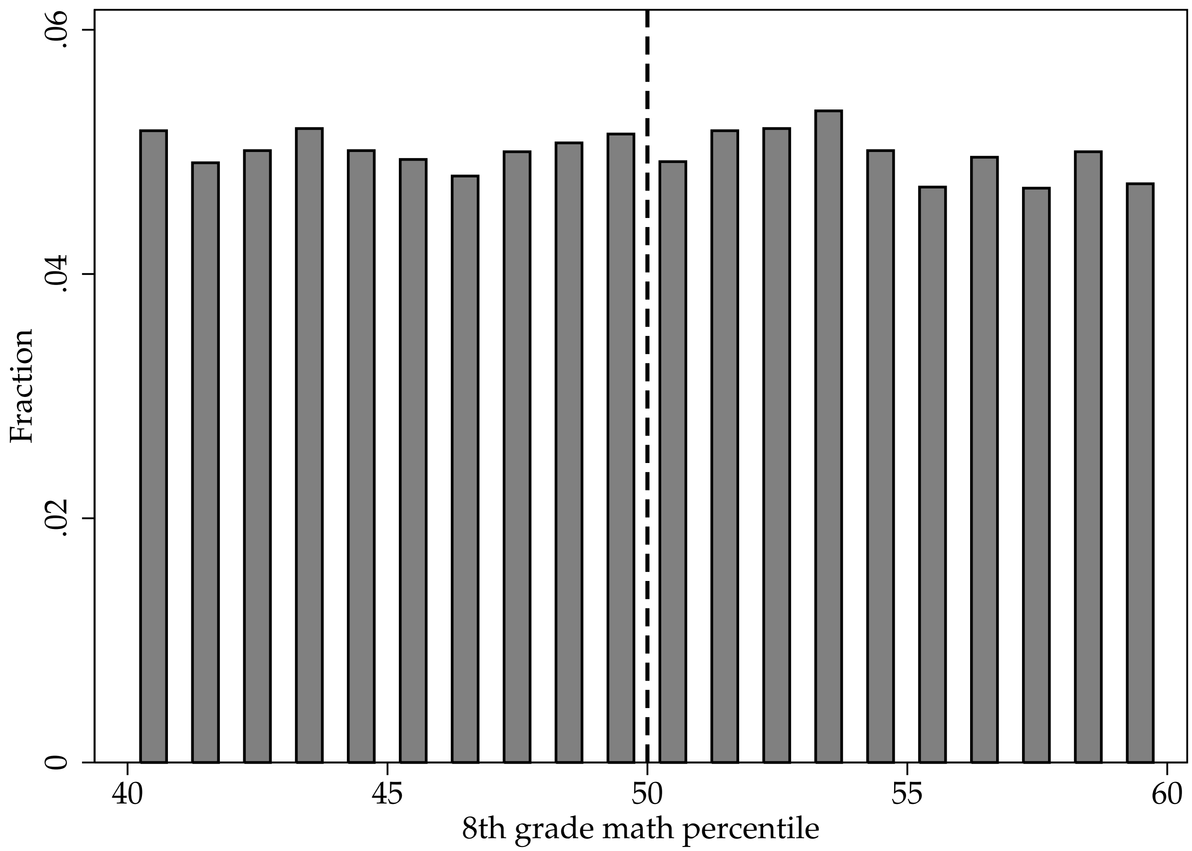


Figure 2: Double-Dosing Rates and Instructional Time

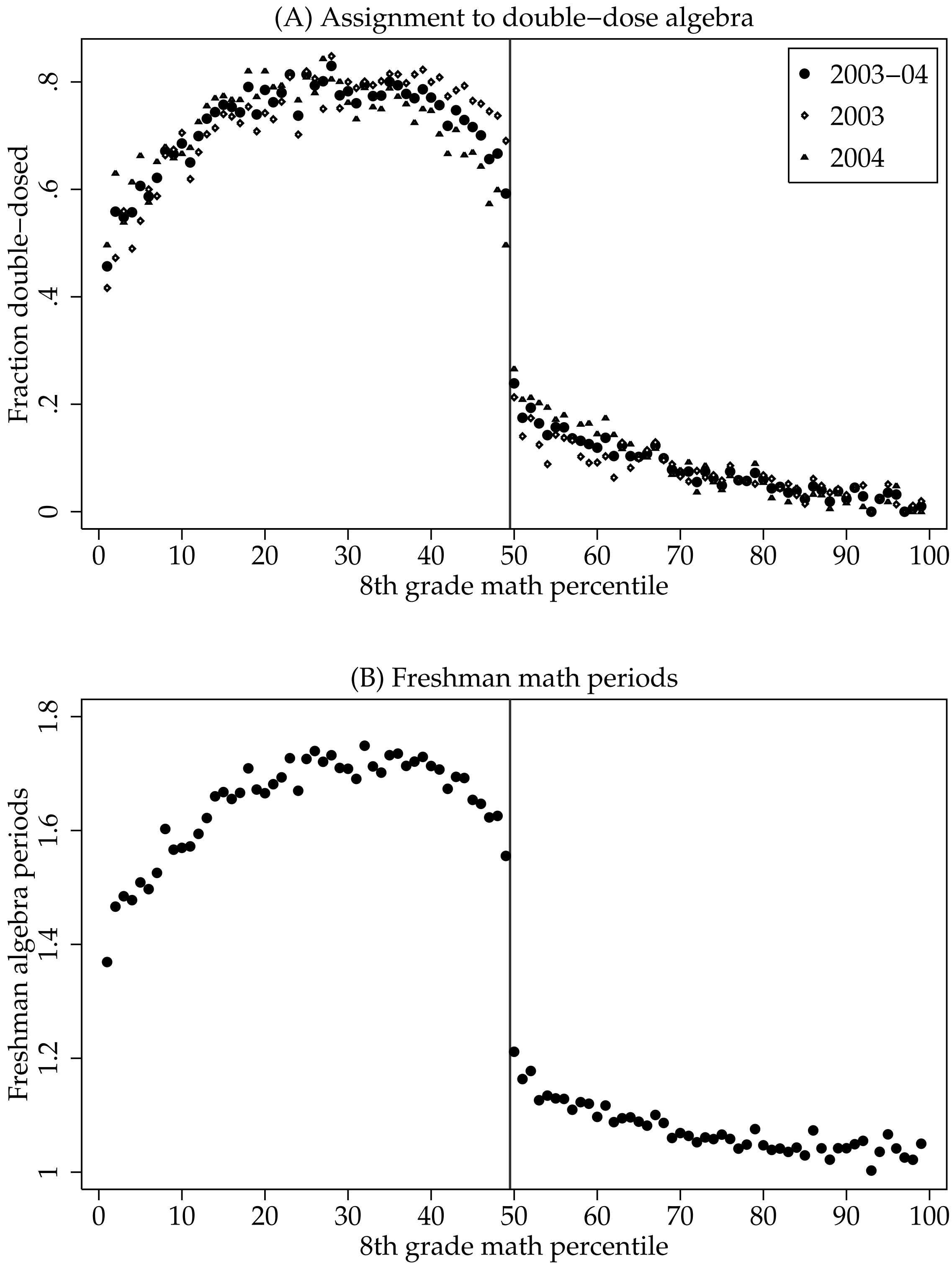
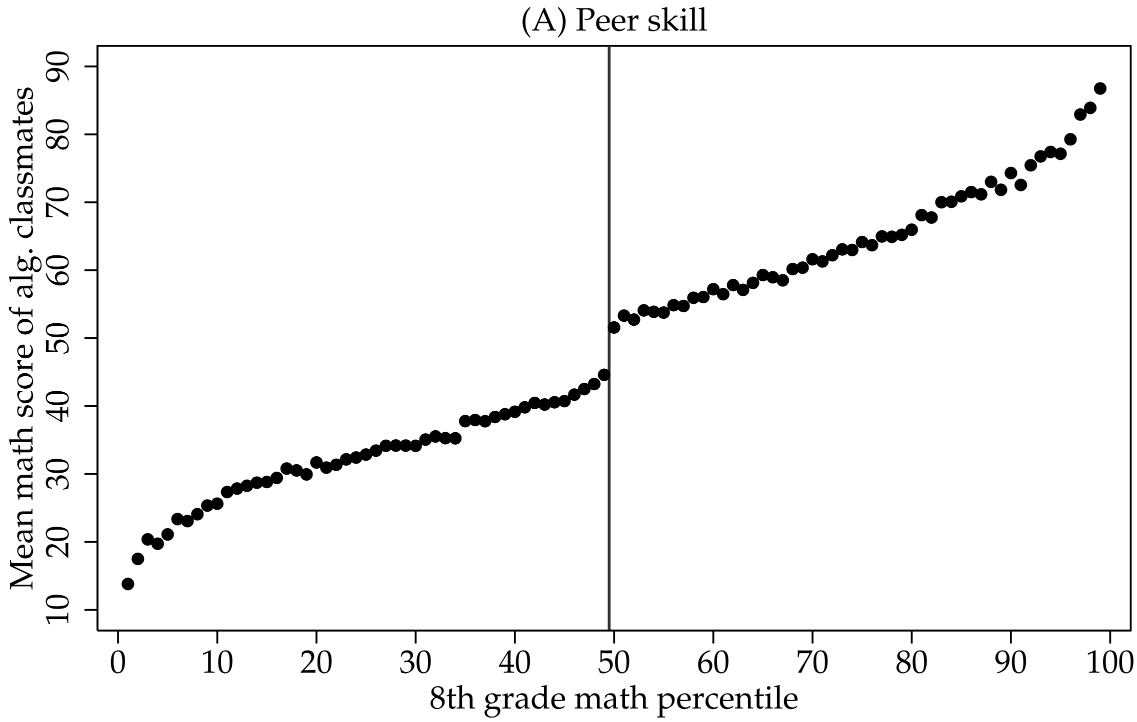


Figure 3: Peer Composition



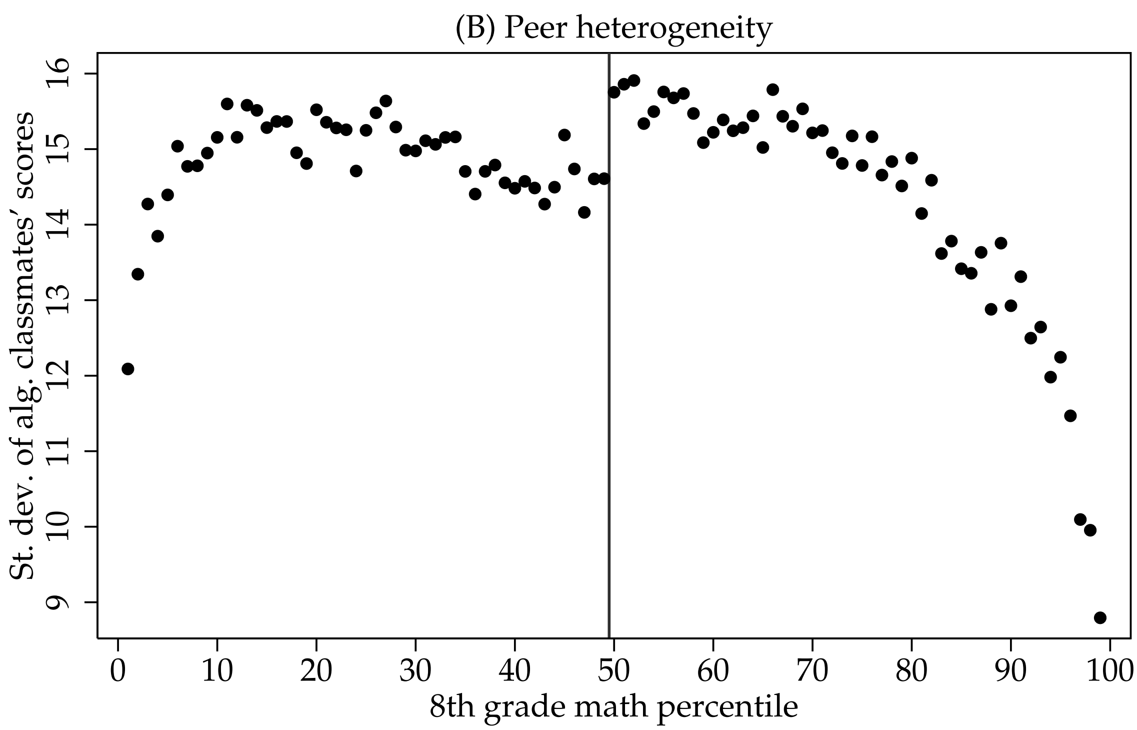


Figure 4: Freshman Algebra Grades

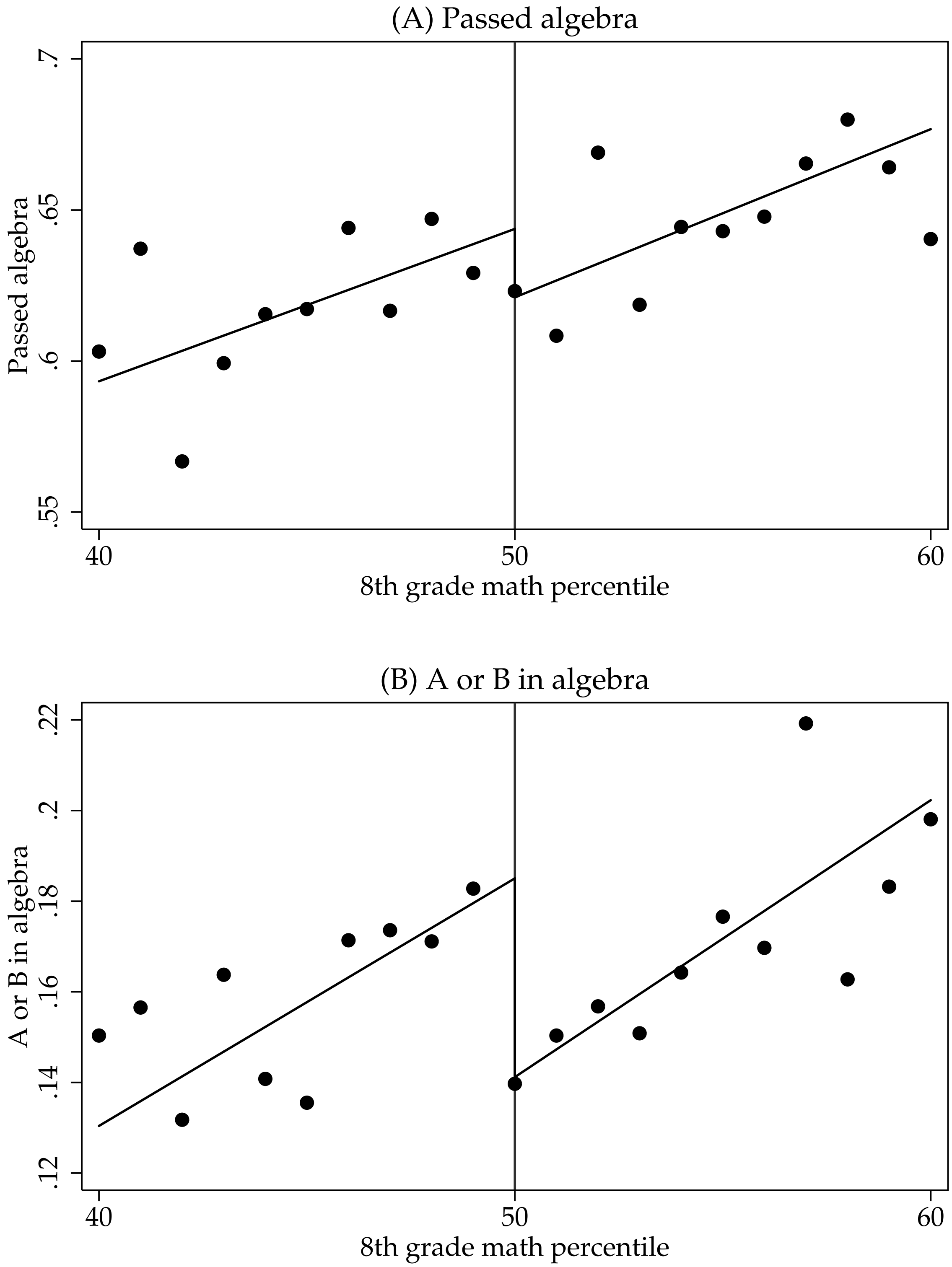


Figure 5: Math Achievement

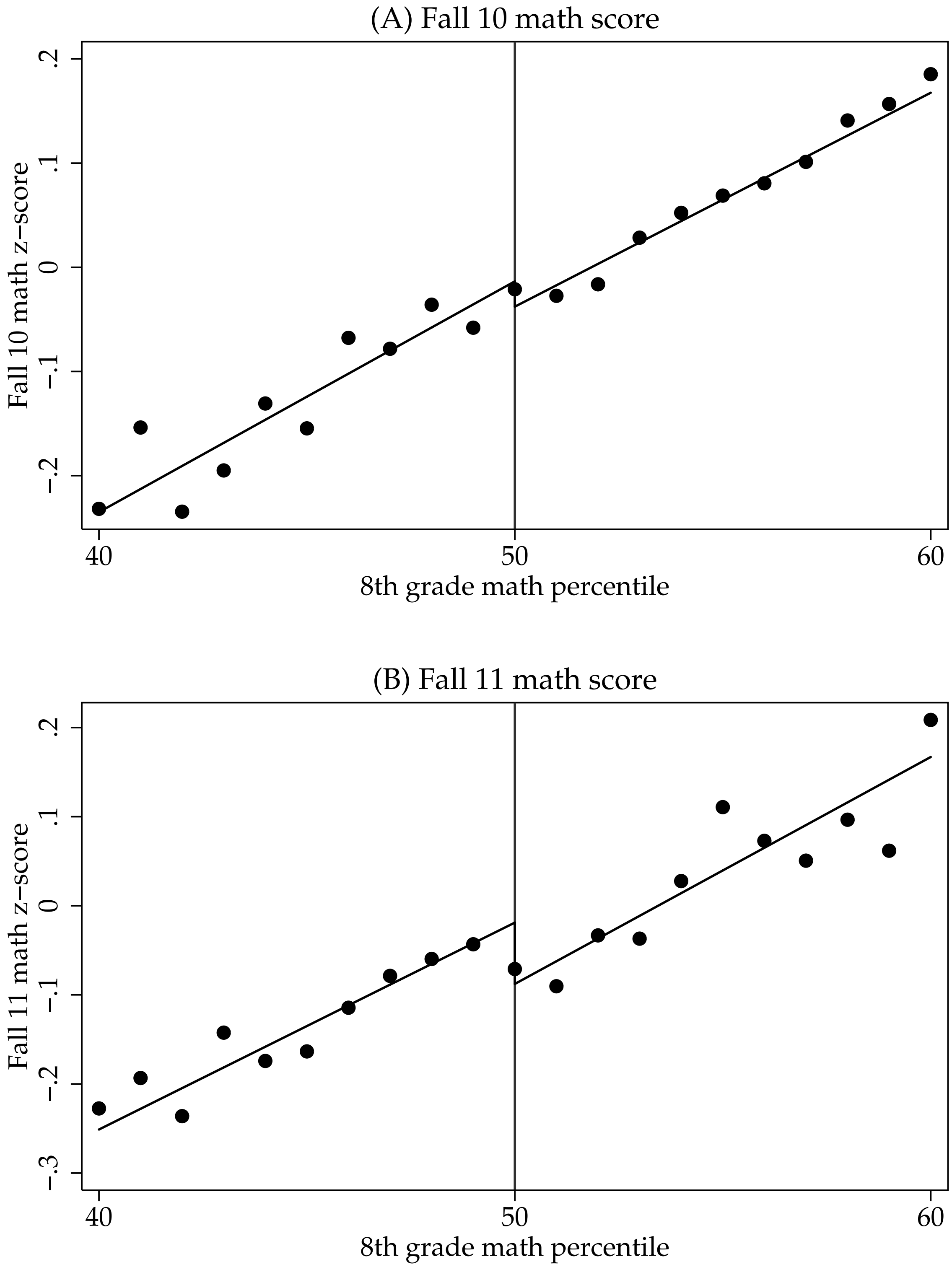


Figure 6: High School Graduation

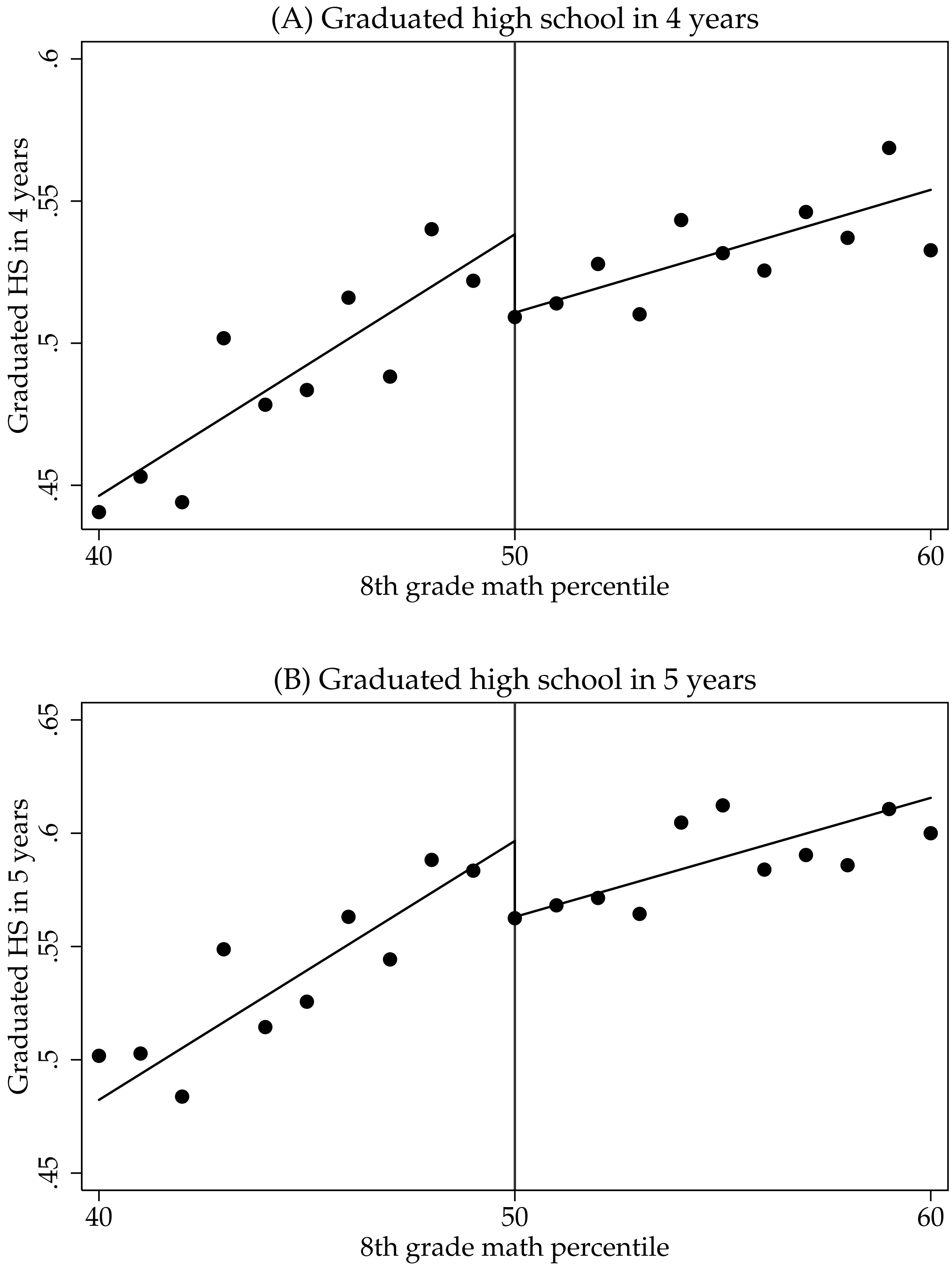


Figure 7: College Enrollment

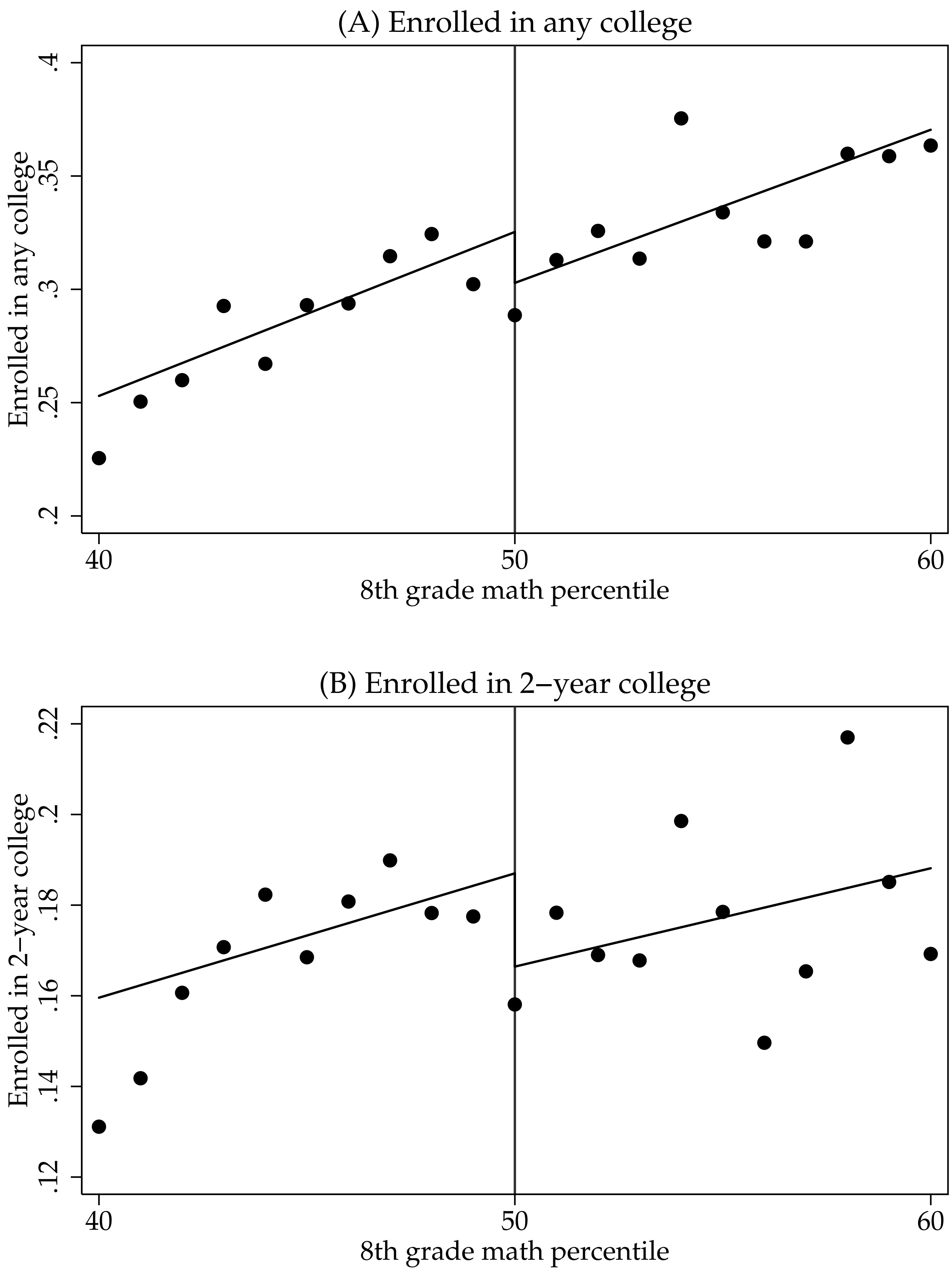


Table 1: Summary Statistics

	(1) Both cohorts, full sample	(2) Both cohorts near threshold	(3) 2003 cohort, near threshold	(4) 2004 cohort, near threshold
(A) Controls				
Female	0.50	0.54	0.55	0.54
Black	0.57	0.57	0.58	0.55
Hispanic	0.33	0.36	0.34	0.37
Special education	0.20	0.08	0.08	0.09
8th grade reading percentile	43.28	46.26	46.19	46.33
(B) Double-dose				
8th grade math percentile	45.63	49.44	49.55	49.33
Double-dose eligible	0.55	0.50	0.49	0.51
Double-dosed	0.44	0.44	0.45	0.42
Freshman math courses	1.40	1.40	1.40	1.40
Consecutive periods	0.62	0.64	0.76	0.51
Same teacher	0.66	0.70	0.83	0.56
Extent of tracking	0.92	0.92	0.94	0.90
(C) Achievement				
Passed algebra	0.62	0.63	0.63	0.64
Passed geometry	0.57	0.58	0.58	0.59
Passed trigonometry	0.52	0.53	0.51	0.54
Fall 10 math z-score	0.00	-0.04	-0.05	-0.03
Fall 11 math z-score	-0.00	-0.06	-0.06	-0.06
ACT math z-score	0.00	-0.22	-0.22	-0.23
(D) Attainment				
Graduated HS in 4 years	0.49	0.51	0.50	0.52
Graduated HS in 5 years	0.54	0.56	0.55	0.57
Enrolled in any college	0.29	0.31	0.29	0.32
Enrolled in 2-year college	0.16	0.17	0.16	0.19
N	41,410	11,057	5,507	5,550

Notes: Mean values of each variable are shown by sample. Column (1) is the full sample of students from the 2003 and 2004 cohorts. Column (2) limits the sample to students within 10 percentiles of the double-dose threshold. Columns (3) and (4) separate column (2) by cohort.

Table 2: Distribution of Covariates at Threshold

	(1) Black	(2) Hispanic	(3) Special education	(4) Block poverty	(5) Block SES	(6) Date of birth	(7) Female	(8) Reading score
(A) BW = 3								
Double-dose eligible	-0.008	0.007**	0.023**	-0.131***	0.027	-15.931**	-0.068***	-2.127***
	(0.004)	(0.003)	(0.008)	(0.027)	(0.029)	(4.134)	(0.012)	(0.430)
(B) BW = 6								
Double-dose eligible	-0.009	0.017	0.016	-0.059	0.015	-1.734	-0.058***	-1.517**
	(0.006)	(0.010)	(0.010)	(0.047)	(0.046)	(8.657)	(0.014)	(0.557)
(C) BW = 10								
Double-dose eligible	-0.014	0.017*	0.011	-0.029	0.005	0.091	-0.043***	-1.042*
	(0.008)	(0.009)	(0.010)	(0.042)	(0.042)	(7.365)	(0.015)	(0.530)

Notes: Heteroskedasticity robust standard errors clustered by 8th grade math percentile are in parentheses (* $p<.10$ ** $p<.05$ *** $p<.01$). Panels (A)-(C) present estimates of the relationship between eligibility for double-dose algebra and student covariates. The estimates are generated by local linear regression weighted with an edge kernel using bandwidths of 3, 6 and 10 percentiles respectively.

Table 3: Eligibility as an Instrument for Double-Dose Algebra

	(1) All	(2) 2003	(3) 2004
(A) BW = 3, no controls			
Double-dose eligible	0.326***	0.454***	0.198***
	(0.018)	(0.018)	(0.022)
F	317.9	667.8	82.2
(B) BW = 6, no controls			
Double-dose eligible	0.361***	0.484***	0.241***
	(0.025)	(0.023)	(0.030)
F	205.5	442.5	64.0
(C) BW = 10, no controls			
Double-dose eligible	0.394***	0.518***	0.273***
	(0.026)	(0.024)	(0.030)
F	237.8	449.1	83.6
(D) BW = 10, with controls			
Double-dose eligible	0.391***	0.513***	0.269***
	(0.026)	(0.026)	(0.029)
F	228.7	387.4	84.7

Notes: Heteroskedasticity robust standard errors clustered by 8th grade math percentile are in parentheses (* p<.10 ** p<.05 *** p<.01). Panels (A)-(C) present first-stage estimates of the relationship between eligibility for and assignment to double-dose algebra. The estimates are generated by local linear regression weighted with an edge kernel using bandwidths of 3, 6 and 10 percentiles respectively. Panel (D) replicates panel (C) but includes controls for gender, race, special education status, date of birth, cohort, 8th grade reading score and Census block poverty and socioeconomic measures. Column (1) includes students in both treated cohorts. Columns (2) and (3) separate the two cohorts. Below each estimate is the value from an F-test of the instrument.

Table 4: Double-Dose Algebra, Freshman Coursework and Peers

	(1) Math courses	(2) Academic courses	(3) Other courses	(4) Total courses	(5) Mean of peer skill	(6) St. dev. of peer skill
(A) BW = 3, no controls						
Double-dosed	0.978***	-0.089***	-0.712***	0.177***	-18.893***	-3.051***
	(0.010)	(0.014)	(0.053)	(0.051)	(0.586)	(0.338)
(B) BW = 6, no controls						
Double-dosed	0.966***	-0.104***	-0.723***	0.139*	-19.018***	-3.841***
	(0.019)	(0.020)	(0.058)	(0.076)	(0.615)	(0.329)
(C) BW = 10, no controls						
Double-dosed	0.959***	-0.135***	-0.697***	0.127**	-19.337***	-3.129***
	(0.015)	(0.023)	(0.054)	(0.063)	(0.521)	(0.360)
(D) BW = 10, with controls						
Double-dosed	0.961***	-0.157***	-0.665***	0.138**	-18.881***	-3.295***
	(0.015)	(0.025)	(0.049)	(0.062)	(0.539)	(0.373)
μ	1.211	3.512	2.256	6.979	51.580	15.753

Notes: Heteroskedasticity robust standard errors clustered by 8th grade math percentile are in parentheses (* p<.10 ** p<.05 *** p<.01). Panels (A)-(C) present estimates of the impact of double-dose algebra on the given outcome, with treatment instrumented by eligibility. The estimates are generated by local linear regression weighted with an edge kernel using bandwidths of 3, 6 and 10 percentiles respectively. Panel (D) replicates panel (C) but includes controls for gender, race, special education status, date of birth, cohort, 8th grade reading score and Census block poverty and socioeconomic measures. Also listed is the mean value of each outcome at the eligibility threshold. Column (2) includes all courses in science, English and social studies. Column (3) includes all courses other than those subjects.

Table 5: The Impact of Double-Dose Algebra on Math Coursework

	(1) A or B in algebra	(2) Passed algebra	(3) Passed geometry	(4) Passed trigonometry	(5) Total math credits
(A) BW = 3, no controls					
Double-dosed	0.152***	0.064	-0.022	0.099***	0.225***
	(0.015)	(0.039)	(0.037)	(0.021)	(0.042)
(B) BW = 6, no controls					
Double-dosed	0.133***	0.050*	0.052	0.097**	0.242***
	(0.017)	(0.030)	(0.049)	(0.041)	(0.057)
(C) BW = 10, no controls					
Double-dosed	0.112***	0.058*	-0.001	0.070**	0.133**
	(0.017)	(0.035)	(0.038)	(0.033)	(0.057)
(D) BW = 10, with controls					
Double-dosed	0.121***	0.073**	0.011	0.085**	0.178**
	(0.020)	(0.033)	(0.034)	(0.041)	(0.071)
μ	0.140	0.623	0.575	0.546	2.203

Notes: Heteroskedasticity robust standard errors clustered by 8th grade math percentile are in parentheses (* p<.10 ** p<.05 *** p<.01). Panels (A)-(C) present estimates of the impact of double-dose algebra on the given outcome, with treatment instrumented by eligibility. The estimates are generated by local linear regression weighted with an edge kernel using bandwidths of 3, 6 and 10 percentiles respectively. Panel (D) replicates panel (C) but includes controls for gender, race, special education status, date of birth, cohort, 8th grade reading score and Census block poverty and socioeconomic measures. Also listed is the mean value of each outcome at the eligibility threshold.

Table 6: The Impact of Double-Dose Algebra on Math Achievement

	(1) Fall 10 math	(2) Fall 11 math	(3) Spring 11 ACT math	(4) Fall 10, imputed	(5) Fall 11, imputed	(6) ACT, imputed
(A) BW = 3, no controls						
Double-dosed	-0.090***	0.137***	-0.009	-0.049**	0.101***	-0.004
	(0.034)	(0.033)	(0.014)	(0.023)	(0.018)	(0.008)
(B) BW = 6, no controls						
Double-dosed	0.025	0.216***	0.098**	0.026	0.157***	0.059**
	(0.054)	(0.049)	(0.046)	(0.036)	(0.030)	(0.027)
(C) BW = 10, no controls						
Double-dosed	0.058	0.175***	0.117***	0.045	0.121***	0.068***
	(0.044)	(0.036)	(0.037)	(0.029)	(0.021)	(0.022)
(D) BW = 10, with controls						
Double-dosed	0.089***	0.217***	0.142***	0.062***	0.134***	0.072***
	(0.034)	(0.033)	(0.033)	(0.021)	(0.024)	(0.018)
μ	-0.021	-0.071	-0.205	-0.090	-0.136	-0.224

Notes: Heteroskedasticity robust standard errors clustered by 8th grade math percentile are in parentheses (* p<.10 ** p<.05 *** p<.01). Panels (A)-(C) present estimates of the impact of double-dose algebra on the given outcome, with treatment instrumented by eligibility. The estimates are generated by local linear regression weighted with an edge kernel using bandwidths of 3, 6 and 10 percentiles respectively. Panel (D) replicates panel (C) but includes controls for gender, race, special education status, date of birth, cohort, 8th grade reading score and Census block poverty and socioeconomic measures. Also listed is the mean value of each outcome at the eligibility threshold. Columns (4)-(6) replace missing scores with a value of -0.25 standard deviations, just below the 40th percentile of the skill distribution.

Table 7: The Impact of Double-Dose Algebra on Educational Attainment

	(1) Earned 24 credits in 4 years	(2) Graduated high school in 4 years	(3) Graduated high school in 5 years	(4) Enrolled, any college	(5) Enrolled, two-year college	(6) Semesters, any college
(A) BW = 3, no controls						
Double-dosed	0.044*	0.079***	0.101***	0.061***	0.058***	0.451***
	(0.023)	(0.029)	(0.019)	(0.015)	(0.018)	(0.147)
(B) BW = 6, no controls						
Double-dosed	0.062***	0.075***	0.107***	0.085**	0.055***	0.541***
	(0.023)	(0.027)	(0.018)	(0.035)	(0.021)	(0.192)
(C) BW = 10, no controls						
Double-dosed	0.042*	0.064**	0.077***	0.057*	0.052**	0.372
	(0.022)	(0.025)	(0.019)	(0.033)	(0.022)	(0.226)
(D) BW = 10, with controls						
Double-dosed	0.061***	0.086***	0.097***	0.079***	0.060***	0.539***
	(0.017)	(0.020)	(0.019)	(0.025)	(0.020)	(0.190)
μ	0.463	0.509	0.563	0.289	0.158	1.789

Notes: Heteroskedasticity robust standard errors clustered by 8th grade math percentile are in parentheses (* $p<.10$ ** $p<.05$ *** $p<.01$). Panels (A)-(C) present estimates of the impact of double-dose algebra on the given outcome, with treatment instrumented by eligibility. The estimates are generated by local linear regression weighted with an edge kernel using bandwidths of 3, 6 and 10 percentiles respectively. Panel (D) replicates panel (C) but includes controls for gender, race, special education status, date of birth, cohort, 8th grade reading score and Census block poverty and socioeconomic measures. Also listed is the mean value of each outcome at the eligibility threshold.

Table 8: Robustness Checks

	(1) Passed freshman algebra	(2) A or B in freshman algebra	(3) Fall 11 math score	(4) ACT math score	(5) Graduated high school in 4 years	(6) Graduated high school in 5 years	(7) Enrolled, any college	(8) Enrolled, two-year college
(A) Other specifications								
BW = 10, edge kernel, linear	0.058*	0.112***	0.175***	0.117***	0.064***	0.077***	0.057*	0.052**
	(0.034)	(0.017)	(0.035)	(0.036)	(0.025)	(0.019)	(0.033)	(0.021)
BW = 10, unweighted, linear	0.050	0.086***	0.109**	0.097**	0.070***	0.057**	0.060*	0.071***
	(0.034)	(0.019)	(0.043)	(0.039)	(0.024)	(0.022)	(0.032)	(0.023)
BW = 10, unweighted, cubic	0.015	0.132***	0.178**	0.006	0.102**	0.130***	0.140**	0.074**
	(0.050)	(0.042)	(0.073)	(0.080)	(0.044)	(0.042)	(0.054)	(0.036)
BW = 20, unweighted, cubic	0.103**	0.146***	0.223***	0.134**	0.096**	0.122***	0.105**	0.066*
	(0.048)	(0.029)	(0.069)	(0.064)	(0.040)	(0.032)	(0.047)	(0.034)
(B) Placebo tests								
ITT, treated cohorts	0.023	0.044***	0.070***	0.047**	0.025**	0.030***	0.022	0.020**
	(0.015)	(0.005)	(0.016)	(0.017)	(0.011)	(0.008)	(0.013)	(0.008)
ITT, untreated cohorts	0.012	0.017**	-0.004	-0.013	0.005	0.000	-0.006	0.014
	(0.013)	(0.007)	(0.025)	(0.017)	(0.009)	(0.010)	(0.021)	(0.010)
ITT, cutoff at 45	-0.018	-0.008	-0.011	-0.066**	-0.011	-0.020	-0.017	-0.005
	(0.014)	(0.016)	(0.026)	(0.027)	(0.016)	(0.019)	(0.011)	(0.009)
ITT, cutoff at 55	-0.011	-0.025**	-0.060*	0.027	0.002	-0.010	0.023	0.013
	(0.013)	(0.012)	(0.031)	(0.033)	(0.011)	(0.016)	(0.020)	(0.016)

Notes: Heteroskedasticity robust standard errors clustered by 8th grade math percentile are in parentheses (* p<.10 ** p<.05 *** p<.01). The first row of panel (A) replicates estimates from previous tables using local linear regression weighted by an edge kernel of bandwidth 10 percentiles. The second row replicates the first but uses a uniform kernel. The third row replicates the second but fits a cubic on either side of the threshold. The fourth row replicates the third but expands the bandwidth to 20 percentiles. All of the regressions in panel (B) present reduced form estimates of eligibility on the listed outcomes, using local linear regression weighted by an edge kernel of bandwidth 10 percentiles. The first row presents baseline ITT estimates for the treated cohorts. The second row replicates the first but uses the untreated cohorts. The third and fourth rows replicate the first but respectively assume 45 and 55 are the eligbility cutoffs instead of 50.

Table 9: Heterogeneity by Academic Skill

	(1) Passed freshman algebra	(2) Spring 11 ACT math score	(3) Graduated high school in 4 years	(4) Enrolled, any college	(5) Enrolled, 2-year college
(A) RD, overall					
Double-dosed	0.058*	0.117***	0.064***	0.057*	0.052**
	(0.034)	(0.036)	(0.025)	(0.033)	(0.021)
N	11,057	6,415	11,057	11,057	11,057
(B) RD, by reading skill					
Double-dosed * poor reader	0.103***	0.194***	0.120***	0.116***	0.079***
	(0.029)	(0.036)	(0.029)	(0.038)	(0.028)
Double-dosed * good reader	0.013	0.083*	0.015	0.007	0.026
	(0.047)	(0.049)	(0.031)	(0.034)	(0.025)
p ($\beta^{below} = \beta^{above}$)	0.023	0.037	0.001	0.000	0.061
μ (at threshold)	0.635	-0.189	0.526	0.289	0.159
N	11,057	6,415	11,057	11,057	11,057
(C) DD, overall					
Double-dosed	0.069***	0.017	0.036***	0.015	0.020***
	(0.010)	(0.024)	(0.010)	(0.009)	(0.008)
μ (below * before)	0.546	-0.538	0.408	0.177	0.122
N	79,540	44,580	79,540	79,540	79,540

Notes: Heteroskedasticity robust standard errors clustered by 8th grade math percentile are in parentheses (* p<.10 ** p<.05 *** p<.01). Panel (A) presents regression discontinuity estimates from previous tables of the impact of double-dose algebra on the given outcome, using local linear regression weighted with an edge kernel of bandwidth 10 percentiles. Panel (B) replicates panel (A) but interacts both the instrument and the treatment variable with reading group indicators, as well as controlling directly for such indicators. Below each coefficient in panel (B) is the p-value from an F-test of the equality of the two coefficients shown. Panel (C) presents difference-in-difference estimates of the treatment effect, using students with the entire range of 8th grade math scores and using the 2001 and 2002 cohorts as the pre-period. To control for compositional changes in the student body over those four years, panel (C) also includes controls for gender, race, special education status, date of birth, cohort, 8th grade reading score and Census block poverty and socioeconomic measures. Below panel (C) is the mean value of each outcome for students below the eligibility threshold in the 2001 and 2002 cohorts.

Table 10: Spillovers onto Other Subjects

	(1) Nonmath GPA, year 1	(2) Nonmath GPA, years 2+	(3) Fall 11 verbal score	(4) ACT verbal score	(5) ACT science score	(6) Passed chemistry
(A) BW = 3, no controls						
Double-dosed	-0.057***	0.243***	-0.015	0.207***	0.417***	0.134***
	(0.018)	(0.020)	(0.079)	(0.032)	(0.030)	(0.004)
(B) BW = 6, no controls						
Double-dosed	-0.103***	0.171***	-0.045	0.157**	0.140	0.148***
	(0.032)	(0.024)	(0.066)	(0.072)	(0.105)	(0.012)
(C) BW = 10, no controls						
Double-dosed	-0.115***	0.138***	-0.069	0.105	0.045	0.101***
	(0.038)	(0.042)	(0.067)	(0.068)	(0.108)	(0.020)
(D) BW = 10, with controls						
Double-dosed	-0.059	0.207***	0.050	0.222***	0.100	0.121***
	(0.046)	(0.057)	(0.067)	(0.075)	(0.117)	(0.023)
μ	1.930	1.791	-0.001	-0.119	-0.163	0.445

Notes: Heteroskedasticity robust standard errors clustered by 8th grade math percentile are in parentheses (* p<.10 ** p<.05 *** p<.01). Panels (A)-(C) present estimates of the impact of double-dose algebra on the given outcome, with treatment instrumented by eligibility. The estimates are generated by local linear regression weighted with an edge kernel using bandwidths of 3, 6 and 10 percentiles respectively. Panel (D) replicates panel (C) but includes controls for gender, race, special education status, date of birth, cohort, 8th grade reading score and Census block poverty and socioeconomic measures. Also listed is the mean value of each outcome at the eligibility threshold.

Table 11: Compliance with Implementation Guidelines

	(1) Passed freshman algebra	(2) Fall 10 math score	(3) Spring 11 ACT math score	(4) Graduated high school in 5 years	(5) Enrolled, any college
(A) By compliance					
Double-dosed	0.067**	0.116***	0.064**	0.057*	0.052**
	(0.032)	(0.037)	(0.026)	(0.033)	(0.025)
Double-dosed * compliance	-0.155*	0.043	-0.006	-0.009	-0.004
	(0.084)	(0.071)	(0.070)	(0.076)	(0.069)
(B) By treatment cohort					
Double-dosed	0.039	0.121***	0.074***	0.068**	0.041*
	(0.031)	(0.043)	(0.027)	(0.030)	(0.022)
Double-dosed * 2004	0.055	-0.009	-0.031	-0.034	0.028
	(0.044)	(0.053)	(0.049)	(0.032)	(0.029)

Notes: Heteroskedasticity robust standard errors clustered by 8th grade math percentile are in parentheses (* p<.10 ** p<.05 *** p<.01). Panel (A) and (B) present instrumental variables estimates generated by regression discontinuity using local linear regression weighted with an edge kernel of bandwidth 10 percentiles. Panel (A) interacts the instrument and treatment with a continuous measure of each school's compliance with the three implementation guidelines, as well as including that measure directly. That measure is a school-level average of the fraction of double-dosed students with the same teacher for both algebra periods, the fraction with the two periods consecutive, and the fraction of peers in algebra who were also double-dosed. Panel (B) interacts the instrument and treatment with a 2004 cohort indicator, as well as including that indicator directly.