Doubling Up:
The Long Run Impacts of Remedial Algebra on High School Graduation and College Enrollment

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Abstract

Success or failure in freshman algebra has long been thought to have a strong impact on subsequent high school outcomes. We study a remedial algebra policy implemented by the Chicago Public Schools (CPS) for cohorts entering high school in 2003 and 2004. Students scoring below the national median on an eighth grade exam were assigned in ninth grade to a double-dose algebra course, which doubled instructional time and increased tracking by ability. This assignment rule allows difference-in-difference and regression discontinuity estimates of average and local average treatments effects. Using longitudinal data that tracks students from eight grade to college enrollment, we confirm prior work showing positive short-run impacts on freshman GPA, passing rates and math scores. We show long run positive and substantial impacts of remediation on college entrance exam scores, high school graduation rates and college enrollment rates. This is the first evidence we know of to demonstrate long run impacts of remediation in an American urban high school setting.

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I. Introduction

There is an increasing recognition and concern that too few high school students, especially those in urban areas, are graduating with the necessary skills needed for college and the workforce. The high school completion rate has been declining over the past decade among students at all income levels and only about half of racial minority students finish high school. Despite increases in college enrollment, there has been little increase in college graduation rates among African-American and Latino students, who often struggle on entering college. High schools, particularly urban ones, are often blamed for not graduating students with the skills and coursework they need to be successful in college.

There is now a national movement calling for more rigorous high school requirements that are explicitly linked to the skills that students will need for work and college. The National Governor's Association, for example, has recommended enacting high school reform through rigorous college preparatory graduation requirements, programs to encourage disadvantaged students to take Advanced Placement exams and college-preparatory classes, and the design of literacy and math support courses for students with below-grade level performance. These recommendations are already being followed. A number of states are in the process of raising curricular requirements for graduation and other states have enacted incentives to particularly encourage disadvantaged students to take rigorous high school course loads.

Calls to increase both curricular offerings and requirements are based on a large amount of evidence tying school curriculum to student outcomes. Since the late 1980s there has been evidence that requiring students to take college preparatory classes could produce higher overall levels of achievement and reduce racial and socioeconomic achievement gaps (Bryk, Lee and Holland, 1993; Goodman, 2009). Some of this evidence comes from studies of the curricular organization of public and Catholic high schools. Of particular interest is the nature of remedial education in the PK-12

setting, which has historically been quite different in these two educational sectors. In public high schools, remedial coursework constituted an entire set of courses that could be used to satisfy graduation requirements. In contrast, in Catholic schools, students were required to enroll in additional courses to build their skills through a "double dose" of coursework. Some researchers have attributed at least part of the apparent success of Catholic schooling to such rigorous curricular requirements.

Unfortunately, the vast majority of studies on the impact of remedial coursework and other curricula on student achievement have relied on cross-sectional samples analyzed with empirical strategies prone to generate substantial selection bias. The few studies on remedial education that have seriously grappled with the issue of causal inference have either looked at short-run outcomes such as test scores (Jacob and Lefgren, 2004) or longer-run outcomes in the context of U.S. colleges or high schools in other nations (Calcagno and Long, 2008; Bettinger and Long, 2009; Lavy and Schlosser, 2005). None of these studies have analyzed longer-term outcomes in the urban American high school.

However, the one study of remedial education in an urban American high school is by Nomi and Allensworth (2009) that study on which this current study builds on, the authors examined the short-term impact of a remedial math policy known as "double-dose" algebra, enacted by the Chicago Public Schools in 2003. Under this policy, students scoring below the national median on their 8th grade math exam were required to take two periods of algebra a day during the 9th grade. Students placed into these remedial classes thus received substantially more instruction time in algebra. The policy also called for increased tracking by ability, so that the policy caused students to be exposed to different peer groups than they otherwise would have been. Nomi and Allensworth (2009) analyzed the *early* high school outcomes of this policy by following students only through the 10th grade, and found positive and substantial impacts on G.P.A, course failure rates, and

standardized test scores. The time frame of their initial study did not, however, allow them to explore other important outcomes beyond 10th grade, such as learning in higher mathematics, high school graduation, and college attendance.

Our study examines the impact of Chicago Public Schools remedial math policy on *longer-term outcomes*, which are ultimately the outcomes that students, parents, and school districts care most about. Specifically, we analyze advanced math coursework and performance, ACT scores, high school graduation rates and college enrollment using longitudinal transcript data from the Chicago Public Schools (CPS), which allow us to track students from 8th grade through college enrollment.

To analyze the effect of this innovative double-dose curriculum, we employ two complementary quasi-experimental techniques. The first is a difference-in-differences analysis, which compares how the differential outcomes between students above and below the remediation threshold changed upon implementation of the policy. The second is a regression discontinuity design, which compares the outcomes of students just above and just below the remediation threshold in years when the remedial math policy was in place. These empirical strategies allow us to compare students treated with the remediation policy to a similar group of untreated students who serve as the control group. Also, both of these techniques will produce reduced form estimates of the impact of the policy on all students as well as instrumental variables estimates of the impact of the policy on students induced to enroll in remedial math.

Using longitudinal data that tracks students from 8th grade to college enrollment, we confirm prior work showing positive short-run impacts on freshman GPA, passing rates and math scores. We show long run positive and substantial impacts of remediation on college entrance exam scores, high school graduation rates and college enrollment rates. This is the first evidence we know of to demonstrate long run impacts of remediation in an American urban high school setting.

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 longer-term effects on students' academic outcomes. The findings from our study suggest that CPS's double-dose algebra policy had an effect on not only student's pre-college entrance exams (i.e., ACT math scores), but also high school graduation and college enrollment.

II. Empirical Strategies

In this section we describe in greater detail the two complementary multivariate regression strategies to measure the impact of the remedial math program under consideration.

A key feature of the double-dose algebra policy is that enrollment in double-dose algebra was assigned on the basis of a cutoff core on the standardized test. Specifically, the policy required that all 9th grade students scoring below the national median on the 8th grade ITBS math test were required to enroll in double-dose algebra.

A. Difference-in-Differences Approach

Our first empirical strategy employs a differences-in-differences (DD) approach to compare how differences in student outcomes between low- and high-scoring students changed with the introduction of the remedial math program. The following model specification is estimated by

ordinary least squares (OLS) to analyze the effect of double-dose algebra on academic outcomes of remedial math students,

$$Y_{it} = \alpha + \beta \cdot post_i + \gamma \cdot lowscore_i + \delta \cdot post_i \cdot lowscore_i + X_{it} \cdot \phi + \varepsilon_{it} \qquad (1)$$

where Y_{it} represents an academic outcome (such as a standardized test score or college attendance) for student *i* in grade cohort *t*. $post_i$ is an indicator variable that equals to one if a given student scores below the national median on his or her 8th grade *Iowa Tests of Basic Skills* (ITBS) math exam and equals to zero for scores above the national median. $lowscore_i$ is an indicator variable that equals to one in 2003 and 2004, when the program was in place; and equals to zero in 2002, when the program was not in place. $post_i$ multiplied by $lowscore_i$ is an interaction of these variables. X_{it} is vector demographic characteristics for student *i* in grade cohort *t*. More specifically, X_{it} includes gender, race/ethnicity (non-Hispanic white (omitted category); black, and Hispanic), special education, socioeconomic status, concentration of poverty, and 8th grade ITBS math exam.[1] ε_{it} is a normally distributed random error term. Lastly, heteroskedasticity robust standard errors will be clustered at the high school level to account for within-school correlations in the error term ε_{it}. Equation (1) will be estimated without and with a full set of student- and school-level controls to check the robustness of the results to underlying compositional changes in the student body or distribution of students among high schools.

Our difference-in-differences approach will measure the average treatment effect (ATE) of the remedial math program on low-scoring students using high-scoring students as a control group. This will yield unbiased estimates of the program's impact under the assumption that no changes other than the remedial math program differentially affected low- and high-scoring students over

[1] The socioeconomic status and concentration of poverty variables are defined as the census-block level for each student. More specifically, socioeconomic status variable is based on 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 percent of males over 18 who are employed one or more weeks during the year and the percent of families above the poverty line.

this time period. Hence, the coefficients of the above difference-in-differences framework have the following interpretation: β captures the average cohort differences in outcomes over time that are *common* to both the low-scoring students (i.e., treatment group) and the high-scoring students (i.e., control group); γ captures the average permanent differences between the treatment and the control group; and lastly, of particular interest is the coefficient δ, which is the difference-in-differences estimator that captures the effect of CPS remedial math program. More specifically, this parameter measures the differences in outcomes (i.e., standardized test score or college attendance) before and after for the low-scoring students, compared to the corresponding difference for the high-scoring students.

B. Regression Discontinuity Approach

If other factors are differentially changing between low- and high-scoring students over this time period, the underlying assumption critical to the difference-in-differences approach may be violated. Hence, in this section we present a second empirical strategy that tackles this issue. Our second empirical strategy employs a regression discontinuity (RD) approach to compare how outcomes differed between students just above and below the cutoff for assignment to remedial math education. This empirical approach can be employed in this study because the CPS remedial policy gives us the exact cut off point needed in this framework. The general set up for our RD design in this paper is as follows,

$$Y_{it} = \alpha + \gamma \cdot lowscore_i + \theta \cdot f(itbsmath_i) + X_{it} \cdot \phi + \eta_{it} \qquad (2)$$

where Y_{it} represents an academic outcome for student *i* in grade cohort *t*. $lowscore_i$ is an indicator variable equal to one if a given student scores *below* the national median on his or her 8th grade ITBS math exam and equal to zero for scores at or *above* the national median. $f(itbsmath_i)$ is a continuous function of each student's score on his or her 8th grade ITBS math exam, with a polynomial of

degree N in $itbsmath_i$. X_{it} is vector demographic characteristics for student *i* in grade cohort *t* (same as in equation (1)). η_{it} is a normally distributed random error term. Again, heteroskedastic robust standard errors will be clustered at the high school level to account for within-school correlations in the error term η_{it}. Equation (2) will be estimated without controls, and also with a full set of student- and school-level controls to check the robustness of the results to underlying compositional changes in the student body or distribution of students among high schools.

The assignment mechanism outlined above is simple: those students whose ITBS math score fall below the cut-off point (i.e., *below* the national median) are placed in the treatment group ($lowscore_i = 1$), whereas those students on or above that cut-off point (*below* the national median) are placed in the control group ($lowscore_i = 0$). Hence, this assignment rule occurs through a known deterministic decision based on the student's score on his or her 8th grade ITBS math exam; and the regression discontinuity estimate will then come from the coefficient γ on the *lowscore* indicator variable.

The RD approach will measure the impact of the program on students just below the remediation cutoff, using students just above the remediation cutoff as a control group. This will yield unbiased estimates of the program's local average treatment effect (LATE) under the assumption that students scoring just at the national median on the ITBS math exam are nearly identical to students scoring just below the national median, but for the fact the latter are remediated.

C. Instrumental Variables Approach

In case the assignment to remediation by the ITBS cutoff score was not perfectly followed, for both the difference-in-differences and regression discontinuity approaches, we will also provide two sets of estimates of the program's effects. We will provide reduced form estimates that measure the intention-to-treat (ITT) impact of the policy. These estimates measure the average impact of the

policy on students, regardless of whether the students were actually remediated. We will also use the cutoff rule as an instrumental variable that changes the probability that a given student is remediated. Instrumental variables estimates from these regressions will yield treatment on the treated (TOT) estimates of the policy. These estimates measure the impact of the policy on students who were remediated as a result of the policy.

For example, in the regression discontinuity framework, this is implemented by running the following first stage regression to generate a predicted probability that a given student is remediated:

$$Remediated_{it} = \alpha + \gamma \cdot lowscore_i + \theta \cdot f(itbsmath_i) + \xi_{it} \quad (3)$$

This predicted probability can then be used in the context of the following regression to provide an instrumental variable estimate of the impact of remediation on an outcome Y_{it}:

$$Y_{it} = \alpha + \pi \cdot Remediated_{it} + \theta \cdot f(itbsmath_i) + \psi_{it} \quad (4)$$

Thus, the coefficient π represents the local average treatment effect of the policy on students who have actually been remediated as a result of the policy. The difference-in-differences framework can be similarly combined with an instrumental variables approach to yield such TOT estimates of the average treatment effect.

III. Data and Sample Characteristics

A. Data Sources

This study uses multiple data sources provided by the Chicago Public Schools (CPS) district. Our data cover the universe of students in CPS from 2002-03 to 2010-11 school year. Specifically, we use student-level administrative records, transcript files, and grade files to construct the analytical sample. The administrative records provide demographic information, including student enrollment status, gender, race, and special education status. We link each student's administrative record to his or her individual transcript and grade file. The transcript files contain semester-by-semester course

transcripts and the grade data files contain detailed class information, such as teacher IDs, class periods, subject names, subject specific course codes, and course grades. These were used to classify students' algebra courses.

Elementary achievement test score is based on the Iowa Test of Basic Skills (ITBS), taken by all CPS students in the 8th grade. High school achievement test scores come from two exams: the PLAN exam, a test that is part of the EPAS system developed by ACT, Inc., which CPS students take in the fall of the 10th grade; and the ACT exam, which is taken by all 11th graders. In the Midwest, most students take the ACT to apply to college. Since 2001, Illinois has included the ACT in its annual state high school test, Prairie State Examination (PSE). As a result, all Chicago juniors take the ACT which assesses skills in math, science, language arts, and social science. To compare observations from different years in our sample, each test score is converted into a z-score, whereby each student's raw test score is standardized using the mean and standard deviation across all students in our sample that took the relevant examination in a given year.

Additionally, 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 were derived from the census data about the economic conditions in students' residential block groups.[2]

A key feature of the CPS database is the ability to track students across and within schools as this allows us to chart students' progress across time. We analyze three cohorts of first-time 9th grade students—one pre-policy and two post-policy cohorts—who entered high school in 2002-03, 2003-04, and 2004-05 school year. The ability to track students allows us to follow each student cohort for four years from the fall of their freshmen year to the fall after they should have graduated from CPS. This allows us to determine whether each student graduated, dropped out, or transferred out of CPS. Students are coded as graduates if they received a regular CPS diploma. They are coded as

[2] These census block indicators distinguish student socioeconomic status much better than more commonly available data on free or reduced lunch as 85 percent of CPS students are eligible for such meal programs.

dropouts if they are no longer active and have been listed as leaving for any reason other than an official transfer to a regular high school outside CPS.

In addition to analyzing on-time graduation and dropout rates of CPS students, we also analyze postsecondary enrollment. CPS linked our data with the National Student Clearinghouse (NSC) data on the college enrollment of CPS graduates, beginning with the graduating class of 2006 and ending with the graduating class of 2008.[3] This allows us to construct several measures of enrollment: whether a student enrolled in a postsecondary institution within a year after graduating from high school, and whether a student enrolled in a 2-year versus a 4-year institution within a year after high school.

B. Descriptive Statistics and Sample Characteristics

The summary statistics of the analytic sample are shown in Table 1. As seen in panel (A), the mean CPS eight grader scored below the 50th percentile on the math portion of the Iowa Test of Basic Skills. As a result, 56% of students qualified for remediation in 2003 and 2004, according to the district's rule that any student scoring below the 50th percentile should be remediated. Even though 56% qualified for remediation, only 42-44% were actually remediated, suggesting imperfect compliance with the rule.

This imperfect compliance is seen in Figure 1, which graphs by cohort the proportion of students remediated for each eight grade math percentile. If compliance with the remediation rule were perfect, we would expect to see 100% remediation rates to the left of the 50th percentile threshold. Instead, remediation rates reach at most about 80% for students in the 30-40th

[3] The NCS collects information on students' postsecondary education, which includes semester-by-semester college enrollment and attainment. The NCS is a non-profit corporation that was established in 1993 to assist higher education institutions in verifying enrollment and degree completion. 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.

percentiles. Students just below the remediation threshold are less likely to comply, as are students who are very low scorers (who may be taking low-level courses that do not technically qualify as double-dose algebra). Interestingly, compliance for students just below the threshold is substantially lower in the 2004 cohort than the 2003 cohort, perhaps due to families or students learning about the program and objecting to remediation when it may not be warranted. Also interesting is the fact that students above the threshold are remediated, perhaps because schools cannot perfectly divide students into appropriately sized classes by the remediation rule.

The most obvious impact of remediation, seen in the last row of panel (A), is that the mean number of yearlong math courses taken by CPS students rose from 1.0 to 1.4. Figure 2 shows this graphically. Because remediation entailed taking two math courses instead of one, Figure 2 is basically Figure 1 scaled up by a factor of 2. Doubling of instructional time was not, however, the only impact of the program. Because remediated students were also supposed to placed into classes with similarly remediated students, tracking in freshman math courses increased. This can be seen in Figure 3, which graphs the average eighth grade math score of students' classmates in freshman math. Remediated students gained instructional time but were in classrooms with substantially lower ability peers than they would otherwise have been.

Panel (B) of Table 1 shows measures of student achievement in mathematics. The mean CPS student had a freshman math GPA of roughly 1.6, somewhere between a C and a D. Nonetheless, 84% of students passed freshman algebra. Only 36% earned a B or higher. We also observe a 10^{th} grade math score and an 11th grade math score from the ACT college entrance exam, both of which we have normalized by CPS cohort.

Panel (C) lists educational attainment figures for this sample (with the caveat that our measures for high school attainment in the 2004 cohort are currently unavailable). Roughly 50% of CPS students graduate in 4 years, with 22% known to be dropouts and another 30% whose fates are

unknown. These latter students may have moved out of the district or may simply be missing from district's system. The National Student Clearinghouse data suggest that 34% of our sample enroll in college within a year of graduating, evenly split between two- and four-year institutions.

IV. Results and Discussion

Table 2 shows the first-stage results using eligibility as an instrument for remediation. Panel (A) is a difference-in-difference specification with all three cohorts. Panel (B) replicates panel (A) but allows the difference-in-difference estimate to vary by treated cohort. Panel (C) is a regression discontinuity specification with the two treated cohorts. It fits a straight line using a bandwidth of 10 percentiles on either side of the threshold. Panel (D) replicates panel (C) but allows the discontinuity to vary by treated cohort. Column (1) includes no additional controls. Column (2) adds controls for gender, race, poverty status, census block poverty and socioeconomic status, and eighth grade math scores. Column (3) includes those controls and high school fixed effects. Column (4) instead uses middle school fixed effects.

The estimates from the difference-in-difference specification in panel (A) imply that students scoring below the remediation threshold were 60 percentage points more likely to be remediated than students scoring at or above the threshold. Panel (B) suggests the magnitude of this impact was slightly larger in 2003 (63 percentage points) than in 2004 (57 percentage points). The regression discontinuity estimates from panel (C) suggest that students just below the threshold were 40 percentage points more likely to be remediated than students just above the threshold. Panel (D) suggests, as is clear from Figure 1, that this discontinuity was much larger in 2003 (49 percentage points) than in 2004 (31 percentage points). This motivates our decision throughout the paper to explore differential impacts by cohort. Finally, the robustness of the first stage estimates to inclusion of a wide variety of controls and school fixed effects suggest that these estimates are driven neither

by changing composition of the student body over time nor by discontinuities in any variables other than remediation itself.

Table 3 shows the impact of remediation on the freshman academic experience. All of the coefficients shown come from regressions in which remediation has been instrumented by eligibility, as in Table 2. As such, these estimates represent treatment on the treated (TOT) effects, the impact of remediation on those actually remediated. As seen in column (1) of panel (A), the most obvious impact of remediation across the entire set of remediated students was to increase the number of freshman math courses taken by one, as would be expected from the double-dose strategy. Columns (2)-(4) show that students fit this additional math course into their schedule by replacing a small number of other core academic courses (i.e., in English, social studies, or science) and a larger number of non-core academic courses (primarily fine arts, language, etc.). The result was no overall change in the number of courses taken freshman year, just a substitution of algebra for other subjects.

Columns (5) and (6) highlight the fact that remediation did not simply entail extra instructional time. The double-dose strategy as implemented by CPS increased ability tracking in freshman math classes. As a result, remediated students ended up in courses with peers whose eight grade scores were 11.2 percentiles lower than the peers of non-remediated students. Though remediated students ended up with lower ability peers, they were also in slightly more homogeneous classrooms, with their distance from their average peer's ability dropping by 1.1 percentiles. For the average remediated student, remediation lowered their peers' ability but pushed them slightly closer to the class' mean ability. Panel (B) suggests all of these results differed little by cohort.

The regression discontinuity estimates in panel (C) tell a similar story about coursework. Students remediated just below the threshold also took one additional math course, which replaced mostly non-core academic courses. Increased ability tracking did, however, affect students at the

threshold quite differently than the average remediated student from panel (A). Because students just below the threshold ended up as some of the highest ability students in their remedial classes, remediation lowered their classmates' average math score by 21.0 percentiles. Remediation also increased their distance from their mean peer's ability by 4.6 percentiles because these students had previously been near the middle of the ability distribution in their classes but now were at the top. For students near the threshold, remediation thus lowered their peers' ability and pushed them farther from the class' mean ability. Panel (D) suggests these treatment effects differed little by cohort.

Remediation thus doubled instructional time in math by replacing other coursework, lowered peer ability and change students' distance from their classmates' mean ability. In table (4) we explore whether such changes impact academic achievement, as measured by grades and standardized test scores. Panel (A) suggests that remediation raised the average remediated students' freshman algebra GPA by 0.26 points from a prior mean of 1.33. This improvement came from all parts of the grade distribution. Passing rates (i.e., receiving a D or higher) rose 6.5 percentage points from a base of 78.6 percent and the fraction of students receiving a B or higher rose 16.4 percentage points from a base of 24.3 percent. Panel (C) suggests even larger GPA effects for students near the remediation threshold, though nearly none of this comes from increased passing rates, perhaps because of the relatively high passing rates of this group of students.

Improved GPA and passing rates are positive and important outcomes in and of themselves. They may, however, be poor measures of student achievement given teachers' subjectivity and tendency to grade on a curve. Remediation might, for example, improve a student's grade not because he has learned more but because he looks better relative to his now lower ability peers. We see some evidence of this in column (4), which uses as an outcome a student's average GPA across math courses in any subsequent years he appears in our data. There is little evidence here that

remediation improves subsequent math GPA and we can certainly reject effect sizes similar to those seen in freshman algebra.

Columns (5) and (6) use as outcomes two additional measures of achievement. The first is a standardized math test that students take near the beginning of 10th grade. The second is the math portion of the ACT college entrance exam, which students take during 11th grade. Here we see some evidence of persistent impacts of remediation on achievement. In panel (A), for example, the average treatment effect of remediation is to raise 10th grade math scores by a statistically significant 0.09 standard deviations, though the effect is driven entirely by a 0.17 standard deviation improvement by the 2003 cohort. There is little evidence of improvement on the ACT in panel (A). In panel (C), the local average treatment effect of remediation is to raise 10th grade math scores by a statistically insignificant 0.05 standard deviations, though the estimate is fairly noisy. Remediation does, however, improve achievement on the ACT exam by a statistically significant 0.14 standard deviations, an impact that is roughly constant across the two treated cohorts. In total, these results suggest remediation improves freshman math grades and passing rates, has little impact on subsequent GPA, but does have longer-run impacts on standardized measures of achievement including the ACT college entrance exam.

Table 5 shows the impact of remediation on educational attainment. There is little evidence in column (1) that remediation improves the rate at which students progress to 10th grade on time. Column (2) shows, however, that the average treatment effect of remediation raises high school graduation rates by 4.8 percentage points, a more than 10% improvement from a base graduation rate of 39.3 percent. The local average treatment effect point estimate in panel (C) is also positive, but at 2.9 percentage points is statistically insignificant. Column (4) suggests that remediation also improves college enrollment rates by 3.1 percentage points, a more than 10% improvement from a base enrollment rate of 23.7 percent. Most this increase is due to a 2.5 percentage point increase in

the rate of two-year college enrollment, an unsurprisingly result given that remediated students have relatively low academic ability and are thus more likely to be drawn to two-year colleges. The estimate in panel (C) also suggests a positive impact of remediation on college enrollment, but at 5.3 percentage points is statistically insignificant. Interestingly, remediation a marginally significant impact on the college enrollment of students near the threshold in the 2003 cohort, but this effect is not apparent for the 2004 cohort.

V. Robustness and Heterogeneity Analysis

Table 6 shows robustness checks for some of the central results of the previous tables. Panel (A) is the baseline difference-in-difference specification used in previous tables. Panel (B) adds to that specification controls for gender, race, poverty status, census block poverty and socioeconomic status, eighth grade math scores and high school fixed effects. Adding these controls has little impact on the central achievement estimates and slightly increases the estimated impacts of remediation on high school graduation and college enrollment rates. Panel (C) is the baseline regression discontinuity specification used in previous tables. Panel (D) adds to that specification controls for gender, race, poverty status, census block poverty and socioeconomic status, eighth grade math scores and high school fixed effects. Addition of these controls has little impact on the estimates, suggesting no presence of discontinuities in these background variables at the threshold Panel (E) includes those same controls and expands the bandwidth from 10 percentiles to 25 percentiles on either side of the threshold. The central estimates are robust to this change of bandwidth and, if anything, increase in magnitude and statistical significance.

Tables 7 through 9 explore heterogeneity of the treatment effects by race, gender and academic ability. Table 7 suggests that black and Hispanic student generally benefitted in quite similar ways from remediation, though white students (fewer than 10% of the sample) actually had lower achievement as a result of the policy. Table 8 suggests few differences by gender in the impact

of remediation on freshman GPA and educational attainment, although the difference-in-difference estimates on math scores do vary by gender.

Panel (A) of Table 9 interacts remediation (and its instruments) with three levels of mathematical ability as measured by eighth grade test scores. Remediation improved the GPA of those in the 20-49th percentiles but not those in 0-19th percentiles. 10^{th} grade test scores were, however, improved similarly across these three groups. Remediation also had no differential impact on attainment across these three groups, as shown by the p-value from the test that all three coefficients are equal.

At the discontinuity, all students have equal math ability, so we instead interact remediation with a measure of eight grade reading ability, dividing the sample into those above and below the median reading ability of students at the remediation threshold. There is some indication from columns (4)-(6) that the longer-run impact of remediation on ACT scores and educational attainment was larger for lower ability students at the threshold.

Finally, Table 10 explores whether increased focused on mathematics and substitution away from other freshman coursework had any negative impacts on achievement in other disciplines. Panel (A) suggests, consistent with the educational attainment results, that remediation made students more likely to actually take the 10^{th} grade exams and ACTs. Furthermore, remediation seems to have improved their 10^{th} grades sciences scores by 0.12 standard deviations and their ACT science scores by 0.08 standard deviations. There is no statistically significant impact on a composite measure of reading and vocabulary scores in either 10^{th} or 11^{th} grade. The discontinuity estimates show no statistically significant impacts of remediation on testing rates or science or verbal achievement.

VI. Conclusion

We show long run positive and substantial impacts of remediation on college entrance exam scores, high school graduation rates and college enrollment rates. This is the first evidence we know of to demonstrate long run impacts of remediation in an American urban high school setting. Given the number of school districts that struggle with low-performing and at-risk students, the possibility that the double-dose math program improved high school graduation and college enrollment rates is extraordinarily promising.

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Figure 1: Remediation rates

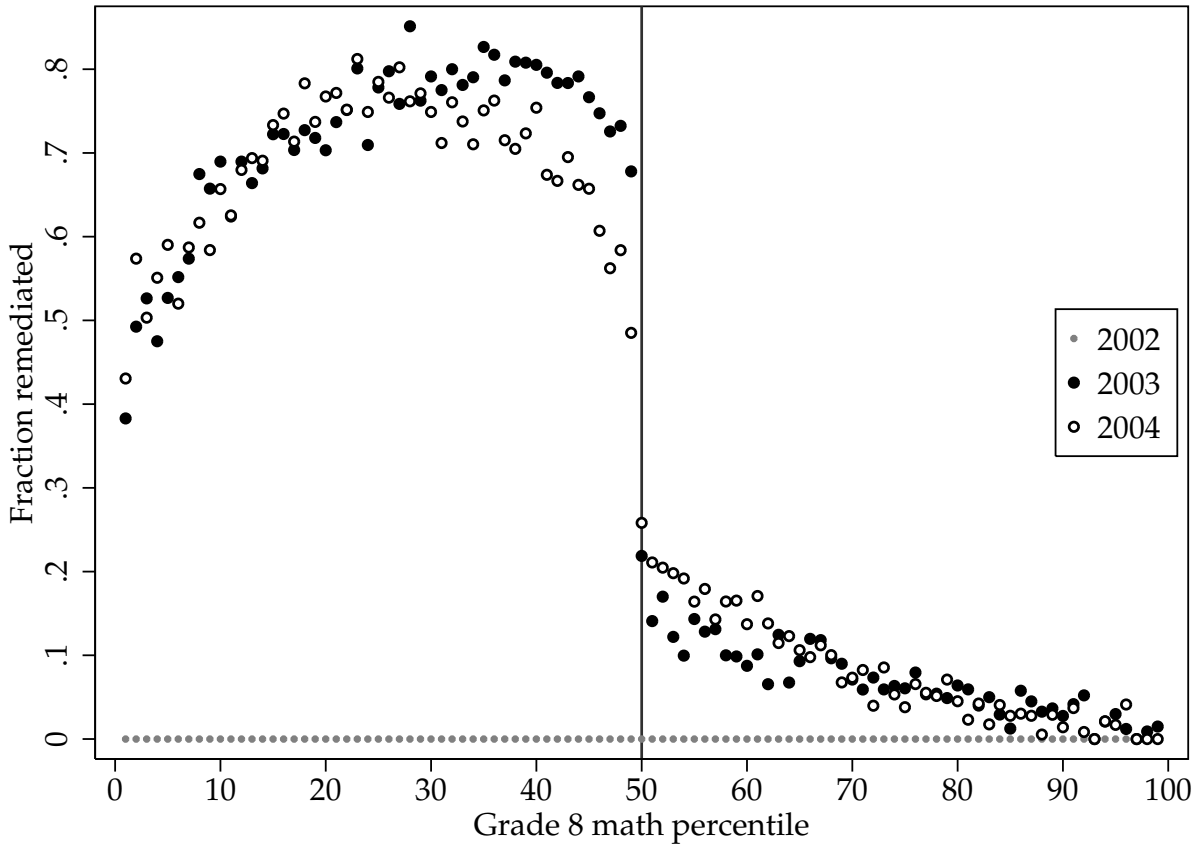


Figure 2: Freshman math courses

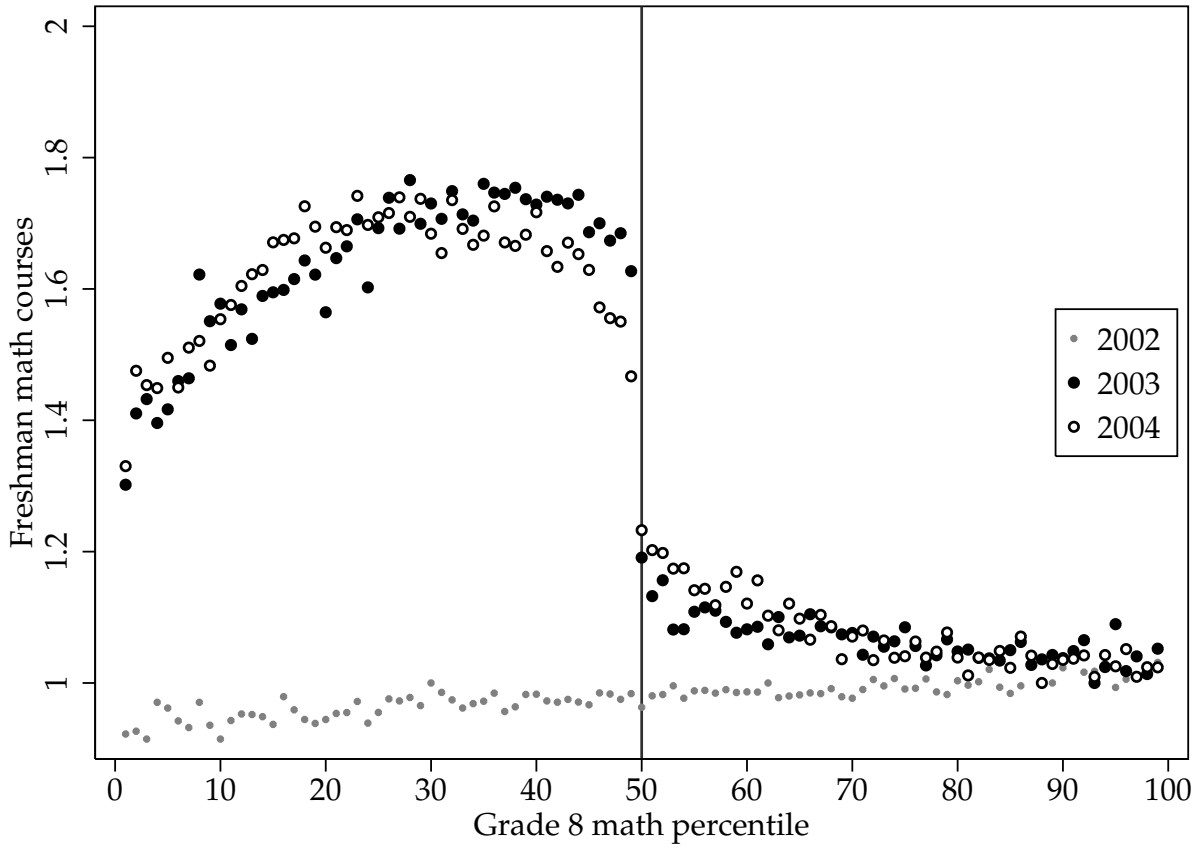


Figure 3: Peer math percentile

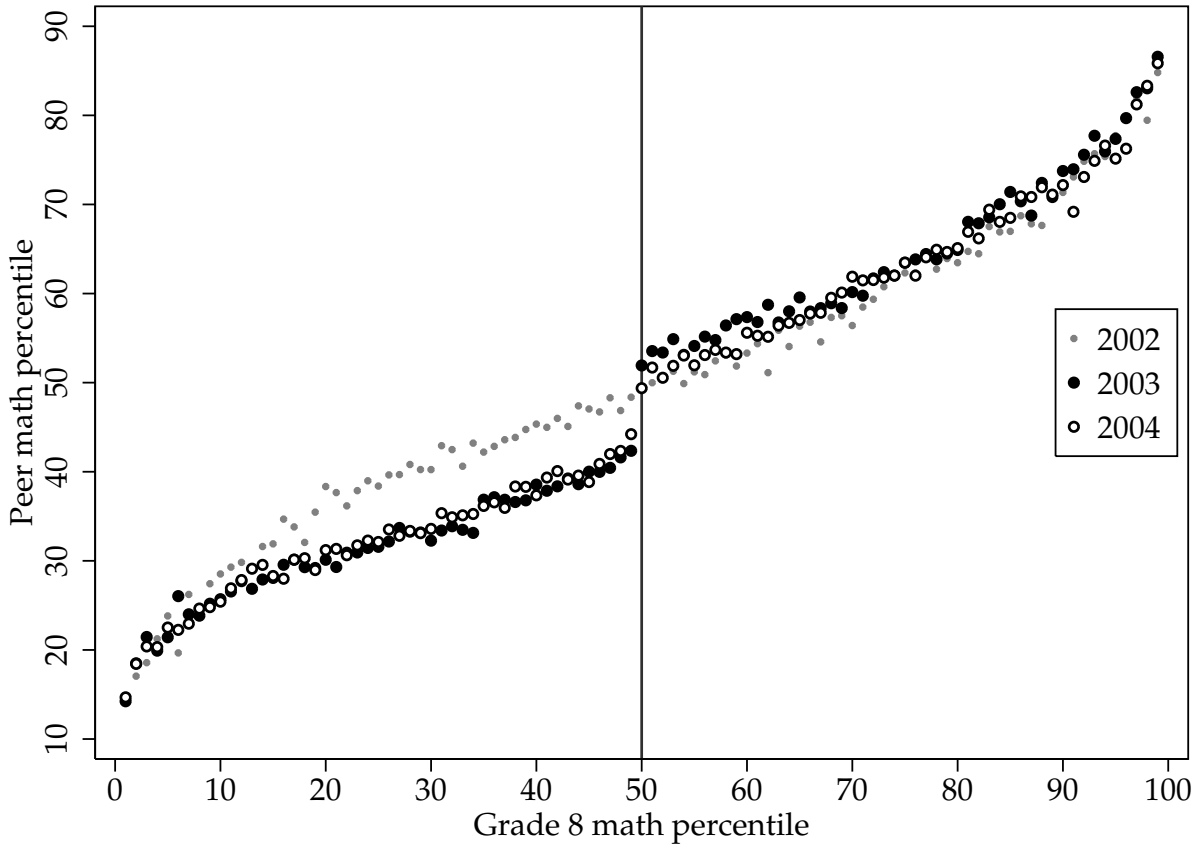


Figure 4: Freshman math GPA

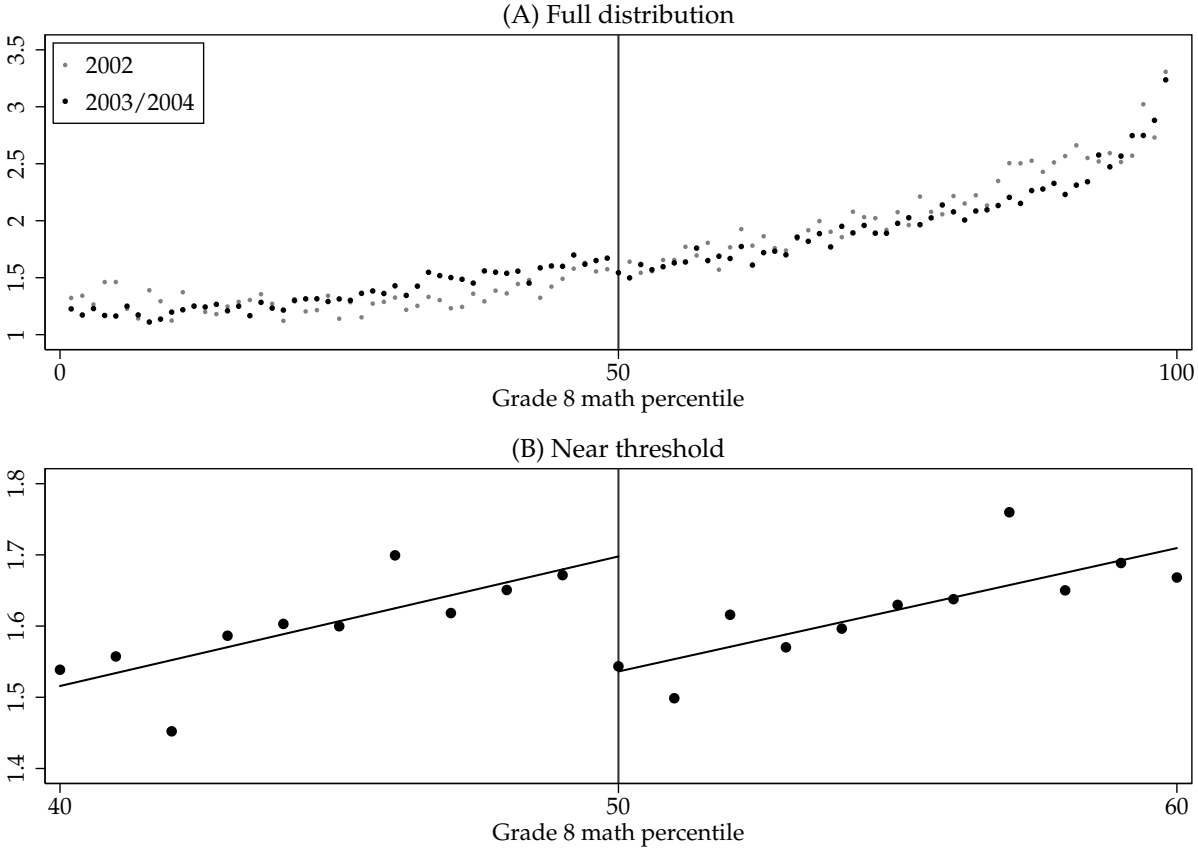


Figure 5: 10th grade math score

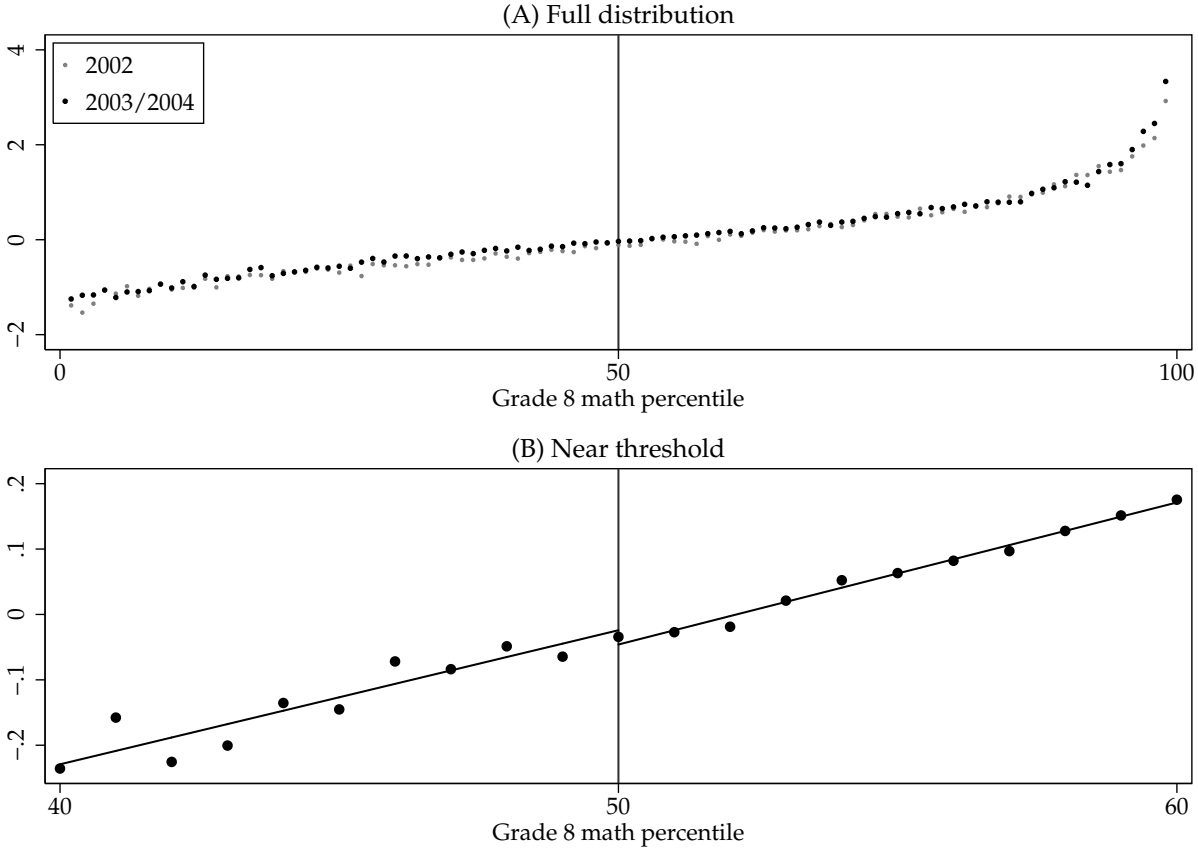


Figure 6: ACT math score

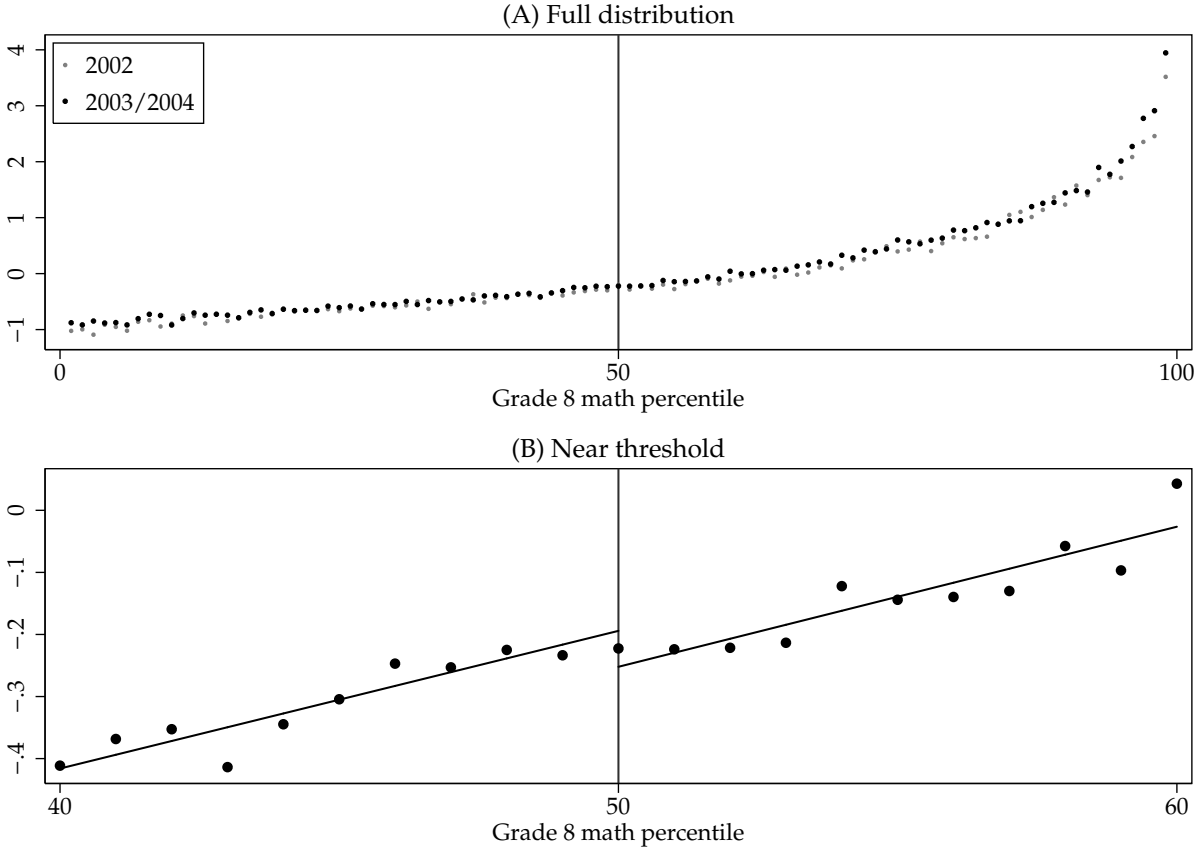


Figure 7: Graduated in four years

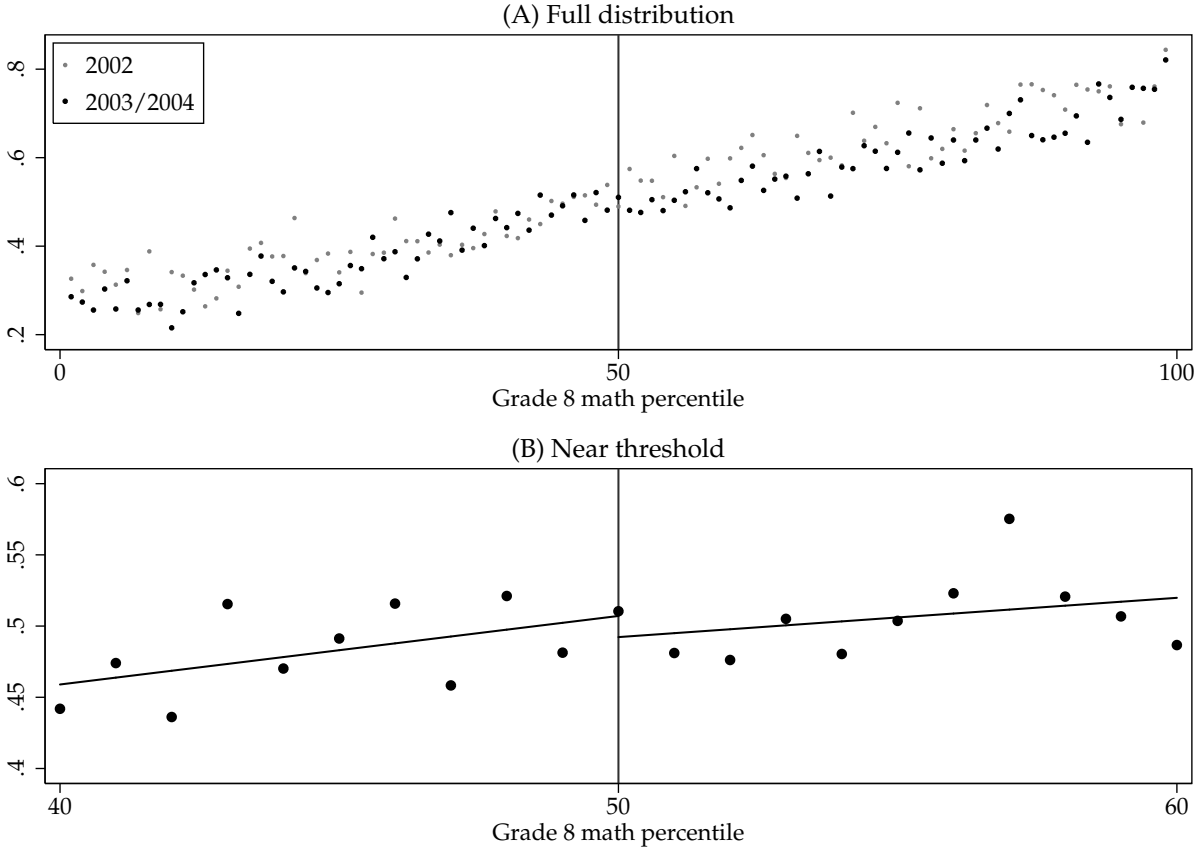


Figure 8: Any college

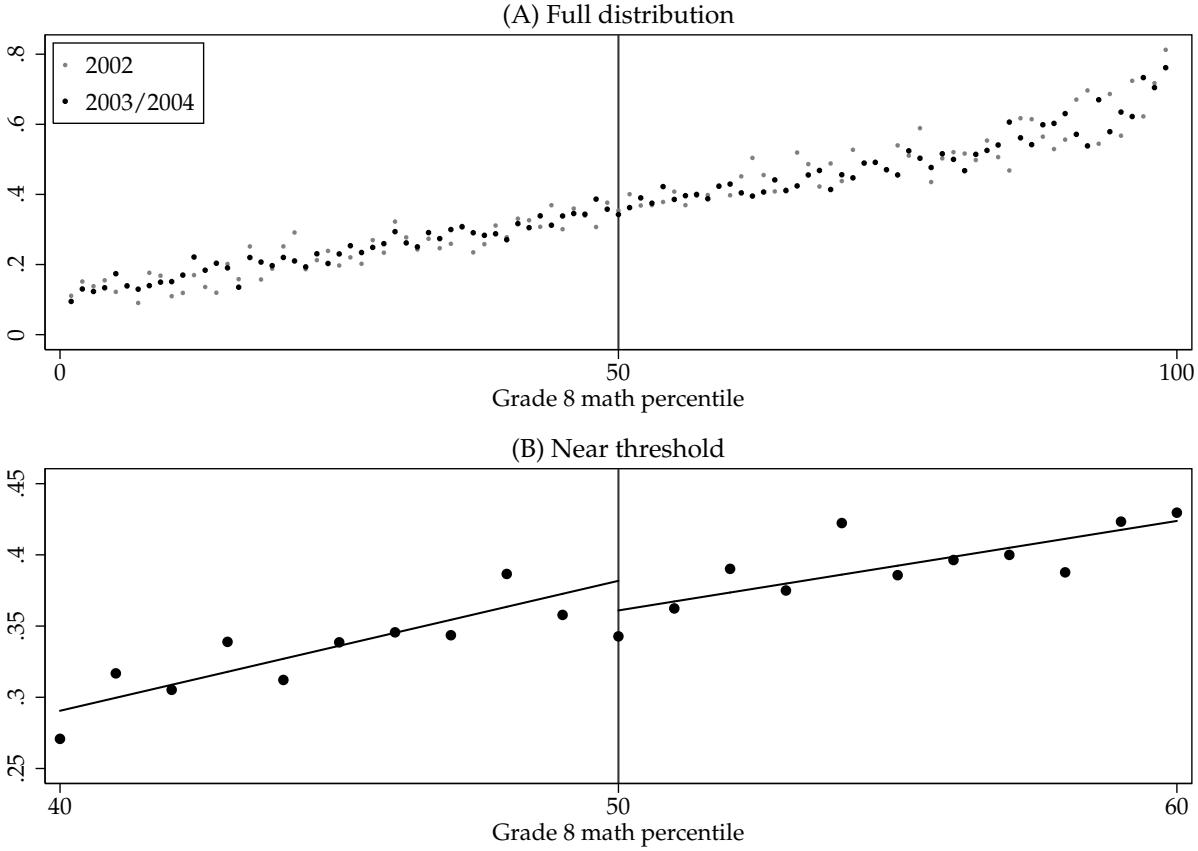


Table 1: Summary Statistics

Cohort	(1) 2002	(2) 2003	(3) 2004
(A) Remediation			
Grade 8 math percentile	47.86	45.36	45.23
Eligible for remediation	0.51	0.56	0.56
Remediated	0.00	0.44	0.42
Grade 9 math courses	0.98	1.39	1.39
(B) Achievement			
Grade 9 math GPA	1.66	1.59	1.63
Passed algebra	0.84	0.84	0.85
Math GPA $\geq$ B	0.36	0.36	0.39
Grade 10 math score	0.00	-0.00	-0.00
Grade 11 math score	0.00	0.00	0.00
(C) Attainment			
Graduated in year 4	0.51	0.46	.
Dropped out by year 4	0.22	0.22	.
Unknown status in year 4	0.29	0.33	.
Any college	0.35	0.34	0.34
Two-year college	0.18	0.17	0.16
Four-year college	0.18	0.17	0.18
N	19,384	21,887	22,302

Notes: Mean values of each variable are shown by cohort.

Table 2: Eligibility as an Instrument for Remediation

Y = remediated	(1) Baseline specification	(2) Demographic controls	(3) High school fixed effects	(4) Middle school fixed effects
(A) DD - overall				
Eligible * after	0.600***	0.594***	0.591***	0.592***
	(0.024)	(0.024)	(0.024)	(0.009)
N	63,573	63,573	63,573	63,573
(B) DD - by cohort				
Eligible * 2003	0.629***	0.623***	0.620***	0.621***
	(0.024)	(0.025)	(0.025)	(0.010)
Eligible * 2004	0.572***	0.566***	0.562***	0.563***
	(0.030)	(0.030)	(0.030)	(0.011)
N	63,573	63,573	63,573	63,573
(C) RD - overall				
Eligible for remediation	0.396***	0.395***	0.403***	0.398***
	(0.037)	(0.037)	(0.035)	(0.021)
N	11,988	11,988	11,988	11,988
(D) RD - by cohort				
Eligible * 2003	0.485***	0.485***	0.489***	0.490***
	(0.038)	(0.038)	(0.036)	(0.022)
Eligible * 2004	0.306***	0.306***	0.317***	0.307***
	(0.046)	(0.046)	(0.044)	(0.023)
N	11,988	11,988	11,988	11,988

Notes: Heteroskedasticity robust standard errors clustered by high school are in parentheses (* p<.10 ** p<.05 *** p<.01). Each column in each panel represents the first-stage regression of remediation on eighth grade math score. Panel (A) is a difference-in-difference specification with all three cohorts, and also includes eligibility and cohort indicators. Panel (B) replicates panel (A) but allows the difference-in-difference estimate to vary by treated cohort. Panel (C) is a regression discontinuity specification with the two treated cohorts. It fits a straight line using a bandwidth of 10 percentiles on either side of the threshold, and also includes cohort indicators. Panel (D) replicates panel (C) but allows the discontinuity to vary by treated cohort. Column (1) includes no additional controls. Column (2) adds controls for gender, race, poverty status, census block poverty and socioeconomic status, and eighth grade math scores.Column (3) includes those controls and high school fixed effects. Column (4) uses middle school fixed effects.

Table 3: Remediation, Coursework and Peer Ability

	(1) Math courses	(2) Academic courses	(3) Other courses	(4) Total courses	(5) Mean peer ability	(6) Distance to mean peer
(A) DD - overall						
Remediated	0.957***	-0.243***	-0.671***	0.043	-11.190***	-1.182*
	(0.014)	(0.070)	(0.083)	(0.076)	(1.440)	(0.632)
(B) DD - by cohort						
Remediated * 2003	0.941***	-0.240***	-0.651***	0.050	-11.726***	-0.685
	(0.017)	(0.068)	(0.077)	(0.074)	(1.381)	(0.628)
Remediated * 2004	0.975***	-0.248***	-0.693***	0.035	-10.611***	-1.717**
	(0.013)	(0.089)	(0.105)	(0.094)	(1.820)	(0.754)
μ	0.961	3.488	2.341	6.790	37.146	14.069
N	63,573	63,573	63,573	63,573	63,150	63,150
(C) RD - overall						
Remediated	0.970***	-0.213**	-0.707***	0.051	-21.039***	4.561***
	(0.033)	(0.094)	(0.091)	(0.092)	(1.401)	(1.316)
(D) RD - by cohort						
Remediated * 2003	0.954***	-0.207**	-0.690***	0.057	-20.469***	4.565***
	(0.032)	(0.087)	(0.089)	(0.082)	(1.316)	(1.122)
Remediated * 2004	1.001***	-0.224*	-0.738***	0.039	-22.102***	4.554**
	(0.036)	(0.124)	(0.108)	(0.125)	(1.963)	(1.883)
μ	1.211	3.496	2.259	6.967	50.687	11.362
N	11,988	11,988	11,988	11,988	11,963	11,963

Notes: Heteroskedasticity robust standard errors clustered by high school are in parentheses (* p<.10 ** p<.05 *** p<.01). Each column in each panel represents the instrumental variables regression of an outcome on an indicator for remediation, where the first-stage is given by column (1) in table 2. Panels (A) and (B) are difference-in-difference specifications with all three cohorts. Panels (C) and (D) are regression discontinuity specification with the two treated cohorts. Specifications and included controls are the same as those in column (1) of table 2.

Table 4: Course grades and test scores

	(1) GPA in algebra	(2) Passed algebra	(3) $\geq$ B in algebra	(4) Later math GPA	(5) Grade 10 math score	(6) ACT math score
(A) DD - overall						
Remediated	0.263***	0.065***	0.164***	0.043	0.087**	-0.014
	(0.061)	(0.018)	(0.023)	(0.042)	(0.040)	(0.048)
(B) DD - by cohort						
Remediated * 2003	0.267***	0.050***	0.150***	0.067	0.165***	0.041
	(0.062)	(0.019)	(0.024)	(0.045)	(0.041)	(0.052)
Remediated * 2004	0.259***	0.081***	0.180***	0.017	0.001	-0.072
	(0.074)	(0.022)	(0.029)	(0.049)	(0.048)	(0.052)
μ	1.333	0.786	0.243	1.199	-0.591	-0.588
N	63,340	63,340	63,340	56,502	43,041	35,798
(C) RD - overall						
Remediated	0.406***	0.040	0.233***	0.115	0.053	0.142**
	(0.106)	(0.031)	(0.047)	(0.090)	(0.067)	(0.067)
(D) RD - by cohort						
Remediated * 2003	0.415***	0.031	0.224***	0.152*	0.054	0.130**
	(0.104)	(0.027)	(0.046)	(0.084)	(0.058)	(0.061)
Remediated * 2004	0.389***	0.056	0.249***	0.044	0.050	0.163**
	(0.137)	(0.046)	(0.063)	(0.121)	(0.094)	(0.083)
μ	1.544	0.853	0.316	1.393	-0.034	-0.223
N	11,966	11,966	11,966	10,864	8,531	6,970

Notes: Heteroskedasticity robust standard errors clustered by high school are in parentheses (* p<.10 ** p<.05 *** p<.01). Each column in each panel represents the instrumental variables regression of an outcome on an indicator for remediation, where the first-stage is given by column (1) in table 2. Panels (A) and (B) are difference-in-difference specifications with all three cohorts. Panels (C) and (D) are regression discontinuity specification with the two treated cohorts. Specifications and included controls are the same as those in column (1) of table 2.

Table 5: Educational attainment

	(1) On time	(2) Graduate	(3) Dropout	(4) College	(5) 2 year	(6) 4 year
(A) DD - overall						
Remediated	0.028	0.048**	0.004	0.031**	0.025**	0.007
	(0.022)	(0.020)	(0.015)	(0.015)	(0.012)	(0.013)
(B) DD - by cohort						
Remediated * 2003	0.028	0.048**	0.004	0.033*	0.015	0.018
	(0.020)	(0.020)	(0.015)	(0.018)	(0.015)	(0.013)
Remediated * 2004	0.028			0.030*	0.035**	-0.005
	(0.028)			(0.018)	(0.014)	(0.016)
μ	0.702	0.393	0.277	0.237	0.156	0.081
N	63,573	41,271	41,271	63,573	63,573	63,573
(C) RD - overall						
Remediated	-0.002	0.029	-0.001	0.053	0.039	0.014
	(0.039)	(0.047)	(0.038)	(0.047)	(0.040)	(0.029)
(D) RD - by cohort						
Remediated * 2003	-0.005	0.029	-0.001	0.072*	0.035	0.037
	(0.034)	(0.047)	(0.038)	(0.041)	(0.038)	(0.024)
Remediated * 2004	0.003			0.016	0.045	-0.029
	(0.055)			(0.063)	(0.051)	(0.045)
μ	0.785	0.510	0.205	0.343	0.174	0.169
N	11,988	6,001	6,001	11,988	11,988	11,988

Notes: Heteroskedasticity robust standard errors clustered by high school are in parentheses (* p<.10 ** p<.05 *** p<.01). Each column in each panel represents the instrumental variables regression of an outcome on an indicator for remediation, where the first-stage is given by column (1) in table 2. Panels (A) and (B) are difference-in-difference specifications with all three cohorts. Panels (C) and (D) are regression discontinuity specification with the two treated cohorts. Specifications and included controls are the same as those in column (1) of table 2.

Table 6: Robustness checks

	(1) GPA in algebra	(2) Passed algebra	(3) Grade 10 math score	(4) ACT math score	(5) HS graduate	(6) Any college
(A) DD - baseline						
Remediated	0.263***	0.065***	0.087**	-0.014	0.048**	0.031**
	(0.061)	(0.018)	(0.040)	(0.048)	(0.020)	(0.015)
(B) DD - with controls						
Remediated	0.256***	0.062***	0.064**	-0.036	0.056***	0.034**
	(0.058)	(0.017)	(0.030)	(0.032)	(0.019)	(0.014)
(C) RD - baseline						
Remediated	0.406***	0.040	0.053	0.142**	0.029	0.053
	(0.106)	(0.031)	(0.067)	(0.067)	(0.047)	(0.047)
(D) RD - with controls						
Remediated	0.441***	0.036	0.022	0.119*	0.037	0.062
	(0.103)	(0.029)	(0.062)	(0.067)	(0.042)	(0.043)
(E) RD - bandwidth = 25						
Remediated	0.328***	0.051**	0.054	0.127***	0.047**	-0.006
	(0.069)	(0.020)	(0.040)	(0.038)	(0.020)	(0.022)

Notes: Heteroskedasticity robust standard errors clustered by high school are in parentheses (* p<.10 ** p<.05 *** p<.01). Each column in each panel represents the instrumental variables regression of an outcome on an indicator for remediation. Panel (A) is the baseline difference-in-difference specification used in previous tables. Panel (B) adds to that specification controls for gender, race, poverty status, census block poverty and socioeconomic status, and eighth grade math scores. Panel (C) is the baseline regression discontinuity specification used in previous tables. Panel (D) adds to that specification controls for gender, race, poverty status, census block poverty and socioeconomic status, and eighth grade math scores. Panel (E) includes those controls and expands the bandwith from 10 percentiles to 25 percentiles on either side of the threshold.

Table 7: Heterogeneity by race/ethnicity

	(1) GPA in algebra	(2) Passed algebra	(3) Grade 10 math score	(4) ACT math score	(5) HS graduate	(6) Any college
(A) DD						
Remediated * black	0.311***	0.069***	0.086**	0.051	0.057**	0.016
	(0.069)	(0.020)	(0.043)	(0.048)	(0.025)	(0.019)
Remediated * Hispanic	0.198***	0.059***	0.166**	-0.029	0.036	0.073***
	(0.076)	(0.021)	(0.071)	(0.091)	(0.025)	(0.022)
Remediated * white	-0.118	0.044	-0.622**	-1.211***	-0.010	-0.082
	(0.158)	(0.030)	(0.289)	(0.418)	(0.068)	(0.068)
N	63,340	63,340	43,041	35,798	41,271	63,573
(B) RD						
Remediated * black	0.390***	0.026	0.052	0.133**	0.029	0.048
	(0.107)	(0.033)	(0.068)	(0.066)	(0.046)	(0.049)
Remediated * Hispanic	0.417***	0.058*	0.061	0.147**	0.026	0.075
	(0.127)	(0.034)	(0.072)	(0.073)	(0.054)	(0.047)
Remediated * white	0.443	0.063	-0.014	0.190	0.076	0.021
	(0.275)	(0.068)	(0.138)	(0.159)	(0.100)	(0.089)
N	11,966	11,966	8,531	6,970	6,001	11,988

Notes: Heteroskedasticity robust standard errors clustered by high school are in parentheses (* p<.10 ** p<.05 *** p<.01). Panels (A) and (B) are respectively the baseline difference-in-difference and regression discontinuity specifications used in previous tables with further interactions by race/ethnicity, as well as black and Hispanic indicators.

Table 8: Heterogeneity by gender

	(1) GPA in algebra	(2) Passed algebra	(3) Grade 10 math score	(4) ACT math score	(5) HS graduate	(6) Any college
(A) DD						
Remediated * male	0.252***	0.045**	-0.018	-0.138***	0.046**	0.045***
	(0.064)	(0.020)	(0.047)	(0.053)	(0.022)	(0.016)
Remediated * female	0.275***	0.085***	0.171***	0.077	0.045**	0.019
	(0.063)	(0.017)	(0.039)	(0.049)	(0.020)	(0.016)
N	63,340	63,340	43,041	35,798	41,271	63,573
(B) RD						
Remediated * male	0.492***	0.054	0.091	0.147*	0.052	0.100**
	(0.118)	(0.033)	(0.081)	(0.076)	(0.051)	(0.050)
Remediated * female	0.419***	0.044	0.033	0.131**	0.030	0.042
	(0.107)	(0.032)	(0.068)	(0.067)	(0.045)	(0.044)
N	11,966	11,966	8,531	6,970	6,001	11,988

Notes: Heteroskedasticity robust standard errors clustered by high school are in parentheses (* p<.10 ** p<.05 *** p<.01). Panels (A) and (B) are respectively the baseline difference-in-difference and regression discontinuity specifications used in previous tables with further interactions by gender, as well as a female indicator.

Table 9: Heterogeneity by ability

	(1) GPA in algebra	(2) Passed algebra	(3) Grade 10 math score	(4) ACT math score	(5) HS graduate	(6) Any college
(A) DD						
Remediated * (0≤ math <20)	0.025	0.019	0.091*	0.062	0.031	0.042**
	(0.076)	(0.024)	(0.051)	(0.053)	(0.025)	(0.018)
Remediated * (20≤ math <40)	0.322***	0.087***	0.092***	-0.037	0.043**	0.027*
	(0.054)	(0.018)	(0.033)	(0.042)	(0.019)	(0.015)
Remediated * (40≤ math <50)	0.312***	0.048***	0.108**	-0.016	0.065***	0.025
	(0.054)	(0.015)	(0.043)	(0.043)	(0.024)	(0.020)
p	0.000	0.003	0.919	0.009	0.514	0.646
N	63,340	63,340	43,041	35,798	41,271	63,573
(B) RD						
Remediated * below average reader	0.443***	0.050	0.068	0.211***	0.049	0.092*
	(0.119)	(0.037)	(0.080)	(0.068)	(0.053)	(0.052)
Remediated * above average reader	0.350***	0.026	0.052	0.083	0.004	0.011
	(0.105)	(0.029)	(0.062)	(0.074)	(0.047)	(0.048)
p	0.227	0.305	0.784	0.009	0.246	0.014
N	11,961	11,961	8,530	6,969	5,998	11,983

Notes: Heteroskedasticity robust standard errors clustered by high school are in parentheses (* p<.10 ** p<.05 *** p<.01). Panels (A) and (B) are respectively the baseline difference-in-difference and regression discontinuity specifications used in previous tables with further interactions by academic ability. In panel (A), the treatment variable is interacted with three indicators for different levels of eighth grade math ability. In panel (B), the treatment variable is interacted with indicators for whether the student's eighth grade reading score was above or below the median score of students at the remediation threshold.

Table 10: Other testing outcomes

	(1) Grade 10 test-taker	(2) Grade 10 science	(3) Grade 10 verbal	(4) Grade 11 test-taker	(5) Grade 11 science	(6) Grade 11 verbal
(A) DD - overall						
Remediated	0.034*	0.117***	-0.048	0.036*	0.075**	0.041
	(0.019)	(0.044)	(0.047)	(0.020)	(0.034)	(0.048)
(B) DD - by cohort						
Remediated * 2003	0.021	0.063	-0.022	0.051**	0.098***	0.058
	(0.020)	(0.043)	(0.041)	(0.024)	(0.037)	(0.051)
Remediated * 2004	0.048**	0.178***	-0.076	0.020	0.050	0.023
	(0.022)	(0.054)	(0.060)	(0.022)	(0.045)	(0.055)
μ	0.582	-0.500	-0.528	0.478	-0.561	-0.601
N	63,573	42,894	43,180	63,573	35,764	35,801
(C) RD - overall						
Remediated	0.007	-0.040	0.027	-0.034	-0.033	0.095
	(0.042)	(0.087)	(0.082)	(0.038)	(0.089)	(0.083)
(D) RD - by cohort						
Remediated * 2003	-0.045	0.114	0.051	-0.030	0.100	0.178*
	(0.042)	(0.100)	(0.085)	(0.042)	(0.084)	(0.091)
Remediated * 2004	0.102	-0.310	-0.015	-0.041	-0.253	-0.042
	(0.090)	(0.192)	(0.143)	(0.087)	(0.180)	(0.161)
μ	0.691	-0.042	-0.065	0.570	-0.164	-0.122
N	11,988	8,506	8,549	11,988	6,968	6,970

Notes: ???