Abstract Title Page

Title:

College-Preparatory Curriculum for All:
The Consequences of Raising Mathematics Graduation Requirements on Students' Course Taking and Outcomes in Chicago

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Background/context: There is a national movement to universalize the high-school curriculum so that all students graduate with college-preparatory coursework. The National Governor's Association (2005), for example, recommended toughening high school graduation requirements to insist on college-preparatory coursework for all students. Policy reports from ACT (2004) and the American Diploma Project (2004) advocated raising science and mathematics standards to improve alignment between secondary and post-secondary curricula. The arguments supporting the current reform movement come from several lines of research showing that students who enroll in college-preparatory coursework have better academic outcomes than students who do not do so, including research on Catholic versus public schools (Bryk, Lee, & Holland, 1993), tracking (Oakes, 2005; Gamoran & Mare, 1989; Lucas, 1999; Powell, Farrar, & Cohen, 1985), and college preparation (Adelman, 1995; Horn & Kojaku, 2001). However, the existing research is limited in its applicability to the case of a *universal mandate* where *all* schools are required to change their curricular offerings, and *all* students are required to take college preparatory coursework. Limitations include: 1) selection bias, 2) lack of generalizability to urban schools, and 3) inattention to differential effects by ability.

Selection bias may arise at both the student- and school-levels; students typically select which courses to take and schools decide which curriculum to offer to their students. Prior studies often compared outcomes between students in academic and non-academic tracks, or between schools with and without universal college-preparatory curriculum by statistically controlling for academic and demographic characteristics of students and schools. However, these statistical controls could not adjust for other differences, such as the availability of qualified teachers, principal leadership, student motivation, and parental influences. If unmeasured factors are related to students' course enrollment and later outcomes we are not able to make valid inferences about the effects of mandating universal college-preparatory curriculum.

In addition, the findings of the extant studies may not be generalizable to schools in particularly challenging urban school contexts. Curricular policies can demand substantial structural changes in schools with many low-achieving students with high enrollment in remedial courses. These schools may lack sufficient qualified staff to teach a large expansion of college-preparatory courses or resources for professional development on instructional strategies to incorporate low-skill students in college-preparatory courses. Also, in schools with high levels of failure and absenteeism, it may be difficult to effectively increase instructional rigor in a way that promotes better academic outcomes for all students[1].

A third limitation is that few studies have examined whether the policy effects differ by students' incoming abilities. Most studies of the high school curriculum have used linear controls for ability. Yet, very low-ability students may particularly have difficulties in challenging classes, and may even drop out before graduation[2]. Higher-ability students might also be adversely affected by a college-prep policy if teachers modify content and pacing to accommodate low-ability students who would otherwise have been in separate remedial classrooms.[3]

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1 In Chicago, where we base this study, 25 percent of students failed their math course prior to the policy, and students averaged over three weeks of course absence per semester.

2 One prior study found that students at or below the 20th ability percentile benefited less than more able students from taking college-prep classes on a test of math achievement in grade 10 (Gamoran & Hannigan, 2000).

3 Rosenbaum (1999) documents this struggle in a school that attempted to de-track its curriculum.

Purpose/objective/research question/focus of study: This study evaluates a curriculum policy that required college preparatory coursework for all students, using data from Chicago Public Schools (CPS). Beginning in 1997, CPS ended remedial coursework and required all students to complete a college-preparatory course sequence for graduation. In this study, we constrain our analysis to the effects of requiring students to begin high school taking Algebra, rather than remedial math. We examine changes in the extent to which students received credit in algebra in ninth grade, their ninth grade math grade, math test scores, and credits in higher-level math in later years.[4]

The way in which the policy was implemented provides an ideal opportunity to address the limitations of prior research. The fact that the Chicago reform applies to all students in all schools and brought about an abrupt change in course enrollment allows us to deal with selection bias. In addition, this study illustrates how the curriculum policy affects students in a challenging context where many students and schools would not ordinarily take/offer college preparatory courses, many of which struggle with low achievement and weak instructional capacity.

We address three research questions. The first question is concerned with school adherence to the policy: 1) To what extent did enrollment in college-preparatory courses increase as a result of the policy? The second question examines the effect of taking one type of class versus another (college-prep vs. remedial) on students' academic outcomes in math: 2) Did students' academic outcomes improve by taking college-prep instead of remedial classes, and did the effects differ by students' incoming abilities? The third question examines the overall policy effects. The overall policy effects not only depend on whether a given student enrolled in college-prep instead of remedial math, but also on his/her likelihood of taking a college-prep class in the absence of the policy. For example, among average-ability students, taking Algebra I instead of remedial math might greatly affect their math grades; however, because few students with average ability would have taken remedial math in the absence of the policy, the total policy effect on these students would be small. Furthermore, the policy could have affected students' outcomes in ways other than changing their enrollment, such as by affecting climate and instruction in college preparatory classes. These effects could accrue to students whose course enrollment was not affected by the policy. Thus, we also ask: 3) What were the overall effects of the policy on students' academic outcomes?

Setting: Chicago has the third-largest school system in the United States. The student population is about 50% African-American, 38% Latino, 9% White, and 3% Asian. Approximately 85% of students are eligible for free/reduced priced lunches. In our analyses, we include all CPS high schools in existence before and after the policy was implemented (n=59 schools).

Population/Participants/Subjects: The study population consists of 11 cohorts of first-time 9th-grade students who enrolled in CPS high schools between 1994 and 2004. The cohorts range in size from 21,587 students in 1997 to 26,197 students in 2004.

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[4] In other work, available from the authors, we examine the broader policy which ended remedial coursework in English, and specified coursework in a number of different subjects beyond the freshman year

Intervention/Program/Practice: The intervention under the current study is the curriculum policy mandating college-preparatory coursework for all students. In 1997 the Chicago Public Schools (CPS) raised graduation requirements requiring Algebra I in the ninth grade, followed by Geometry and Algebra II in the subsequent years. Prior to the policy, students were required to complete two to three years of mathematics in any subject and many students began high school with remedial coursework (e.g., pre-algebra or general mathematics).

Research Design: Quasi-experiment, interrupted time series, combined with within-cohort comparisons.

Data Collection and Analysis: This study draws from multiple data sources provided by CPS. Course transcripts and semester grade files contain information on students' course enrollment (e.g., subject names, subject specific course codes, course absences, and course grades). They were used to identify college-preparatory course enrollment (Algebra I, Geometry, and Algebra II or higher), course absences, and grades. Administrative records contain student demographic information, including gender, age, race/ethnicity, special-education status, residential mobility. Student socio-economic variables were constructed using the 2000 U.S. census block-level data linked to students' home addresses. We used the data on the Iowa Tests of Basic Skills to measure students' incoming abilities. High school achievement test scores come from the Tests of Academic Proficiency (TAP), given at the end of the ninth grade (see Table 1 for details).

We operationalize the policy intervention at the school level in two ways. Our first indicator—used to address Research Question 2—captured the degree to which course enrollment changed for students with different incoming ability levels. We first divided students into four groups by their incoming ability levels. We then computed the proportion of students enrolled in college-prep courses pre-policy in each ability group for each school, and then computed the change in enrollment between pre- and post-policy periods. The second indicator—used to address Research Question 3—was a dummy-coded variable of whether or not the school was affected by the policy. We considered schools that enrolled at least 25% of their lowest-ability students in remedial coursework pre-policy as influenced by the policy (coded 1), whereas those that already enrolled 75% or more of their lowest-ability students pre-policy were coded 0, as they were largely unaffected by the policy—all (or almost all) of their students would have taken college preparatory courses in the absence of the policy[5].

In the analysis on student outcomes, we use an interrupted time series design, combined with within-cohort comparisons to isolate the enrollment/policy effects from the effects of other cohort changes on student outcomes. An interrupted time serried design takes an advantage of the fact that there was a clear shift in college-preparatory course enrollment when the policy was implemented in 1997. Thus, if the policy had an effect, we should observe a shift in the outcome during the same periods.

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[5] We only use lowest-ability students for this definition because these students would be enrolled in remedial classes if they were available at the school. Including higher ability students would confound our definition, as it would not only depend on whether schools offered remedial classes, but also on what proportion of students in the school were low-ability. All schools had sufficient students in the lowest-ability category on which to base this definition.

One disadvantage of this design is that it could lead to false conclusions about the enrollment/policy effects if other policy or programmatic changes affected student outcomes during the same period. Fortunately, the way that schools structured their course offerings pre-policy provided a natural comparison group of CPS schools that were not affected by the curriculum policy. Our analysis of ninth-grade course enrollment showed considerable variability across all types of schools in pre-policy remedial course enrollment among students with the same ability levels.[6] Given student ability levels, only schools that enrolled their students in remedial courses pre-policy were affected by the mandate to end remedial coursework, while all schools would be affected by other CPS policies. Thus, to estimate the policy, we used a difference-in-difference approach; we compared post-policy *changes* in students' outcomes between schools that were affected by the curriculum policy and schools not affected by this policy—the comparison schools serve as a control for other reforms occurring simultaneously.[7] See Appendix B for statistical models.

Findings/Results: *The policy effects on Course Enrollment.* Once the curriculum policy mandated college-preparatory courses in 1997, a large shift occurred in Algebra I course enrollment; virtually all CPS ninth graders were enrolled in Algebra I immediately with the policy (Figure 1). The policy most strongly influenced low-ability students, but had almost no effect on the coursework of high ability students. Surprisingly, once students' incoming abilities were taken into account, school characteristics did not predict the degree to which schools enrolled students in remedial coursework pre-policy.[8]

Course Enrollment Effects on Student Academic Outcomes. Table 2 shows the effects of enrolling in Algebra instead of remedial math on students' academic outcomes. However, because coefficients from the statistical models are difficult to interpret we also present the results in the form of a simulation (Table 3). Table 3 illustrates the changes in academic outcomes accompanying a 20 percentage point increase in Algebra I enrollment. For simplicity, we present only the mid-policy period contrast (1998-99).

Not surprisingly, all students were more likely to earn credit in Algebra I with the policy. However, beyond gaining course credit, there were no observable benefits to enrolling in Algebra I instead of remedial math. Moreover, there were some adverse consequences for both low- and average-ability students. Math failure rates increased among low-ability students by 3.0%, and 8.9 % among average-ability students. Math grades also decreased, declining the most among average-ability students by 0.18 grade points. Absenteeism increased among average-ability students by 1.6 more days. Math test scores were unaffected by taking Algebra I, although

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6 No measured school characteristics were related to school remedial course enrollments pre-policy, including type of school (magnet, vocational, neighborhood), size, average incoming ability level, or demographic composition.

7 There was a possibility that our comparison schools (i.e., schools unaffected by the policy because they did not offer remedial coursework pre-policy) were systematically different from the treated schools (schools that increased Algebra I enrollment) in unmeasured characteristics that affected student outcomes. However, we found that the relationships between student outcomes and increases in Algebra coursework in treated schools post-policy were the same as the relationships in comparison schools pre-policy, which no systematic unmeasured differences between the treatment and comparison schools were related to the relationships being studied.

8 For example, schools' pre-policy college-pep enrollment rates for lowest-ability students were not related to school mean ability levels (see Figure 2)

it is possible that the test was not sensitive to the change in curriculum.[9] Despite increases in Algebra I completion rates the policy had few effects on later outcomes. Students in the two lowest-ability groups were slightly more likely to earn upper-level math credits beyond geometry, but not beyond Algebra II. Even though post-policy students could potentially take up to pre-Calculus, because they started Algebra I in ninth rather than tenth grade, they were not more likely to do so.

Overall Effects from the Policy. The pattern of overall policy effects is similar to that of enrollment effects (Table 4 and Table 5). However, the lowest-ability students' academic outcomes were most strongly affected by the policy. This is reasonable because the policy most strongly affected their course enrollments. In comparison, average-ability students were less likely to change their enrollment as a result of the policy because few were taking remedial courses pre-policy. Thus, overall policy effects were smaller for average-ability students than low-ability students. In general, post-policy students in the two lower-ability groups were more likely to earn Algebra I credits than their pre-policy counterparts. However, failures for lowest-ability students increased by 7.4 percentage points post-policy. Average-ability students were absent more often by 3.14 days, and the lowest-ability students' math GPAs declined by .15 points. The policy had few effects on math test scores, or advanced math course enrolment.

Conclusions: Changing requirements led to more students taking and receiving credit in rigorous-sounding courses, but grades suffered slightly and later course-work were unaffected. Thus, most of the benefits of the "College Prep for All" policy suggested by the extant research were unrealized in Chicago contexts. We offer several explanations for these disappointing results.

First, prior research was limited in its applicability to a universal mandate and affected by selection bias. Students who completed rigorous course sequences in the absence of the policy were those whose families had selected particular schools for them to attend, those who were particularly motivated within their schools, and those who performed well in earlier grades. Their outcomes were strong for a number of reasons, not just because they enrolled in rigorous coursework.

Second, the policy focused only on curricular content, but instructional quality and classroom climate may matter at least as much as content. Policies that focus on curriculum often fail to recognize that curricular changes will place new demands on teachers, and affect the ways that students are grouped in classes. These changes could have additional effects on students' outcomes, beyond any effects of changing curricular content. It seems likely that teaching courses with high-level content to students without a record of high-level performance requires substantial changes in the process of instruction. In addition, classroom composition became more heterogeneous in college-preparatory courses, and de-tracking itself could have resulted in instructional difficulties.

Finally, content may matter little if students are not engaged in their coursework. Pre-policy, the average ninth-grade math grade in CPS was below a C, and for students with very low abilities,

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9 Only seven of 48 questions on the TAP exam test Algebra knowledge.

the average grade was a D+. These disturbingly low grades did not improve post-policy. If students are earning Ds in their courses, can we really expect the content that they are barely learning to matter? As long as students continue to be minimally engaged in their courses and to attend school irregularly, we should not expect substantial improvements in learning.

Appendix A. References

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Appendix B. Statistical Models

Analyses of enrollment/policy effects on academic outcomes use three-level hierarchical models, with students nested within cohorts within schools. The student-level model to estimate the outcome Y for student *i* in cohort *j* in school *k* is written as:

$$Y_{ijk} = \pi_{1jk}(\text{ability level1})_{ijk} + \pi_{2jk}(\text{ability level2})_{ijk} + \pi_{3jk}(\text{ability level3})_{ijk} + \pi_{4jk}(\text{ability level4})_{ijk} +$$

$$\sum_{p=1}^{P} \pi_{4+pjk}(X_{ij}) + e_{ijk},$$

where X is a vector of student-level control variables (incoming ability, race, mobility, age, etc.).

This model does not include an intercept; cohort effects are estimated at each ability level independently. The first four coefficients (π_{1jk}, π_{2jk}, π_{3jk}, and π_{4jk}) provide the mean outcome (e.g., test score, course failure, college enrollment) for students in each cohort in each school at each ability level, controlling for individual background characteristics ($X_{1\ldots p}$). At the cohort-level, we specify these means as a function of cohort year, controlling for the academic composition of students in that school in that cohort. For each ability level *m*:

$$\pi_{jk} = \beta_{m0k} + \beta_{m1k}(\text{early post-policy})_{jk} + \beta_{m2k}(\text{mid post-policy})_{jk} + \beta_{m3k}(\text{late post policy})_{jk} +$$

$$\beta_{m3k}(\text{cohort average latent ability})_{jk} + r_{mj.}$$

The intercept β_{m0k} represents the average *pre-policy* outcome at ability level *m* in school *k*, and the coefficients $\beta_{m1k,}$ $\beta_{m2k,}$ and β_{m3k} represent the *change* in the average outcome for students in ability group *m* at each school from pre-policy to the respective post-policy period. If there was no policy effect, these coefficients should be equal to zero. If the policy had an effect on the outcomes of students in ability group *m*, these coefficients should be different than zero.

At the school-level, we estimated the average pre-policy outcomes (β_{m0k}) and the average post-policy change in outcomes ($\beta_{m1k,}$ $\beta_{m2k,}$ and β_{m3k}) as a function of school characteristics. Schools in which few students in a given ability group enrolled in college prep courses pre-policy should have shown more change in outcomes for that ability group than schools where almost all students in the group enrolled in college prep courses before the policy. (If all of the students of that ability already enrolled in college prep courses pre-policy then the policy should have had no effect.)

In the analysis of enrollment effects, we included a variable at the school level representing the degree to which college prep course enrollment changed for students in that ability group in that school compared to pre-policy levels. Initial models included variables representing school characteristics, but these variables were removed for parsimony as they did not change the estimates of policy effects:

$$\beta_{m0k} = \gamma_{m00} + \gamma_{m01}(\text{\% college-prep enrollment pre-policy for group m})_k + u_{00k}$$
$$\beta_{m1k} = \gamma_{m10} + \gamma_{m11}(\text{change in \% college-prep enrollment post-policy for group m})_k$$
$$\beta_{m2k} = \gamma_{m20} + \gamma_{m21}(\text{change in \% college-prep enrollment post-policy for group m})_k$$
$$\beta_{m3k} = \gamma_{m30} + \gamma_{m31}(\text{change in \% college-prep enrollment post-policy for group m})_k$$

In the above models, the intercept γ_{m00} represents average pre-policy outcome for students in ability group *m* in schools with 100% pre-policy college-preparatory course enrollment (i.e., schools that did not have to change course enrollment for students in ability group *m* with the policy because all students had already enrolled in college preparatory courses). Post-policy intercepts (γ_{m10}, γ_{m20}, and γ_{m30}) represent, respectively, the average early, mid, and late post-policy changes in outcomes for schools with no changes in remedial/college-prep enrollment. The coefficients of interest are γ_{m11}, γ_{m21}, and γ_{m31}; these represent the extent to which changes in college preparatory course enrollment were associated with changes in academic outcomes in early, mid, and late post-policy periods, respectively, for students at each ability level. If enrolling in college preparatory courses instead of remedial courses affected students' outcomes, we should see that schools that increased college preparatory enrollment with the policy made greater changes in students' outcomes than schools unaffected by the policy. The numbers in Table 2 are based on the coefficients γ_{m11}, γ_{m21}, and γ_{m31}.

The analyses of total policy effects are similar to those of enrollment effects, but instead of using change in enrollment by ability group as the key independent variable, we use a dummy variable indicating whether the school was affected by the policy. Schools are considered to have been affected by the policy if they enrolled at least 25 percent of their lowest-ability (group 1) students in remedial courses pre-policy. (Schools did not enroll average-ability students in remedial classes unless they also enrolled low-ability students in remedial classes. If a school already enrolled almost all of very low ability students in college preparatory courses pre-policy, it would not be substantially affected by the policy—its students took college preparatory courses in the absence of the policy.) The coefficients of interest are the same as in the previous analyses, but they represent the total effect of the policy on schools that did not already enroll their lowest-ability students in college preparatory courses prior to the policy.

Schools with many low-ability students would likely experience more course programming changes and demands on school capacity than schools with few low-ability students, and the degree of change in the school might affect all students' outcomes. Therefore, we also included a variable for the percent of low-ability students in the school, and an interaction of the percent low-ability with whether the school was affected by the policy. The numbers in Table 3 are based on the coefficients γ_{m11}, γ_{m21}, and γ_{m31} from models that use the dummy variables for whether a school was affected by the policy rather than the percentage change in college preparatory enrollment.

Appendix C. Tables and Figures

Table 1. Variable descriptions

Student level variables	
9th grade outcomes	
Credit received	A dummy variable indicating a full-yr credit in college-prep math
GPA	Average math GPA on a 4 point scale
Course failure	A dummy variable indicating whether students failed math courses
Course absence	Number of days absent per math course, including course cutting
Test scores	Math test scores measured in 9th grade spring semester (TAP)
End of HS outcomes	
Higher math credit	1) Dummy variables indicating whether students received post geometry credits 2) Dummy variables indicating whether students received post algebra II credits
Control variables	
Student ability	Two variables constructed using latent ITBS math scores measured in the 8th grade spring term (standardized across all cohorts with a mean of 0 and SD of 1): 1) A set of dummy variables indicating student ability levels—Four levels with level 1 being the lowest and level 4 being the highest level; 2) A set of math scores for each ability group centered on the lowest scores of that group and students in other ability groups having a value of zero
Special education	A dummy variable with 1= special education students and 0=otherwise
Gender	A dummy variable with 1=male and 0=female
Race/Ethnicity	A set of dummy variables indicating African-American (ref. group), Asian, Hispanic, and White.
SES	Two variables constructed from the U.S. census data on students' residential block groups: 1) Concentration of poverty (a composite of male unemployment rate and % families under the poverty line. 2) Social status (a composite of the median family income and the average educational attainment. Both were standardized to have a mean of 0 and SD of 1.
Mobility	A set of dummy variables: no moves, one move, two or more moves in the 3 yrs before high school
Age at HS entry	1) Number of months old for high school 2) a dummy variable indicating if students are slightly old 3) a dummy variable indicating if students are young for starting high school
Cohort-level variables	
Post-policy periods	A set of dummy variable distinguishing a pre-policy period (ref. category) and three post-policy periods (period 1=1997, period 2=1998-2000 for and period3=2001-2002
School-level variables	
Pre-policy college prep enrollment	Average percentage of pre-policy students enrolled in college-prep math courses
Post-policy changes in college prep enrollment	Changes in % students enrolled in college-prep math (pre-policy average enrollment is subtracted from post-policy average enrollment)
% special education	Percent of students receiving special education services
School ability compositions	A set of dummy variables distinguishing four groups of schools, based on the average incoming ability in the school and the standard deviation of ability in the school: high mean-high heterogeneity; high mean-low heterogeneity; low mean-high heterogeneity; low mean-low heterogeneity
School size	Dummy variable indicating large school (over 1800 students)
School type	Dummy variables indicating vocational schools, magnet school

Tables 2. Change in enrollment predicting students' math outcomes by ability levels
(Results from statistical models: Coefficients are used for Table 3)

Table 3 uses the coefficients indicated under "Mid Post-Policy," "Change in %".[1]		Algebra I or Higher Credit (in logits)	Math Course Failure (in logits)	Math Course Absences (in log odds)	Math GPA	Math Test Scores
Lowest Ability						
Pre-Policy	Intercept	-1.78 **	-0.06	3.03 **	0.81 **	16.55 **
	% enrolled	0.26 **	0.03 ~	-0.01 **	-0.02 *	0.23 *
Early Post-Policy	Intercept	0.31 **	0.33 **	-0.08 **	-0.26 **	1.62 *
	Change in %	0.25 **	0.01	0	-0.02	0.24
Mid Post-Policy	Intercept	0.62 **	0.01	-0.09 **	-0.09 **	4.71 **
	Change in %	0.22 **	0.06 **	0	-0.03 **	0.17
Late Post-Policy	Intercept	0.74 **	-0.12	-0.1 **	0.07 ~	
	Change in %	0.26 **	0.07 *	0	-0.04 **	
Low Ability						
Pre-Policy	Intercept	-0.16 **	-0.88 **	2.91 **	1.26 **	30.85 **
	% enrolled	0.27 **	0.02	-0.01 ~	-0.03 ~	0.14
Early Post-Policy	Intercept	0.12	0.14	-0.11 **	-0.17 **	1.79 **
	Change in %	0.27 **	0	0.01	-0.03	-0.03
Mid Post-Policy	Intercept	0.26 **	0.03	-0.07 **	-0.13 **	3.05 **
	Change in %	0.24 **	0.08 *	0.02 ~	-0.04 *	-0.07
Late Post-Policy	Intercept	0.4 **	-0.14	-0.09 **	0.1 **	
	Change in %	0.31 **	0.03	0	-0.02	
Average Ability						
Pre-Policy	Intercept	0.44 **	-1.26 **	2.81 **	1.55 **	39.55 **
	% enrolled	0.29 **	0.07	0	-0.06 ~	-0.23
Early Post-Policy	Intercept	0.05	-0.03	-0.1 **	-0.08 ~	0.18
	Change in %	0.36 **	0.05	0.03	-0.03	-0.24
Mid Post-Policy	Intercept	0.21 **	-0.1	-0.09 **	-0.06 ~	2.06 **
	Change in %	0.19 *	0.24 *	0.05 *	-0.09 *	-0.21
Late Post-Policy	Intercept	0.14 ~	-0.08	-0.08 **	0	
	Change in %	0.20	0	0.09 *	-0.11 *	
High Ability						
Pre-Policy	Intercept	0.82 **	-1.7 **	2.73 **	1.82 **	45.97 **
	% enrolled	0.34 **	0.06	0	-0.05	-0.51
Early Post-Policy	Intercept	-0.03	0.22 **	-0.06 ~	-0.11 *	0.04
	Change in %	0.14	0.19	0.04	-0.17 *	-2.01 *
Mid Post-Policy	Intercept	0.22 **	0.04	-0.06 *	-0.07 *	2.01 **
	Change in %	0.35 **	0.12	0.02	-0.05	-1.09 ~
Late Post-Policy	Intercept	-0.08 ~	0.01	-0.08 *	-0.1 *	
	Change in %	0.48 **	-0.07	-0.06	0.02	

~p<.10 *p<.05 **p<.01: [1] The coefficients for "Change in %" indicate the change in students' outcomes associated with 10% increases in the school college-preparatory course enrollment rates post-policy

Table 2 (continued). Change in enrollment predicting students' math outcomes by ability levels (Results from statistical models: Coefficients are used for Table 3)

Table 2 uses the coefficients indicated under "Mid Post-Policy," "Change in %".[1]		Post-Geometry Credit	Post-Algebra II Credits
Lowest Ability			
Pre-Policy	Intercept	-1.94 **	-3.24 **
	% enrolled	0.11 **	0.01
Early Post-Policy	Intercept	0.76 **	-0.05
	Change in %	0.05	-0.08 *
Mid Post-Policy	Intercept	0.95 **	-0.71 **
	Change in %	0.08 **	0.03
Low Ability			
Pre-Policy	Intercept	-0.85 **	-2.49 **
	% enrolled	0.11 **	-0.13 **
Early Post-Policy	Intercept	0.54 **	-0.36 **
	Change in %	-0.01	-0.21 **
Mid Post-Policy	Intercept	0.65 **	-0.67 **
	Change in %	0.07 ~	0.04
Average Ability			
Pre-Policy	Intercept	-0.34 **	-2.03 **
	% enrolled	0.06 ~	-0.11
Early Post-Policy	Intercept	0.41 **	-0.34 *
	Change in %	0.08	-0.18
Mid Post-Policy	Intercept	0.53 **	-0.55 **
	Change in %	0.01	-0.06
High Ability			
Pre-Policy	Intercept	0.03	-1.84 **
	% enrolled	0.11	-0.06
Early Post-Policy	Intercept	0.31 **	-0.05
	Change in %	0.02	-0.73 **
Mid Post-Policy	Intercept	0.35 **	-0.23 **
	Change in %	0.05	-0.33 **

~p<.10 *p<.05 **p<.01

[1] The coefficients for "Change in %" indicate the change in students' outcomes associated with 10% increases in the school college-preparatory course enrollment rates post-policy

Table 3. Effects of 20 percentage point increase in ninth-grade college-prep math enrollment on students' math outcomes (simulated results[1])

	Effect Sizes					Natural metrics (Percentage Points, Days, Grade Points)				
Ninth Grade Math	Algebra or Higher Credit	Course Failure	Course Absences	GPA	Test Scores	Algebra or Higher Credit	Course Failure	Course Absences	GPA	Test Scores
Lowest Ability	1.11**	0.31 **	0.00	-0.25 **	0.06	8.9%	3.0%	-	-0.06	-
Low Ability	1.22**	0.41 *	0.08 ~	-0.34 *	-0.02	11.6%	3.5%	0.70	-0.08	-
Average Ability	0.96*	1.24 *	0.21 *	-0.76 *	-0.07	8.0%	8.9%	1.60	-0.18	-
The end of HS	Post-Geometry Credit	Post-Algebra II credit				Post-Geometry Credit	Post-Algebra II credit			
Lowest Ability	0.40**	0.07				3.3%	-			
Low Ability	0.35~	0.10				3.5%	-			
Average Ability	0.05	-0.14				-	-			

~p<.10 *p<.05 **p<.01

[1]This table shows the effects for mid-policy years (1998-1999) only; similar effects were observed in other post-policy periods(see Table2). Effect sizes were calculated by multiplying the mid post-policy percent change coefficient by 2 (for a 20% change) and dividing that value by the school level standard deviation in the respective outcome from the fully unconditional models. The values in the right panel were converted into their natural metric (if the coefficient was not statistically significant at p<.10 no value is displayed).

Table 4. Total policy effects on students' math outcomes by ability levels
(Results from statistical models: Coefficients are used for Table 5)

Table 5 uses the coefficients indicated under "Mid Post-Policy, School Affected by Policy".		Algebra I or Higher Credit	Course Failure	Course Absences	GPA	Test scores
Lowest Ability						
Intercept	Intercept	-1.01 **	-0.27 *	3.45 **	0.89 **	17.65 **
	% Lowest Ability Student	0.07	-0.08 ~	0.02	0.06 **	0.55 ~
	School Affected by Policy	-1.12 **	0.00	0.02	0.02	-1.00 ~
Early Post-Policy	Intercept	-0.21	0.32 *	-0.24 **	-0.22 **	1.04
	% Lowest Ability Student	-0.03	-0.02	-0.11 **	0.01	0.47 ~
	School Affected by Policy	0.88 **	0.17	0.10	-0.12	1.32
Mid Post-Policy	Intercept	0.10	0.01	-0.24 **	-0.08 ~	3.93 **
	% Lowest Ability Student	0.08 ~	-0.12 **	-0.04 *	0.03	0.49 *
	School Affected by Policy	0.73 **	0.31 *	0.09	-0.15 **	0.63
Late Post-Policy	Intercept	0.01	-0.06	-0.21 *	0.04	6.55 **
	% Lowest Ability Student	0.02	0.00	-0.02	0.00	0.00
	School Affected by Policy	1.12 **	0.20	0.05	-0.10	0.00
Avg ability	Intercept	0.05	-0.21	-0.32 **	0.14 ~	0.00
Low Ability						
Intercept	Intercept	0.36 **	-1.16 **	3.20 **	1.39 **	31.60 **
	% Lowest Ability Student	0.09 *	-0.11 *	-0.01	0.08 **	0.27
	School Affected by Policy	-0.56 **	0.07	-0.01	-0.02	-0.47
Early Post-Policy	Intercept	-0.24 ~	0.19	-0.26 **	-0.19 **	1.34 *
	% Lowest Ability Student	0.04	-0.07	-0.06 ~	0.03	0.39
	School Affected by Policy	0.50 **	0.07	0.10	-0.04	0.29
Mid Post-Policy	Intercept	-0.07	0.12	-0.18 **	-0.19 **	2.45 **
	% Lowest Ability Student	0.06 ~	-0.09 *	-0.03	0.02	0.18
	School Affected by Policy	0.30 **	0.17	0.11	-0.07	0.13
Late Post-Policy	Intercept	-0.07	0.04	-0.14 ~	-0.03	4.69 **
	% Lowest Ability Student	0.03	0.00	0.00	0.00	0.00
	School Affected by Policy	0.54 **	0.04	0.02	0.03	0.00
Avg ability	Intercept	0.24	-0.37 *	-0.40 **	0.19 *	0.00
Average Ability						
Intercept	Intercept	0.79 **	-1.55 **	3.07 **	1.70 **	40.37 **
	% Lowest Ability Student	0.07 ~	-0.12 **	-0.04	0.10 **	0.25
	School Affected by Policy	-0.26 **	0.11	-0.08	-0.08 ~	-0.25
Early Post-Policy	Intercept	-0.14	0.11	-0.19 *	-0.14 ~	-0.83
	% Lowest Ability Student	0.04	0.02	-0.07 *	0.00	0.32
	School Affected by Policy	0.19	-0.03	0.09	0.05	1.24
Mid Post-Policy	Intercept	0.03	0.03	-0.19 **	-0.16 **	1.68 **
	% Lowest Ability Student	0.04	-0.04	-0.01	-0.02	0.20
	School Affected by Policy	0.04	0.08	0.16 *	-0.01	-0.17
Late Post-Policy	Intercept	-0.04	0.10	-0.11	-0.12 ~	4.72 **
	% Lowest Ability Student	0.05	0.03	0.00	-0.02	0.00
	School Affected by Policy	0.07	0.02	0.03	0.02	0.00
Avg ability	Intercept	0.27 ~	-0.28 ~	-0.51 **	0.07	0.00
High Ability						
Intercept	Intercept	1.17 **	-2.15 **	2.86 **	2.09 **	46.90 **
	% Lowest Ability Student	0.09 *	-0.20 **	-0.05 *	0.14 **	0.50

	School Affected by Policy	-0.19 ~	0.24 **	-0.03	-0.20 **	0.25
Early Post-Policy	Intercept	-0.24 *	0.46 **	-0.13	-0.19 *	-1.27
	% Lowest Ability Student	0.01	-0.04	-0.09 *	-0.01	0.37
	School Affected by Policy	0.20 ~	-0.29 *	0.07	0.04	1.00
Mid Post-Policy	Intercept	0.00	0.25 **	-0.09	-0.28 **	0.91
	% Lowest Ability Student	0.01	0.02	-0.01	-0.04 ~	-0.06
	School Affected by Policy	-0.02	0.05	0.10	0.10	0.50
Late Post-Policy	Intercept	-0.19 *	0.37 **	-0.01	-0.37 **	6.08 **
	% Lowest Ability Student	0.03	0.02	0.01	-0.04	0.00
	School Affected by Policy	0.05	-0.01	-0.05	0.18 ~	0.00
Avg ability	Intercept	0.52 **	-1.04 **	-0.67 **	0.26 **	0.00

Table 4. (continued) Total policy effects on students' math outcomes by ability levels (Results from statistical models: Coefficients are used for Table 5)

Table 5 uses the coefficients indicated under "Mid Post-Policy, School Affected by Policy".		Post-Geometry Credit	Post-Algebra II Credit
Lowest Ability			
Intercept	Intercept	-1.79 **	-3.67 **
	% Lowest Ability Student	0.06	-0.14 **
	School Affected by Policy	-0.24 **	0.29 **
Early Post-Policy	Intercept	0.44 **	0.11
	% Lowest Ability Student	0.04	-0.09 ~
	School Affected by Policy	0.11	-0.43 **
Mid Post-Policy	Intercept	0.43 **	-0.51 **
	% Lowest Ability Student	0.03	0.12 **
	School Affected by Policy	0.13	0.00
Avg ability	Intercept	0.40 *	-1.02 **
Low Ability			
Intercept	Intercept	-1.07 **	-2.50 **
	% Lowest Ability Student	0.04 *	0.02
	School Affected by Policy	-0.10 **	0.43 **
Early Post-Policy	Intercept	0.30 **	-0.30 **
	% Lowest Ability Student	0.02	0.09 *
	School Affected by Policy	-0.02	-0.48 **
Mid Post-Policy	Intercept	0.25 **	-0.65 **
	% Lowest Ability Student	0.01	0.11 **
	School Affected by Policy	0.04	-0.09
Avg ability	Intercept	0.33 **	-0.19
Average Ability			
Intercept	Intercept	-0.79 **	-2.30 **
	% Lowest Ability Student	0.05 **	-0.01
	School Affected by Policy	-0.03	0.44 *
Early Post-Policy	Intercept	0.19 **	-0.16
	% Lowest Ability Student	0.00	0.00
	School Affected by Policy	-0.05	-0.33 ~
Mid Post-Policy	Intercept	0.15 **	-0.39 **
	% Lowest Ability Student	-0.01	0.00
	School Affected by Policy	-0.01	-0.16
Avg ability	Intercept	0.31 **	-0.17

High Ability			
Intercept	Intercept	-0.69 **	-1.67 **
	% Lowest Ability Student	0.01	0.17 **
	School Affected by Policy	0.03	0.26 **
Early Post-Policy	Intercept	0.13 **	-0.16 *
	% Lowest Ability Student	0.00	-0.01
	School Affected by Policy	-0.02	-0.15 ~
Mid Post-Policy	Intercept	0.12 **	-0.40 **
	% Lowest Ability Student	-0.01	-0.06 **
	School Affected by Policy	-0.04	-0.08
Avg ability	Intercept	0.15 **	0.69 **

~p<.10 *p<.05 **p<.01

Table 5. Total policy effects on students' math outcomes by ability levels

Ninth Grade Math	Algebra or Higher Credit	Course Failure	Course Absences in days	GPA	Test Scores[i]
Lowest Ability	8.8% **	7.7% *	2.24	-0.15 **	0.63
Low Ability	7.4% **	3.6%	2.40	-0.07	0.13
Average Ability	1.0%	1.3%	3.14 *	-0.01	-0.17
High Ability	-0.4%	1.2%	1.67	0.10	0.50

The end of HS	Post-Geometry Credit	Post-Algebra II Credit
Lowest Ability	1.0%	-0.3%
Low Ability	0.6%	-2.0%
Average Ability	-0.3%	-2.5%
High Ability	-0.9%	-1.9%

~p<.10 *p<.05 **p<.01: This table shows the effects for mid-policy years (1998-1999) only. Values were calculated by taking the difference between the pre/post policy change for schools that changed enrollment and those that did not change enrollment. The values were converted into their natural metric.

[i] normal curve equivalents

Figure 1. Percent of 9th-grade students enrolled in Algebra I or higher by ability levels and cohorts

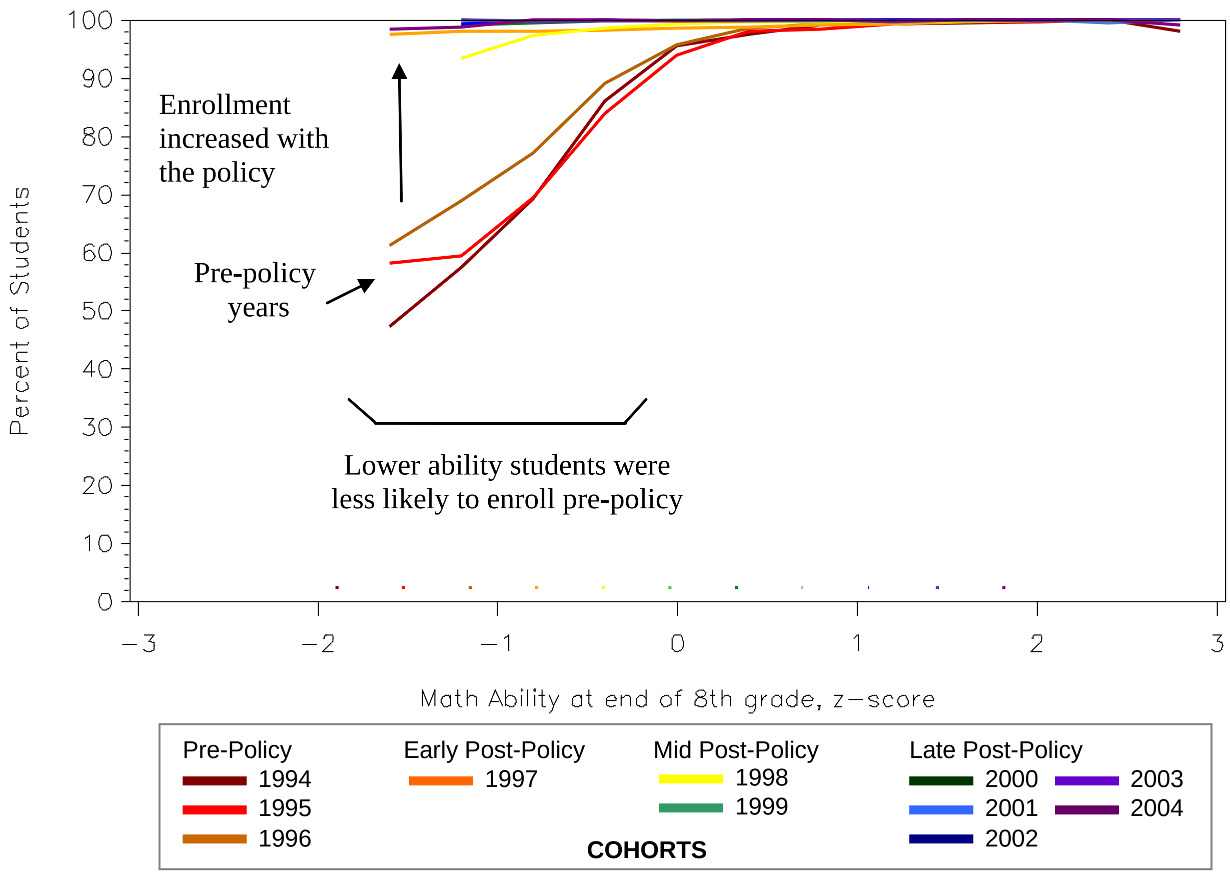

Figure 2. Schools' pre-policy percent of lowest-ability students enrolled in college-pep math courses by school mean ability (each dot represents a school)