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The Unintended Consequences of an Algebra-for-All Policy on High-Skill Students: Effects on Instructional Organization and Students' Academic Outcomes

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In 1997, Chicago implemented a policy that required algebra for all ninth-grade students, eliminating all remedial coursework. This policy increased opportunities to take algebra for low-skill students who had previously enrolled in remedial math. However, little is known about how schools respond to the policy in terms of organizing math classrooms to accommodate curricular changes. The policy unintentionally affected high-skill students who were not targeted by the policy—those who would enroll in algebra in its absence. Using an interrupted time-series design combined with within-cohort comparisons, this study shows that schools created more mixed-ability classrooms when eliminating remedial math classes, and peer skill levels declined for high-skill students. Consequently, their test scores also declined.

Keywords: *curricular reform, high school, algebra, social organization of classrooms, quasi-experimental design*

A recent report by the Mathematics Advisory Panel (National Mathematics Advisory Panel, 2008) referred to algebra as a "gateway" to later achievement. To address low academic performance in algebra, an increasing number of states and school districts have begun requiring algebra for all students in ninth grade or earlier. This policy is supported by a large body of research, which shows that students who enrolled in more rigorous coursework have better academic outcomes than students who did not do so. This includes research on Catholic versus public schools (Bryk, Lee, & Holland, 1993; Lee & Bryk, 1988), constrained curriculum (Lee, Croninger, & Smith, 1997), opportunities to learn (McKnight et al., 1987; Schmidt, Wang, & McKnight, 2005; Westbury, 1992), tracking (Gamoran & Mare, 1989; Lucas, 1999; Oakes, 2005), and curricular requirements (Attewell & Domina, 2008; Chaney, Burgdorf, & Atash, 1997; Gamoran & Hannigan, 2000).

However, these studies typically compare outcomes among students who took different coursework, such as algebra versus remedial math, and such comparisons do not necessarily inform us about the consequences of a systemic reform—a districtwide reform that mandates that schools change their curricular offerings. In particular, prior research has not considered potential difficulties of universalizing algebra in terms of organizing instruction effectively to meet the needs of all students who enter high school with diverse academic skills while providing a rigorous curriculum for all students (McPartland & Schneider, 1996).

To deal with the problem of students' diversities, U.S. high schools have historically used tracking; schools used differentiated curriculum and organized instruction according to students' skills, interests, and future occupational paths (Powell, Farrar, & Cohen, 1985). However, the problem of instructional organization has been largely absent from current policy conversations about universalizing academic curriculum. Consequently, we know little about how an algebra-for-all policy may affect the way in which schools organize classrooms for teaching algebra and how this, in turn, affects students' outcomes. Such compositional changes would affect all students, even high-skill students who are not targeted by the policy—those who would enroll in algebra regardless of the policy.

How the Curricular Policy Could Affect the Outcomes of High-Skill Students

The algebra-for-all policy intends to improve the outcome of low-skill students by expanding opportunities to take algebra to those who would not otherwise take such a course. However, the curricular policy may also affect students' outcomes if it, both intentionally and unintentionally, induces changes in instructional organization to accommodate these curricular changes. For example, when eliminating remedial math, schools may incorporate low-skill students, who would normally take remedial classes, in the same algebra classes with higher skill students. Then, the peer skill levels in algebra classes would decline.

The extant research on tracking and detracking does not distinguish between curriculum structure (e.g., offering tracked curriculum vs. nontracked curriculum) and instructional organization (e.g., homogeneous vs. heterogeneous grouping); tracking means a differentiated curriculum and organizing instruction based on students' skills, whereas detracking refers to offering, in principle, the same rigorous content in heterogeneous settings. Thus, the effect of tracking compared to detracking would be the combined effects of the course students take and classroom peer composition.

On the whole, tracking affects students' learning because it affects instruction given in the classroom. Although the curricular structure of the school and instructional organization both shape classroom instruction, it is useful to distinguish these two concepts to understand the mechanisms through which the algebra-for-all policy affects different subgroups of students. The algebra policy would affect the outcomes of low-skill students who are targeted by the policy—those who would take remedial math in the absence of the policy—through curricular changes as well as changes in instructional organization (Allensworth, Nomi, Montgomery, & Lee, 2009; Hong & Nomi, forthcoming). The policy would also affect the outcomes of students who are not targeted by the policy—high-skill students who would take algebra regardless of the policy—primarily through changes in instructional organization. The policy effects on these high-skill students are the focus of the current study.

Changes in the organization of instruction are likely to affect student learning even when the curriculum is the same because classroom academic composition not only shapes instruction (e.g., what content is taught, the amount of time spent on given content, difficulty, and pacing) but can influence learning climates and expectations (e.g., see Barr & Dreeben, 1983; Gamoran, 1986, 1987; Oakes, 2005; Page, 1991). For example, students in high-ability classrooms often are taught more challenging content, have higher teacher expectations placed on them, and experience less disruptive environments than students in low-ability classrooms. Thus, detracking tends to have negative consequences on high-skill students due to declines in peer ability levels especially when additional instructional supports are not provided to low-skill students to help them learn more rigorous content. For example, in detracked high schools, high-performing students are likely to become bored and disaffected in classes with lower skill peers (Loveless, 2009; Rosenbaum, 1999). Also, Hoffer (1997) suggests that expanding algebra to low-skill students would diminish the benefit of taking algebra, likely because it leads to a dilution of algebra curriculum as the class becomes more diverse.

The purpose of this study is to understand unintended consequences on high-skill students of a policy that expanded opportunities to take algebra for low-skill students. Specifically, the study focuses on the role of academic composition as a mechanism of the policy effects. Three research questions are addressed:

> *Research Question 1:* To what extent did the algebra-for-all policy in Chicago affect classroom academic composition?

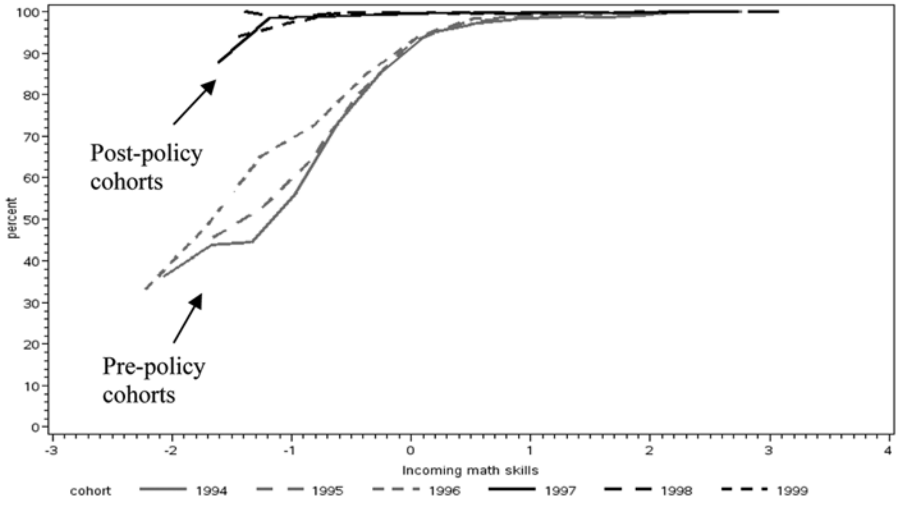

FIGURE 1. *Percentage of students enrolled in algebra by incoming ability: Pre- and Post-policy cohorts.*

Research Question 2: For high-skill students who were not targeted by the policy, how did the policy affect math achievement through changes in academic composition in algebra classrooms?

Research Question 3: What was the effect of academic composition on math achievement among high-skill students?

It is hypothesized that during pre-policy years, classroom peer ability levels for high-skill students were higher in schools that offered remedial math than in schools that did not offer remedial math. This is because schools with remedial math would enroll high-skill students in algebra classes together with other high-skill students, whereas lower skill students would take remedial math together with other low-skill students. Thus, test scores would be higher for high-skill students in schools with remedial math than for their counterparts in schools with full algebra enrollment during pre-policy years.

However, in schools that initially offered remedial math, peer ability levels would decline for high-skill students when the policy universalized algebra. This is because these schools created mixed-ability grouping when eliminating remedial math, and therefore high-skill students began taking algebra with lower-skill peers. Thus, high-skill students' test scores would decline post-policy compared to similar students in schools unaffected by the policy. As a result, pre-policy test score differences between the two types of schools would diminish post-policy.

Algebra-for-All Policy in Chicago

The Chicago Public Schools implemented an algebra-for-all policy in 1997 as part of a large curricular reform; the district raised high school graduation requirements, eliminating the large array of remedial courses in all subjects. In mathematics, students were required to take Algebra I in the ninth grade, followed by Geometry and Algebra II in the subsequent years.[1] The algebra-for-all policy had a large and immediate effect on students' algebra course taking, and the magnitude of the effect was particularly large for low-performing students (Allensworth et al., 2009). Figure 1 describes relationships between algebra course enrollment and students' incoming math skills during both pre- and post-policy years. Approximately 40 percent of students whose incoming math skills were more than half a standard deviation below the system average took remedial math (e.g., prealgebra and general math) pre-policy. In comparison, many higher performing students enrolled in algebra pre-policy.

When the policy was adopted in 1997, this created an exogenous shock in algebra enrollment; schools enrolled almost all students in algebra immediately after the introduction of the policy. Thus, the policy was successful in eliminating remedial math and creating equal access to algebra for all students. For high-performing students, the policy did not affect algebra enrollment because almost all of them took algebra both before and after the policy.

Despite large improvements in algebra course taking, the policy had few effects on the academic outcomes of students targeted by the policy—low- and average-ability students who were likely to take remedial math in the absence of the policy (Allensworth et al., 2009). Allensworth et al. (2009) showed that the policy did not improve their test scores, and math course failure rates slightly increased for students with average incoming skills as a result of the policy.[2] This study, however, did not examine how the policy might have unintentionally affected high-skill students.

Research Design

The current study uses a short interrupted time-series design with a nonequivalent control group to examine the effect of the algebra-for-all policy on classroom academic composition and student math achievement (Research Questions 1 and 2). An interrupted time-series design takes advantage of the fact that there was a sharp increase in algebra enrollment for low-skill students when the policy was introduced in 1997 (see Figure 1). Thus, the policy effect on the outcome is captured by a shift in the outcome trend during the same periods.

A key assumption required for the time series design is that factors unrelated to the policy do not affect the shift in the outcome trend. This study makes two types of adjustment to strengthen this design. First, covariate adjustment is used to control for cohort changes in observed characteristics, such as students' incoming skills and demographic backgrounds. Second, a nonequivalent control group is used to adjust for other unmeasured historical confounding effects. For example, a retention policy was introduced 1 year before the algebra policy, and school accountability was tightened during this period, which could affect test scores regardless of the algebra policy. A nonequivalent control group is the group that would not have been affected by the algebra policy but affected by all other historical factors. In Chicago, pre-policy algebra enrollment rates varied considerably across schools, and some schools had already offered algebra to nearly all of their students, including low-skill students, during the entire pre-policy periods. These schools serve as a control group as they did not need to increase algebra enrollment with the policy but would have been affected by all other historical factors.

Using a nonequivalent control group, this study employs a difference-in-differences analysis wherein policy effects are defined as differences in the outcome changes from pre- to post-policy periods between schools affected by the policy and schools unaffected by the policy. The key assumption required for a difference-in-differences analysis is that historical confounding effects are similar between the two groups of schools. As shown in the Results section, pre-policy trends in both math achievement and classroom academic composition are similar between schools affected by the policy and schools unaffected by the policy although their characteristics differed in the base year. This suggests other pre-policy reforms, such as a retention policy, seem to have affected similarly the two groups of schools.

It is important to note that observed school characteristics are not significantly related to pre-policy algebra enrollment rates, given students' incoming skills. This implies that although schools that offered remedial math pre-policy have somewhat disadvantaged characteristics compared to schools offering algebra for all pre-policy (e.g., lower incoming skills and lower socioeconomic characteristics; see appendix), these differences are not likely to be correlated with the degree to which the policy increased algebra enrollment rates. Thus, base-year differences in observed school characteristics are not likely to confound policy effects on the outcome once students' characteristics are adjusted for. The preliminary analysis showed that including school characteristics as control variables did not change the estimates of policy effects although they might reduce standard errors.

Moreover, trends in school academic composition are also similar between schools affected and unaffected by the algebra policy (see appendix). This suggests that post-policy shifts in classroom academic composition observed for schools affected by the policy are likely to be induced by the policy, and they would not be attributed to the shift in school academic composition. Taking advantage of this policy-induced change in classroom peer composition, subsequent analysis uses an instrumental variable strategy to examine the effect of classroom academic composition on student achievement (Research Question 3).

Data

Chicago Public Schools is the third-largest school system in the United States. The district serves predominantly low-income and minority students; approximately 85% of students are eligible for free/reduced-price lunch, and racial composition is about 50% African American, 38% Latino, 9% White, and 3% Asian. This study uses six cohorts of ninth-grade students—three pre-policy and three post-policy cohorts from 1994 and 1999—in nonmagnet high schools that existed in both pre- and post-policy periods (18,005 students in 58 schools).

Analysis of instructional organization examines characteristics of classrooms attended by all students, excluding students with disabilities. Analysis of students' outcome focuses on students with high skills. For the purpose of this study, high-skill students are defined as those whose incoming math abilities are more than .7 standard deviation above the overall average. These students scored approximately at or above the 60th national percentile on the eighth-grade Iowa Tests of Basic Skills in mathematics.[3] Virtually all of these students took algebra pre-policy; thus, the policy would have affected their outcomes not through changes in algebra course taking but only through changes in classroom academic composition.

This study draws from multiple data sources provided by the Chicago Public Schools. Data on course transcripts and semester grade files are used to identify students' algebra enrollment and their teachers and classmates. Administrative records contain student demographic information, including gender, age, race/ethnicity, special-education status, and residential mobility prior to entering high school. The 2000 U.S. census block-level data are also used to construct student socioeconomic variables by linking to students' home addresses. Data on the Iowa Tests of Basic Skills mathematic scores are used to measure students' incoming skills and academic composition of students' algebra classes. High school achievement test scores come from the Tests of Academic Proficiency administered in the ninth-grade spring semester.

Measures

The main achievement outcomes are students' math scores on the standardized test (Tests of Academic Proficiency), which is administered in the ninth-grade spring semester. To measure students' incoming math skills, a latent math score is constructed using a vector of students' Iowa Tests of Basic Skills test scores from third through eighth grade.[4] The score is standardized across cohorts with a mean of 0 and standard deviation of 1. Classroom academic composition is measured by taking the average of the latent math scores in students' math classes. This variable captures the average initial skill levels of students in math classes upon entering high school. In addition, latent math scores are used to create two measures of school academic composition. One measure is the average of student latent math scores for each school in each year. The other measure is the standard deviation of students' incoming skills in the school, capturing the overall skill heterogeneity. They are used as control variables because school compositional changes are likely to affect classroom composition irrespective of the algebra policy and they also would be correlated with the outcomes. In addition, a small number of schools increased algebra enrollment in 1996 when the district increased the number of math courses from 2 to 3 years of mathematics. Thus, a categorical indicator is created, with 1 indicating schools that increased algebra enrollment rates more than 5% in 1996 and 0 otherwise. The size of ninth-grade cohorts is also used as a control variable as it is correlated with both classroom composition and outcomes.[5]

Student algebra enrollment is a dichotomous variable, where 1 indicates taking algebra or higher and 0 indicates taking below algebra (e.g., prealgebra and remedial math). A school-level dichotomous variable distinguishes between schools affected by the policy (treated schools) and those unaffected by the policy (control schools). Specifically, schools are defined as affected by the policy if they increased algebra enrollment among low-skill students by more than 5% from pre- to post-policy. Schools are defined as not affected by the policy if they increased algebra enrollment among low-skill students by less than 5% post-policy. These schools had offered algebra for nearly all students, including low-skill students, pre-policy.

It is important to note that this measure also controls for school academic composition to capture the policy effect on algebra enrollment rates. Because the overall pre-policy algebra enrollment is lower for schools with more low-skill students, defining the policy effects based on the overall changes in algebra enrollment would confound the policy effects with school academic composition. For the purpose of this study, low-skill students are those with incoming latent math scores greater than .5 standard deviation below the overall average. These students are least likely to enroll in algebra in the absence of the policy; thus, the policy would affect their algebra enrollment the most.

A set of dichotomous variables distinguishes the year students entered high school, with the baseline year (1994) being an omitted category. Other student control variables include a dummy variable for gender, with 1 indicating male, and a set of dummy variables for race/ethnicity distinguishing African American (omitted group), Hispanic, White, and Asian students. Two measures of socioeconomic status variables are constructed using the block-level 2000 U.S. census data, linked to students' home addresses.[6] The first measure captures neighborhood poverty levels, and the second measure captures social status, and both of them are standardized to have a mean of 0 and standard deviation of 1. Neighborhood poverty is a composite measure of the male unemployment rate and the percentage of families under the poverty line in the block group. Social status is a composite measure of average educational attainment and percentage of employed persons who are managers, executives, or professionals in the block group. Residential mobility is measured by a set of dummy indicators distinguishing no moves (omitted category), moving once, and moving twice or more in the 3 years prior to entering high school. Age at entry into high school is measured by three variables—number of months old for entering high school, a dummy variable indicating if students are slightly old, and a dummy variable indicating if students are young for starting high school. Table 1 provides descriptive statistics.

Analytic Strategy

The first analysis examines policy effects on classroom academic composition and math outcomes of high-skill students (Research Questions 1 and 2). The statistical model nests students within cohorts within schools.[7] The outcome Y (e.g., classroom academic composition and math outcomes) for student *i* in cohort *j* in school *k* is estimated as follows:

$$Y_{ijk} = \pi_{0jk} + \sum_{p=1}^{P} \pi_{\mathbf{pjk}} (\mathbf{X})_{\mathbf{ijk}} + e_{ijk} ,$$

where X is a vector of student-level control variables, including students' incoming skills, race, gender, socioeconomic status, age, and residential mobility.[8] Student-level covariates are grand mean centered so that the intercept, π_{0ijk}, represents the mean outcome for typical high-skill students in the system across the cohorts. At the cohort level, the mean outcome is specified as a function of cohort year and cohort characteristics in each school:

$$\pi_{0jk} = \beta_{00k} + \sum_{m=1}^{M} \beta_{\mathbf{0mk}} (\mathbf{CohortYear_m})_{\mathbf{jk}} + \beta_{\mathbf{0(m+1)k}} (\mathbf{C})_{\mathbf{jk}} + r_{jk} ,$$

where the variable CohortYear is a set of dichotomous variables with m = 1, 2 … M, distinguishing cohorts in which the omitted category is the base year (1994) cohort.[9] Outcome trends are captured by these dichotomous variables because there are only three pre-policy cohorts and a linear trend is difficult to specify with a short interrupted time-series design. The variable C is a vector of cohort characteristics, such as average incoming skills, heterogeneities in students' skills, and demographic characteristics in each school in each year. The cohort variables are centered on the average

TABLE 1
Descriptive Statistics (All Regular-Education Students)

	Pre-Policy		Post-Policy	
	Mean	Standard Deviation	Mean	Standard Deviation
Student characteristic				
Percentage algebra enrollment	.81	.39	.99	.11
Incoming math skills	–.13	.84	.17	.91
Ninth-grade math scores	35.00	16.44	41.89	16.18
Classroom average math skills	–.19	.59	.06	.65
Percentage male	.48	.50	.48	.50
Race/Ethnicity				
Percentage White	.08	.27	.08	.27
Percentage Hispanic	.28	.45	.32	.47
Percentage Asian	.02	.14	.02	.15
Age at high school entry				
Young	.11	.31	.03	.16
Month old for ninth-grade	1.58	3.22	1.71	3.29
Socioeconomic status				
Social status	–.35	.77	–.31	.76
Poverty level	.38	.87	.29	.86
Mobility prior to high school				
Percentage moved once	.25	.43	.25	.43
Percentage moved twice or more	.08	.26	.07	.25
School characteristic				
Average incoming skills	–.03	.12	.22	.14
Heterogeneity	.81	.13	.91	.15
Number of ninth-grade students	376.90	138.10	321.99	133.36
Percentage vocational school	.12	.33	.12	.33

values in the base year in each school and capture fluctuations in the outcome due to year-to-year changes in the characteristics of the incoming cohorts.[10]

The intercept β_{00k} thus represents the average outcome in the base year in school k, and the coefficient β_{0mk} represents the average difference in the outcome from the baseline year to the respective cohort year, m, controlling for changes in student and cohort characteristics. The relationship between students' incoming skills and outcomes are allowed to vary randomly across cohorts. All other πs are fixed at the cohort level.

The school-level model specifies the average baseline year outcome and the subsequent outcome differences (β_{00k} through β_{0mk}) as a function of school type—whether schools are affected by the policy—and the base-year school academic composition.[11] This is written as

$$\beta_{00k} = \gamma_{000} + \gamma_{001}(\text{Affected})_k + \gamma_{002}(\text{S})_k + u_{00k},$$

and for each cohort year, m, where m = 1, 2 … M

$$\beta_{0mk} = \gamma_{0m0} + \gamma_{0m1}(\text{Affected})_k,$$

where the variable Affected is a dichotomous variable indicating whether schools are affected by the policy (i.e., schools that increased algebra enrollment post-policy for low-ability students by more than 5%) and the omitted category is unaffected schools (i.e., schools that increased algebra enrollment by less than 5% for low-ability students post-policy). The variable S indicates the average skill level of the baseline cohort centered on the average of unaffected schools. All other βs are fixed at the school level without predictors.

The intercept γ_{000} for the Level 2 intercept (β_{00k}) represents the average baseline year outcomes for schools unaffected by the policy. The coefficient γ_{001} represents the difference in the base-year outcome between unaffected and affected schools, controlling for the base-year school average ability and other student characteristics. It is expected that both high-skill students' peer ability levels and their test scores would be higher for schools offering remedial math than schools with full algebra in the base year ($\gamma_{001} > 0$). Also, initial test score differences would be explained by differences in classroom peer ability levels between the two groups.

The intercept γ_{0m0} for each β_{0mk}, where m = 1, 2 … M, represents the average differences between the baseline year and the subsequent cohort year, m, in schools unaffected by the policy. The coefficient of interest γ_{0m1} represents the difference in these outcome differences between unaffected and affected schools, controlling for the base-year school average abilities and changes in cohort and student characteristics. Pre-policy outcome trends are expected to be similar between the two types of schools ($\gamma_{0m1} = 0$ for the pre-policy cohort m), suggesting similarity in the effects of historical factors during pre-policy years. However, during post-policy years, peer ability levels and test scores would decline for high-skill students in schools that increased algebra enrollment compared to their counterparts in schools unaffected by the policy ($\gamma_{0m1} < 0$ for the post-policy cohort m). In addition, post-policy declines in test scores would be explained by declines in classroom average ability.[12]

The above analysis compares the total outcome differences between schools that increased algebra enrollment and schools that did not. This does not directly assess the effect of classroom academic composition (i.e., Research Question 3). Also, the degree of policy-induced changes in classroom academic composition differed across schools, and the total policy effects should be larger for schools that made greater changes in classroom academic composition. Therefore, the final analysis uses instrumental variable strategies to directly estimate the effect of classroom peer academic composition on student achievement. Specifically, interaction terms between the policy year and schools that offered remedial math pre-policy are used as instruments.

Using the two-stage least squares, the first-stage equation estimates classroom academic composition as a function of the policy year, schools students attended, and student and cohort characteristics:

$$T_{ijk} = \gamma_{0jk} + \gamma_1(CohortYear)_{\mathbf{jk}} + \gamma_2(W)_j + \gamma_3(PostYear_{jk} * Affected_j) + \gamma_4\mathbf{X}_{ijk} + \gamma_5(\mathbf{C})_{\mathrm{jk}} + e_{ijk},$$

where T indicates peer ability levels for student i in cohort k in school j, the variable *CohortYear* distinguishes the year students entered high schools with the base-year cohort (1994) as the omitted year, and W is school fixed effects. The instruments are the interaction terms between *PostYear* and *Affected*—a set of dichotomous variables distinguishing post-policy years (i.e., 1997, 1998, 1999) and school fixed effects among those that are affected by the policy. These instruments identify exogenous variation in classroom peer ability levels induced by the policy in schools that increased algebra enrollment in a given post-policy year. Control variables include a vector of student characteristics, X, and a vector of cohort characteristics, C, capturing year-to-year fluctuations in the characteristics of the entering ninth-grade students.

The second-stage equation estimates the effect of classroom composition on students' academic outcomes Y:

$$Y_{ijk} = \gamma_{0jk} + \gamma_1(CohortYear)_{\mathbf{jk}} + \delta(\hat{T}) + \gamma_2 W_j + \gamma_3\mathbf{X}_{ijk} + \gamma_4(\mathbf{C})_{\mathbf{jk}} + r_{ijk},$$

where $\hat{T}$ is the predicted value of peer ability levels estimated from the first equation and other notations are the same as in the first-stage equation. Here, the coefficient γ_1 captures the average within-school differences in the outcomes between the base-year cohort and subsequent cohorts that are not related to policy-induced changes in classroom academic composition. The coefficient of interest δ provides the effect on the outcomes of peer academic composition. A key assumption is that the policy would affect the outcomes of high-performing students in schools that are affected by the policy—schools that increased algebra enrollment—only through its effect on classroom peer academic composition, controlling for time-invariant characteristics of the school, general year-to-year outcome changes (i.e., year fixed effects), and student and cohort characteristics.[13]

Results

The Policy Effects on Classroom Academic Composition

The first analysis shows how the algebra-for-all policy affected classroom academic

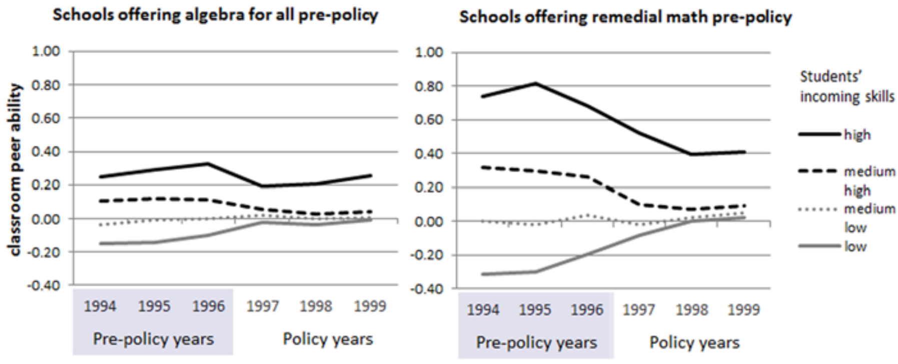

FIGURE 2. *Change in classroom peer ability before and after algebra for all.*
Note: Classroom peer ability adjusts for school academic composition by subtracting students' own incoming skills from the school mean ability in each year. The figure is based on students without disabilities.

composition. This begins by descriptively examining relationships between students' incoming skills and the average skill levels in their math classes. Figure 2 shows classroom average peer ability levels from 1994 to 1999 for four groups of students (high skill, medium high, medium-low skill, and low skill). The figure on the left is based on schools that offered algebra for nearly all students pre-policy, and the figure on the right is based on schools offering remedial math pre-policy and increased algebra enrollment with the policy.

These two figures show stark differences in how these two types of schools organized classrooms, highlighting the importance of school curricular structure in shaping how schools organize instruction based on students' incoming skills. Prior to the policy, schools that offered remedial math were much more likely to segregate classrooms based on students' prior skills than schools offering algebra for all where the degree of sorting was much more modest. In schools with remedial math classes, lower skill students were more likely to attend classes with lower skill peers, and higher skill students were more likely to have higher skill peers than their counterparts with similar initial skills in schools with full algebra enrollment. As the policy of mandating algebra for all students led these schools to detrack math curriculum, these schools also created more mixed-ability classrooms. In schools that increased algebra enrollment, average classroom peer skill levels declined considerably for high-skill students and improved for low-skill students, compared to similar students in schools that did not increase algebra enrollment post-policy.

Table 2 shows the results of statistical models that estimated the average skill levels of algebra classes attended by high-skill students in each year, controlling for their background characteristics, school academic composition, and changes in school composition over time. Among the baseline cohort (1994), the average peer skill levels of high-skill students were higher by .21 standard deviation in schools that offered remedial math (schools affected by the policy) than the skill levels in schools with full algebra enrollment (unaffected schools) after controlling for differences in students' characteristics and school academic composition ($p < .001$). This reflects the fact that these schools tracked students in math classes prior to the policy. Changes in classroom academic composition were similar between these two types of schools during pre-policy years; thus, the initial difference in peer skill levels remained at the same level during pre-policy years.

However, the overall trend began to shift when the algebra-for-all policy was implemented in 1997, and the initial gap disappeared completely by the 2nd year of the policy in

TABLE 2
Changes in Peer Ability Levels for High-Skill Students

	Coefficient	*SE*
Intercept (1994)	.31	.04****
Difference for affected schools[a]	.21	.05****
Pre-policy difference from 1994 to		
1995	.00	.05
Difference for affected schools	.00	.06
1996	.03	.05
Difference for affected schools	.00	.06
Post-policy difference from 1994 to		
1997	–.03	.05
Difference for affected schools	–.09	.06
1998	.05	.06
Difference for affected schools	–.21	.06***
1999	.02	.06
Difference for affected schools	–.24	.06***

Note: The models control for school academic composition in 1994, changes in school academic composition, cohort size, and students' background characteristics.
a. These are schools that offered remedial math pre-policy.
***$p < .01$. ****$p < .001$.

1998. The initial difference (.21 standard deviation) was narrowed by .09 standard deviation in 1997 (not significant) and by .21 standard deviation in 1998, eliminating the pre-policy differences. Similarly, peer ability levels declined in 1999 by .23 standard deviation from the base year in schools that increased algebra enrollment. These results suggest that schools created more mixed-ability classrooms when they increased algebra enrollment, although some schools might have continued to sort students by ability in the 1st year of the policy.

The Policy Effects on Math Achievement of High-Skill Students

The next analysis examines how the algebra-for-all policy affected math achievement of high-skill students. Model 1 in Table 3 estimates math test scores in the baseline year (1994) and subsequent differences for high-skill students in schools that were affected by the policy compared to similar students in schools that were not affected by the policy. Figure 3 shows the outcome trend from pre-policy to post-policy years based on the model estimates.

Math scores in the baseline year (1994) were higher by 2.04 points for high-skill students in schools that offered remedial math than for students with similar incoming skills in schools that offered algebra for all students ($p < .05$) after controlling for school academic composition and students' background characteristics. The magnitude of the base-year difference was about .16 standard deviation.[14] During pre-policy years, there were no significant differences in the outcome trend between the two types of schools, as expected. The test score difference from the baseline year to the subsequent pre-policy years was similar between schools with remedial math and schools with full algebra enrollment. In general, test scores improved during pre-policy years in both types of schools.

However, this pattern shifted in 1998, 1 year after the policy was implemented. For high-skill students in schools with full algebra enrollment pre-policy, math scores improved by 3.84 and 4.18 points in 1998 and 1999, respectively. In comparison, test score improvements were smaller in schools that expanded algebra during these years; test scores improved by only 1.64 (3.84–2.20) in 1998 and 1.44 (4.18–2.74) in 1999 for high-skill students in schools affected by the policy. The policy did not have statistically significant effects on math scores in the 1st year of policy implementation, and this corresponds with the policy effects on classroom academic composition; the decline in peer skill

TABLE 3
Changes in Ninth-Grade Test Scores for High-Skill Students

	Model 1		Model 2	
	Coefficient	*SE*	Coefficient	*SE*
Intercept (1994)	53.65	0.75****	54.20	0.77****
Difference for affected schools[a]	2.04	0.87**	0.70	0.88
Pre-policy difference from 1994 to				
1995	0.35	0.90	–0.19	0.98
Difference for affected schools	–0.66	1.05	–0.26	1.09
1996	2.04	0.84**	1.64	0.87*
Difference for affected schools	–0.81	1.04	–0.48	1.03
Post-policy difference from 1994 to				
1997	0.88	0.89	0.38	0.92
Difference for affected schools	–1.34	0.99	–0.71	1.00
1998	3.84	1.01****	3.52	1.00***
Difference for affected schools	–2.20	0.98**	–1.34	0.97
1999	4.18	1.01****	3.45	1.01***
Difference for affected schools	–2.74	1.02***	–1.28	1.02
Class average skills			4.85	0.25****

Note: The models control for school academic composition, changes in school academic composition, cohort size, and students' background characteristics.
a. These are schools that offered remedial math pre-policy.
*p < .1. **p < .05. ***p < .01. ****p < .001.

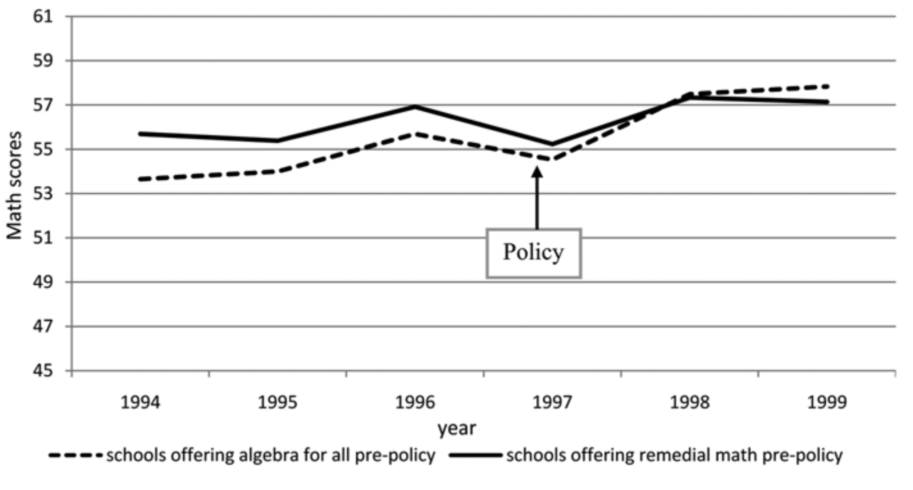

FIGURE 3. *Trends in the average ninth-grade math test scores for high-skill students.*

levels did not occur until 2 years after the policy was implemented. Similarly, the pre-policy test score differences between the two schools were eliminated by the 2nd year of the policy.

An additional analysis included classroom peer skill levels as an explanatory variable (Model 2 in Table 3). This model shows that differences in classroom academic composition explain the differences in math scores for the base-year cohort (1994 cohort) between schools that offered remedial math and schools with full algebra enrollment pre-policy. Classroom compositional changes also explain differences in the post-policy trends in math scores between

TABLE 4
The Effect of Classroom Academic Composition on Math Test Scores: Instrumental Variable Models

	Coefficient	*SE*
Intercept (1994)	53.72	2.33****
1995	–0.45	0.46
1996	1.55	0.42****
1997	0.36	0.48
1998	3.21	0.63****
1999	3.14	0.64****
Class average skills	3.15	1.19***

Note: These are pre-policy cohorts.
****p* < .01. *****p* < .001.

the two types of schools. Post-policy differences in achievement are no longer significant between the two schools once classroom academic composition is taken into account.

Instrumental Variable Analysis: The Effect of Peer Academic Composition

The above analyses estimated the policy effects by comparing schools that increased algebra enrollment due to the policy and schools that did not. Yet this does not directly assess the effect of classroom academic composition on math outcomes of high-skill students. Thus, the final analysis estimates the effect of classroom academic composition by exploiting the policy-induced shifts in classroom academic composition for high-skill students (Research Question 3).

The result of instrumental variable analysis is shown in Table 4. The coefficients for the year variables indicate the differences in test scores among high-skill students from the base-year cohort (1994) to subsequent years that are not related to changes in classroom academic composition induced by the policy. The estimated outcome changes were similar to earlier results, showing general improvements in test scores over time. The coefficient for classroom average skill indicates differences in test scores that are attributable to changes in peer ability levels induced by the policy. As peer skill levels declined for high-skill students in schools that increased algebra enrollment post-policy, their test scores were expected to decline accordingly. For example, for schools where peer ability levels declined by 1 standard deviation for high-skill students, these students' math scores were predicted to decline by 3.15 (about a quarter of a standard deviation).

Conclusions

The policy of universalizing a college-preparatory curriculum is an increasingly popular strategy to address problems of low achievement in high school. In particular, algebra is often considered a gateway for later achievement, and a recent report by the National Mathematics Advisory Panel (2008) underscores the importance of improving algebra learning in secondary school. Today, a growing number of states and districts requires algebra for all students in ninth grade or earlier.

A large body of research supports this policy, suggesting that students who enroll in core academic coursework have better academic outcomes than students in remedial or general coursework, controlling for their initial skills and other background characteristics. Moreover, tracking is widely criticized for impeding the academic progress of students placed in low-level, "dead-end" tracks. Thus, eliminating curricular differentiation is often thought to improve educational inequality. However, few studies address the policy question of how a policy of universalizing a college-prep curriculum would affect student achievement, compared to no policy. More important, we know little about the systemic impacts of the policy, including how schools organize classes for instruction. This would affect all students, including high-skill students who are not targeted by the policy. As many states and districts have begun implementing such a reform, this is a critical limitation in prior research.

A recent study by Allensworth et al. (2009), which evaluated the overall effects of the college-prep-curriculum-for-all policy in Chicago, is an exception. This study focused on policy target students—low- and average-ability students who would take remedial math in the absence of the policy. Their results showed that even though the policy equalized opportunities for algebra and led more students to receive algebra credits, their test scores did not improve, and math failure rates increased, particularly among average-ability students.

A recent study by Loveless (2008) also addresses challenges of the universal algebra policy in improving students' achievement at the middle school level. For example, simply mandating algebra for all eighth-grade students would create a mismatch between students' skills and the content, leading to the watering down of algebra instruction. The policy may also have negative effects on high-performing students. In Massachusetts, fewer students reached proficient levels in schools offering algebra for all eighth graders than in schools offering both algebra and remedial math (Loveless, 2009). Moreover, the universal algebra policy is likely to be most detrimental to high-performing students in low-income and high-minority schools because such policies are more common in these schools than in schools that serve higher income and fewer minority students.

This study also showed potential challenges of a policy aiming to universalize a college-prep curriculum in ninth grade in a large urban district. When eliminating remedial math classes, schools are likely to put lower performing students in algebra classes together with high-performing students. Thus, peer skill levels declined for high-skill students. Because teachers often adjust instruction to the middle students in a classroom, declines in peer ability levels could have resulted in less challenging content and slower paced instruction. Similarly, declines in peer ability levels might have resulted in lower expectations and greater disruption (Oakes, 2005; Page, 1991; Rosenbaum, 1999). These factors may explain why test scores of high-skill students suffered from this policy. In Chicago, even though overall math scores were rising during the 1990s, test score improvements were smaller in schools that expanded algebra with the policy. However, it is also important to note that the validity of this study's findings rests on a strong assumption required for the difference-in-differences analysis—historical factors would affect similarly schools affected by the algebra policy and schools unaffected by the policy. This assumption would be violated if any concurrent changes, such as increased accountability and shifts in unmeasured student or school characteristics, uniquely affected one type of school.

A critical limitation in the algebra-for-all policy in Chicago is that although many students enter high schools with very weak math skills, the policy did not offer additional supports for struggling students to master algebra or professional development for teachers around how to teach algebra in mixed-ability classrooms. In the heterogeneous classroom, instruction is often more challenging than in the homogeneous setting, and successful detracking seems to require a host of supports, such as a shared belief in diversity among staff, successful professional development that leads teachers to use inclusive pedagogical practices, and additional supports for struggling students (Boaler & Staples, 2008; Oakes, 2005; Rubin, 2008). Alternatively, a policy could use homogeneous grouping while requiring algebra for all students and providing additional instructional time and supports for struggling students and their teachers. This "double-dose" approach was introduced in Chicago, following the algebra-for-all policy, and this was successful in raising test scores for all students (Nomi & Allensworth, 2009).[15] The current study, combined with our earlier study (Allensworth et al., 2009), suggests that simply mandating a college-prep curriculum for all students is not sufficient to improve the academic outcomes of all students.

Appendix

Additional analyses are conducted to rule out potential selection bias problems that would arise if the treated and control schools differed in a way that affects the outcome trends. First, Table A1 shows differences in base-year school characteristics. On average, schools that offered remedial math pre-policy (i.e., treated schools) had lower average incoming skills, greater skill heterogeneity, lower social status, a larger cohort size, and more African American students—but fewer Hispanic students than schools that offered algebra for all pre-policy (i.e., control schools). However, these school characteristics are not associated with pre-policy variation in algebra enrollment rates, given students' incoming skills. For example, Figure A1 describes relationships between pre-policy algebra enrollment rates among low-skill students (students with incoming skills lower than .5 standard deviation below the overall average) and school average incoming skills. The figure shows that pre-policy algebra enrollment rates vary considerably across schools with similar academic composition. Some schools serving many low-skill students enrolled nearly all low-skill students in algebra pre-policy. Also, some schools serving fewer low-skill students did not enroll low-skill students in algebra pre-policy. These results indicate that the policy effects on algebra expansion are not related to observed characteristics of the school, given students' incoming skills.

TABLE A1
Base-Year School Characteristics

School Characteristic	Schools Offering Algebra for All Pre-Policy	Schools Offering Remedial Math Pre-Policy
Average math skills	–0.07	–0.34
Skill heterogeneity	0.74	0.81
Average social status	–0.19	–0.02
Average poverty level	0.21	0.23
Cohort size in hundreds	3.99	4.32
Percentage Black	0.66	0.71
Percentage White	0.07	0.08
Percentage Asian	0.01	0.02
Percentage Hispanic	0.26	0.19

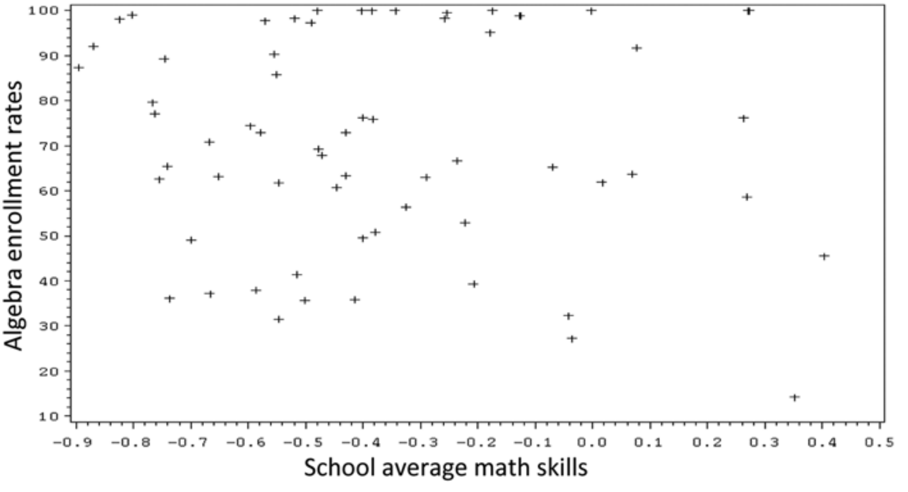

FIGURE A1. *Schools' pre-policy algebra enrollment rates of low-ability regular-education students, by school average incoming math skills.*
Note: Low-ability students are defined as students with incoming math ability below –.5 standard deviation. Each dot represents a school.

The next analysis examines whether the treated and control schools had similar trends from pre- to post-policy years in school academic composition in terms of average incoming skills and skill heterogeneity. Similarity in these trends would ensure that post-policy changes in classroom academic composition and student achievement are not due to changes in school academic composition. As Figure A2 shows, changes in the average incoming skills were similar between the treated and control schools although they were different at the initial level; students' incoming skills improved over time for both types of schools. Also, although schools with full algebra enrollment pre-policy were more homogeneous than the control schools pre-policy, there were no differential trends in skill heterogeneity between the two groups (see Figure A3).

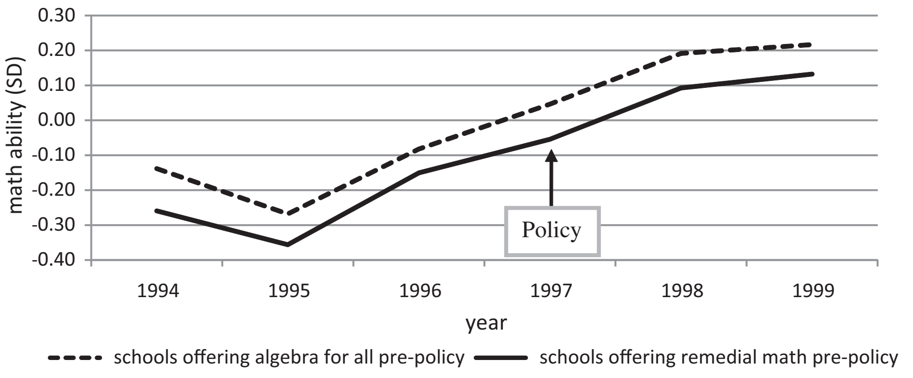

FIGURE A2. *Trends in the incoming math ability of ninth-grade cohorts from 1994 to 1999 for schools unaffected by the policy (schools offering algebra for all pre-policy) and schools that increased algebra enrollment (schools offering remedial math pre-policy).*

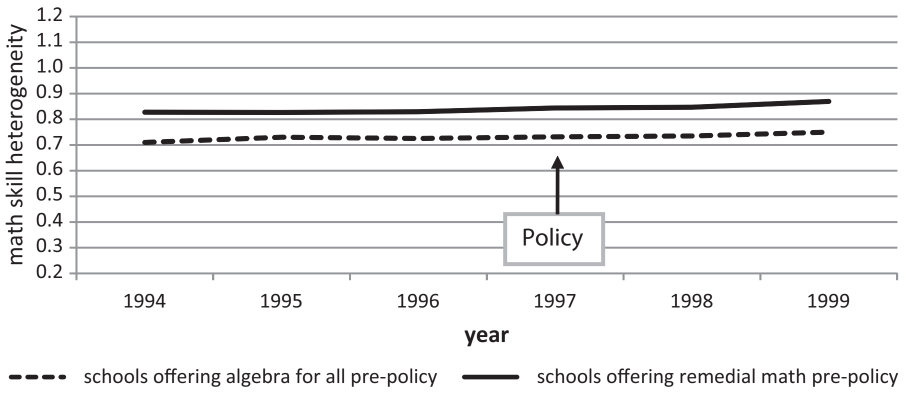

FIGURE A3. *Trends in skill heterogeneity from 1994 to 1999 for schools unaffected by the policy (schools offering algebra for all pre-policy) and schools that increased algebra enrollment (schools offering remedial math pre-policy).*

Last, as discussed in the Results section, the analysis of academic outcomes shows that pre-policy outcome trends were similar between the treated and control schools. For example, although a retention policy was introduced in 1996, the outcome differences between 1994 and 1996 were similar between the two groups; both schools had higher test scores in 1996 than in 1994. However, the outcome trends became dissimilar soon after the algebra-for-all policy was introduced. These combined results increase confidence in the results of this study, showing that the algebra-for-all policy had a negative effect on the outcomes of high-skill students in schools that expanded algebra coursework.

Notes

1. Prior to 1996, students were required to complete 2 years of any mathematics for graduation. In 1996, the district changed the graduation requirement from 2 to 3 years of any mathematics so that schools would have time to make a transition to an algebra-for-all policy.

2. The study by Allensworth et al. (2009) examined only the overall effects of the policy on low- and average-ability students but did not examine the policy mechanisms.

3. Selecting students whose latent math scores were 1 standard deviation or more above the overall average (i.e., students who were at approximately the 70th percentile or higher) did not change the pattern of the result.

4. Iowa Tests of Basic Skills scores are equated first through Rasch analysis to remove form and level effects. Using a Rasch score, a latent ability is estimated by modeling each student's learning trajectory from third through eighth grade. Specifically, a three-level measurement model, nesting measurements within years within students, estimates students' outcomes as a function of grade and grade-squared, which are allowed to vary across students. In addition, a dummy variable for grade retention in year t is included to adjust for learning that occurred the second time in a grade.

5. Including other cohort-level demographic variables (e.g., race composition, socioeconomic status composition, and percentage special education students) did not change the results; thus, they are excluded from the final analysis.

6. These socioeconomic status measures provide a different value for students who live in different census block groups. There are 2,450 census block groups represented among students in 2004. Although some students live in the same census block, these variables are much better at distinguishing economic status among students than the commonly used indicator of free and reduced-price lunch status. In Chicago, more than 80% of students qualify for free/reduced-price lunch, so this variable provides little information. On the other hand, the variables based on census block show vast differences in the economic conditions of students among those who qualify for free/reduced-price lunch. In addition, census-based socioeconomic status indicators are more strongly related to student outcomes than free/reduced-price lunch eligibility.

7. The results of this model are similar to the results of a model that nested students within classrooms within schools and entered cohort variables at the student level. Results are available on request.

8. To model classroom academic composition as an outcome, one can conceive that students are assigned to a classroom with a certain peer ability level.

9. The 1994 cohort is used as the baseline cohort rather than the 1996 cohort because schools were informed of the new algebra policy in the 1996 school year. In 1996, the district increased the mathematics requirement from 2 to 3 years of any mathematics, and some schools that had previously offered remedial math increased algebra enrollment to some degree in that year.

10. Preliminary analysis decides which cohort characteristics are entered in the final model. The final mode includes cohort average skills, heterogeneity, and cohort size.

11. Other observed school characteristics are not associated with the degree to which schools offered algebra pre-policy or the average classroom skill levels; thus, these variables are not included for a parsimonious model.

12. For high-skill students, an influx of low-ability students to algebra classes leads to declines in peer ability levels and increases skill heterogeneity in classrooms simultaneously. It is possible that the estimated effect of classroom peer ability levels may include the effect due to increased skill heterogeneity. However, it is beyond the scope of this study to examine the extent to which skill heterogeneity alone affects student achievement.

13. A preliminary analysis showed that class size for high-skill students did not change as a result of the policy.

14. The base-year standard deviation among high-skill students was 12.51.

15. Although the double-dose policy was successful in raising test scores for all students, failure rates increased for higher achieving students due to improvements in peer skill levels (Nomi & Allensworth, 2009). Also, Nomi & Allensworth (2011) discuss the mechanisms by which the double-dose algebra affected students' outcomes.

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