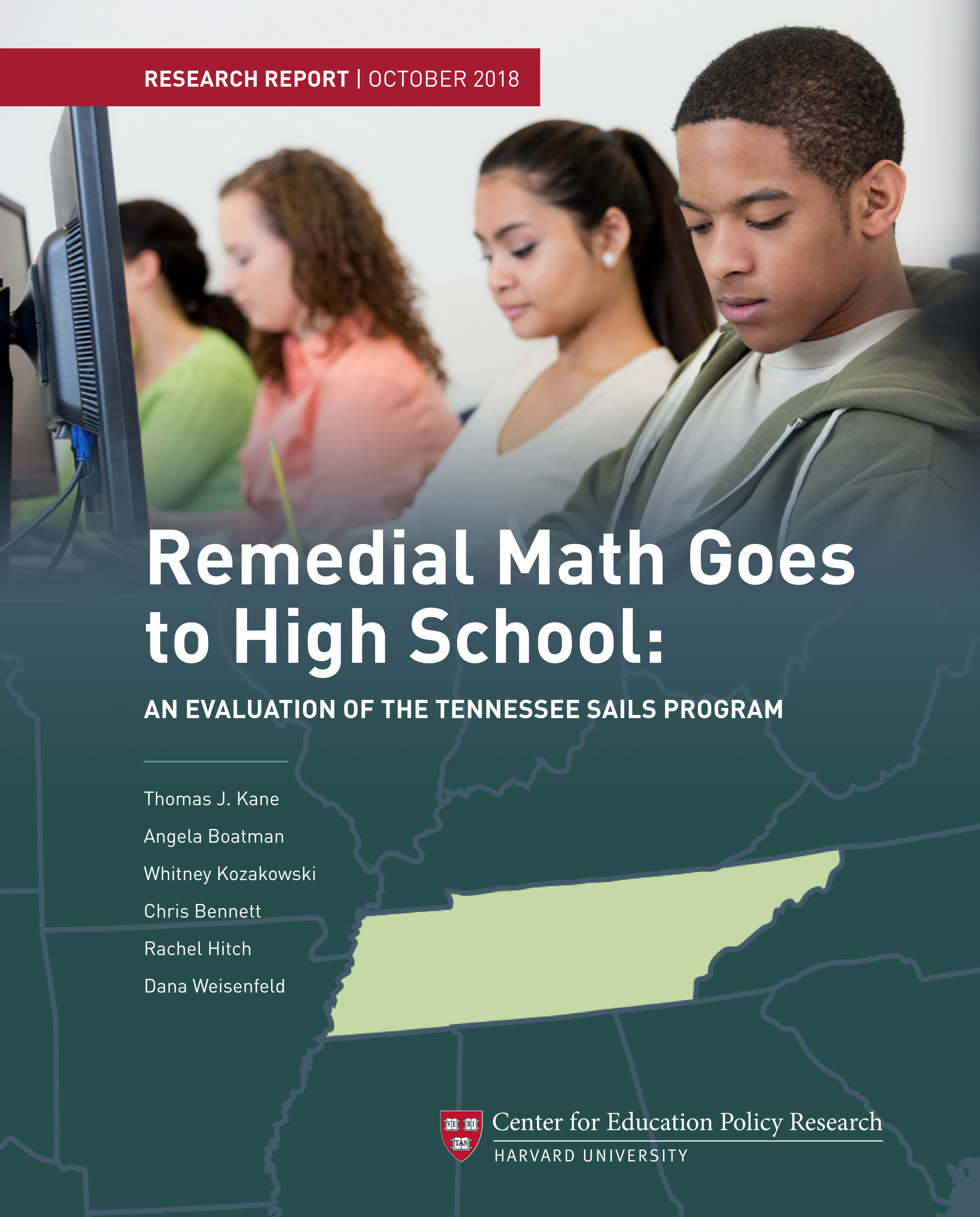

RESEARCH REPORT | OCTOBER 2018

Remedial Math Goes to High School:

AN EVALUATION OF THE TENNESSEE SAILS PROGRAM

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TABLE OF CONTENTS

INTRODUCTION

As college enrollment rates have increased, large numbers of students are arriving at college without the math and literacy skills traditionally expected for college-level work. Among first- and second-year college students nationally in 2011-12, 29% of students at public four-year institutions and 41% of those at public two-year institutions reported taking at least one remedial course (Skomsvold, 2014). Critics claim that remediation often does more harm than good, needlessly delaying students and draining students' financial aid to pay for courses that do not earn them credit toward a degree. Given the high proportion of remedial students who never complete degrees, advocacy organizations, such as Complete College America, have described pre-requisite remediation as a "bridge to nowhere" (Complete College America, 2012).

Tennessee has been a national leader in the effort to redesign college remediation. In 2012, Chattanooga State Community College launched a pilot initiative, known as the Seamless Alignment and Integrated Learning Support (SAILS) program, which the state would eventually scale up to reach a majority of Tennessee high schools. The goal of the program was to shift the locus of math remediation from college back to high school. Unlike most states in which students learn of their remediation status only after arriving at college and taking a placement test, Tennessee notifies students of their remediation status in high school (based on their junior year ACT score) while they still have a chance to refresh their skills. Students in SAILS-participating high schools who score below the remediation threshold of 19 on the ACT math test (roughly half of the seniors in those schools) can fulfill their math remediation by completing an online math course during their senior year. Those who complete all five modules are exempted from math remediation when they enroll at a Tennessee community college.

In this report, we evaluate the impact of the SAILS program on students' ability to take and pass college-level math and to accumulate college-level credits. We evaluate the program's impact under two different remediation policies: first, we measure the consequences for the high school seniors of 2013-14, when Tennessee community colleges still required students to complete remediation before their college-level coursework ("pre-requisite remediation"); we also measure impacts for the seniors of 2014-15 and 2015-16, after Tennessee community colleges began allowing students to enroll in remediation concurrently with their college-level classes ("co-requisite" remediation). Currently, at least eight states have a form of co-requisite remediation analogous to Tennessee's.

We use two different research designs to discern the impact of SAILS. First, because the SAILS program was rolled out in stages with high schools implementing the program in different years, we measure impacts by comparing changes in the outcomes of remediation-eligible students in schools implementing SAILS to the changes in outcomes for schools that had not yet implemented SAILS (or had implemented SAILS in a prior year). The staggered timing of SAILS implementation allows us to distinguish the effect of SAILS from other policy changes affecting high school students in Tennessee.

Second, we compare outcomes for students with junior year ACT math scores just below the remediation cutoff—who were recommended for remediation—to the outcomes for those just above the same cutoff—who had similar academic achievement but escaped remediation. In other words, we take those with ACT scores just above the cutoff as a control group for measuring the effect of the remediation treatment. We do the same analysis in high schools with and without the SAILS program. Moreover, we can compare the impacts for the class of 2013-14 (who faced pre-requisite remediation) to the impacts for the class of 2015-16 (who faced co-requisite remediation).

The two methods rely on different assumptions and different comparison groups. The first assumes that the changes in outcomes at participating and non-participating high schools would have been the same if not for the SAILS program; the second assumes that those with ACT math scores just above the remediation cutoff (and those who were not required to take remediation) are roughly similar to those just below (at least not in a way that cannot be controlled for with differences in their observed ACT scores). We present both sets of results which yield similar findings.

Ironically, little is known about the impact of math remediation on students' mastery and understanding of math. The reason is that while students are sent to remediation based on a test score, there is usually no posttest available for comparing the gains of program participants with those of non-participants.

Therefore, to learn more about the impact of SAILS on student's math achievement and attitudes towards math, we administered a posttest and student survey to high school seniors in 119 schools participating in the SAILS program. By comparing the achievement and survey responses of seniors who scored below the threshold as juniors (i.e., the students most likely to participate in SAILS) to the achievement of those who scored just above the threshold (who were unlikely to participate in SAILS), we estimate impacts of SAILS participation on math achievement and other survey-based student outcomes.

Below, we summarize four primary findings:

First, under the pre-requisite policy, the students with ACT math scores below 19 enrolled in college-level math at higher rates after their high schools implemented SAILS. During the first year in community college, SAILS participants were 29 percentage points more likely to enroll in college math. Roughly half of those students passed the course, yielding a 13-percentage point increase in the percentage of students having passed college math by the end of their first year. However, the impacts on college math enrollment and completion were smaller by the second year, as students from non-participating high schools completed their remediation and caught up.[1] By the end of their second year in college, SAILS participants completed 4.5 additional college credits (or 1.5 courses) compared to their counterparts in high schools without SAILS.

Second, SAILS did seem to improve students' perceptions of the usefulness and enjoyment of math. Students just below the ACT math cutoff of 19 (a group much more likely to enroll in SAILS) were 6.5 percentage points more likely to perceive that their math course content would be useful in their careers, 10 percentage points more likely to indicate they were better prepared for college math, and 6 percentage points more likely to say that they were interested in math than those immediately above the remediation threshold, who took a different high school math course. The impacts were particularly large for Black students.

1 It is also possible that the co-requisite policy implemented during their second year helped shrink the difference.

Third, despite the positive impacts on students' perceptions of math, the SAILS program did not improve students' math achievement or boost their likelihood of passing college math once they took the course. There was no difference in math performance on the posttest we administered for those immediately above and below the remediation threshold. Although the posttest was not specifically aligned to the SAILS curriculum (it was designed to reflect the ACT scoring scale), we did not find that SAILS participation improved performance on any of the subsets of items identified by SAILS program staff as aligned with the SAILS curriculum. As noted above, about half of the new students who enrolled in college math as a result of SAILS passed the course during their first year in college. The passing rates for SAILS graduates were not higher than for students with similar ACT scores who were just above the remediation threshold.

Fourth, after the co-requisite policy was introduced in the fall of 2015, SAILS no longer had an impact on the percentage of students taking or passing college math during their first year, nor on the total number of credits completed at the end of their second year. In lifting the barrier to entry to college-level courses, the co-requisite policy largely superseded the SAILS program by allowing students to do their remediation alongside college-level courses, rather than before them.

Once the co-requisite policy was in place, the primary impact of the SAILS program was to shift the timing of remediation from college back to high school for 29% of the students recommended for remediation. The SAILS program would have produced a larger decrease in college remediation if not for two reasons: first, fewer than half (roughly 40%) of remediation-eligible students enrolled in the SAILS course while in high school (either due to constraints on the number of computers in the high school, the availability of high school teachers trained in SAILS, or lack of student interest); and second, students found other ways to avoid remediation. One-third of remediation-eligible students from non-participating high schools avoided remediation by re-taking the ACT test, enrolling in non-degree programs, or otherwise being exempted from remediation.

POLICY IMPLICATIONS

During the time when Tennessee community colleges were still requiring remediation as a pre-requisite to entry into college courses, the SAILS program increased the proportion of students taking college-level math during their first year of community college. The introduction of co-requisite remediation seems to have had a similar effect—by allowing students to enroll in college-level math directly. Indeed, once the co-requisite policy was in effect, the incremental effect of SAILS on college course-taking essentially disappeared. Moreover, both the SAILS program and the co-requisite policy resulted in modest increases in the total number of credits students completed by community college entrants by the end of the second year after high school, from approximately 25 to 29.5 college credits (or 1.5 courses).

Put simply, pre-requisite remediation was preventing many students from enrolling in college-level math during their first year in college. Lifting that barrier—either with SAILS or with a co-requisite policy—did allow more students to take college math during their first year of college. Yet the SAILS program did little to improve students' chances of success in college math. The program had no impact on math achievement; and only about half of the incremental students who enrolled in college math passed it.

Critics have decried pre-requisite remediation as a major *cause* of non-completion. At least in Tennessee, our results suggest that was not the case. Shifting the timing of remediation—to high school with a program such as SAILS or to concurrent remediation with a co-requisite policy—did allow the half of remediation-recommended students who would have passed college math to do so. However, by the end of their second year after high school, the impact on the number of credits completed was 4.5 credits, roughly equivalent to 1.5 courses.

In order to move the needle on credit accumulation and degree completion, higher education institutions will need to identify and clear other academic bottlenecks that are preventing students from degree completion, such as better advising, adjusting when majors are chosen, helping students meet administrative deadlines, and helping students to improve their study skills.

Replacing pre-requisite remediation with high school remediation—such as SAILS—or with co-requisite remediation, as many states are now doing, will allow more students to complete the first lap, but they will likely need much more support to finish the race.

PROGRAM DESCRIPTION

In the fall of 2012, 68% of first-year, first-time community college students in Tennessee were enrolled in one or more developmental or remedial courses (Tennessee Higher Education Commission, 2014). For first-time, full-time students who entered Tennessee community colleges in fall 2011, only 28% completed a credential within 6 years (Tennessee Higher Education Commission, 2018). The high rates of remediation and the low rates of degree and certificate completion led leaders in Tennessee to rethink the state's approach to remediation.

Unlike most other states, Tennessee notifies students of their remediation status in high school. Tennessee uses students' 11th grade ACT math test scores to assign students to remedial courses. The vast majority of students (around 82% each year) take the ACT in school during their junior year. Students scoring below 19 on the ACT math test are told that they will need to take a remedial course in math if they enroll in a public college or university in Tennessee. Students who complete the SAILS curriculum in high school can avoid that requirement if they proceed to enroll in a Tennessee community college.

In 2011, the faculty at the Chattanooga State Community College overhauled their remedial math course, "Learning Support Math," to align with the five college-level math standards adopted by the Tennessee Board of Regents (TBR). The Chattanooga State faculty developed a version of the course that they could deliver online in self-paced modules (Educause, 2014). After piloting the model in a high school setting in 2012, Chattanooga State received funding to expand the program (now called SAILS) to other high schools in Tennessee.

The SAILS program has several unique features:

» It is self-paced, with students progressing through the material on their own schedule, either over one semester or across the whole school year;

» The course material is delivered entirely online, including videos, homework problems, and assessments. Most of the work is completed in class, although students have the option of working outside of class as well;

» Teachers serve as learning partners and coaches—although they occasionally provide direct instruction, their primary role is to monitor students' progress through the online modules and provide guidance when students are stuck;

» Students and teachers are assisted by a team of field coordinators housed at each community college and overseen by the Tennessee Board of Regents; and

» Although officially a high school course, the SAILS course is explicitly aligned with postsecondary math requirements.

The program was rolled out to 20 high schools in 2012-13. With additional state support, the program expanded each year, serving 122 high schools in 2013-14, 182 high schools in 2014-15, and 243 high schools in 2015-16.[2]

Beginning in 2012, students in Tennessee high schools were required to complete four years of high school math in order to receive their diploma. Therefore, for most students enrolled in SAILS, the course is substituting for another high school math course such as Pre-Calculus, Statistics, and Advanced Algebra and Trigonometry, or Bridge Math. Bridge Math covers similar remedial math concepts as SAILS, although it is taught in a traditional, direct-instruction format. A key difference is that students who successfully complete Bridge Math courses do not receive automatic exemption from math remediation in Tennessee community colleges. Although Bridge Math continues to be taught in SAILS-participating high schools, our results below suggest that SAILS participants are not being drawn from Bridge Math, but from the other senior math courses such as Advanced Algebra and Trigonometry and Statistics.[3]

THE TENNESSEE PROMISE AND CO-REQUISITE REMEDIATION

As the SAILS program was expanding, Tennessee became the first state in the country to offer statewide, tuition-free community college education for recent high school graduates (beginning with the high school class of 2014-15). The Tennessee Promise scholarship covers the cost of tuition and fees for recent high school graduates attending community and technical colleges, along with a small number of public and private four-year institutions offering associate degrees. It is a "last dollar" scholarship, covering the cost of tuition and required fees that remain after subtracting grant aid from other sources (e.g., Pell Grant, Tennessee HOPE scholarship). There are no minimum ACT or high school GPA eligibility requirements for the Tennessee Promise.

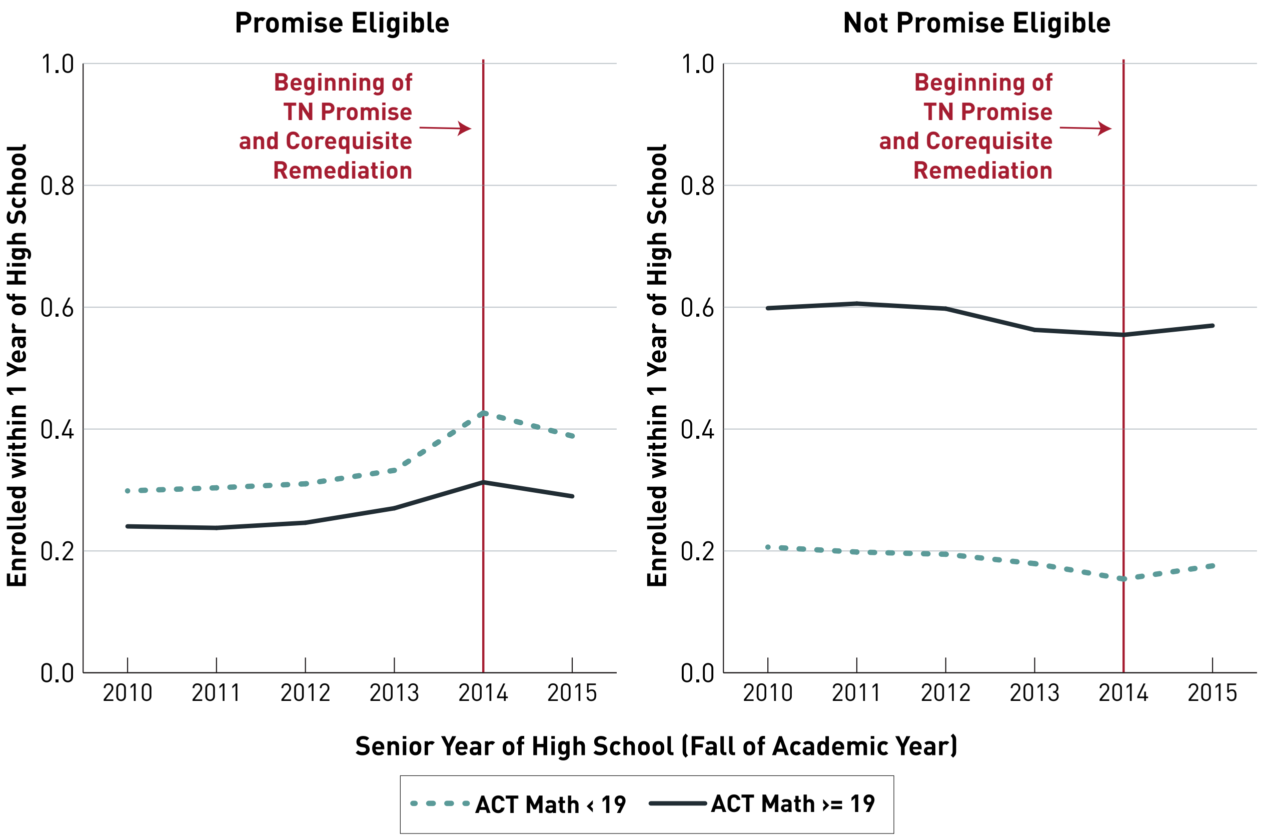
Also in fall 2015, Tennessee's community colleges transitioned from a policy of pre-requisite remediation requiring students to complete remedial courses prior to enrolling in college-level

2 We exclude the 20 high schools implementing the SAILS program in 2012-13 from our analysis. Beyond being a relatively small group of high schools and an early iteration of the SAILS program, we did not have accurate data on the students participating in the SAILS program in that first year. As a result, we dropped them from both the treatment and the comparison groups.

3 As reported in Table 4 below, there was no statistically significant difference in Bridge Math enrollments at the remediation threshold in the SAILS participating schools. Appendix Figure A3 portrays the proportion of students enrolling in different courses with ACT scores above and below the cut-off.

courses, to a co-requisite policy, in which students are required to enroll in a remedial course simultaneously with their college-level course. Like SAILS, the new remediation policy was intended to allow students to enroll directly in college-level coursework (Tennessee Board of Regents, 2016). However, a key difference is that SAILS allows students to avoid having to take the remedial class at all, thus freeing up time to take other classes.

Figure 1. Postsecondary Enrollment by Institutions' TN Promise Eligibility

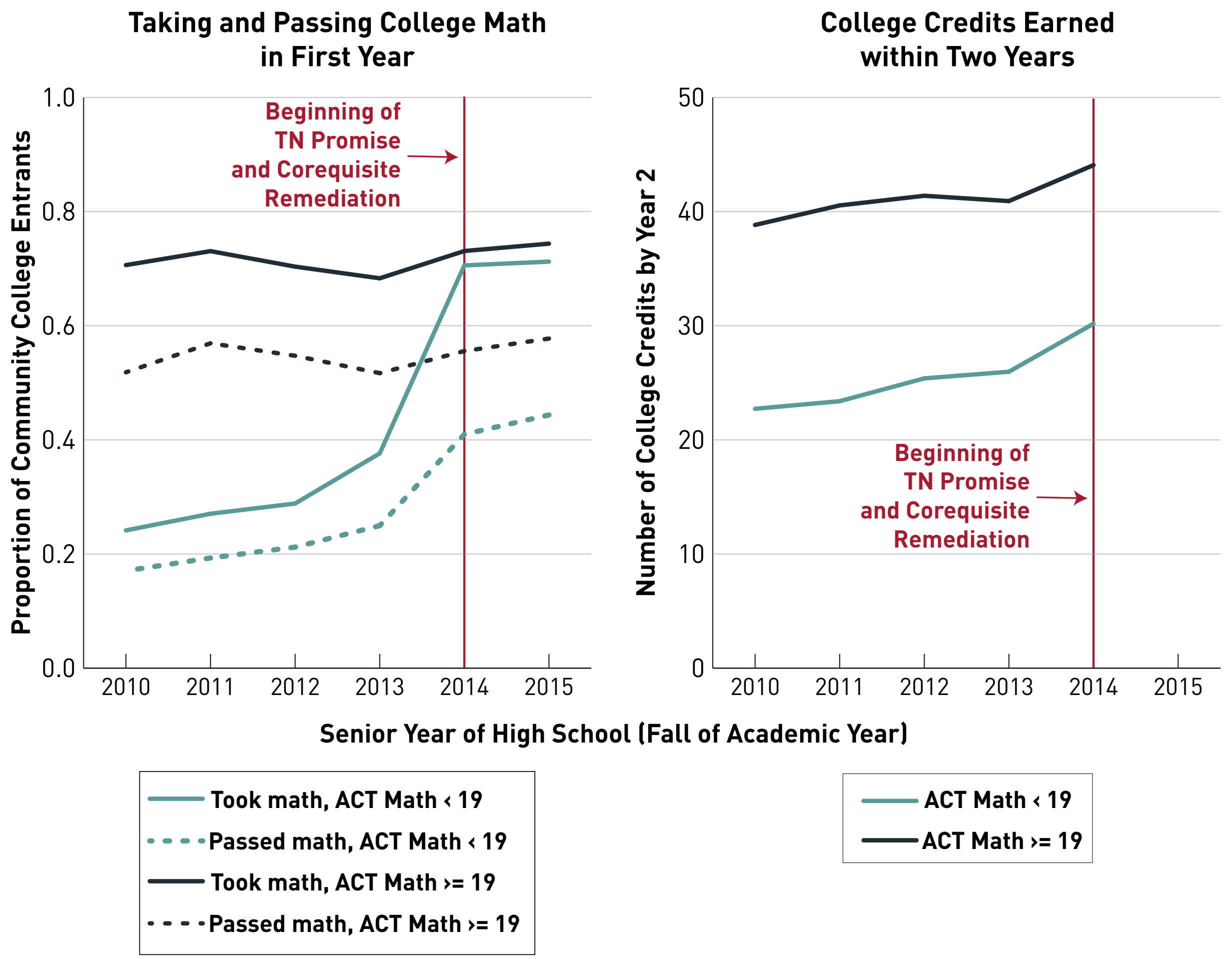


Because the Tennessee Promise and the co-requisite policy were implemented at the same time, it is difficult to isolate the independent effect of each. However, it seems that the combination of policy changes had a substantial impact, as they coincided with an increase in postsecondary enrollment and a sharp change in the proportion of community college entrants enrolling in college-level math during their first year after high school.

Figure 1 portrays the proportion of students enrolling in postsecondary education for the high school seniors of 2010-11 through 2015-16. We report trends separately for those with ACT math scores below 19 (the remediation-eligible) and for those with ACT math scores of 19 and above. We also report separately for the institutions that were and were not eligible for Tennessee Promise scholarships (eligible institutions included Tennessee community colleges, Tennessee Colleges of Applied Technology, and some four-year colleges with associate degree programs). There was more than a 10-point increase in the percentage of remediation-eligible students enrolling in institutions eligible for the Tennessee Promise. There was a smaller increase in college entry among those with ACT math scores of 19 and above. These increases were accompanied by a small decline in entry at institutions not eligible for the scholarship.

Figure 2 portrays the trend in the proportion of community college entrants taking and passing college math during their first year as well as the average number of college credits students completed by the end of their second year after high school. As in Figure 1, we report the trends separately for those with junior year ACT scores above and below the remediation threshold of 19.

Figure 2. Postsecondary Progress for Community College Entrants



When the co-requisite was implemented, there was a substantial (33-point) increase in the percentage of remediation-eligible students taking college math. It was accompanied by a smaller increase—15 percentage points—in the proportion of community college entrants *passing* college math (meaning that about half of the new entrants passed the course). In fact, in the first year of the co-requisite policy, the percentage of remediation-eligible community college entrants taking college math (approximately 70%) was nearly equivalent to that of those with ACT math scores of 19 or above (approximately 75%). In other words, having an ACT score of 19 was no longer a barrier to entry into college-level math. However, there was a large difference in passage rates between the two groups: while nearly 80% of those with ACT math scores above the remediation threshold passed college math when they took the course, the passage rates for students with ACT scores below the threshold were closer to 60%. This is the reason why the 33-point increase in college math enrollment yielded a much smaller gain in the percentage of students passing college math in their first year. (Although this applies to the co-requisite policy, we report similar impacts for SAILS participation later in the report.)

As reported in the second panel of Figure 2, the remediation-eligible seniors of 2014-15 accumulated four additional college credits by the end of their second year after high school (from 26 to 30) compared to 2013-14 seniors with ACT math scores below 19.

Given the changing policy landscape, we measure the impact of SAILS before and after the implementation of co-requisite remediation and the Tennessee Promise. Aside from the change in the cost of college, the practical consequences for students of completing SAILS differed under pre-requisite and co-requisite policies in colleges: under the former, SAILS allowed students to circumvent remediation and go directly into college-level math courses, while under the latter, the primary benefit of SAILS was to avoid co-requisite courses; SAILS students may have had more room in their schedules to take other classes. As a result, we measure the effects under pre-requisite and co-requisite remediation separately.

LITERATURE REVIEW OF COLLEGE REMEDIATION

Math remediation programs may produce two types of effects: on one hand, they refresh students' content knowledge in math (the positive "achievement effect"); on the other hand, they may delay (in the case of pre-requisite remediation) or displace (in the case of co-requisite remediation) students' ability to accumulate degree credits during their initial years in college (the negative "delay/displacement effect").

The achievement effect and the delay or displacement effects could partially or fully offset each other. For instance, if a remedial course improves students' math skills, it may accelerate students' progress in subsequent college-level courses. However, the time required to take the remedial course also diverts a student from accumulating college-level credits for a degree. The former is hopefully positive, the latter negative. The net effect of any remediation policy depends on the sum of the two effects.

The prior literature on the impacts of college remediation focused on the net effect of remediation. Researchers asked, did students assigned to traditional remedial math courses complete fewer credits and make slower progress toward a degree compared to their academically similar peers not assigned to remedial courses? A common analytic strategy used in these evaluations involved a statistical technique called "regression discontinuity," and involved comparing those immediately above and below a remediation threshold.[4] For instance, Calcagno and Long (2008) compared community college entrants on either side of the remediation threshold in Florida. While they found a small positive effect of being recommended for remediation on persistence, they ultimately found no differences in degree completion, transfer, or completion of college credits. Martorell and McFarlin (2011) studied over 250,000 students in public two-and four-year colleges in Texas and found that being identified for remediation slightly reduced credit accumulation and years in college, but had no impact on degree attainment. Scott-Clayton and Rodriguez (2012) found that students recommended for remediation in a large, urban community college system were no less likely to enroll in postsecondary education or earn a college degree and completed roughly the same number of

4 Bettinger and Long (2009) use an alternative identification strategy called an *instrumental variables* approach to estimate the effect of remediation. Using variation based on proximity to college and differences in remediation placement policies across colleges, they find that students placed into remediation are more likely to persist in college.

terms and college credits as those immediately above the cutoff. Using data from Tennessee, Boatman and Long (2018) found that in the years before the SAILS program students on the margins of needing one developmental math course were less likely to persist to the second year in college, and 5 percentage points less likely to have completed a degree within eight years.

LITERATURE REVIEW OF PRE-COLLEGE REMEDIATION

Two recent studies have examined the effects of remedial interventions offered *before* students enroll in college. Early Start, adopted in 2012 in California, requires incoming California State University students to complete their remedial math and/or English requirements in the summer prior to beginning college. Using a regression discontinuity approach, Kurlaender, Lusher, and Case (2017) found no significant improvements in persistence or academic performance for students identified as needing math remediation under Early Start.

The second study was focused on the City University of New York's (CUNY) Start program, which was launched in 2009. The program delays college enrollment for one semester for incoming students who are assessed as needing remediation in math, reading, and writing, and instead provides intensive instruction in math, reading, and writing during that semester. It also provides a number of services that go beyond remediation: providing advising, tutoring, and study skills courses. In a randomized experiment, eligible students were assigned to CUNY Start or to traditional remedial courses. Early results indicate that the program leads students to remain enrolled in CUNY colleges at higher rates during the second semester (Scrivener et al., 2018). The authors also examined the effects on student learning. They found that the treatment group had higher performance in math, reading and writing. However, because a much higher fraction of the treatment group took the tests than the control group did, it is impossible to know if the treatment caused the improvement or if the difference was driven by differences in the composition of test-takers in the two groups (e.g., the students who did not improve were less likely to take the test, especially in the treatment group.) Given these limitations, the authors describe their results as "suggestive."

DISTINGUISHING BETWEEN THE ACHIEVEMENT EFFECT AND THE DELAY/DISPLACEMENT EFFECT OF REMEDIATION

In sum, the previous research on the impacts of college remediation suggests that the net effect of being recommended for remediation has a moderately negative to null effect on degree completion. Because these studies were done in the context of pre-requisite remediation, there are two possible hypotheses that could explain small net effects:

» **The "High Delay/High Achievement Boost" Hypothesis:** It could be the case that the need to complete remediation before enrolling in college-level courses is a barrier to many students and is preventing a large fraction of students from completing degrees. However, since the net effect of being assigned to remediation has been estimated to be small, it would also have to be the case that the subset of students who complete remediation make better progress toward a degree as a result of remediation (in order to offset the large delay effect).

» **The "Low Delay/Low Achievement Boost" Hypothesis:** The second possibility is that both the achievement effect and the delay effects are small, that the delays associated with pre-requisite remediation are not the cause of low completion rates for most students, and that the achievement effects of remediation are also failing to help the average student make better progress.

Other combinations—such as the hypothesis that remediation is a major cause of non-completion and that the achievement effect of remediation is minimal—would not be consistent with the prior literature evidence, because it would imply large negative net effects of being assigned to remediation.

The unique features of the SAILS program provide an opportunity to distinguish between the two hypotheses above. To do so, we first measure the impact on remediation-eligible students when their high schools began offering SAILS under pre-requisite remediation. The SAILS program essentially gave students the opportunity to forgo the delay effect of remediation in college, while maintaining any achievement effects. Under the high delay/high achievement boost hypothesis, we would have expected to see a large increase, not only in college math enrollment, but also in credit accumulation when schools implemented SAILS.

Later, when public colleges in Tennessee adopted the co-requisite policy, we had an opportunity to measure the displacement effect of co-requisite remediation courses. When high schools implemented SAILS in the context of co-requisite remediation, students completing SAILS could avoid taking a co-requisite remedial course. Again, if the displacement effect of co-requisite remediation were large, we would expect to see a large increase in credit accumulation for students at the schools implementing SAILS for the first time.

In addition to measuring the effect of SAILS on postsecondary progress, we also measure the impacts of SAILS on students' math achievement by comparing achievement on a posttest for those immediately above and below the cutoff for remediation. If the high delay/high achievement boost hypothesis were true, and there were, in fact, a large achievement boost from remediation, we would expect to see a large impact from participating in SAILS on students' math achievement.

DATA

For the analysis, we obtained administrative data from the public K-12 system as well as from the public two-year and four-year colleges in Tennessee:

» The **Tennessee Department of Education** (TDOE) provided us with K-12 administrative records, including student enrollment, demographic characteristics, courses, and ACT scores on a 1-36 scale for all high school seniors in the 2010-11 through 2015-16 school years.

» Using the high school enrollment information above, we identified each student's primary high school (based on days attended) and added school characteristics (such as urbanicity and the proportion of students eligible for free or reduced-price lunch) from the **Common Core of Data** (CCD).

» We received information on student and school participation from the **SAILS program** itself.

» The **Tennessee Higher Education Commission** (THEC) provided data on students' postsecondary enrollment from 2010-11 through 2016-17, and postsecondary completion from 2010-11 through 2015-16.

» The **Tennessee Board of Regents** (TBR) provided data on postsecondary course-taking, credit accumulation and course grades for 2010-11 through 2016-17. Because the SAILS exemption from remediation applied to all Tennessee community colleges, we focus on analyzing student's outcomes at these colleges.

» **ACT** provided a rescaled version of the ACT math test scores taken in students' junior year for the graduating high school class of 2015-16—including a finer-grained scale than the integer scale which they use for public reporting.

» **P20 Connect** (formerly MeasureTN), which manages Tennessee's longitudinal data system, served as an intermediary in the data collection process. The agency cleaned, matched, and transferred K-12 and postsecondary data from TDOE, THEC and TBR.

In addition to administrative data, we administered a **posttest** and **survey** to a sample of approximately 16,000 high school seniors at 119 high schools that were implementing SAILS in 2015-16. We excluded high schools that were in their first year of SAILS. We focused our data collection for the posttest and survey on students enrolled in certain senior year math classes: all Bridge Math and SAILS classes, as well as all Advanced Algebra and Trigonometry, Pre-Calculus, and Statistics classes.[5] To lessen the burden on schools, we excluded students in advanced classes such as Advanced Placement and International Baccalaureate classes, since these students were likely to be far above the cut-off. We also excluded students in Geometry classes (which, when taken by seniors, consisted of students scoring far below the threshold).[6] Among students in the targeted classes, we received posttest and survey results for 69% of students (approximately 11,000 students). Excluding students without valid answer sheets and those that ACT and MeasureTN were unable to match to their records, we collected responses from approximately 61% of students in targeted classes.

The 50-minute, 35-item posttest administered to seniors was an abbreviated version of the ACT math test. It measured the same constructs as the original test. Students took the assessment at the end of their senior year math course either in November 2015 (for fall semester courses) or April 2016 (for spring semester or full-year courses). The posttest was scored by ACT and matched to students' prior ACT records and K-12 records from TDOE. The new scale, which ranged from 333 to 680, provided a finer level of detail than the integer scale ACT uses for public reporting (which allowed us to identify students within a fraction of a point above and below the remediation cutoff of 19). During the last five minutes of the posttest, students completed a 15-

5 The research design that we use to estimate the effect of SAILS on students' posttest scores is a regression discontinuity design, described in greater detail in the Empirical Methods section. The sample used in such a design only includes students scoring within a narrow band above and below the cutoff, which is why our posttest data collection focused on this group.

6 We also included classes labeled Finite Math, Discrete Math, Core Mathematics, and Algebra II when we saw large numbers of seniors enrolled. However, other courses, particularly junior year courses with less than three seniors enrolled, were also excluded to alleviate the burden of in-class testing for these teachers.

item student survey on topics such as perceptions of their math courses, attitudes towards math, and postsecondary aspirations.

Finally, we invited all 370 teachers in the targeted classrooms to complete an **online survey that focused on teacher background, experience and teaching methods**. Eighty-five percent of targeted teachers responded. We used this to test for differences in teacher characteristics and the nature of the instruction offered to students above and beyond the remediation threshold. (We summarize the teacher survey results in the Appendix.)

Table 1. Characteristics of High Schools by First Year of SAILS Implementation

	YEAR OF SAILS IMPLEMENTATION			AS OF 2015-16	
Student characteristics:	2013-14	2014-15	2015-16	Ever SAILS	Never SAILS
Male	0.51	0.50	0.51	0.51	0.51
White	0.80	0.67	0.70	0.75	0.65
Black	0.16	0.30	0.26	0.22	0.30
Hispanic	0.07	0.05	0.06	0.06	0.06
English Language Learner	0.07	0.04	0.05	0.06	0.06
IEP	0.13	0.12	0.11	0.12	0.12
Days absent	12.76	12.89	10.62	12.40	10.61
Graduate high school	0.92	0.89	0.86	0.90	0.88
Urban high school	0.31	0.26	0.34	0.30	0.42
Suburban/town high school	0.33	0.42	0.30	0.34	0.41
Rural high school	0.36	0.32	0.36	0.36	0.17
Took senior math	0.90	0.89	0.87	0.89	0.88
Took SAILS	0.29	0.28	0.16	0.26	0.00
Have ACT math (rescaled)	0.68	0.69	0.71	0.69	0.70
ACT math	18.37	18.06	18.47	18.30	19.23
ACT math < 19	0.50	0.53	0.49	0.51	0.46
12^{th}-grade students (*N*)	24,511	11,311	12,224	49,622	24,115
Schools beginning SAILS (*N*)	101	66	58	-	-
Total schools using SAILS (*N*)	120	180	236	245	166

Notes: The table reports characteristics for 2015-16 seniors in schools that had started SAILS in 2013-14, 2014-15 or 2015-16. The label, "Ever SAILS," refers to schools that ever participated in SAILS between 2013-14 and 2015-16. Rescaled scores refer to the finer-grained ACT scores ranging from 333 to 680.

SAMPLE

Table 1 presents the characteristics of 2015-16 seniors, by the year in which their high school began offering the SAILS program. (Because records were incomplete for the 20 schools which implemented the program in 2012-13, we excluded those schools from all years.) The 101 high schools which began implementing SAILS in 2013-14 had somewhat higher percentages of White students, fewer Black students, and higher high school graduation rates for seniors than schools that implemented SAILS in the two subsequent years. However, the three waves of high schools each had similar proportions of students scoring below the remediation threshold of

19 (50%, 53%, and 49% respectively). Relative to the schools that had never offered the SAILS program as of 2015-16, the SAILS schools were more likely to be rural (36% vs. 17%), more likely to have students with ACT math scores below 19 (51% vs. 46%), and more likely to have a higher share of White students (75% vs. 65%).

Of the students who enrolled in the SAILS course in 2015-16, the vast majority (92%) completed the course successfully. Completion rates were the same in 2014-15 but were lower in 2013-14 (69%).[7]

Before arriving at our sample for the posttest analysis, we went through three stages of non-random selection: First, out of the 187 schools that were in their second or third year of SAILS implementation in 2015-16, only a subset of 119 schools agreed to participate. Second, within those 119 schools, we chose a subset of math classes in which to test students, focusing on classes with students most likely to be near the remediation cutoff. Third, either due to student absences, non-consent, invalid answer sheets, or the inability to match student codes to the longitudinal data, we obtained usable math scores for 61% of the students in the targeted math classes.

Table 2 compares the student characteristics of the sample to the underlying population at each phase of selection. The first column describes the characteristics of all students in all eligible schools (schools that were in their second or third year of implementation in 2015-16). The second column reports the characteristics of all students in the subset of schools that agreed to participate. The participating schools were very similar to the eligible schools in most measures, including demographic characteristics and test scores.

The third column reports the characteristics of all students in the targeted math classes in the participating schools. Because our study was focused on students just above and below the remediation threshold, we excluded advanced math classes (such as Advanced Placement) as well as classes for students who were far behind for their grade (such as Geometry).[8] Students in the targeted classes tended to have lower than average ACT scores, with 63% scoring below 19 compared to 54% in the full sample. They were also more likely to participate in SAILS than the full population of students in the participating schools (43% vs. 33%).[9]

The fourth column reports the characteristics of the students in the targeted classes for whom we have posttest outcomes. The sample with posttest scores was similar to those in the targeted classes, with the exception that they have slightly fewer days absent and are more likely to graduate. Students who dropped out during their senior year or who were frequently absent may not have been present to take the test.

7 Higher Education For Higher Standards, "Precollege Interventions Help Increase College Readiness, Reduce Remediation" December, 2016. http://higheredforhigherstandards.org/scalingsails/

8 Geometry was frequently the special education track courses.

9 They were also more likely to take senior year math (99% vs. 92%) and were more likely to graduate conditional on beginning their senior year (96% vs. 93%).

Table 2. Comparing the Posttest Sample to the Full Sample of Eligible Schools (2015-16)

	ELIGIBLE SCHOOLS (*N*=187)	ELIGIBLE SCHOOLS PARTICIPATING IN THE POST-TEST AND SURVEY (*N*=119)		
Student characteristics	All students	All students	Students in targeted classes	Students with posttests
Male	0.51	0.50	0.49	0.49
White	0.76	0.75	0.75	0.78
Black	0.21	0.22	0.22	0.19
Hispanic	0.06	0.06	0.07	0.06
English Language Learner	0.06	0.06	0.06	0.05
IEP	0.12	0.12	0.06	0.05
Days absent	12.98	11.97	12.58	10.73
Graduate high school	0.91	0.93	0.96	0.99
Urban high school	0.29	0.28	0.28	0.25
Suburban/town high school	0.36	0.36	0.37	0.38
Rural high school	0.36	0.35	0.35	0.37
Took senior math	0.90	0.92	0.99	1.00
Took SAILS	0.30	0.33	0.43	0.46
Has ACT math	0.78	0.80	0.86	0.89
ACT math	18.24	18.13	17.54	17.79
ACT math <19	0.52	0.54	0.63	0.63
Have ACT math (rescaled)	0.69	0.75	0.83	0.89
ACT math (rescaled)	499.40	499.05	494.74	497.72
12^{th}-grade students (*N*)	37398	22317	15808	9560

Notes: The eligible schools are the 187 schools which were in their second or third year of SAILS implementation in 2015-16. The eligible, participating schools are the subset of 119 schools which agreed to participate in the posttest and student survey. Column 3 limits the sample to those students which were in classes that administered the posttest. Column 4 limits this sample further to those students which actually took the posttest. Rescaled scores refer to the finer-grained ACT scores ranging from 333 to 680.

EMPIRICAL STRATEGY

We use two different research strategies to measure the impact of the SAILS program:

First, we exploit the fact that SAILS was implemented in waves, with high schools implementing the program in different years. For this part of the analysis, we use a difference-in-difference design, comparing the change in outcomes in schools in the year they began implementing SAILS against the change for those that had not yet implemented or had implemented SAILS in some prior year. By focusing on changes in outcomes, we implicitly control for pre-existing differences between schools as well as for policy changes which might have affected all schools in a given year. The key assumption underlying the difference-in-difference strategy is that the year-to-year change in SAILS-participating and non-participating schools would have been the same if not for the implementation of SAILS.

The co-requisite policy was implemented in the fall of 2015, affecting the high school seniors of 2014-15 and subsequent years. Therefore, by using the cohort of students who graduated in 2013-14, we can estimate the effect of the SAILS program in the context of pre-requisite remediation. Similarly, we use the cohorts of students graduating in 2014-15 and 2015-16 to estimate the effect of the SAILS program in the context of co-requisite remediation.

Second, like much of the prior research literature cited above, we use a regression discontinuity design to identify the impact of being assigned to remediation of any kind (SAILS, co-requisite and pre-requisite) versus no remediation. Essentially, we compare the outcomes of all students immediately above or below the remediation cutoff (not just those enrolled in SAILS). The outcomes we study include college enrollment, persistence, the likelihood of taking remedial and college-level math, passing college-level math, and subsequent math achievement. The key assumption that we make is that there is no discontinuous change in other student characteristics at the cutoff that could be driving any observed outcome differences; that is, students slightly above and slightly below the cutoff should be generally similar and any differences can be adjusted for by controlling for ACT differences themselves. If the assumption is true (and we provide evidence in the Appendix that there were few differences in observable characteristics at the cutoff), then the difference at the cutoff will provide an estimate of the impact of being recommended for remediation. We interpret the remediation cutoff as akin to random assignment for those near the threshold, with those on one side of the line being assigned to remediation and those on the other side serving as the control group.

Moreover, because we know whether or not a student's school is participating in the SAILS program, we can compare the effect of being assigned to remediation in schools that do and do not offer SAILS. Furthermore, we can compare such a difference in 2013-14, when non-participating schools faced the pre-requisite math policy and into 2015-16, when the non-participating schools faced the co-requisite policy.

The two methods rely on different statistical assumptions in order to yield estimates of the impact of SAILS remediation. In the first, we are assuming that the changes in outcomes would have been the same for remediation-recommended students in participating and non-participating schools if not for SAILS. In the second, we are assuming that the students not recommended for remediation, with scores just above the remediation cutoff, are similar to those below. Both methods yield a similar story.

We provide additional details on the statistical models in the following textboxes.

RESULTS

We start by comparing the change in outcomes for students recommended for remediation when their schools began implementing SAILS (using the difference-in-difference method described in the previous section). We do so separately for schools that started SAILS in 2013-14, when the pre-requisite remediation policy was still in place, and for those that started in 2014-15 and 2015-16, after the co-requisite remediation policy had been adopted. Subsequently, we present our estimates based on the regression discontinuity method described in the previous section, comparing the outcomes for those immediately above and below the remediation threshold.

How We Measure the Effect of SAILS vs. Pre-Requisite or Co-Requisite Remediation

In order to learn about the effect of SAILS remediation compared to other forms of remediation, we exploit the fact that the rollout of SAILS was staggered, with high schools implementing in different years. Using a sample of students who are recommended for remediation, we compare the change in outcomes for students from high schools that implemented SAILS in a given year against the change in outcomes for schools that had not yet implemented SAILS (or had implemented in a prior year). More specifically, we use the following difference-in-differences model:

$$(1)\ Y_{ist} = \beta_0 + \beta_1 SAILS\_HS\_Trad_{st} + \beta_2 SAILS\_HS\_Coreq_{st} + \gamma_s + \psi_t + \alpha X_{is} + \varepsilon_{ist}$$

In equation (1), Y_{ist} is an outcome for student i in high school s in year t. The two main treatment variables of interest are $SAILS\_HS\_Trad_{st}$ and $SAILS\_HS\_Coreq_{st}$. $SAILS\_HS\_Trad_{st}$ equals one for high schools implementing SAILS in a year when students will face traditional pre-requisite remediation (i.e., those graduating before 2014-15). $SAILS\_HS\_Coreq_{st}$ equals one for all high schools using SAILS for students who will face co-requisite remediation in college (i.e., those graduating in 2014-15 or later). This model allows us to estimate how the effect of SAILS differs between an environment with traditional remediation versus co-requisite remediation.

γ_s and ψ_t are high school and year fixed effects respectively. X_{is} is a vector of student demographic characteristics. ε_{ist} is a stochastic error term. Standard errors are clustered at the high school level. The sample is limited to 12th-grade students who are flagged as needing remediation based on whether they score below 19 on their first junior year ACT score.

One advantage of a difference-in-differences design is that it controls for other statewide policy changes that may have affected all 12th graders in the state over this period of time. For example, in fall 2015 Tennessee began the Tennessee Promise program, which provides students studying at two-year colleges with a last dollar scholarship. Because both the SAILS-participating schools and the non-participating schools are facing the same changes in the policy environment, the difference in the change in outcomes for the two groups of high schools (i.e., the difference in the difference) allows us isolate the effect of the SAILS program.

The identifying assumption made in this design is that if SAILS high schools had not been using SAILS, students' outcomes would have followed the same trend as that observed in the non-SAILS high schools. One violation of this assumption would be if the adoption of SAILS was related to underlying trends in the outcome variables. For example, if schools that adopted SAILS had remediation rates which were increasing faster or slower than the rates at non-adopting schools. Appendix Table A4 shows that for the majority of outcomes, the trends for SAILS and non-SAILS schools were parallel in the lead-up to the adoption of SAILS.

How We Measure the Effect of Remediation versus No Remediation

To estimate the effect of remediation on math achievement and student survey outcomes, we exploit the fact that remediation in Tennessee targets students with an ACT math score below 19. We use the following regression discontinuity model to compare the outcomes of students just above and just below the remediation cutoff:

$$(2)\ Y_{is} = \beta_0 + \beta_1 Below19_{is} + \beta_2 ACTMath_{is} + \beta_3 ACTMath_{is} * Below19_{is} + \delta_s + \lambda X_{is} + \varepsilon_{is}$$

In this model, Y_{is} represents the outcome for a student i in school s. $Below19_{is}$ is an indicator for student i in school s having an ACT math score below 19. $ACTMath_{is}$ represents a student's junior year ACT Math score (centered at the cutoff) and serves as the running variable. For the class of 2015-16, we have a rescaled version of the ACT with finer units than the integer units used for public reporting. (For 2013-14, we have only the integer scores.) δ_s are high school fixed effects, and X_{is} denotes a vector of student-level demographic controls for race and gender. ε_{is} signifies the idiosyncratic error term. In this model, the coefficient of interest is β_1, which captures any discontinuous shift in the expected value of Y_{is} at the threshold. We cluster errors at the level of the high school.

Because the remediation threshold is publicly known to students, and students might retake the ACT multiple times to avoid remediation, we use a student's *first* ACT math score in their junior year as the running variable. (Very few students take the test before junior year). Following McCrary (2008), we check that the density of students is smooth through the cutoff for each of the subsamples we use for our estimates in Figure A1 and A2.

We also test for balance in the covariates near the threshold. In Appendix Tables A5, A6, and A7, we see that student characteristics are generally smooth through the cutoff. The few differences that we observe are consistent with the number of false positives that we would expect when testing this many hypotheses. Moreover, we control for these demographic characteristics in our main estimates and find that the results are not sensitive to their inclusion.

We estimate equation (2) using a sample of students in the 119 SAILS high schools that took the posttest and student survey in 2015-16. We further restrict this sample to different cohorts of students in order to study the effects of different types of remediation.

We report estimates of β_1 for four distinct samples: in high schools with and without the SAILS program, in 2013-14 (when the control group schools faced the pre-requisite policy) and in 2015-16 (when the control group schools faced the co-requisite policy).

MEASURING THE EFFECT OF SAILS UNDER PRE-REQUISITE AND CO-REQUISITE REMEDIATION

In Table 3, we estimate the effect of SAILS by examining the change in student outcomes for remediation-recommended students when their high school began using SAILS. We split the impact estimates into two groups: students who are exposed to a pre-requisite remediation policy in college, and students exposed to a co-requisite remediation policy. To provide some perspective in interpreting the magnitude of the impacts, we also report in the first column the mean outcomes for the comparison students (those who had an ACT math score below 19 in high schools that were not participating in SAILS).

In the top panel, we report impacts on outcomes which apply to all high school seniors, whether or not they enter postsecondary education. As reported in the top row, when a high school began implementing SAILS, about 40% of remediation-recommended students enrolled in SAILS. Our interviews with school staff suggested that individual student enrollment decisions were based on a number of factors including college-readiness or comfort with technology and that there were capacity constraints limiting SAILS participation in schools (IMPAQ International, 2016). Specifically, many schools had limits on the number of computers available for the computer-based curriculum. It is precisely because we may not observe all the various ways that students might be selected into SAILS that we measure the effect of SAILS implementation on the average student recommended for remediation (not the selected subset of students that participated in SAILS.) To generate an estimate of the impact of SAILS participation, we can then scale the estimated impacts by dividing by the proportion of students who enrolled in SAILS.

As reported in the remaining rows in the top panel, SAILS implementation was not accompanied by any increase in high school graduation, postsecondary enrollment, or a change in the type of postsecondary institution attended. Although the program was not intended to boost high school graduation rates, our results also imply that the prospect of taking SAILS did not discourage students from finishing their senior year either. The ability to complete remediation in high school also did not lead students to attend college at higher rates.[10]

In the lower panel of Table 3, we report impacts on a range of outcomes for students who entered community college within one year after graduating from high school (thus, the smaller sample sizes in the bottom panel). Our estimates suggest that implementation of SAILS was accompanied by a 28-percentage point decline in the percentage of community college entrants taking remedial math during their first year in college. In other words, SAILS implementation succeeded in shifting the locus of remediation from college back to high school for 28% of remediation-recommended community college students. The impact was similar under the pre-requisite and co-requisite policy.

The results in Table 3 also help to explain why there was not a 100% reduction in college remediation among students attending SAILS high schools. First, as noted above, only about 40% of remediation-recommended students at participating high schools enrolled in SAILS.

10 The latter finding, especially the lack of change in the proportion of students attending community colleges, is relevant to our analysis of outcomes for community college entrants. A change in college going might have signaled a change in the unmeasured characteristics of students attending community college.

Table 3. Measuring the Effect of SAILS Implementation under Pre-Requisite and Co-Requisite Remediation

Dependent variable	Mean outcomes for seniors from non-SAILS schools	SAILS IMPACT: Under pre-requisite remediation	SAILS IMPACT: Under co-requisite remediation	*N*
SAILS participant	0.00	0.407***	0.409***	198,091
		(0.024)	(0.019)	
High school (HS) graduate	0.92	-0.000	-0.001	198,091
		(0.005)	(0.004)	
Enrolled in any college by spring of 1st year after HS	0.52	-0.004	0.017*	198,091
		(0.010)	(0.010)	
Enrolled in two-year college by spring of 1st year after HS	0.27	0.001	0.012	198,091
		(0.009)	(0.009)	
Enrolled in TBR CC by spring of 1st year after HS	0.23	0.002	0.008	198,091
		(0.009)	(0.008)	
Enrolled in TCAT by spring of 1st year after HS	0.04	0.000	0.002	198,091
		(0.004)	(0.005)	
Enrolled in four-year college by spring of 1st year after HS	0.26	-0.006	0.004	198,091
		(0.007)	(0.006)	
Outcomes for TN CC Enrollees				
Took remedial math by spring of 1st year	0.66	-0.283***	-0.284***	49,502
		(0.023)	(0.018)	
Took remedial math by spring of 2nd year	0.72	-0.299***	-0.322***	39,985
		(0.024)	(0.022)	
Took math by spring of 1st year	0.45	0.140***	-0.035***	49,502
		(0.019)	(0.013)	
Took math by spring of 2nd year	0.53	0.066***	-0.055***	39,985
		(0.017)	(0.014)	
Pass math by spring of 1st year	0.30	0.063***	-0.054***	49,502
		(0.014)	(0.012)	
Pass math by spring of 2nd year	0.37	0.021	-0.062***	39,985
		(0.014)	(0.014)	
Pass math by spring of 1st year (if took college math by spring of 1st year)	0.66	-0.066***	-0.047***	22,621
		(0.024)	(0.017)	
Pass math by spring of 2nd year (if took college math by spring of 2nd year)	0.71	-0.038**	-0.038**	21,865
		(0.018)	(0.016)	

Table 3. Continued

Dependent variable	Mean outcomes for seniors from non-SAILS schools	SAILS IMPACT: Under pre-requisite remediation	SAILS IMPACT: Under co-requisite remediation	*N*
Withdrew from math by spring of 1st year (if took college math by spring of 1st year)	0.08	0.038*** (0.012)	0.042*** (0.009)	22,621
College credits earned by spring of 1st year	16.33	0.965*** (0.274)	0.210 (0.298)	42,398
College credits earned by spring of 2nd year	25.04	2.210*** (0.548)	0.736 (0.707)	34,339
Retook ACT (if enrolled in TN CC)	0.58	-0.024 (0.020)	-0.018 (0.013)	49,502
Return for 2nd year of college	0.64	-0.011 (0.014)	-0.013 (0.013)	39,985
Earned associate degree by 2nd year	0.04	0.002 (0.006)	-0.003 (0.005)	39,985
Earned certificate by 2nd year	0.04	0.008 (0.008)	-0.003 (0.007)	39,985

Notes: For the outcome in each row, we report coefficients on two indicators: one for attending a SAILS high school during a period of pre-requisite remediation (i.e. those graduating in 2013-14) and another for attending a SAILS high school during co-requisite remediation (those graduating after 2013-14). The excluded category represents students attending non-SAILS schools in that year. The sample consists of students with junior year ACT math scores below 19, who were high school seniors in a public high school between 2010-11 and 2015-16. All specifications include high school fixed effects and demographic controls for race, ethnicity, and sex. In the second panel, the sample is limited to students enrolling in a Tennessee community college by the spring of 1 year after high school, regardless of degree intention. The sample size for outcomes 2 years after graduation are smaller because they are not available for the 2015-16 cohort of seniors. The count of total credits is missing for approximately 13% of the sample. Heteroskedastic-robust standard errors are clustered by high school and are reported in parentheses (**p*<.10, ***p*<.05, ****p*<.01).

Second, many students with junior year ACT math scores below 19 did not take remedial math during their first year in college. As reported in Table 3, 66% of students at non-participating high schools with ACT math scores below 19 (all of whom would have been recommended for remediation) actually took remedial math in their first year of college.[11] The product of these two percentages (40% x 66% = 27%) is similar to the estimated reduction in remediation in Table 3 (28%).

In the final rows in Table 3, we report impacts on the proportion of students taking and passing college math during their first and second years in community college. We start by focusing

11 There are several ways that students could avoid remediation. One is that 58% of community college entrants retook the ACT at least once, giving them an opportunity to raise their scores. Secondly, students were also able to take the ACCUPLACER test, and if they received a score above a certain level, they could skip remediation. A third is that some students may be enrolling in non-degree programs. A fourth reason is that the remediation requirements may be waived for some students. A fifth reason is that students may delay taking their remedial requirements to later years.

on the impacts of SAILS implementation under the pre-requisite policy (the first of the two columns of impact estimates). For the graduating class of 2013-14, SAILS implementation was accompanied by a 14-point boost in the percentage of remediation-recommended students taking college math during their first year (relative to 45% in the non-participating schools). However, many of these students did not pass college math. After accounting for failures and withdrawals, there was a 6.3-point boost in the percentage of remediation-recommended students passing college math by the end of their first year in college (still 20% higher than the percentage of students from non-participating high schools that had passed college math, 30%).

In other words, we estimate that SAILS-implementation led to a 14-percentage point increase in college math enrollment. A subset of these students—about 6.3 percentage points—passed college math. The ratio (6.3/14) implies that fewer than half of the new enrollees in college math passed the course.

We also report in Table 3 the estimated impact on the percentage of students passing college math under the condition that they took the course. (The prior percentages used as a denominator the number of community college entrants, not the smaller number of community college entrants who had enrolled in college-level math.) Our estimates imply that the passing rate for students enrolled in college math *declined* by 6 percentage points when schools began implementing SAILS. (This does not mean that any individual student was less likely to pass as a result of taking SAILS. Rather, it is more likely that the *incremental* students enrolling in college math after completing SAILS were simply less likely to pass college-level math than the remediation-recommended students who succeeded in taking college math from the non-participating high schools.)

The estimated impacts on the percentage of students taking and passing college math were smaller by the end of the second year in college, as students from the non-participating high schools had the opportunity to complete their remedial courses and enroll in college-level classes. By the end of the second year, the percentage of students from non-participating high schools that had taken college math grew by 8 percentage points, from 45% to 53%. That increase was accompanied by a corresponding decrease in the estimated impact of SAILS implementation from 14 percentage points for students in their first year back to 7 percentage points for students in their second year. By the end of the second year, the estimated impact on the percentage of students passing college math was still positive—2 percentage points—but it was no longer statistically significant.

We estimate that SAILS implementation under the pre-requisite policy also resulted in a small increase in the number of college credits students completed: 1.0 credits during the first year and 2.2 credits by the end of the second year.

The estimates in Table 3 represent the impact on the average remediation-recommended student when a high school joined the SAILS program—what researchers would call the "intent to treat" effect. However, that's different from the impact of participating in SAILS—what researchers would call the impact of the "treatment on the treated." Only 40% of remediation-recommended students actually enrolled in SAILS. Among community college enrollees, 49% enrolled in SAILS. If we are willing to assume that SAILS had no effect on the outcomes of remediation-eligible students who did not participate (which seems reasonable), we can infer the effect of

SAILS participation by dividing the average impact on each outcome by the participation rate. For example, the 14-point increase in the percentage of remediation-recommended seniors taking college math in their first year translates to a 29-percentage point increase in math enrollments for SAILS participants. The 6.6-point increase in the percentage of students passing college math translates to a 13-point increase in the percentage of SAILS participants passing college math. The 2.2 credit increase in credits by the end of the second year translates to a 4.5 credit increase for SAILS participants. These are all similar to the improvements we reported in Figure 2 between 2013-14 and 2014-15, when community colleges moved to the co-requisite policy and allowed students with ACT scores below 19 to enroll directly in college-level math. (Since the policy applied to all remediation-recommended students, the "treatment on the treated" and the "intent to treat" effects were the same for the co-requisite policy.)

Although positive, the impact on credit completion—whether viewed overall or for SAILS participants—are smaller than would be required to generate large changes in degree completion within two years. The average community college entrant from a SAILS high school would have completed 27.2 credits by the end of the second year after high school (2.2 credits more than 25 (the average number of credits completed by the control group). Yet the typical associate degree in Tennessee requires 60 credits. Only 4% of remediation-recommended community college entrants completed an associate degree within 2 years. An additional 4% of remediation-recommended students completed a certificate program. Some students may not have been seeking an associate degree or certificate, and two years is shorter than the length of time many students require to complete. Nevertheless, we saw no statistically significant impact of SAILS implementation either on the proportion of students completing an associate degree or completing a certificate within 2 years.

The second column of impact estimates in Table 3 is based on the two waves of schools implementing SAILS under the co-requisite remediation policy: the high school seniors of 2014-15 and 2015-16. As reported in the top panel, there was a similar shift in the locus of remediation from college back to high school, as the students from SAILS participating high schools were 28 percentage points less likely to enroll in college remediation during their first year in college. However, the subsequent estimates of the impact of SAILS under the co-requisite policy are quite different from those under the pre-requisite policy. We no longer estimate a positive impact on the proportion of students taking or passing college math during their first year. In fact, the estimates are negative (although we explain below why we think the negative finding is anomalous). Likewise, the estimated impacts on college credits completed by the end of the first and second year of college is not significantly different from zero.

That is not entirely surprising, since the two policies—SAILS and co-requisite remediation—serve largely the same purpose. Both eliminate the barriers to entry into college math: in the case of SAILS, by allowing students to complete their remediation in high school and, in the case of co-requisite, by allowing students to do their remediation concurrently with college math. There are other differences between the two policies which might have led us to observe an impact of SAILS under the co-requisite policy. For instance, by allowing students to do their remediation in high school rather than in college, it is possible that SAILS would have allowed students to take more college-level courses during their first year of college. On the other hand, the knowledge imparted during a refresher course in math may fade between the senior year in high school and the first year in college. The students in non-SAILS high schools may have benefited from doing their remediation concurrently with their college math course (i.e., "just in time" remediation.) Whether or not either of these advantages for the two approaches were substantive, they seem to have canceled each other out, as the net effect of SAILS implementation on credit accumulation under the co-requisite policy was zero.

Although the SAILS program had no incremental effect on credit accumulation after the co-requisite policy was introduced, SAILS still may have generated cost savings. Virtually all of the reduction in college remediation expenses for the 28% of students shifting their remediation to high school would have represented a net savings. There are two reasons: first, after implementation of the Tennessee Promise program, it was often the Tennessee taxpayer (or state lottery participant) and not the student who would have been paying the tuition associated with co-requisite courses; second, Tennessee high school students are required to take four years of math, for which the SAILS course counted.[12] In other words, as long as the cost of offering a SAILS course was comparable to the cost of offering another high school math course, any reduction in instructional expenditures on co-requisite math instruction on college campuses would have represented a net cost savings.

Explaining the Negative Estimated Impacts of SAILS on Taking College-Level Math under Co-Requisite Remediation

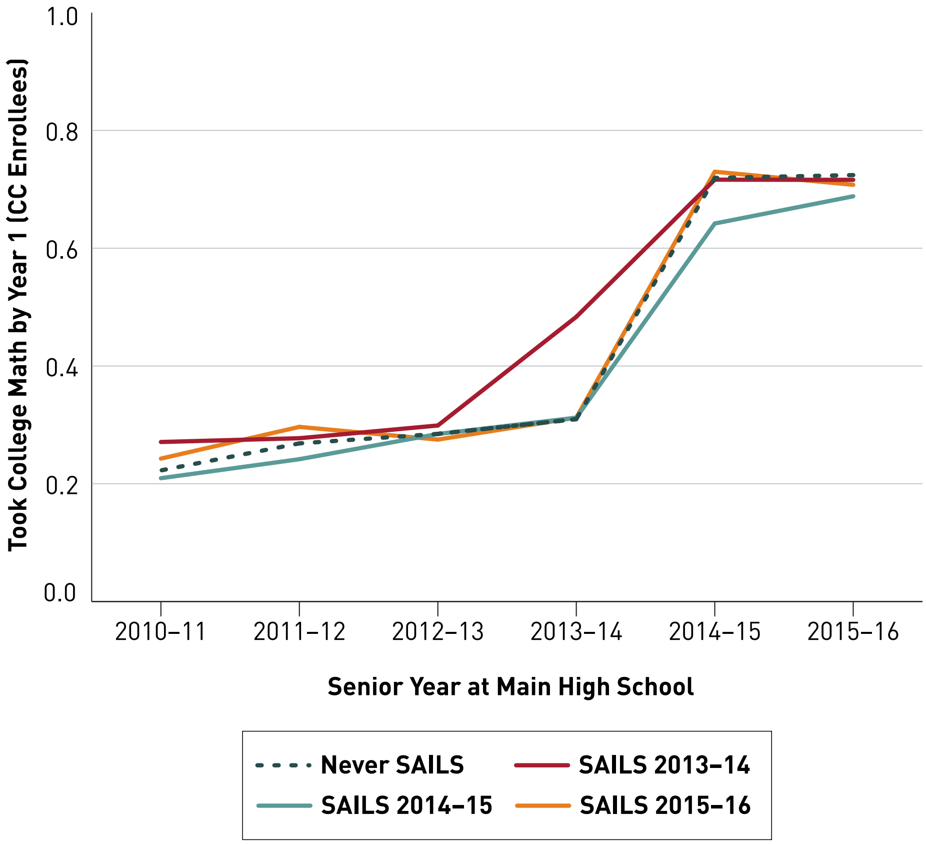
In Table 3, we estimate that the SAILS implementation had a negative impact on the percentage of students taking and passing college math under co-requisite remediation. If interpreted literally, that would mean that students were better off doing their remediation concurrently. Although it is possible that "just in time" remediation is more effective than remediation in high school, we are not ready to draw such a conclusion based on the evidence we have seen. One reason is that in our regression discontinuity analysis (in Table 6 later in the report), we do not see evidence that remediation-recommended students were worse off in SAILS-participating schools in 2015-16. A second reason is evidence that the negative impact estimate of SAILS on taking college-level math under co-requisite remediation (in Table 3) was driven by the uneven implementation of the co-requisite policy in 2014-15, specifically in the colleges serving the high schools launching SAILS in 2014-15. We believe that the true effect of SAILS implementation was more likely zero than negative.

Figure 3 demonstrates why. We report the trend in the proportion of community college entrants taking college-level math for four different sets of high schools: the high schools that first implemented SAILS in 2013-14, 2014-15, 2015-16 and those that had yet to implement SAILS as of 2015-16. For those who were seniors in high school prior to 2013-14 at all four groups of high schools, the proportion of community college-entrants taking college-level math during their first year was the same: roughly 40%. However, for the set of high schools that implemented SAILS in 2013-14 (the red line), the proportion of college entrants taking college-level math increased by roughly 14 percentage points for the high school class of 2013-14. The fact that the increase occurred *only for* the high schools that were implementing SAILS in that year is the reason for the positive 14 percentage point impact of SAILS in Table 3.

12 Approximately three quarters of first-time freshman at Tennessee community colleges received the TN Promise in 2014-15 (Tennessee Higher Education Commission & Student Assistance Corporation, 2018).

In 2014-15, with the implementation of the co-requisite policy and the Tennessee Promise, all four sets of high schools saw an increase in college math-taking, to about 70%. But notice that the rates of math course-taking in the set of high schools that implemented SAILS the year earlier, in 2013-14, were no higher than the other three sets of high schools. Also notice that the cohort of high schools that implemented SAILS in 2015-16 saw no increase in college math course-taking in that year. Both of these facts lead us to conclude that the effect of SAILS on college math course-taking was zero in the context of the co-requisite policy. Although SAILS continued to generate time and cost savings by shifting remediation back to high school, the program's impact on college math enrollments was superseded by the implementation of the co-requisite policy.

Figure 3. Rates of College Math-Taking, by SAILS Cohort (ACT<19)



To understand the source of the negative impact estimate on college math enrollment, observe the trend in math course-taking for the set of high schools that implemented SAILS for the first time in 2014-15 (the green line): their math course-taking also jumped up in 2014-15 (with the co-requisite policy) but *not by as much as in the other three sets of schools*. This was the primary reason for the negative estimated impact of SAILS implementation in Table 3: the schools that implemented SAILS in 2014-15 had *lower* college math course-taking in that year than other schools. However, the difference was short-lived—by 2015-16, college math course-taking from these high schools had caught up with the other three sets of high schools. When we drop from our estimation the high schools implementing SAILS in 2014-15, the estimated impact of SAILS implementation on college course-taking shrinks and is no longer statistically different from zero.

Impact of SAILS Implementation by Student Subgroup

In Section II of the Appendix, we report impacts for various subgroups of students, by ACT score, gender and race. SAILS implementation seemed to have a larger effect on college-math taking for students with ACT scores further below the remediation cut-off (who were less likely to be able to avoid remediation by retaking the ACT). Moreover, the program's effects seemed to be concentrated among female students. For instance, even under the pre-requisite policy, the program had no statistically significant impact on the proportion of male students passing college math during their first year, nor on the number of credits they completed. There were no differences in program impacts by race.

THE CONSEQUENCES OF BEING RECOMMENDED FOR REMEDIATION IN SAILS-PARTICIPATING AND NON-PARTICIPATING SCHOOLS

In Table 4, we report the differences in student outcomes at the remediation threshold (score of 19 on math ACT). We report the difference four times: by whether or not the school was participating in SAILS and by high school graduating year, 2013-14 and 2015-16. While the impact estimates in Table 3 were identified based on the differential changes in outcomes when schools began implementing SAILS, the impacts in Table 4 are identified by differences in outcomes for those just above and below the threshold of 19. The comparison group in Table 3 consisted of similar students in high schools that had not yet implemented SAILS (or had implemented SAILS in some prior year), while the comparison group in Table 4 is the set of students with similar achievement and characteristics who just missed being recommended for remediation. We conduct both analyses to determine if our results are robust to different comparison groups.

In SAILS-participating schools in 2013-14, students just below the remediation threshold were 27 percentage points more likely to enroll in SAILS than those just above the threshold. This is somewhat smaller than the 41-percentage-point estimate of participation in SAILS in Table 4. One possible reason is that estimates in Table 3 are for all students scoring below 19, while the results in Table 4 would apply to those with ACT scores right at the remediation cutoff.

For simplicity, Table 4 reports effects on a subset of the college-going outcomes (for the full set of impact estimates, see appendix tables A8 and A9). At SAILS-participating schools in 2013-14, we estimate that those who were just below the remediation threshold were 26.9 percentage points *more likely* than similar students just above the threshold to take a remedial math class during their first year at a community college. However, that difference was even larger at the non-participating schools, where those below the threshold were 48.5 percentage points more likely to take a remedial class. In other words, if a school were to switch from non-participating to participating in SAILS, the difference between the two (48.5 - 26.9 = 21.6) would determine the reduction in remediation at the threshold.

In non-SAILS-participating schools in 2013-14, students recommended for remediation faced barriers to entry in college-level math courses. Those with an ACT score just below the remediation threshold were 13.8 percentage points less likely than those just above the threshold to take a college math course and 7.8 percentage points less likely to pass a college math course during their first year in a community college. They also accumulated 1.8 fewer college credits during their first year in college.

Table 4. Difference at the Remediation Threshold in SAILS-Participating and Non-Participating Schools: 2013-14 and 2015-16

	SAILS-PARTICIPATING SCHOOLS		NON-PARTICIPATING SCHOOLS		
Dependent variable	Difference at threshold	Control mean	Difference at threshold	Control mean	Difference in Differences
2013-14 High school seniors					
SAILS participant	0.271***	0.14	0.000	0.00	0.271***
	(0.035)		(0.000)		(0.035)
	[11259]		[23287]		
Took Bridge Math	-0.030	0.20	0.162***	0.30	-0.192***
	(0.027)		(0.020)		(0.034)
	[11259]		[23287]		
Outcomes for TN CC enrollees					
Took remedial math by spring of 1st year	0.269***	0.03	0.485***	0.04	-0.216***
	(0.035)		(0.025)		(0.043)
	[3206]		[5232]		
Took math by spring of 1st year	-0.062	0.69	-0.138***	0.67	0.076
	(0.049)		(0.033)		(0.059)
	[3206]		[5232]		
Pass math by spring of 1st year	0.008	0.47	-0.078**	0.49	0.086
	(0.044)		(0.036)		(0.057)
	[3206]		[5232]		
Total college credits earned by spring of 1st year	0.096	22.31	-1.797**	21.84	1.893
	(1.108)		(0.770)		(1.349)
	[2805]		[4621]		
2015-16 High school seniors					
SAILS Participation	0.340***	0.07	0.001	0.00	0.339***
	(0.024)		(0.002)		(0.024)
	[12438]		[5972]		
Took Bridge Math	-0.007	0.11	0.093***	0.20	-0.100***
	(0.017)		(0.026)		(0.031)
	[12073]		[5806]		

Table 4. Continued

Dependent variable	SAILS-PARTICIPATING SCHOOLS: Difference at threshold	SAILS-PARTICIPATING SCHOOLS: Control mean	NON-PARTICIPATING SCHOOLS: Difference at threshold	NON-PARTICIPATING SCHOOLS: Control mean	Difference in Differences
2015-16 High school seniors					
Outcomes for TN CC enrollees					
Took remedial math by spring of 1st year	0.146***	0.02	0.277***	0.01	-0.131***
	(0.020)		(0.033)		(0.039)
	[3939]		[1553]		
Took math by spring of 1st year	0.050	0.72	-0.052	0.76	0.102*
	(0.037)		(0.054)		(0.065)
	[3939]		[1553]		
Pass math by spring of 1st year	0.080*	0.50	-0.011	0.59	0.091
	(0.041)		(0.063)		(0.075)
	[3939]		[1553]		
Total college credits earned by spring of 1st year	-0.288	24.78	0.775	24.05	-1.063
	(0.900)		(1.220)		(1.516)
	[3440]		[1388]		

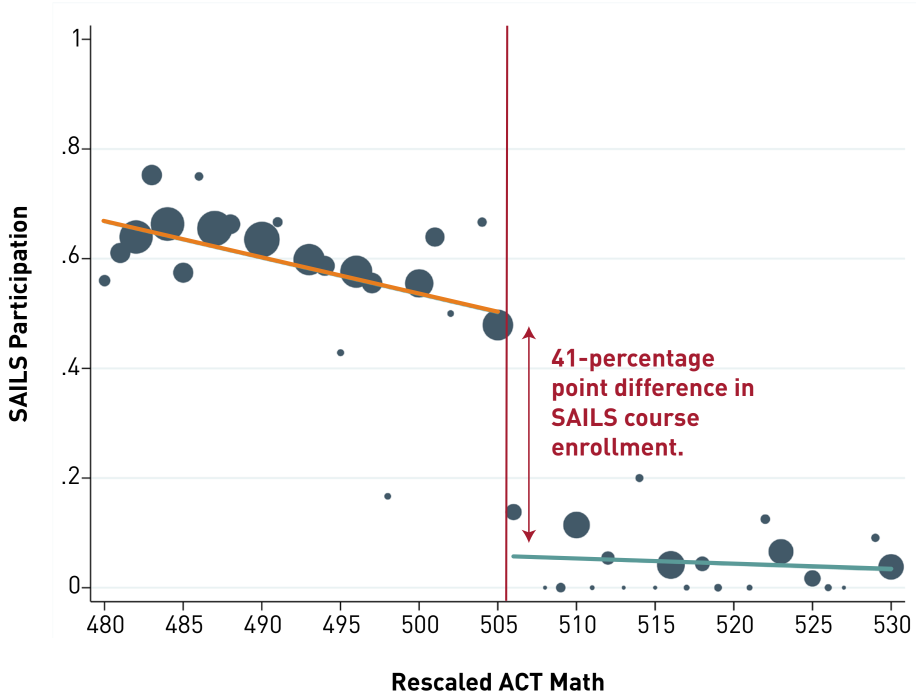
Notes: For the outcome in each row, we report the discontinuity at the remediation cut-off of 19. We assume a linear relationship between the outcome and ACT score (the running variable), although we allow for different slopes above and below the cut-off. (For 2013-14, we used the integer scores usually reported by ACT. For 2015-16, we used a rescaled version of the ACT with finer-grained scale.) All regressions also include high school fixed effects and demographic controls for race, ethnicity, and sex. The sample includes all high school seniors in the noted year, e.g. 2013-14 or 2015-16. In 2013-14, we present estimates using a bandwidth of 4 integer points; in 2015-16, we use a bandwidth of 25 rescaled ACT points. For robustness, we provide estimates in the Appendix Tables A8 and A9 using a variety of bandwidths. Control means are the mean outcomes for students within 10 points above the cut-off on the rescaled measure or a single point above the cut-off using the integer ACT measure. Heteroskedastic robust standard errors are clustered by high school and included in parentheses (*p<0.10, **p<0.05, ***p<0.01).

In SAILS-participating schools, the differences at the remediation threshold were smaller and not statistically significant. Although there were 101 high schools that began offering SAILS in 2013-14, this represents only a quarter of all high schools in the state. As such, we cannot generate a precise estimate of the difference in parameters in the SAILS-participating and non-participating schools in 2013-14. Nevertheless, our results indicate that there was a difference at the remediation threshold in the proportion of students taking and passing college-level math in the schools not participating in SAILS. In the schools with SAILS, those differences were not statistically significant, suggesting that SAILS helped to reduce the negative effects of assignment to remediation.

The bottom panel of Table 4 reports analogous differences in 2015-16, after the implementation of the co-requisite policy. Again, the difference in the magnitude of the shift at the remediation threshold between SAILS-participating and non-participating schools suggests that community college entrants from SAILS schools were 13.1 percentage points less likely to take remedial math than at non-participating schools. Although about half as large as we estimated in Table 3, the results imply that SAILS did shift the locus of remediation from college back to high school.

However, under the co-requisite policy in 2015-16, there were no longer any statistically significant differences at the remediation threshold in the proportion of students taking or passing college-level math—either in the SAILS-participating or in the non-participating schools. This is consistent with our earlier conclusion that SAILS implementation had no effect on college math enrollment or passage once the co-requisite policy was in place. There was also no difference in credit accumulation during the first year in community college between those immediately below and above the remediation threshold of 19.

Figure 4. Proportion of Students Taking SAILS by Rescaled ACT Score



Differences in Student Survey and Test Scores in 119 SAILS-Participating Schools

For the remaining results, we focus on students in the 119 SAILS-participating schools that administered the posttest and student survey in 2015-16. In Figure 4, we report the proportion of students in the 119 SAILS-participating high schools who actually enrolled in SAILS in 2015-16 as a function of their ACT score. To more precisely locate where the jump in participation occurs, we use the finer-grained measure of math achievement provided to us by ACT. The vertical line corresponds to an ACT score of 19. About 50% of those with ACT scores immediately below 19 enrolled in the SAILS program in these high schools, with a small percentage (roughly 10%) of those immediately above the ACT cut-off of 19 enrolled in SAILS. In other words, those who scored a fraction of a point below the remediation cut-off were roughly 40 percentage points more likely to be enrolled in SAILS than those just above the cut-off.

To measure the causal impact of SAILS, we focus on differences in outcomes for all students above and below the remediation threshold, whether or not those students are enrolled in SAILS. We avoid comparing student participants and non-participants in SAILS schools because an individual student's enrollment decision is likely to be influenced by other factors for which we cannot control: e.g., the strength of the students' plans to complete college, a counselor's recognition that they need extra help, their school's ability to pay for computer equipment, etc. For all these reasons and more, the individual students enrolling in the SAILS course are likely to differ from those with similar ACT scores who are not enrolled in SAILS. Rather than assume that we can control for all the relevant differences for students' enrolling in SAILS (which seems unlikely), we are more comfortable assuming that the *average* student (regardless of SAILS enrollment) just above and below the remediation cut-off are similar.

Nevertheless, it is still possible to estimate the impact for the average student enrolling in SAILS using the regression discontinuity method. The sharp change in SAILS participation at the remediation threshold in Figure 4 provides a way to do so. Specifically, as long as being recommended for remediation only impacts a student's posttest scores (and other outcomes) if he or she actually enrolled in the SAILS math course, then we can essentially divide any difference in outcomes at the threshold by the 41-percentage point difference in SAILS enrollment at the threshold. While the difference in student outcomes at the remediation threshold tells us the average effect of *offering* SAILS to students, dividing that difference by the difference in students enrolled in SAILS provides an estimate of the average effect on the students *participating* in SAILS.

Table 5. Difference in Posttest Outcomes at Remediation Threshold in Sampled SAILS HS

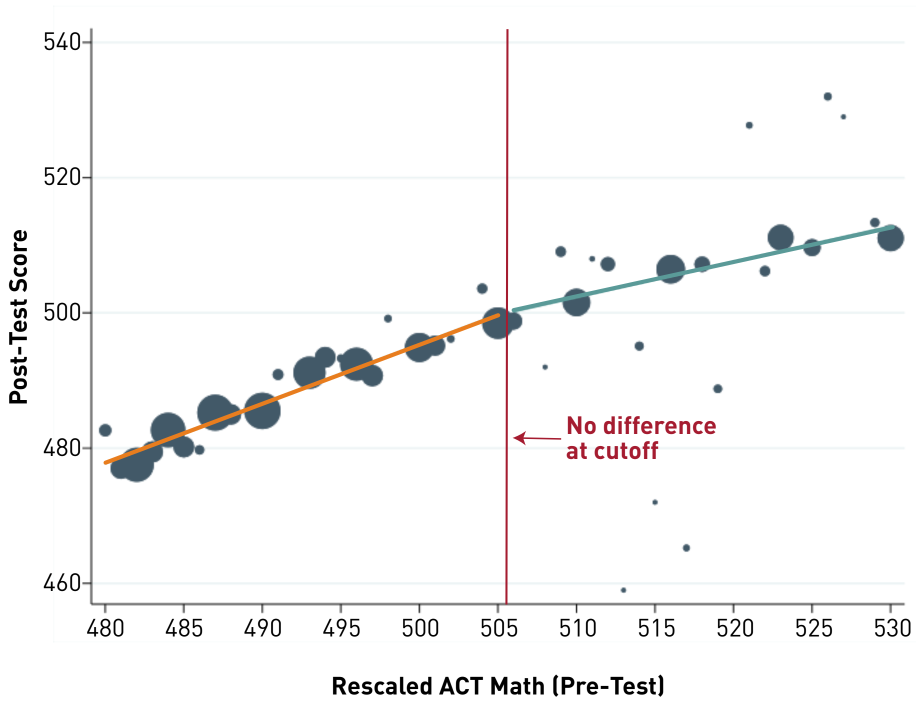
Dependent variable	Difference at cutoff	Control mean
Posttest math scale score	-0.199	501.67
	(2.242)	
Posttest math, % correct	-0.001	0.40
	(0.010)	
SAILS-aligned posttest math, % correct	-0.002	0.46
	(0.011)	
% correct on questions covered in module 1	-0.022*	0.39
	(0.013)	
% correct on questions covered in module 2	0.006	0.47
	(0.014)	
% correct on questions covered in modules 3, 4, and 5	0.007	0.50
	(0.020)	

Notes: For the outcome in each row, we report the discontinuity at the remediation cut-off of 19, assuming a linear relationship between the outcome and the rescaled ACT score (the running variable) and allowing for different slopes above and below the cut-off. We use a bandwidth of 25 on the rescaled ACT, of the rescaled ACT, which runs from 333 to 680. Each specification also includes high school fixed effects and demographic controls for race, ethnicity, and sex. Posttest Math % Correct indicates the % correct on the posttest, as opposed to the scale score on the posttest. SAILS-aligned Posttest % Correct indicates the % correct on the items on the posttest that the SAILS staff indicated as aligned with the SAILS curriculum. Sample includes only high school seniors in the schools that participated in the posttest and student surveys in the 2015-16 school year. Control means are the mean outcomes for students within 10 points above the cutoff. Standard errors are clustered at the high school level (* $p<0.10$, ** $p<0.05$, *** $p<0.01$).

The differences in various high school outcomes at the remediation threshold are reported in Figure 5 and in Table 5. On the ACT math posttest, it appears that the students in the SAILS classes improved no more (or less) than the students taking other math classes during their senior year. Thus, the estimated impact of the school's offering SAILS is not statistically different from zero. (There is little point in dividing by the difference in SAILS enrollment rates (.41), since the impact per enrolled student is also not statistically different from zero.)

Of course, the skills tested on the ACT may differ from the Tennessee Board of Regents Learning Support competencies, on which the SAILS curriculum is based. As a result, we provided a copy of the 35-item posttest to the SAILS team for their review, asking them to identify specific test items which appeared to be aligned with each of the five modules included in the SAILS curriculum. We then constructed a measure of the percent of those items that students answered correctly and conducted a similar analysis, looking for evidence of differences in student performance above and below the cut-off. Those results are reported in Table 5. Although the alignment exercise was a crude one, we did not find evidence of differences between SAILS-enrolled and non-SAILS students for those items judged to be aligned with the five modules.

Figure 5. Score on Posttest by Pretest Score



It is important to remember that the comparison students—those scoring just above the remediation threshold—are also typically enrolled in a math course, since Tennessee students are required to complete four years of math in order to graduate. Other than SAILS and the other remedial math course, Bridge Math, the three most popular courses taken by students with an ACT math score of 19 in our sample of high schools were Advanced Algebra and Trigonometry (34.3%)[13], Pre-Calculus (26.5%), or Statistics (21.9%). Approximately 10% of these students were

13 We also include small numbers of students who enrolled in Discrete Math and Finite Math here.

enrolled in more than one math class.[14] Thus, the estimated impacts on the math posttest in Table 5 is not relative to the absence of a math course, but relative to the average math course that high school seniors with ACT math scores in the neighborhood of 19 typically take.[15]

In Table 6, we report differences in several other survey-based outcomes for those immediately above and below the ACT cut-off of 19. We did not find any significant differences in students' postsecondary college plans for those with ACT scores immediately above or below 19. For instance, there was no difference in students' intention to attend college or their plan to complete an associate or bachelor degree. This is consistent with our earlier findings that when high schools implemented the SAILS program we saw no jump in college enrollment relative to schools which did not introduce SAILS in that year.

However, we did find that students just below the cut-off of 19 were 6.6 percentage points *more* likely to perceive that their math course content would be useful in their careers, 10 percentage points *more* likely to report that they felt prepared for college math, and 6 percentage points *more* likely to say that they were interested in math.

We also found that students just below the 19 cutoff were 8 percentage points less likely to say that their classes stayed busy, which may have been due to the self-paced instructional model used in the SAILS program.

To convert these into impacts per student enrolled in SAILS, we would simply divide by the difference in SAILS enrollment at the cut-off (41 percentage points). Therefore, *enrolling* in SAILS would seem to produce a 16-point increase in the perceived usefulness of math in their careers, a 25-point increase in the perception of being prepared for college math, and a 14-point increase in the percentage of students interested in math. Of course, that also means that enrolling in SAILS implied a 20-percentage point *decline* in the percentage of students perceiving that their class stayed busy.

Table 6. Difference in Student Survey Outcomes at Remediation Threshold in Sampled SAILS High School

Dependent variable	Difference at cut-off	Control mean
Intend to attend college	-0.001 (0.028)	0.86
Intend to attend college full-time	0.006 (0.035)	0.70
Plan to attend two-year	-0.040 (0.035)	0.36
Plan to attend four-year	0.002 (0.039)	0.51

14 Less than 2% of this group seemed not to be enrolled in any math course during their senior year.
15 Appendix Figure A3 shows the fraction of students taking a given math course in their senior year above and below the cutoff in SAILS schools in 2015-16. Just below the cutoff for assignment to remediation, SAILS participation seems to mostly come at the expense of taking Advanced Algebra and Trigonometry, and to a lesser extent Pre-Calculus and Statistics.

Table 6. Continued

Dependent variable	Difference at cut-off	Control mean
Highest expected attainment—AA or certificate	-0.011 (0.033)	0.18
Highest expected attainment—BA/BS	0.003 (0.038)	0.50
Highest expected attainment—BA/BS or Higher	0.011 (0.033)	0.82
Course content useful in career	0.065* (0.037)	0.42
Better prepared for college math	0.102** (0.040)	0.58
More interested in math	0.059** (0.030)	0.20
Class stays busy	-0.080** (0.040)	0.74
Teacher knows when class understands	0.025 (0.042)	0.67
Teacher wants us to use thinking skills	-0.044 (0.035)	0.80
Spend more than 1 hr/night on math homework	0.008 (0.035)	0.26
Spend more than 1 hr/night on any homework	-0.030 (0.035)	0.40

Notes: For the outcome in each row, we report the discontinuity at the remediation cut-off of 19, assuming a linear relationship between the outcome and the rescaled ACT score (the running variable) and allowing for different slopes above and below the cut-off. We use a bandwidth of 25 on the rescaled ACT. Each specification also includes high school fixed effects and demographic controls for race, ethnicity, and sex. The sample includes only high school seniors in the schools that participated in the posttest and student surveys in the 2015-16 school year. Control means are the mean outcomes for students within 10 points above the cutoff. Standard errors are clustered at the school level (* $p<0.10$, ** $p<0.05$, *** $p<0.01$).

In Table 7, we report differences at the remediation threshold for different subgroups of students. It is worth noting that the impact on student perception of being better prepared for college math was particularly large and positive for Black students (43 percentage points). Dividing by the SAILS enrollment rate, this implies more than a 100% improvement in the percentage of Black students feeling prepared for college math. Given the impact on Black students' perceptions of math, it is puzzling that we did not see an impact on posttest scores in Table 7. We also did not see a disproportionate increase in college math enrollment, math passage, or accumulated credits for Black students in Appendix Table A4.

Table 7. Student Survey Impacts at Remediation Threshold by Subgroup

			STUDENT SURVEY			
	SAILS enrollment	Posttest score	Course content useful in career	Better prepared for college math	More interested in math	Class stays busy
Female	0.409***	1.157	0.056	0.126**	0.068	-0.080
	(0.047)	(3.446)	(0.054)	(0.061)	(0.046)	(0.056)
	[2057]	[2057]	[1996]	[1996]	[1994]	[1992]
Male	0.403***	-1.185	0.069	0.089*	0.059	-0.081
	(0.051)	(3.181)	(0.058)	(0.052)	(0.047)	(0.053)
	[1788]	[1788]	[1723]	[1725]	[1721]	[1722]
Black	0.395***	1.771	0.196	0.433***	0.170	-0.183
	(0.073)	(5.977)	(0.122)	(0.133)	(0.107)	(0.119)
	[684]	[684]	[651]	[651]	[649]	[650]
White	0.400***	-0.553	0.081*	0.077*	0.052	-0.051
	(0.038)	(2.511)	(0.041)	(0.042)	(0.034)	(0.041)
	[3072]	[3072]	[2982]	[2984]	[2980]	[2978]

Notes: For the outcome in each column, we report the discontinuity at the remediation cut-off of 19 for the sample of students in the corresponding row, assuming a linear relationship between the outcome and the rescaled ACT score (the running variable) and allowing for different slopes above and below the cut-off. We use a bandwidth of 25 on the rescaled ACT. Each specification also includes high school fixed effects and demographic controls for race, ethnicity, and sex. The sample is limited to high school seniors in 2015-16 in the schools which implemented the posttest and student survey. Sample sizes (in brackets) vary slightly across outcomes due to response rates. Standard errors are clustered at the school level (* p<0.10, **p<0.05, *** p<0.001).

Impacts by High School Subgroup

We also investigated differences at the remediation threshold for various subgroups of high schools: rural vs. non-rural, high-poverty vs. low-poverty, and in schools where students finished the SAILS modules early vs. late in the semester or year. Those results are reported in Table 8. There was little difference in the impact of SAILS in high-poverty vs. low-poverty high schools. However, we find that SAILS was associated with a small negative difference in math achievement in the rural schools (-6.3 points on the rescaled ACT test). When divided by the percentage-point difference in students enrolling in SAILS (44.4 percentage points), this would imply about a half-point decline in ACT scores on a 36-point scale for students in rural high schools enrolling in the SAILS course.

We also calculated the percentage of students who had completed all five modules with 10% of the class time remaining. In effect, this is a measure of students' rate of progression in the SAILS curriculum prior to the final one tenth of the semester or academic year. We observed that a large share of some schools completed the modules in the final weeks and days of the semester, while students in other schools made more steady progress throughout the course. In the 25% of SAILS schools with the lowest completion rate by the last tenth of the semester, only 4% of students had completed all five modules. In the top 25% of SAILS schools, 77% of students had completed SAILS by that point. In the schools where many students completed the modules in the last days and weeks (or didn't complete SAILS at all), students immediately below the threshold were 23.7 percentage points less likely to report that their class stayed busy than the students just above the threshold. In the schools where a higher proportion of students had completed the five modules with time remaining, students were no more or less likely to report that their class stayed busy. In other words, students' perception that SAILS kept them less busy than other math classes was concentrated in schools where large numbers of students completed the program in the closing days or weeks.

Table 8. Heterogeneity by School Subgroup

			STUDENT SURVEY			
	SAILS participation	Posttest math score	Course content useful in career	Better prepared for college math	More interested in math	Class stays busy
Rural high school	0.444***	-6.317**	0.100*	0.103*	0.028	0.016
	(0.061)	(3.032)	(0.058)	(0.055)	(0.052)	(0.056)
	[1550]	[1550]	[1487]	[1490]	[1486]	[1487]
Non-rural high school	0.391***	4.533	0.043	0.109*	0.082**	-0.145***
	(0.046)	(3.077)	(0.050)	(0.057)	(0.036)	(0.053)
	[2295]	[2295]	[2232]	[2231]	[2229]	[2227]
High-poverty school, 65%+ school FRPL	0.429***	1.062	0.109	0.162	0.112	-0.053
	(0.075)	(4.776)	(0.079)	(0.096)	(0.066)	(0.105)
	[884]	[884]	[840]	[843]	[839]	[842]
Low-poverty school, 35% or less school FRPL	0.422***	-6.895	-0.024	0.079	0.061	-0.093
	(0.068)	(5.564)	(0.057)	(0.106)	(0.056)	(0.097)
	[599]	[599]	[583]	[583]	[583]	[582]
% Complete at 90% through the course: Quartile 1	0.285***	-0.256	0.045	0.161	0.099	-0.237**
	(0.089)	(4.917)	(0.074)	(0.126)	(0.087)	(0.104)
	[614]	[614]	[583]	[583]	[582]	[581]
% Complete at 90% through the course: Quartile 4	0.344***	1.673	0.123	0.087	0.025	0.047
	(0.075)	(3.946)	(0.075)	(0.067)	(0.057)	(0.062)
	[1077]	[1077]	[1060]	[1059]	[1058]	[1058]

Notes: For the outcome in each column, we report the discontinuity at the remediation cut-off of 19 for the sample of students in the corresponding row, assuming a linear relationship between the outcome and the rescaled ACT score (the running variable) and allowing for different slopes above and below the cut-off. We use a bandwidth of 25 on the rescaled ACT. Each specification also includes high school fixed effects and demographic controls for race, ethnicity, and sex. The sample is limited to high school seniors in 2015-16 in the schools which implemented the posttest and student survey. % Complete at 90% through the course is a measure of the percentage of students who have completed all five modules after 90% of the classes in the semester or year had been held. Sample for all outcomes is limited to high school seniors from 2015-16 in the schools which implemented the posttest and student survey. Standard errors are clustered at the high school level (* $p<0.10$, ** $p<0.05$, *** $p<0.01$).

CONCLUSION

In the fall of 2013, Tennessee Governor Bill Haslam announced the "Drive to 55" campaign with the goal of nearly doubling the share of Tennesseans with a college credential from 32% to 55% by 2025 (Drive to 55, 2015). At the time the Governor made the announcement, *half* of Tennessee high school seniors had ACT math scores below the level considered necessary for enrolling in college-level math in the state's community colleges. Therefore, leaders in Tennessee began to rethink the options available to such students, with the goal of eliminating any needless barriers to degree completion.

Over the last five years, the state has implemented two major initiatives to allow students to enroll directly in college-level coursework in community colleges: the SAILS program allows students to complete their math remediation during the senior year in high school, and the co-requisite remediation policy allows students to complete their remediation concurrently with college coursework. Our results suggest that both were effective in opening the doors to college-level coursework, increasing the proportion of remediation-recommended students taking college-level math in their first year at community college by roughly 30 percentage points.

However, the impacts of the SAILS program and the co-requisite policy were not additive. Once the co-requisite policy was in effect, the students in SAILS participating schools were no more likely than non-participants to take or complete college math and there was no difference in credit accumulation. In other words, once the co-requisite policy was adopted, the main effect of SAILS has been to shift the locus of remediation back to high school for roughly a quarter of remediation-recommended students.

However, our evidence suggests that SAILS is not improving students' math achievement or boosting their likelihood of passing college math once they take the course. Of the additional students who were able to take college-level math after the introduction of SAILS, only about half passed college math. In effect, until the co-requisite policy was in place, the SAILS program opened the door to college-level math for many students and about half of the incremental entrants made it through the course successfully. The co-requisite policy opened the same door for the remaining students, with the same result, that about half of the incremental entrants passed. And by the end of their second year in college, they had completed 4.5 more credits. Positive yes, but hardly a watershed.

In other words, pre-requisite remediation was not the primary barrier to student progress, nor is the current form of remediation boosting students' success rates in college math.

Achieving significant improvements in the number of Tennesseans with a postsecondary credential will require identifying and clearing other barriers to college completion, such as providing more intensive supports to college students to improve retention and degree attainment. In a review of remedial education reform efforts in recent years, Bailey et al. (2016) found that programs with comprehensive, integrated, and long-lasting student supports produced the largest increases in college success outcomes (Bailey et al., 2016). For example, programs like the City University of New York's (CUNY) Accelerated Study in Associate Programs (ASAP) that offer students comprehensive advising, tutoring, and financial support have been shown to have impacts on degree completion (Scrivener et al., 2015). Other institutions, such as Georgia

State University, have been reaching out to students during the summer, allowing students to choose "meta-majors" with common course requirements during their freshman year, redesigning their introductory math courses, and automating parts of their advising system.[16] Some of these approaches have been tested with comparison groups and have been shown to produce improvements in postsecondary access and persistence (Page & Gelbach, 2017), though others have not yet been evaluated (Kurzweil & Wu, 2015).

Our findings also suggest a more thorough rethinking of the content and delivery of remediation. The first task must be finding a model of remediation that actually improves students' understanding of math. It could be that senior year in high school is too late to start. In a study of the effectiveness of a double-period algebra course in the 9th grade in the Chicago Public Schools, researchers found positive impacts on students' achievement in algebra (Nomi & Allensworth, 2009). Later work found positive impacts on credits earned in high school, test scores, high school graduation, and college enrollment rates (Cortes, Goodman, & Nomi, 2014).

It is also possible that the self-paced, online course is not well-matched to the needs of low-achieving students. A growing body of work from college and high school settings has found that students with lower levels of academic preparation perform less well in online courses than with traditional instruction (Xu & Jaggars, 2013; Bettinger, Fox, Loeb, & Taylor, 2017; Heppen, et al., 2017). Although SAILS is not purely online (there is a teacher in the room), it is possible that the self-paced format is less effective for the students who have struggled with the material in the past. If states cannot find a model of remediation that actually increases students' success in math, the next step would be to evaluate the consequences of eliminating remediation requirements for more students.

Especially for those without the math and literacy skills traditionally expected for college-level work, we are unlikely to find a single policy to generate large increases in college completion. Rather, leaders in Tennessee and other states will need to continue to innovate—and test their ideas with data—in order to achieve substantial change.

16 Other states have begun using high school grades in combination with test scores for making remediation decisions. A recent study in New York found that some of the students exempted from remediation based on a combined measure were able to pass college math (Barnett et al., 2018). These findings support earlier research with similar conclusions (Belfield & Crosta, 2012; Scott-Clayton, 2012; Scott-Clayton, Crosta, & Belfield, 2014).

APPENDIX

I. TEACHER SURVEY RESULTS

We administered an online teacher survey in spring 2016 in the 119 schools that participated in the posttest. The survey was intended to collect information on teacher characteristics and teaching practices. We invited all the math teachers who taught any of the classes included in the posttest sample to participate in the survey, including those teaching Pre-Calculus, Statistics, SAILS, Bridge Math, and Advanced Algebra & Trigonometry. Of the 370 teachers who were invited, 317 responded yielding a response rate of 85%.

Table A1 presents results from the survey on teacher's characteristics, splitting teachers between those who taught at least one SAILS course and those who taught no SAILS courses. SAILS teachers were slightly less experienced than teachers who never taught SAILS by an average of 2.2 years. Most of the difference in teaching experience appears to come from the fact that SAILS teachers were more likely to have 6 to 15 years of experience, while non-SAILS teachers were more likely to have 16 or more years of experience, though these differences are not statistically significant. However, after 6 years of teaching, the difference in student achievement gains associated with teacher experience flattens out considerably (Kane, Rockoff, & Staiger, 2008). The distribution of teachers with only 1 to 5 years of experience was approximately 20% in both groups. A similar pattern is reported for years of experience teaching math.

Regarding teachers' preparation, SAILS teachers and non-SAILS teachers report taking similar numbers of college courses covering methods for teaching math (about 4), math content for teachers (about 4), and general math courses (about 6). They are also are equally likely to be National Board-certified teachers (about 15%). SAILS teachers appear slightly more likely to enter teaching through an alternative certification program (16% compared to 12% of non-SAILS teachers) rather than a traditional teacher education program, though these differences are not statistically significant. SAILS and non-SAILS teachers are also equally likely to have entered teaching without a formal training program. Taken together, the differences between SAILS and non-SAILS teachers are small.

Table A1. Teacher Background and Experience (SAILS vs non-SAILS)

Measure	SAILS Teachers	Non-SAILS Teachers	Difference
Years taught (all subjects)			
1 to 5 years	20.9%	20.0%	0.9 pp
6 to 15 years	40.3%	34.3%	6.0 pp
16 or more years	38.8%	45.7%	-6.9 pp
Years teaching full time (total)	13.8	16.0	-2.2**
Years taught math			
1 to 5 years	23.0%	22.9%	0.1 pp
6 to 15 years	41.7%	35.4%	6.3 pp
16 or more years	35.3%	41.7%	-6.4 pp
Years teaching HS math (total)	12.4	14.6	-2.2**
Path into teaching			
Teacher Ed Program	71.7%	76.0%	-4.3 pp
Alt Cert Program	15.9%	12.0%	3.9 pp
No Formal Training	12.3%	12.0%	0.3 pp
NBPTS Certified	14.7%	14.5%	0.2 pp
College Mathematics Courses			
Avg # methods for teaching math classes	4.01	3.85	0.17
Avg # math content classes for teachers	4.02	3.76	0.26
Avg # math courses	6.47	6.43	0.04
Observations	139	175	

Notes: The unit of observation in this table is the teacher. Any teacher who taught at least one SAILS course is included in the SAILS teacher column. Any teacher who exclusively taught non-SAILS math courses was included in the non-SAILS teacher column (*p<0.1; ** p<0.05; *** p<0.01).

While the teachers teaching SAILS are mostly similar to those who do no teach SAILS, teachers report significant differences in how they teach SAILS courses compared to other math courses. Almost all teachers (96%) reported that students were self-paced often or almost all the time in SAILS courses, compared to only 11% in other math courses. The vast majority of teachers (85%) also noted that students in SAILS courses use educational software every week or more, compared to only 24% in other math courses. Almost all teachers also noted that students in SAILS courses engaged in independent work often or almost all the time (96% vs 63% in other math courses) and that they tracked student progress every week or more (93% vs 86% in other math courses).

Interestingly, teachers were less likely to report that SAILS courses connect to college work often or almost all the time than they were for other math courses (61% vs 75% in other math courses). This could be due to the fact that SAILS is explicitly covering remedial college material, not college-level work, whereas other math courses like Pre-Calculus and Statistics may be viewed as closer to college-level work.

Overall, it is clear that teachers reported using distinctly different teaching practices in SAILS courses compared to other math courses, with a clear emphasis on self-paced, software-based, independent work in SAILS courses.

Table A2. Frequent Use of Teaching Practice, by Course

Measure	SAILS Course	Non-SAILS Course	Difference
Independent student work (often or almost all the time)	95.70%	62.90%	32.8***
Tracks student progress (every week or more)	92.80%	86.00%	6.8**
Use of educational software (every week or more)	84.90%	23.80%	61.1***
Connects to college work (often or almost all the time)	60.40%	74.60%	-14.2***
Self-paced (often or almost all the time)	96.40%	10.70%	85.7***
Share of 5 practices used frequently	86.00%	51.60%	34.4***
N	139	307	

Notes: The unit of observation in this table is the course level, as teachers were asked about the frequency of these teaching practices in each type of math course they taught (**p*<0.1; ** *p*<0.05; *** *p*<0.01).

II. SUBGROUP EFFECTS

In Table A3, we present estimates of the effect of SAILS implementation by students' junior year ACT score, showing results separately for three groups of students: those with ACT math scores of 16 and below, 17 and 18, or 19 and above. For both groups of remediation-recommended students (with ACT math scores below 19), participation in SAILS was similar—about 40%. Although students with ACT math scores of 19 and above were not recommended for remediation, our results suggest that a small share—5% —of those students ended up enrolling in SAILS anyway. As in Table A4, the impact of SAILS implementation on high school graduation and college-going were small or not statistically significant across all three groups.

SAILS implementation did seem to generate a bigger reduction in college math remediation for community college entrants with ACT math scores of 16 or below—roughly 30 percentage points versus 25 percentage points for those with ACT math scores closer to the remediation threshold of 19.

Table A3. Measuring the Impact of SAILS Implementation by ACT Math Subgroup

	(1) ACT MATH <= 16		(2) ACT MATH = 17 OR 18		(3) ACT MATH ≥ 19	
Dependent variable	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation
SAILS participant	0.406*** (0.025) [142176]	0.398*** (0.021) [142176]	0.413*** (0.026) [55915]	0.435*** (0.019) [55915]	0.052*** (0.007) [117628]	0.028*** (0.003) [117628]
High school (HS) graduate	0.002 (0.006) [142176]	-0.001 (0.005) [142176]	-0.005 (0.006) [55915]	0.001 (0.004) [55915]	0.004 (0.004) [117628]	0.003 (0.002) [117628]
Enrolled in any college by spring of 1st year after HS	0.003 (0.010) [142176]	0.020* (0.011) [142176]	-0.021 (0.014) [55915]	0.003 (0.010) [55915]	0.004 (0.007) [117628]	0.016*** (0.005) [117628]
Enrolled in two-year college by spring of 1st year after HS	0.006 (0.010) [142176]	0.013 (0.010) [142176]	-0.011 (0.012) [55915]	0.006 (0.011) [55915]	-0.011 (0.010) [117628]	-0.004 (0.009) [117628]
Enrolled in TBR CC by spring of 1st year after HS	0.007 (0.010) [142176]	0.008 (0.009) [142176]	-0.009 (0.012) [55915]	0.002 (0.010) [55915]	-0.005 (0.009) [117628]	0.002 (0.008) [117628]
Enrolled in TCAT by spring of 1st year after HS	-0.000 (0.005) [142176]	0.005 (0.005) [142176]	0.001 (0.005) [55915]	-0.002 (0.006) [55915]	-0.003 (0.004) [117628]	-0.006 (0.004) [117628]
Enrolled in four-year college by spring of 1st year after HS	-0.003 (0.007) [142176]	0.006 (0.007) [142176]	-0.012 (0.013) [55915]	-0.005 (0.010) [55915]	0.008 (0.010) [117628]	0.018** (0.009) [117628]

Table A3. Continued

	(1) ACT MATH <= 16		(2) ACT MATH = 17 OR 18		(3) ACT MATH ≥ 19	
Dependent variable	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation
Outcomes for TN CC Enrollees						
Took remedial math by spring of 1st year	-0.304***	-0.296***	-0.248***	-0.256***	-0.001	-0.002
	(0.027)	(0.021)	(0.031)	(0.021)	(0.004)	(0.003)
	[33824]	[33824]	[15678]	[15678]	[21568]	[21568]
Took remedial math by spring of 2nd year	-0.320***	-0.330***	-0.268***	-0.304***	-0.001	-0.004
	(0.027)	(0.025)	(0.032)	(0.027)	(0.004)	(0.004)
	[27509]	[27509]	[12476]	[12476]	[17485]	[17485]
Took math by spring of 1st year	0.163***	-0.039***	0.103***	-0.029	-0.012	-0.050***
	(0.020)	(0.014)	(0.030)	(0.019)	(0.018)	(0.017)
	[33824]	[33824]	[15678]	[15678]	[21568]	[21568]
Took math by spring of 2nd year	0.089***	-0.064***	0.032	-0.048**	-0.013	-0.038**
	(0.019)	(0.016)	(0.028)	(0.019)	(0.018)	(0.017)
	[27509]	[27509]	[12476]	[12476]	[17485]	[17485]
Pass math by spring of 1st year	0.072***	-0.063***	0.057**	-0.042**	-0.026	-0.069***
	(0.014)	(0.013)	(0.025)	(0.020)	(0.021)	(0.017)
	[33824]	[33824]	[15678]	[15678]	[21568]	[21568]
Pass math by spring of 2nd year	0.031*	-0.066***	0.017	-0.064***	-0.019	-0.071***
	(0.016)	(0.017)	(0.025)	(0.021)	(0.022)	(0.020)
	[27509]	[27509]	[12476]	[12476]	[17485]	[17485]
Pass math by spring of 1st year (if took college math)	-0.066**	-0.062***	-0.032	-0.026	-0.019	-0.038***
	(0.031)	(0.021)	(0.032)	(0.022)	(0.021)	(0.014)
	[13410]	[13410]	[9211]	[9211]	[15469]	[15469]
Pass math by spring of 2nd year (if took college math)	-0.048**	-0.048**	-0.006	-0.032	-0.009	-0.047***
	(0.022)	(0.021)	(0.029)	(0.022)	(0.019)	(0.017)
	[13293]	[13293]	[8572]	[8572]	[13970]	[13970]
Withdrew from math by spring of 1st year (if took college math)	0.024	0.042***	0.054***	0.043***	0.002	0.021**
	(0.018)	(0.012)	(0.019)	(0.012)	(0.014)	(0.010)
	[13410]	[13410]	[9211]	[9211]	[15469]	[15469]
Total credits earned by spring of 1st year	1.099***	0.000	0.926*	0.384	-0.574	0.101
	(0.302)	(0.332)	(0.546)	(0.448)	(0.533)	(0.453)
	[28292]	[28292]	[14106]	[14106]	[19858]	[19858]
Total credits earned by spring of 2nd year	2.609***	0.056	1.937*	1.600	-0.948	0.497
	(0.657)	(0.766)	(1.082)	(1.220)	(0.968)	(1.024)
	[23037]	[23037]	[11302]	[11302]	[16106]	[16106]

Table A3. Continued

Dependent variable	(1) ACT MATH <= 16		(2) ACT MATH = 17 OR 18		(3) ACT MATH ≥ 19	
	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation
Outcomes for TN CC Enrollees:						
Retook ACT (if enrolled in TN CC)	-0.015	-0.026*	-0.040	-0.003	0.017	0.020
	(0.021)	(0.015)	(0.029)	(0.019)	(0.028)	(0.016)
	[33824]	[33824]	[15678]	[15678]	[21568]	[21568]
Return for 2nd year of college	0.001	-0.026	-0.029	0.005	-0.006	0.018
	(0.017)	(0.016)	(0.023)	(0.020)	(0.018)	(0.017)
	[27509]	[27509]	[12476]	[12476]	[17485]	[17485]
Earned associate degree by 2nd year	0.001	-0.006	0.004	-0.002	-0.006	-0.003
	(0.007)	(0.005)	(0.013)	(0.011)	(0.014)	(0.012)
	[27509]	[27509]	[12476]	[12476]	[17485]	[17485]
Earned certificate within 2 years	0.006	-0.003	0.012	-0.003	0.008	-0.021
	(0.008)	(0.007)	(0.013)	(0.012)	(0.011)	(0.014)
	[27509]	[27509]	[12476]	[12476]	[17485]	[17485]

Notes: We estimate equation (1) separately for the three subgroups of students by ACT math score (as reflected in the column headings.) For the outcome in each row, we report coefficients on two indicators: one for attending a SAILS high school during a period of pre-requisite remediation (i.e. those graduating in 2013-14) and another for attending a SAILS high school during co-requisite remediation (those graduating after 2013-14). The excluded category represents students attending non-SAILS schools in that year. The sample consists of students with junior year ACT math scores below 19, who were high school seniors in a public high school between 2010-11 and 2015-16. All specifications include high school fixed effects and demographic controls for race, ethnicity, and sex. In the second panel, the sample is limited to students enrolling in a Tennessee community college by the spring of 1 year after high school, regardless of degree intention. The sample size for outcomes 2 years after graduation are smaller because they are not available for the 2015-16 cohort of seniors. The count of total credits is missing for approximately 13% of the sample. Heteroskedastic-robust standard errors are clustered by high school and are reported in parentheses (**p*<.10, ***p*<.05, ****p*<.01).

Similar to the full sample, the impact of SAILS implementation on college math enrollments was positive for remediation-recommended students in 2013-14, when the pre-requisite policy was in place, or zero or negative once the co-requisite policy was in place. However, the impacts were larger for those with ACT math scores further below the cut-off: 16.3 percentage points for those with ACT scores of 16 and below vs. 10.3 percentage points for those with ACT math scores of 17 or 18. Although the impact of SAILS on college math course-taking faded by the second year, as the students from non-participating schools caught up, the impact on the proportion of students passing college math by the spring of their second year was still significant (albeit small at 3 percentage points) for those ACT math scores of 16 and below (and not significant for those with ACT math scores of 17 or 18). The impacts on total credits completed by the end of the first and second year of college—1.1 points and 2.6 points—were statistically significant for those with ACT math scores of 16 and below and marginally significant–0.9 and 1.9—for those with scores of 17 or 18.

The last two columns report impacts for those with ACT math scores of 19 and above. Although these students were not remediation-recommended, they could still have been impacted indirectly, as lower-scoring students enrolled in their college-level math courses. Our estimates do suggest that the proportion of those with ACT scores 19 and above taking and passing college-level math fell under the co-requisite policy. Unlike above, the negative impact remains even after we exclude the anomalous data from the set of schools implementing in 2014-15.

Table A4 reports impacts by gender and race. As with report Table 3, the sample includes all high school seniors with ACT math scores below 19. In the context of pre-requisite remediation, the implementation of SAILS seems to have had a larger impact on female high school graduates: reducing first-year remediation by 32.1 points vs. 23.1 points for males; increasing first-year college math enrollment by 16.6 points vs. 10.1 points for males; increasing the share of college entrants passing college math in their first year by 10.4 points vs. 0 points for males. The impacts on total college credits completed by the end of the first and second year were also positive and statistically significant for female high school seniors (1.5 and 3.5 credits), and small and statistically insignificant for males. Under the co-requisite policy, the SAILS program had similar effects on the proportion of students doing their remediation in high school. However, even for female graduates, the effect of SAILS implementation on the proportion of students taking or passing college-level math or on credit completion were statistically insignificant or negative (we would argue, due to the anomalous implementation of co-requisite remediation in 2014-15) once the co-requisite policy was in place.

The last four columns in Table A4 report results for White and Black students separately.[17] Under the pre-requisite remediation policy, the impacts of SAILS implementation on the share of students taking and passing college-level math were similar by race. The estimated impact on credit completion was not statistically significant for Black students, but, due to limited sample sizes, that estimate was not statistically different from the estimated impacts for White students. Especially because the impacts on college math enrollment and passage were similar, we see no reason to believe that the credit accumulation impacts were actually different by race.

17 There were too few students of other racial/ethnic categories to compare impacts with desired precision.

Table A4. Measuring the Impact of SAILS Implementation by Gender and Race

	(1) FEMALE		(2) MALE		(3) BLACK		(4) WHITE	
Dependent variable	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation
SAILS participant	0.422***	0.420***	0.391***	0.395***	0.396***	0.325***	0.430***	0.458***
	(0.025)	(0.020)	(0.024)	(0.020)	(0.041)	(0.031)	(0.025)	(0.018)
	[102821]	[102821]	[95270]	[95270]	[61919]	[61919]	[121651]	[121651]
High school (HS) graduate	-0.001	-0.002	0.002	0.001	0.002	-0.004	-0.002	-0.002
	(0.006)	(0.004)	(0.006)	(0.005)	(0.008)	(0.007)	(0.006)	(0.005)
	[102821]	[102821]	[95270]	[95270]	[61919]	[61919]	[121651]	[121651]
Enrolled in any college by spring of 1st year after HS	-0.006	0.019*	-0.003	0.014	-0.016	0.010	0.002	0.003
	(0.011)	(0.011)	(0.012)	(0.011)	(0.019)	(0.013)	(0.011)	(0.009)
	[102821]	[102821]	[95270]	[95270]	[61919]	[61919]	[121651]	[121651]
Enrolled in two-year college by spring of 1st year after HS	0.003	0.011	-0.002	0.011	-0.007	0.013	0.004	-0.003
	(0.011)	(0.010)	(0.012)	(0.010)	(0.016)	(0.011)	(0.010)	(0.009)
	[102821]	[102821]	[95270]	[95270]	[61919]	[61919]	[121651]	[121651]
Enrolled in TBR CC by spring of 1st year after HS	0.002	0.012	0.002	0.003	-0.005	0.011	0.003	-0.003
	(0.011)	(0.010)	(0.011)	(0.009)	(0.016)	(0.011)	(0.010)	(0.008)
	[102821]	[102821]	[95270]	[95270]	[61919]	[61919]	[121651]	[121651]
Enrolled in TCAT by spring of 1st year after HS	0.001	-0.001	-0.001	0.006	0.001	0.001	0.001	-0.003
	(0.004)	(0.004)	(0.006)	(0.007)	(0.004)	(0.004)	(0.005)	(0.007)
	[102821]	[102821]	[95270]	[95270]	[61919]	[61919]	[121651]	[121651]
Enrolled in four-year college by spring of 1st year after HS	-0.009	0.006	-0.004	0.002	-0.015	-0.003	-0.002	0.004
	(0.010)	(0.008)	(0.009)	(0.007)	(0.017)	(0.012)	(0.007)	(0.007)
	[102821]	[102821]	[95270]	[95270]	[61919]	[61919]	[121651]	[121651]

Table A4. Continued

Dependent variable	(1) FEMALE Under pre-requisite remediation	(1) FEMALE Under co-requisite remediation	(2) MALE Under pre-requisite remediation	(2) MALE Under co-requisite remediation	(3) BLACK Under pre-requisite remediation	(3) BLACK Under co-requisite remediation	(4) WHITE Under pre-requisite remediation	(4) WHITE Under co-requisite remediation
Outcomes for TN CC Enrollees								
Took remedial math by spring of 1st year	-0.321***	-0.302***	-0.231***	-0.260***	-0.258***	-0.190***	-0.297***	-0.326***
	(0.025)	(0.020)	(0.031)	(0.022)	(0.038)	(0.031)	(0.027)	(0.018)
	[29842]	[29842]	[19660]	[19660]	[12993]	[12993]	[33521]	[33521]
Took remedial math by spring of 2nd year	-0.337***	-0.340***	-0.249***	-0.300***	-0.276***	-0.213***	-0.314***	-0.372***
	(0.026)	(0.024)	(0.032)	(0.027)	(0.041)	(0.040)	(0.027)	(0.022)
	[24118]	[24118]	[15867]	[15867]	[10755]	[10755]	[26974]	[26974]
Took math by spring of 1st year	0.166***	-0.045***	0.101***	-0.022	0.163***	-0.031	0.139***	-0.034**
	(0.021)	(0.015)	(0.024)	(0.017)	(0.034)	(0.021)	(0.021)	(0.014)
	[29842]	[29842]	[19660]	[19660]	[12993]	[12993]	[33521]	[33521]
Took math by spring of 2nd year	0.077***	-0.054***	0.047*	-0.057***	0.064*	-0.038	0.087***	-0.054***
	(0.019)	(0.017)	(0.026)	(0.022)	(0.033)	(0.026)	(0.019)	(0.015)
	[24118]	[24118]	[15867]	[15867]	[10755]	[10755]	[26974]	[26974]
Pass math by spring of 1st year	0.104***	-0.069***	0.000	-0.033**	0.055**	-0.053***	0.071***	-0.061***
	(0.017)	(0.015)	(0.021)	(0.016)	(0.023)	(0.017)	(0.017)	(0.015)
	[29842]	[29842]	[19660]	[19660]	[12993]	[12993]	[33521]	[33521]
Pass math by spring of 2nd year	0.060***	-0.070***	-0.040*	-0.048**	-0.005	-0.026	0.045***	-0.069***
	(0.018)	(0.017)	(0.022)	(0.024)	(0.026)	(0.025)	(0.017)	(0.017)
	[24118]	[24118]	[15867]	[15867]	[10755]	[10755]	[26974]	[26974]
Pass math by spring of 1st year (if took college math)	-0.006	-0.051**	-0.162***	-0.043	-0.128**	-0.052	-0.053**	-0.056***
	(0.027)	(0.020)	(0.039)	(0.028)	(0.056)	(0.037)	(0.026)	(0.020)
	[14075]	[14075]	[8546]	[8546]	[4378]	[4378]	[16664]	[16664]

Table A4. Continued

Dependent variable	(1) FEMALE Under pre-requisite remediation	Under co-requisite remediation	(2) MALE Under pre-requisite remediation	Under co-requisite remediation	(3) BLACK Under pre-requisite remediation	Under co-requisite remediation	(4) WHITE Under pre-requisite remediation	Under co-requisite remediation
Outcomes for TN CC Enrollees								
Pass math by spring of 2nd year (if took college math)	0.012	-0.046**	-0.120***	-0.028	-0.074	-0.018	-0.025	-0.045**
	(0.022)	(0.019)	(0.031)	(0.030)	(0.048)	(0.036)	(0.021)	(0.019)
	[13703]	[13703]	[8162]	[8162]	[4363]	[4363]	[16108]	[16108]
Withdrew from math by spring of 1st year (if took college math)	0.034**	0.038***	0.047**	0.048***	0.023	0.041*	0.041***	0.043***
	(0.016)	(0.012)	(0.020)	(0.015)	(0.040)	(0.021)	(0.015)	(0.011)
	[14075]	[14075]	[8546]	[8546]	[4378]	[4378]	[16664]	[16664]
Total credits earned by spring of 1st year	1.485***	-0.027	0.173	0.623	0.748	-0.089	1.186***	0.442
	(0.381)	(0.355)	(0.431)	(0.402)	(0.517)	(0.447)	(0.327)	(0.347)
	[26038]	[26038]	[16360]	[16360]	[10329]	[10329]	[29419]	[29419]
Total credits earned by spring of 2nd year	3.466***	0.674	0.281	1.025	1.282	0.036	2.953***	1.385*
	(0.763)	(0.842)	(0.883)	(1.057)	(1.087)	(1.055)	(0.646)	(0.835)
	[21069]	[21069]	[13270]	[13270]	[8506]	[8506]	[23831]	[23831]
Return for 2nd year of college	-0.011	-0.009	-0.012	-0.020	-0.014	-0.015	-0.004	-0.016
	(0.016)	(0.015)	(0.022)	(0.022)	(0.031)	(0.027)	(0.016)	(0.015)
	[24118]	[24118]	[15867]	[15867]	[10755]	[10755]	[26974]	[26974]
Retook ACT (if enrolled in TN CC)	-0.017	-0.010	-0.041*	-0.032*	-0.029	-0.035*	-0.010	-0.007
	(0.023)	(0.015)	(0.024)	(0.018)	(0.031)	(0.020)	(0.024)	(0.016)
	[29842]	[29842]	[19660]	[19660]	[12993]	[12993]	[33521]	[33521]
Earned associate degree by 2nd year	0.008	-0.003	-0.009	-0.005	0.002	-0.007	0.003	0.002
	(0.008)	(0.008)	(0.010)	(0.007)	(0.008)	(0.004)	(0.009)	(0.007)
	[24118]	[24118]	[15867]	[15867]	[10755]	[10755]	[26974]	[26974]

Table A4. Continued

	(1) FEMALE		(2) MALE		(3) BLACK		(4) WHITE	
Dependent variable	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation	Under pre-requisite remediation	Under co-requisite remediation
Outcomes for TN CC Enrollees								
Earned certificate within 2 years	0.006	-0.007	0.009	0.002	0.012	-0.005	0.005	0.000
	(0.009)	(0.009)	(0.012)	(0.008)	(0.011)	(0.007)	(0.010)	(0.009)
	[24118]	[24118]	[15867]	[15867]	[10755]	[10755]	[26974]	[26974]

Notes: We estimated the coefficients separately for each subgroup, by gender and by race. For the outcome in each row, we report coefficients on two indicators: one for attending a SAILS high school during a period of pre-requisite remediation (i.e. those graduating in 2013-14) and another for attending a SAILS high school during co-requisite remediation (those graduating after 2013-14). The excluded category represents students attending non-SAILS schools in that year. The sample consists of students with junior year ACT math scores below 19, who were high school seniors in a public high school between 2010-11 and 2015-16. All specifications include high school fixed effects and demographic controls for race, ethnicity, and sex. In the second panel, the sample is limited to students enrolling in a Tennessee community college by the spring of 1 year after high school, regardless of degree intention. The sample size for outcomes 2 years after graduation are smaller because they are not available for the 2015-16 cohort of seniors. The count of total credits is missing for approximately 13% of the sample. Heteroskedastic-robust standard errors are clustered by high school and are reported in parentheses (**p*<.10, ***p*<.05, ****p*<.01).

III. ADDITIONAL APPENDIX TABLES AND FIGURES

In Table A5, we report any significant changes in the characteristics of students in school corresponding with the implementation of SAILS. Even though we control for student demographics, any substantial difference would have been cause for concern, since it might have signaled other unobserved changes in the characteristics of students in schools implementing SAILS. The only statistically significant difference—1 percentage point in gender composition—was sufficiently small that it did not represent cause for concern.

Table A5. Difference-in-Differences Covariate Balance Check (ACT Math < 19)

Dependent variable	(1) Under traditional remediation	(2) Under co-requisite remediation	Mean values for seniors from non-SAILS schools (ACT < 19)
Female	0.000 (0.008) [198091]	0.010* (0.006) [198091]	0.52
Black	0.000 (0.005) [198091]	0.005 (0.005) [198091]	0.41
White	-0.002 (0.005) [198091]	-0.000 (0.006) [198091]	0.51
Hispanic	-0.002 (0.004) [198091]	-0.001 (0.003) [198091]	0.05

Note: For the outcome in each row, we report coefficients on two indicators: one for attending a SAILS high school during a period of pre-requisite remediation (i.e. those graduating in 2013-14) and another for attending a SAILS high school during co-requisite remediation (those graduating after 2013-14). The excluded category represents students attending non-SAILS schools in that year. The sample consists of students with junior year ACT math scores below 19, who were high school seniors in a public high school between 2010-11 and 2015-16. Sample sizes are included in brackets. Control means are the means for students with ACT math scores below 19 in non-SAILS high schools. Heteroskedastic-robust standard errors are clustered by high school and are reported in parentheses (**p*<.10, ***p*<.05, ****p*<.01).

As noted in the text, the key assumption in a difference-in-difference analysis is that the change in the year of implementation would have been similar in SAILS-participating and non-participating schools if not for the SAILS program. One way to test that assumption is to investigate whether there were different trends in the years leading up to implementation. As reported in Table A6, we saw few differences in the key outcomes (e.g., taking and passing college math) for the SAILS high schools in the years leading up to implementation.

Table A6. Event Study

Dependent variable	5 Yrs Pre-SAILS	4 Yrs Pre-SAILS	3 Yrs Pre-SAILS	2 Yrs Pre-SAILS	1st SAILS Yr	2nd SAILS Yr	3rd SAILS Yr	*N*
SAILS participant	0.040***	0.026**	0.013**	0.004	0.386***	0.450***	0.493***	198,091
	(0.012)	(0.012)	(0.005)	(0.003)	(0.019)	(0.023)	(0.032)	
High School (HS) graduate	-0.013	0.009	0.004	0.007	0.002	0.005	0.002	198,091
	(0.016)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.008)	
Enrolled in any college by spring of 1st year after HS	-0.029	0.005	-0.006	-0.001	0.003	0.022**	0.026*	198,091
	(0.019)	(0.012)	(0.009)	(0.007)	(0.007)	(0.011)	(0.014)	
Enrolled in two-year college by spring of 1st year after HS	-0.003	0.016	0.009	0.005	0.008	0.015	0.034**	198,091
	(0.017)	(0.011)	(0.008)	(0.007)	(0.007)	(0.010)	(0.013)	
Enrolled in TBR CC by spring of 1st year after HS	-0.006	0.013	0.012*	0.006	0.007	0.013	0.035***	198,091
	(0.013)	(0.009)	(0.007)	(0.006)	(0.006)	(0.009)	(0.012)	
Enrolled in TCAT by spring of 1st year after HS	-0.009	-0.002	-0.005	-0.004	0.001	-0.002	-0.006	198,091
	(0.011)	(0.007)	(0.005)	(0.003)	(0.004)	(0.006)	(0.007)	
Enrolled in four-year college by spring of 1st year after HS	-0.027**	-0.010	-0.015**	-0.005	-0.005	0.005	-0.010	198,091
	(0.013)	(0.009)	(0.006)	(0.005)	(0.005)	(0.007)	(0.010)	
Outcomes for TN CC Enrollees								
Took remedial math by spring of 1st year	-0.043	-0.023	-0.013	-0.002	-0.272***	-0.305***	-0.322***	49,502
	(0.028)	(0.021)	(0.015)	(0.011)	(0.016)	(0.022)	(0.027)	
Took remedial math by spring of 2nd year	-0.013	-0.007	-0.002	0.001	-0.301***	-0.333***		39,985
	(0.025)	(0.019)	(0.014)	(0.011)	(0.020)	(0.027)		
Took math by spring of 1st year	0.037	0.044**	0.019	-0.007	0.034***	-0.011	-0.036*	49,502
	(0.028)	(0.018)	(0.014)	(0.013)	(0.013)	(0.015)	(0.019)	
Took math by spring of 2nd year	0.018	0.037**	0.024*	0.017	0.019	-0.042**		39,985
	(0.028)	(0.019)	(0.014)	(0.012)	(0.013)	(0.017)		

Table A6. Continued

Dependent variable	5 Yrs Pre-SAILS	4 Yrs Pre-SAILS	3 Yrs Pre-SAILS	2 Yrs Pre-SAILS	1st SAILS Yr	2nd SAILS Yr	3rd SAILS Yr	*N*
Outcomes for TN CC Enrollees								
Pass math by spring of 1st year (if took college math)	0.009	0.010	0.021	0.009	-0.051***	-0.041*	-0.074***	22,621
	(0.043)	(0.029)	(0.019)	(0.017)	(0.017)	(0.021)	(0.026)	
Pass math by spring of 2nd year (if took college math)	0.044	0.026	0.024	0.007	-0.039***	-0.037**		21,865
	(0.034)	(0.024)	(0.016)	(0.013)	(0.015)	(0.019)		
Pass math by spring of 1st year	0.038*	0.037**	0.021*	0.000	-0.005	-0.033**	-0.075***	49,502
	(0.022)	(0.015)	(0.011)	(0.009)	(0.011)	(0.013)	(0.018)	
Pass math by spring of 2nd year	0.039	0.039**	0.028**	0.018	-0.009	-0.055***		39,985
	(0.027)	(0.018)	(0.013)	(0.011)	(0.012)	(0.017)		
Withdrew from first college-level math (if took college math)	-0.037	-0.020	-0.010	-0.012	0.016**	0.029***	0.059***	30,770
	(0.022)	(0.013)	(0.010)	(0.008)	(0.008)	(0.009)	(0.012)	
Number of withdrawals from college-level math in 1st year (if took college math)	-0.030	-0.034*	-0.030**	-0.021*	0.027***	0.044***	0.067***	22,621
	(0.032)	(0.018)	(0.013)	(0.012)	(0.009)	(0.012)	(0.016)	
Withdrew from math by spring of 1st year (if took college math)	-0.033	-0.035*	-0.029**	-0.024**	0.028***	0.045***	0.065***	22,621
	(0.031)	(0.019)	(0.013)	(0.011)	(0.009)	(0.012)	(0.015)	
Total credits earned by spring of 1st year	1.096**	1.003**	0.329	0.176	0.514**	0.442	0.545	42,398
	(0.537)	(0.399)	(0.274)	(0.231)	(0.235)	(0.353)	(0.420)	
Total credits earned by spring of 2nd year	1.764	1.693**	0.844	0.371	1.440***	1.020		34,339
	(1.183)	(0.747)	(0.545)	(0.459)	(0.509)	(0.833)		
Retook ACT (if enrolled in TN CC)	0.043	0.037**	0.011	0.019	-0.016	-0.009	-0.016	49,502
	(0.027)	(0.018)	(0.014)	(0.012)	(0.012)	(0.016)	(0.020)	
Return for 2nd year of college	0.061***	0.028*	0.017	0.025**	-0.008	-0.012		39,985
	(0.020)	(0.015)	(0.011)	(0.010)	(0.012)	(0.015)		

Table A6. Continued

Dependent variable	5 Yrs Pre-SAILS	4 Yrs Pre-SAILS	3 Yrs Pre-SAILS	2 Yrs Pre-SAILS	1st SAILS Yr	2nd SAILS Yr	3rd SAILS Yr	*N*
Outcomes for TN CC Enrollees								
Earned associate degree by 2nd year	0.016	-0.000	0.006	0.001	0.000	-0.005		39,985
	(0.013)	(0.008)	(0.006)	(0.006)	(0.005)	(0.007)		
Earned certificate within 2 years	-0.001	-0.008	-0.005	-0.004	0.004	-0.008		39,985
	(0.014)	(0.011)	(0.007)	(0.005)	(0.006)	(0.007)		

Notes: Estimates in each row come from estimating equation 1, but exchanging the two main indicators of SAILS under traditional remediation and SAILS under co-requisite remediation for a set of binary variables indicating the years relative to when a student's school adopted SAILS (e.g. an indicator for one year before SAILS was adopted, two years before SAILS was adopted, etc. up to five years before SAILS was adopted and an indicator for whether it was the first year of using SAILS, second year of using SAILS or third year of using SAILS). The excluded category represents students attending non-SAILS schools in that year. The sample consists of students with junior year ACT math scores below 19, who were high school seniors in a public high school between 2010-11 and 2015-16. Heteroskedastic-robust standard errors are clustered by high school and are reported in parentheses (**p*<.10, ***p*<.05, ****p*<.01).

In Table A7, we present differences in student covariates at various bandwidths on either side of the cut-score (In the main report, we used a bandwidth of 25 points on the rescaled ACT, but we also show results using a bandwidth of 15 and 35 points). In Table A8, we report similar evidence for the 2013-14 sample. Table A9 reports the differences for the subset of the 2015-16 sample in the 119 schools for whom we had student test scores.

Table A7. RD Covariate Balance Tests, Full Sample 2015-16

	SAILS SCHOOLS				NON-SAILS SCHOOLS			
	BW25, controls	BW15, controls	BW35, controls	Control mean	BW25, controls	BW15, controls	BW35, controls	Control mean
PLAN math score	0.197	0.403**	0.341***	17.50	0.086	0.215	0.290	17.43
	(0.154)	(0.200)	(0.129)		(0.225)	(0.339)	(0.218)	
	[11363]	[5337]	[18306]		[5306]	[2597]	[8455]	
PLAN ELA score	0.158	0.137	0.238**	17.50	0.173	-0.185	0.411**	17.46
	(0.137)	(0.188)	(0.116)		(0.220)	(0.308)	(0.200)	
	[11365]	[5337]	[18307]		[5313]	[2600]	[8464]	

Table A7. Continued

	SAILS SCHOOLS				NON-SAILS SCHOOLS			
	BW25, controls	BW15, controls	BW35, controls	Control mean	BW25, controls	BW15, controls	BW35, controls	Control mean
PLAN composite score	0.143	0.221*	0.249***	17.87	0.085	-0.103	0.285**	17.83
	(0.091)	(0.126)	(0.077)		(0.138)	(0.227)	(0.125)	
	[11352]	[5332]	[18283]		[5304]	[2596]	[8449]	
English language learner	-0.001	0.001	0.001	0.05	0.001	0.010	0.002	0.05
	(0.009)	(0.013)	(0.008)		(0.015)	(0.018)	(0.014)	
	[12438]	[5841]	[20306]		[5972]	[2934]	[9572]	
IEP	-0.006	0.004	-0.022***	0.02	-0.013	-0.002	-0.018*	0.03
	(0.006)	(0.008)	(0.006)		(0.010)	(0.015)	(0.009)	
	[12438]	[5841]	[20306]		[5972]	[2934]	[9572]	
Male	-0.006	-0.015	-0.011	0.49	-0.067**	-0.051	-0.057**	0.50
	(0.022)	(0.034)	(0.018)		(0.034)	(0.047)	(0.028)	
	[12438]	[5841]	[20306]		[5972]	[2934]	[9572]	
Black	-0.014	-0.006	-0.015	0.13	0.001	-0.011	0.004	0.19
	(0.013)	(0.017)	(0.012)		(0.015)	(0.027)	(0.015)	
	[12438]	[5841]	[20306]		[5972]	[2934]	[9572]	
White	0.005	0.001	0.008	0.84	-0.025	-0.019	-0.024	0.78
	(0.013)	(0.018)	(0.012)		(0.019)	(0.031)	(0.017)	
	[12438]	[5841]	[20306]		[5972]	[2934]	[9572]	
Hispanic	-0.002	0.000	0.004	0.06	-0.007	0.012	-0.016	0.05
	(0.008)	(0.012)	(0.008)		(0.013)	(0.021)	(0.011)	
	[12438]	[5841]	[20306]		[5972]	[2934]	[9572]	

Notes: For the outcome in each row, we report the discontinuity at the remediation cut-off of 19. We assume a linear relationship between the outcome and ACT score (the running variable), although we allow for different slopes above and below the cut-off (We used a rescaled version of the ACT with finer-grained scale.) All regressions also include high school fixed effects and demographic controls for race, ethnicity, and sex. The sample includes all high school seniors in 2015-16 who have a math score in the specified bandwidth (BW) noted in the column heads (e.g., +/- 4 points, +/- 3 points, +/- 5 points). Control means are the mean outcomes for students within 10 points above the cut-off on the rescaled measure. Heteroskedastic robust standard errors are clustered by high school and included in parentheses (*p<0.10, **p<0.05, ***p<0.01).

Table A8. RD Covariate Balance Test, Full Sample 2013-14 (Coarsened ACT)

	SAILS SCHOOLS				NON-SAILS SCHOOLS			
Dependent variable	BW4, controls	BW3, controls	BW5, controls	Control mean	BW4, controls	BW3, controls	BW5, controls	Control mean
Female	0.040*	0.055*	0.037*	0.52	0.018	-0.005	0.015	0.53
	(0.023)	(0.032)	(0.022)		(0.016)	(0.020)	(0.015)	
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]	
Black	-0.000	-0.000	0.005	0.12	0.011	0.003	0.016*	0.18
	(0.013)	(0.017)	(0.012)		(0.009)	(0.013)	(0.009)	
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]	
White	-0.013	-0.005	-0.012	0.82	-0.021*	-0.004	-0.015	0.73
	(0.017)	(0.018)	(0.015)		(0.011)	(0.015)	(0.010)	
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]	
Hispanic	0.015*	0.012	0.010	0.04	0.007	0.004	0.002	0.04
	(0.008)	(0.012)	(0.006)		(0.007)	(0.009)	(0.006)	
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]	

Notes: For the outcome in each row, we report the discontinuity at the remediation cut-off of 19. We assume a linear relationship between the outcome and ACT score (the running variable), although we allow for different slopes above and below the cut-off. (We used the integer value ACT math score.) All regressions also include high school fixed effects and demographic controls for race, ethnicity, and sex. The sample includes all high school seniors in 2013-14 who have a math score in the specified bandwidth (BW) noted in the column heads (e.g., +/- 4 points, +/- 3 points, +/- 5 points). Control means are the mean outcomes for students who are a single point above the cut-off using the integer ACT measure. Heteroskedastic robust standard errors are clustered by high school and included in parentheses (*p<0.10, **p<0.05, ***p<0.01).

Table A9. RD Covariate Balance Tests (Posttest Sample, 2015-16)

Dependent variable	BW25, controls	BW15, controls	BW35, controls	Control mean
PLAN math score	0.130	0.437	0.151	17.59
	(0.259)	(0.389)	(0.236)	
	[3598]	[1709]	[5683]	
PLAN ELA score	-0.166	-0.332	-0.059	17.78
	(0.247)	(0.367)	(0.223)	
	[3598]	[1709]	[5681]	
PLAN composite score	-0.032	0.056	0.019	17.97
	(0.155)	(0.251)	(0.137)	
	[3597]	[1708]	[5679]	
English language learner	-0.007	-0.005	0.005	0.04
	(0.012)	(0.018)	(0.011)	
	[3851]	[1833]	[6131]	
IEP	0.014	0.014	-0.000	0.01
	(0.011)	(0.017)	(0.009)	
	[3851]	[1833]	[6131]	
Male	-0.043	-0.075	-0.045	0.53
	(0.039)	(0.058)	(0.034)	
	[3851]	[1833]	[6131]	
Black	0.037*	0.063**	0.026	0.11
	(0.021)	(0.028)	(0.018)	
	[3851]	[1833]	[6131]	
White	-0.039	-0.040	-0.031	0.87
	(0.024)	(0.032)	(0.021)	
	[3851]	[1833]	[6131]	
Hispanic	-0.013	-0.006	-0.000	0.04
	(0.015)	(0.022)	(0.015)	
	[3851]	[1833]	[6131]	

Notes: For the outcome in each row, we report the discontinuity at the remediation cut-off of 19. We assume a linear relationship between the outcome and ACT score (the running variable), although we allow for different slopes above and below the cut-off. (We used a rescaled version of the ACT with finer-grained scale.) All regressions also include high school fixed effects and demographic controls for race, ethnicity, and sex. The sample includes all high school seniors in 2015-16 who took the post-test and have a math score in the specified bandwidth (BW) noted in the column heads (e.g., +/- 4 points, +/- 3 points, +/- 5 points). Control means are the mean outcomes for students within 10 points above the cut-off on the rescaled measure. Heteroskedastic robust standard errors are clustered by high school and included in parentheses (**p*<0.10, ***p*<0.05, ****p*<0.01).

In Tables A10 and A11, we report two different types of robustness checks. First, it includes the results for different bandwidths (3, 4 and 5 integer points on the ACT in 2013-14 and 15, 25 and 35 on the rescaled ACT for 2015-16). Second, it includes the results for the full set of postsecondary outcomes we were able to measure.

Table A10. SAILS and Non-SAILS School RD Estimates, 2013-14, All High School & Post-Secondary Outcomes

	SAILS SCHOOLS, 2013-14				NON-SAILS SCHOOLS, 2013-14				
Dependent variable	BW4, controls	BW3, controls	BW5, controls	Control mean	BW4, controls	BW3, controls	BW5, controls	Control mean	Difference (BW4)
SAILS participant	0.271***	0.221***	0.309***	0.14	0.000	0.000	0.000	0.00	0.271
	(0.035)	(0.035)	(0.033)		(0.000)	(0.000)	(0.000)		
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]		
Took Bridge Math	-0.030	-0.017	-0.019	0.20	0.162***	0.145***	0.191***	0.30	-0.192
	(0.027)	(0.031)	(0.023)		(0.020)	(0.022)	(0.021)		
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]		
High School (HS) graduate	-0.012	-0.007	-0.003	0.98	0.016**	0.014	0.019***	0.95	-0.028
	(0.008)	(0.009)	(0.006)		(0.007)	(0.009)	(0.006)		
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]		
Enroll in any college by spring of 1st year after HS	0.002	0.003	0.014	0.71	0.015	0.004	0.026*	0.70	-0.013
	(0.020)	(0.027)	(0.017)		(0.015)	(0.018)	(0.014)		
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]		
Enroll in two-year college by spring of 1st year after HS	0.009	0.028	0.003	0.40	-0.010	-0.007	0.003	0.46	0.019
	(0.024)	(0.029)	(0.021)		(0.016)	(0.021)	(0.014)		
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]		
Enroll in TN CC by spring of 1st year after HS	0.006	-0.026	0.015	0.30	0.019	0.010	0.016	0.22	-0.013
	(0.021)	(0.026)	(0.018)		(0.014)	(0.017)	(0.012)		
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]		
Enroll in TCAT by spring of 1st year after HS	-0.012	-0.012	-0.003	0.04	0.007	0.005	0.006	0.03	-0.019
	(0.009)	(0.011)	(0.008)		(0.007)	(0.008)	(0.006)		
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]		

Table A10. Continued

Dependent variable	SAILS SCHOOLS, 2013-14: BW4, controls	SAILS SCHOOLS, 2013-14: BW3, controls	SAILS SCHOOLS, 2013-14: BW5, controls	SAILS SCHOOLS, 2013-14: Control mean	NON-SAILS SCHOOLS, 2013-14: BW4, controls	NON-SAILS SCHOOLS, 2013-14: BW3, controls	NON-SAILS SCHOOLS, 2013-14: BW5, controls	NON-SAILS SCHOOLS, 2013-14: Control mean	Difference (BW4)
Enroll in four-year college by spring of 1st year after HS	-0.012	-0.046*	0.008	0.35	0.023	0.011	0.021*	0.27	-0.035
	(0.020)	(0.026)	(0.018)		(0.015)	(0.018)	(0.013)		
	[11259]	[8246]	[13077]		[23287]	[16786]	[27112]		
Outcomes for TN CC Enrollees									
Took remedial math by spring of 1st year	0.269***	0.256***	0.273***	0.03	0.485***	0.449***	0.517***	0.04	-0.216
	(0.035)	(0.037)	(0.035)		(0.025)	(0.030)	(0.022)		
	[3206]	[2434]	[3612]		[5232]	[3896]	[5907]		
Took remedial math by spring of 2nd year	0.278***	0.265***	0.282***	0.03	0.498***	0.465***	0.531***	0.04	-0.220
	(0.034)	(0.037)	(0.035)		(0.027)	(0.033)	(0.024)		
	[3206]	[2434]	[3612]		[5232]	[3896]	[5907]		
Took math by spring of 1st year (if enrolled in TN CC)	-0.062	-0.067	-0.072	0.69	-0.138***	-0.134***	-0.141***	0.67	0.076
	(0.049)	(0.056)	(0.049)		(0.033)	(0.045)	(0.030)		
	[3206]	[2434]	[3612]		[5232]	[3896]	[5907]		
Took math by spring of 2nd year (if enrolled in TN CC)	0.014	0.018	0.001	0.78	-0.066**	-0.038	-0.077***	0.77	0.080
	(0.044)	(0.053)	(0.043)		(0.031)	(0.040)	(0.027)		
	[3206]	[2434]	[3612]		[5232]	[3896]	[5907]		
Pass math by spring of 1st year (if took college math)	0.083*	0.113*	0.102**	0.69	0.049	0.044	0.055	0.72	0.034
	(0.048)	(0.063)	(0.040)		(0.042)	(0.053)	(0.040)		
	[1779]	[1408]	[1978]		[2189]	[1765]	[2467]		
Pass math by spring of 2nd year (if took college math)	0.063	0.072	0.072*	0.75	0.035	0.062	0.052	0.75	0.028
	(0.045)	(0.061)	(0.040)		(0.037)	(0.045)	(0.033)		
	[2210]	[1741]	[2462]		[3145]	[2462]	[3518]		
Pass math by spring of 1st year	0.008	0.031	0.000	0.47	-0.078**	-0.066	-0.074**	0.49	0.086
	(0.044)	(0.056)	(0.041)		(0.036)	(0.046)	(0.031)		
	[3206]	[2434]	[3612]		[5232]	[3896]	[5907]		

Table A10. Continued

Dependent variable	SAILS SCHOOLS, 2013-14 BW4, controls	BW3, controls	BW5, controls	Control mean	NON-SAILS SCHOOLS, 2013-14 BW4, controls	BW3, controls	BW5, controls	Control mean	Difference (BW4)
Outcomes for TN CC Enrollees									
Pass math by spring of 2nd year	0.050	0.069	0.039	0.58	-0.027	0.020	-0.028	0.57	0.077
	(0.043)	(0.060)	(0.043)		(0.036)	(0.045)	(0.030)		
	[3206]	[2434]	[3612]		[5232]	[3896]	[5907]		
Withdrew from math by spring of 1st year (if took college math)	0.017	0.043	0.020	0.08	-0.034	-0.001	-0.044*	0.07	0.051
	(0.026)	(0.039)	(0.026)		(0.025)	(0.033)	(0.025)		
	[1779]	[1408]	[1978]		[2189]	[1765]	[2467]		
Withdrew from math by spring of 2nd year (if took college math)	0.032	0.045	0.036	0.10	-0.062**	-0.054	-0.069***	0.11	0.094
	(0.030)	(0.036)	(0.031)		(0.027)	(0.033)	(0.026)		
	[2210]	[1741]	[2462]		[3145]	[2462]	[3518]		
Total credits earned by spring of 1st year	0.096	0.241	-0.639	22.31	-1.797**	-0.883	-1.948***	21.84	1.893
	(1.108)	(1.385)	(1.074)		(0.770)	(1.103)	(0.705)		
	[2805]	[2149]	[3156]		[4621]	[3496]	[5194]		
Total credits earned by spring of 2nd year	-0.282	1.444	-1.494	36.34	-2.196	-0.028	-2.904**	35.38	1.914
	(1.937)	(2.606)	(1.894)		(1.623)	(2.140)	(1.461)		
	[2805]	[2149]	[3156]		[4621]	[3496]	[5194]		

Notes: For the outcome in each row, we report the discontinuity at the remediation cut-off of 19. We assume a linear relationship between the outcome and ACT score (the running variable), although we allow for different slopes above and below the cut-off. We used the integer ACT scores. All regressions also include high school fixed effects and demographic controls for race, ethnicity, and sex. The sample includes all high school seniors in the noted year (2013-14). We present estimates using bandwidths of 3, 4, and 5 integer points. Control means are the mean outcomes for students within 1 point above the cut-off on the rescaled measure. Heteroskedastic robust standard errors are clustered by high school and included in parentheses (*p<0.10, **p<0.05, ***p<0.01).

Table A11. SAILS and Non-SAILS School RD Estimates, 2015-16, All High School & Post-Secondary Outcomes

	SAILS SCHOOLS, 2015-16				NON-SAILS SCHOOLS, 2015-16				
Dependent variable	BW25, controls	BW15, controls	BW35, controls	Control mean	BW25, controls	BW15, controls	BW35, controls	Control mean	Difference
SAILS participation	0.340*** (0.024) [12438]	0.286*** (0.028) [5841]	0.385*** (0.023) [20306]	0.07	0.001 (0.002) [5972]	0.009* (0.005) [2934]	0.001 (0.002) [9572]	0.00	0.339
Student took at least one likely Bridge Math class	-0.007 (0.017) [12073]	0.014 (0.021) [5663]	-0.015 (0.017) [19701]	0.11	0.093*** (0.026) [5806]	0.117*** (0.035) [2857]	0.094*** (0.028) [9282]	0.20	-0.100
High school graduate	0.001 (0.007) [12438]	0.004 (0.010) [5841]	0.004 (0.006) [20306]	0.97	0.011 (0.013) [5972]	0.028 (0.020) [2934]	0.023** (0.010) [9572]	0.94	-0.010
Enroll in any college by spring of 1st year after HS	0.020 (0.018) [12438]	0.025 (0.028) [5841]	0.042** (0.016) [20306]	0.71	0.038 (0.026) [5972]	0.067 (0.045) [2934]	0.059*** (0.021) [9572]	0.69	-0.018
Enroll in two-year college by spring of 1st year after HS	-0.006 (0.018) [12438]	-0.027 (0.027) [5841]	-0.003 (0.016) [20306]	0.37	0.015 (0.024) [5972]	0.031 (0.042) [2934]	0.009 (0.020) [9572]	0.25	-0.021
Enroll in TN CC by spring of 1st year after HS	-0.014 (0.017) [12438]	-0.030 (0.026) [5841]	-0.010 (0.016) [20306]	0.34	0.016 (0.023) [5972]	0.025 (0.042) [2934]	0.012 (0.019) [9572]	0.23	-0.030
Enroll in TCAT by spring of 1st year after HS	0.006 (0.007) [12438]	0.003 (0.009) [5841]	0.006 (0.006) [20306]	0.03	-0.007 (0.008) [5972]	-0.002 (0.014) [2934]	-0.005 (0.007) [9572]	0.02	0.013
Enroll in four-year college by spring of 1st year after HS	0.026 (0.019) [12438]	0.049* (0.028) [5841]	0.045*** (0.016) [20306]	0.36	0.024 (0.024) [5972]	0.032 (0.045) [2934]	0.045** (0.020) [9572]	0.45	0.002

Table A11. Continued

Dependent variable	SAILS SCHOOLS, 2015-16 BW25, controls	BW15, controls	BW35, controls	Control mean	NON-SAILS SCHOOLS, 2015-16 BW25, controls	BW15, controls	BW35, controls	Control mean	Difference
Outcomes for Community College Enrollees									
Take remedial math by spring after HS	0.146***	0.162***	0.146***	0.02	0.277***	0.299***	0.301***	0.01	-0.131
	(0.020)	(0.028)	(0.018)		(0.033)	(0.050)	(0.030)		
	[3939]	[1854]	[6264]		[1553]	[740]	[2427]		
Take college math by spring after HS	0.050	0.081	0.063**	0.72	-0.052	0.045	0.021	0.76	0.102
	(0.037)	(0.061)	(0.030)		(0.054)	(0.083)	(0.042)		
	[3939]	[1854]	[6264]		[1553]	[740]	[2427]		
Pass college math by spring after HS (If took college math)	0.067	0.040	0.080**	0.70	0.024	-0.030	0.041	0.78	0.043
	(0.044)	(0.075)	(0.034)		(0.066)	(0.088)	(0.052)		
	[2914]	[1382]	[4536]		[1201]	[578]	[1853]		
Withdrew from college math by spring after HS (If took college math)	0.020	0.039	0.004	0.09	-0.026	0.030	-0.029	0.06	0.046
	(0.027)	(0.046)	(0.022)		(0.039)	(0.067)	(0.031)		
	[2914]	[1382]	[4536]		[1201]	[578]	[1853]		
Pass college math by spring after HS	0.080*	0.092	0.096***	0.50	-0.011	0.007	0.052	0.59	0.091
	(0.041)	(0.064)	(0.034)		(0.063)	(0.098)	(0.052)		
	[3939]	[1854]	[6264]		[1553]	[740]	[2427]		
Total credits by spring after HS (Excluding remedial)	-0.288	-0.206	-0.158	24.78	0.775	-0.880	0.460	24.05	-1.063
	(0.900)	(1.420)	(0.722)		(1.220)	(1.966)	(0.984)		
	[3440]	[1636]	[5397]		[1388]	[664]	[2138]		

Notes: For the outcome in each row, we report the discontinuity at the remediation cut-off of 19. We assume a linear relationship between the outcome and ACT score (the running variable), although we allow for different slopes above and below the cut-off. We used a rescaled version of the ACT with a finer-grained scale. All regressions also include high school fixed effects and demographic controls for race, ethnicity, and sex. The sample includes all high school seniors in 2015-16. We use bandwidths of 25, 15, and 35 rescaled ACT points. Control means are the mean outcomes for students within 10 points above the cut-off on the rescaled measure. Heteroskedastic robust standard errors are clustered by high school and included in parentheses (*p<0.10, **p<0.05, ***p<0.01).

The top panel of Figure A1 plots the distribution of rescaled ACT scores (using the fine-grained scale) for the cohort of Tennessee seniors in 2015-16, while the bottom panel limits this to students who also took the posttest. The distribution appears to have three different layered distributions due to differences in when students took the test. The largest distribution represents students who took the ACT test on the statewide test administration day. The second largest distribution represents those who took the ACT on a makeup test date, while the smallest distribution represents students who took the test with an accommodation. Across each of the three distributions, there does not appear to be a sharp change in density at the threshold for assignment to remediation. If anything, it appears that students who took the statewide test on a make-up day were more likely to score below the cutoff than above, which is not consistent with student incentives to avoid remediation.

Figure A1. Distribution of Rescaled ACT Scores for 12th Graders in 2015-16

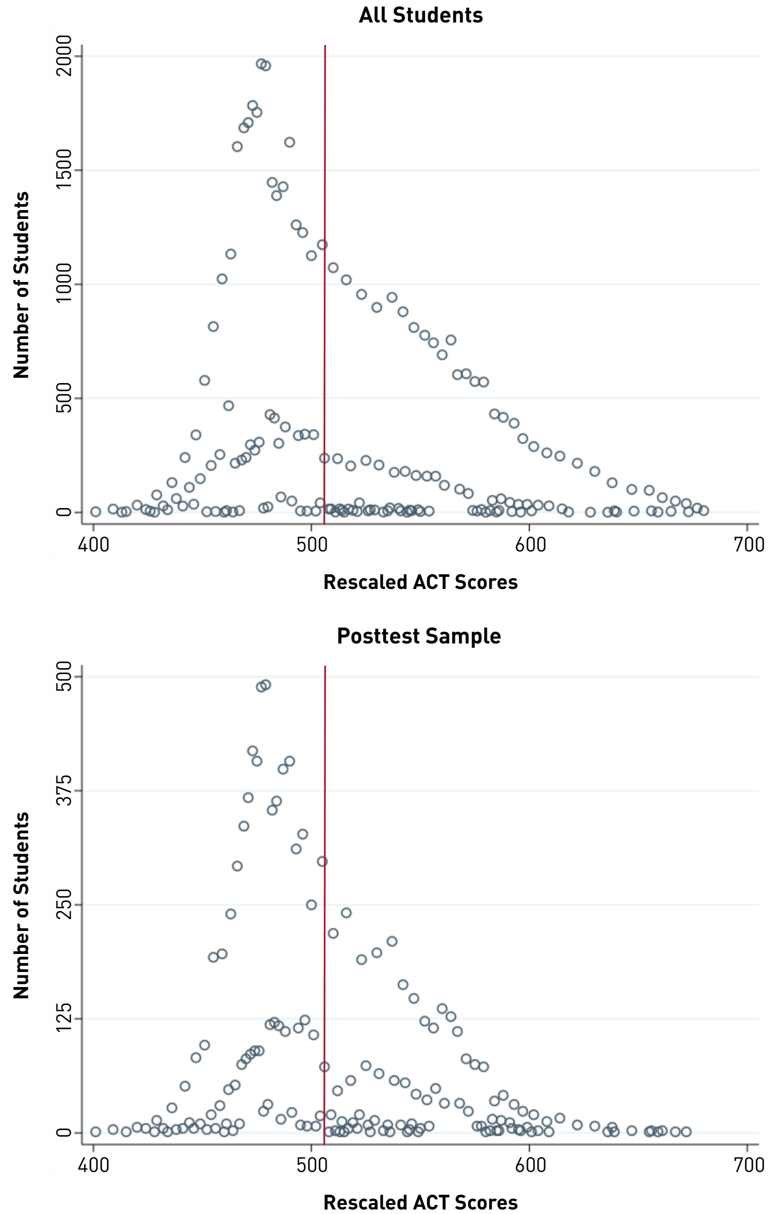


Figure A2. Distribution of Coarse ACT Scores, 12th Graders in 2013-14

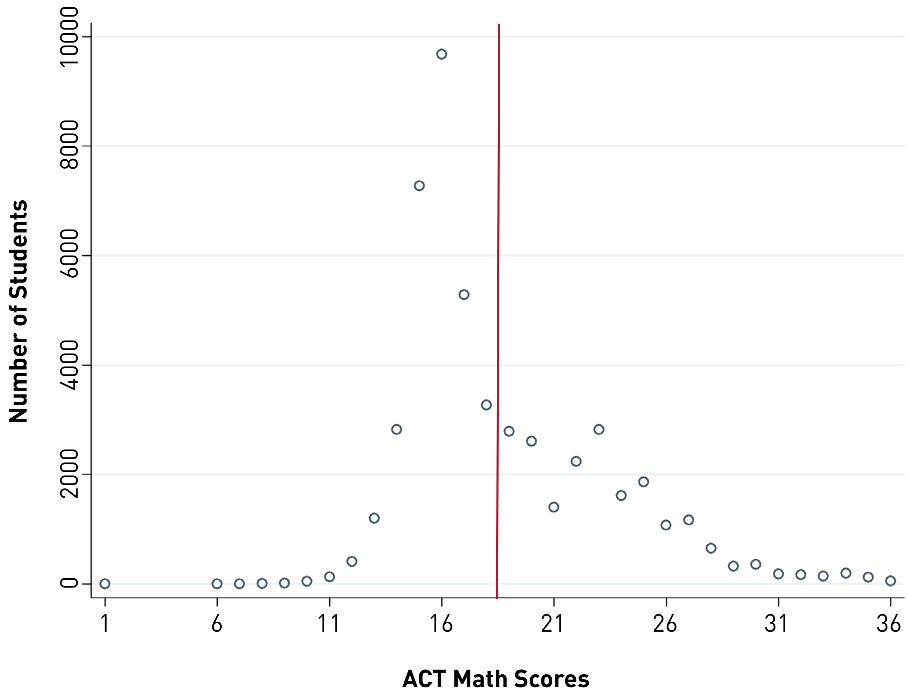
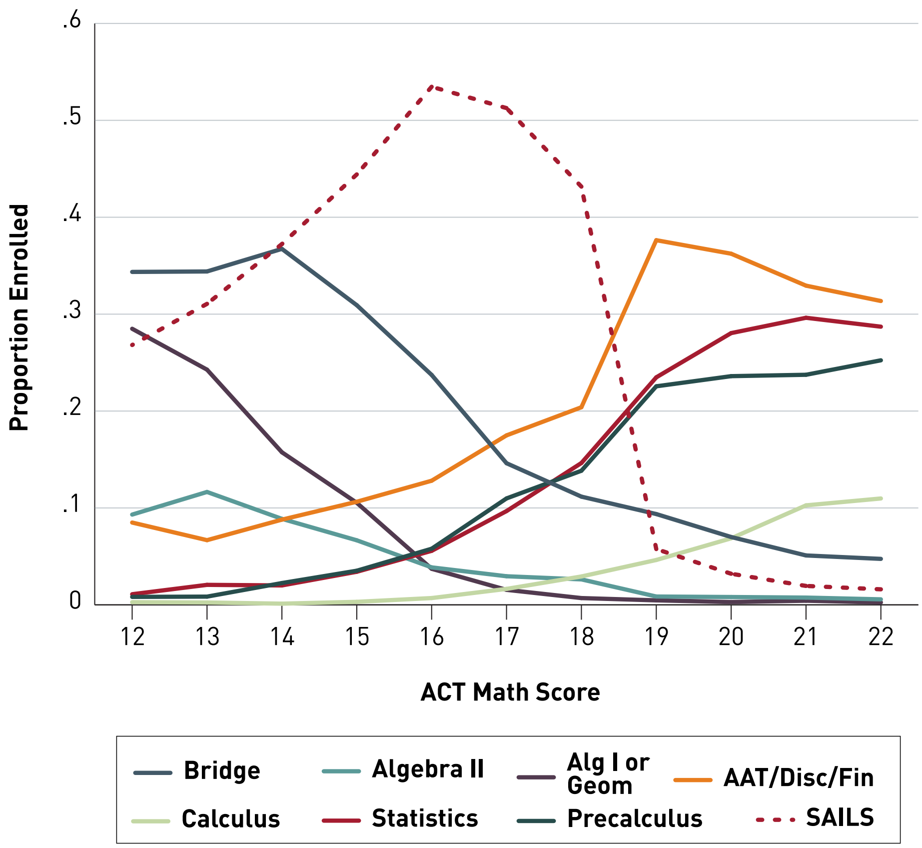


Figure A3. High School Math Course Enrollment for Students in SAILS Schools (2015-16), by ACT Score



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