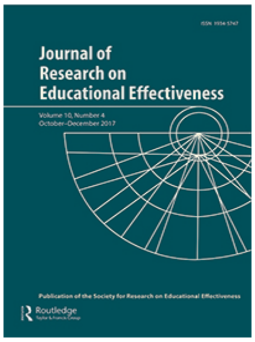

Journal of Research on Educational Effectiveness

ISSN: 1934-5747 (Print) 1934-5739 (Online) Journal homepage: https://www.tandfonline.com/loi/uree20

Can Low-Cost Online Summer Math Programs Improve Student Preparation for College-Level Math? Evidence From Randomized Experiments at Three Universities

Matthew M. Chingos, Rebecca J. Griffiths & Christine Mulhern

JOURNAL OF RESEARCH ON EDUCATIONAL EFFECTIVENESS
2017, VOL. 10, NO. 4, 794–816
https://doi.org/10.1080/19345747.2017.1300362

INTERVENTION, EVALUATION, AND POLICY STUDIES

Check for updates

Can Low-Cost Online Summer Math Programs Improve Student Preparation for College-Level Math? Evidence From Randomized Experiments at Three Universities

Matthew M. Chingos[a], Rebecca J. Griffiths[b], and Christine Mulhern[c]

ABSTRACT

Every year many students enter college without the math preparation needed to succeed in their desired programs of study. Many of these students struggle to catch up, especially those who are required to take remedial math courses before entering college-level math. Increasing the number of students who begin at the appropriate level of math has become an important focus for educators and policymakers. We conducted randomized experiments of low-cost online summer math programs at three universities to test whether this type of intervention can increase access to math preparation, improve placement and enrollment in fall math classes, and improve performance in first-year math courses. Students who received the intervention engaged with the platform, though at relatively low rates, and were more likely to retake the placement test and improve their scores than students in the control group. However, these improved scores did not translate into enrolling in higher level math courses, obtaining more math credits, or improving grades in math-related courses during the first year of college. Thus, providing students access to this online tool did not improve their math skills.

KEYWORDS
remedial math
online instruction
higher education
student learning
randomized experiment

Every year, postsecondary institutions in the United States enroll more than 1.5 million students who are identified as unprepared to succeed in college-level mathematics (National Center for Education Statistics, 2014).[1] This problem is particularly acute in community colleges and open-access four-year institutions, but even selective research universities take in many students whose math skills are not adequate for their desired courses of study (Jones et al., 2012).[2] It is estimated that only one quarter to one third of students entering college are academically prepared for college (Chen, Wu, & Tasoff, 2010; Greene & Forster, 2003). The standard approach for serving students who are deemed "not ready" for college-level math has been to place them in remedial or developmental courses, typically a sequence of

CONTACT Christine Mulhern mulhern@g.harvard.edu Center for Education Policy Research, Harvard University, 4th Floor, 50 Church Street, Cambridge, MA 02141, USA.
[a]Urban Institute, Washington, DC, USA
[b]SRI International, Arlington, Virginia, USA
[c]Harvard University, Cambridge, Massachusetts, USA

[1]NCES Digest of Education Statistics for 2014 Table 311.4 indicates that in academic year 2011–2012, 16.2% of 9.4 million incoming first-year students took remedial math.

[2]For example, 20% of students accepted at the University of Maryland College Park, the state's flagship university, place into remedial math (Complete College America, 2012).

full-semester courses starting as low as basic math (whole numbers, fractions, decimals, etc.). These courses do not count toward college degrees but still incur tuition and fees.

This approach has substantial drawbacks. Approximately half of all undergraduate students take at least one remedial course, and among students who take a remedial course, the average student takes 2.6 such courses (Scott-Clayton, Crosta, & Belfield, 2014). These courses have high failure rates and can be discouraging for students, inhibiting many students who start in remedial courses from ever progressing to college-level courses (Bailey, Jeong, & Cho, 2010). Furthermore, at the national level, students who start out in remedial courses have poor graduation prospects—only about a third of these students in four-year colleges finish in six years, and less than 10% of community college students who start in remedial courses attain a degree in three years (Complete College America, 2011). Moreover, the costs to students and taxpayers are substantial: one study estimated that remediation costs the nation $7 billion annually when considering the tuition paid by students as well as government subsidies (Scott-Clayton et al., 2014).

Furthermore, students who place into college-level math may still place below their programs' preferred initial math course. Discussions with math faculty at universities in Maryland indicated this was a significant concern, especially for students who desired to earn a STEM degree. Students placing into college algebra need to complete many more math courses to earn a STEM degree than students starting in calculus, and there is some evidence that starting in lower level math courses decreases students' probability of earning a STEM degree (Kokkelenberg & Sinha, 2010; Van Dyken, Benson, & Gerard, 2015).

Educators, administrators, researchers, and policymakers are pursuing a variety of approaches to address concerns about students' math placements. Summer bridge programs are one long-standing approach to improving college readiness (Myers & Drevlow, 1982). These are typically intensive, campus-based programs run during the summer between high school and college. They aim to improve multiple dimensions of college readiness, including study habits and life skills, in addition to math and literacy skills (Cabrera, Miner, & Milem, 2011). However, the available evidence on the effectiveness of these programs is limited, and the evidence that does exist points to mixed results and is of variable rigor (Ackermann, 1991; Garcia, 1991; Santa Rita & Bacote, 1997; Tan, 1985).

The most rigorous study of summer bridge programs to date was conducted at six community colleges and two open-enrollment institutions in Texas (Barnett et al., 2012). Student participation was randomly assigned, and the study compared outcomes over a two-year period for students who participated in summer bridge programs with those of students who did not. The researchers found that the programs increased students' pass rates in college-level math by 5–9 percentage points each semester in the first year and a half, and they increased pass rates in college-level writing between 3 and 5 percentage points, but these effects were only marginally significant. They did not, however, find any effects on the number of credits attempted or earned, or persistence rates over a two-year period. The authors surmised that it is not realistic to expect sustained long-term impacts from a short-term, intensive intervention (Barnett et al., 2012). In addition, the study estimated the average costs of these programs at $1,319 to the student, with considerable variance across the eight participating institutions from $835 to $2,349.

The high costs of in-person summer bridge programs is just one of their drawbacks. These programs also require substantial time commitments—typically three to six hours per day for four to five weeks—and not all students are able and willing to commit the necessary time during the summer before their first year of college.[3] Given these challenges, as well as underwhelming long-term impacts from these programs, institutions continue to look for more cost-effective ways to prepare students for college-level math.

Some institutions have sought to reduce student placement into remedial courses by altering placement testing policies or compressing developmental course sequences. Other institutions have eliminated remedial courses altogether and replaced them with cocurricular supports, such as tutoring services (Jones et al., 2012; Logue, Watanabe-Rose, & Douglas, 2016).[4] A study at the City University of New York found that assigning students to college-level statistics with extra help workshops instead of remedial elementary algebra increased pass rates by 16 percentage points (Logue et al., 2016).

A number of state legislatures have also become involved in creating policies to address the poor outcomes from traditional approaches to remediation. Four-year schools in Tennessee and South Carolina, for example, are banned from providing developmental courses (Long & Boatman, 2013), and in Florida all high school graduates are entitled to take college-level courses if they so choose (Fain, 2013).[5]

These policies fit with recent research by Clotfelter, Ladd, Muschkin, and Vigdor (2015), which suggests that community college students in North Carolina are much less likely to ever pass a college course in the relevant subject or achieve a success measure (degree attainment, certificate, or transfer) if they begin in a remedial course. In the four-year sector, Bettinger and Long (2009) find that remedial courses negatively impact credit completion and persistence, but they have positive effects on bachelor's degree completion in four years.

There are also concerns with the cutoff scores used to determine students' placements. A study conducted by Belfield and Crosta (2012) found that a significant portion of students who placed into developmental courses could have succeeded in college-level ones, and urged institutions to consider a broader set of indicators when making placement decisions. Research on community college students in Florida and Texas suggests that students who just miss the cutoff score for placement into college-level courses have lower persistence rates and credit accumulation than their peers who scored just above the cutoff (Calcagno & Long, 2008; Martorell & McFarlin, 2011; Scott-Clayton & Rodriguez, 2015). Thus, student placement scores may not be accurate reflections of a student's ability to succeed in college-level courses, especially if it has been a long time since the student took a math course or if she or he is unfocused on the day of the test. Improving the placement tests, providing brief reviews before students take the test, and increasing the number of times a student can take the math placement test are potential strategies for increasing marginal students' enrollment in higher level college courses.

There has also been a push to deliver the content in remedial courses more effectively, with some focus on online learning technologies as a potential solution (Page & Scott-Clayton, 2016). Online learning technologies may be able to substitute for some parts of the

[3]At one university in Maryland, for example, only 1% of summer session students reportedly had children, compared to 60% for the student body as a whole.

[4]Examples include the SAILS program in Tennessee; Austin Peay State University replaced remedial courses with credit-bearing ones with extra supports; and the University of Maryland College Park places the top 60% of students who place into developmental math in a credit-bearing corequisite math course that meets five days per week (Chattanooga State Community College, 2017; Complete College America 2012, p. 9).

[5]Preliminary evidence from Florida suggests that this law led to reduced pass rates in college-level courses but increased the proportion of entering students who passed English and math gateway courses (Hu et al., 2016).

math readiness component of summer bridge programs or remedial courses, and they have the potential to improve student learning outcomes, increase access, and lower costs (Bowen, 2013). Existing evidence on the impact of online learning technologies on student learning outcomes and costs points to mixed conclusions. Some studies find that students perform worse in online courses than face-to-face ones (Alpert, Couch, & Harmon, 2016; Figlio, Rush, & Yin, 2013; Xu & Jaggars, 2013). However, a meta-analysis from 2010 indicates that students performed slightly better in online sections of courses than face-to-face ones (Means, Toyama, Murphy, Bakia, & Jones, 2010). In addition, more recent experiments have failed to detect significant improvements (or decrements) associated with adoption of technology in hybrid formats, which combine face-to-face and online instruction (Alpert et al., 2016; Bowen, Chingos, Lack, & Nygren, 2014; Chingos, Griffiths, Mulhern, & Spies, 2017).

Looking specifically at technology-enhanced remedial math content, two studies have found positive effects. Evans and Henry (2015) examine an intervention in a community college using a computer-based adaptive placement test, targeted feedback based on students' test scores, and an opportunity to retake the placement test. The intervention increased college-level math placement by 7 to 9 percentage points without any decrement in course success. Boatman (2012) evaluates remedial programs revised to include online learning or self-directed technology components using a regression-discontinuity approach and finds positive impacts on credits attempted and persistence. Given the mixed evidence on the impact of online learning technologies on student performance, and more recent work suggesting the potential for positive effects in remedial education, additional research is needed to determine in which setting, if any, online learning can improve student learning. Furthermore, no experimental studies to date have examined how online learning programs in the summer before a student begins at a four-year college can impact students' math skills, placement test scores, and their first-year performance.

Summer programs offer an appealing context for examining both learning impacts and the potential for cost reductions associated with emerging technologies. Online technologies can make it easier to educate a large number of students starting with different levels of understanding because many of them can be customized at the individual level. Adaptive technologies aim to capture information about individual students as they interact with the software and use this information to create customized learning paths that can be worked through at one's own pace. Putting math readiness programs online can also increase access, especially for students who need to work or care for a family. However, online summer programs may not be effective if students do not have regular summer Internet access or do not devote time to working on the program.

Finally, online versions of summer programs may cost much less than their traditional counterparts. Software with instructional content, embedded assessments, and automated grading can substantially decrease time demands on instructors. Because institutions typically rely on short-term contracts with summer instructors, a reduction in staff time demands can translate into direct cost savings.

The Study

This study aimed to test whether offering low-cost, online-only summer math preparation programs could improve students' math skills prior to beginning college and their performance in the first year of college. Specifically, we sought answers to the following questions:

What is the impact on student outcomes associated with use of an adaptive learning product as part of various types of summer math preparation programs? Can emerging technologies be used to make these programs more accessible and/or affordable? And finally, would we be able to detect any short-term or sustained benefits from interventions that are delivered entirely online?

During the summer of 2014, we worked with three institutions in Maryland to conduct a series of randomized experiments using MyFoundationsLab (MFL), an online learning product provided by Pearson, in summer programs. The three institutions involved were: the University of Maryland, Baltimore County (UMBC); Towson University; and Bowie State University.

The research team worked with administrators at the University System of Maryland to identify these partner institutions. They were selected because they serve a large number of students who test below the desired level in math and they expressed interest in improving pathways to higher level math courses. Five of the universities in the university system expressed interest but after meetings with the research team only three were willing to follow the implementation protocol desired by the research team. These institutions participated voluntarily, largely because the research dovetailed with their needs and desire to try out new approaches to improve math readiness of incoming students. They were provided funds to cover the costs of a program facilitator, data collection, and additional placement testing. Pearson provided free licenses to MyFoundationsLab for students participating in these programs.

Two of the participating institutions (Towson University and Bowie State University) offer face-to-face summer bridge programs, but administrators and math faculty believed that an online-only, free program focused on improving math knowledge would appeal to a segment of students who did not have the desire or ability to attend the in-person programs.

We focused on investigating whether provision of an online-only summer program could produce gains in student placement test scores, placement into higher level math courses, and performance in students' first year math courses, relative to not participating in any formal summer program. The intervention consisted of an invitation for students to participate in the program, free access to MyFoundationsLab, and a series of e-mail messages from program facilitators encouraging students to engage with the program. As is typically the case for summer programs, participation was optional for students.

Given the findings of earlier research on campus-based summer bridge programs and remedial math interventions, we did not expect to find dramatic gains associated with an entirely online intervention focused only on math skills. We did, however, see value in testing whether a relatively low-cost program, made possible by emerging technologies, could "move the needle" in terms of students' math preparation and in obtaining a clearer understanding of the value of the online tools. Furthermore, previous studies had focused on community colleges and open-access four-year institutions, and we thought it would be valuable to see how students attending more selective institutions might benefit from technology-enhanced summer programs.

Research Design

In order to be eligible for inclusion in our study, students had to:

- be enrolled to start at one of these institutions in fall 2014,

Table 1. Intervention components.

Intervention components	UMBC		Towson University		Bowie State University	
	Treatment	Control	Treatment	Control	Treatment	Control
Invitation to participate in summer program	✓		✓		✓	✓
Access to MFL	✓		✓		✓	
Access to computer lab on campus					✓	✓
Access to static online resources						✓
Program facilitator	✓		✓		✓	✓
Option to retake placement test	✓	✓	✓	✓	✓	✓
Assistance switching to different math courses if placement score improved	✓	✓	✓	✓	✓	✓

- have taken the placement test before the required deadline,
- have placed into developmental math or a math course below the desired level for their degree program, and
- not have registered for the residential summer bridge program (where available).

At Towson University and Bowie State University, invitations to the online program were withheld until after students had chosen whether or not to enroll in the campus-based summer bridge program so as not to attract students who would otherwise enroll in the more intensive face-to-face program.

At each institution, eligible students were randomly assigned to either treatment or control groups. Half of the eligible students at UMBC and Bowie State University were assigned to the treatment group and Towson University assigned 58% of eligible students to the treatment group.[6] Students in the treatment groups received invitations to participate in the intervention and free access to MyFoundationsLab, while students in the control groups were simply notified of the option to retake the placement test at the end of the summer.[7] Students in the control group at Bowie State University also received an invitation to access resources available through the campus Learning Resource Center via Blackboard. Invitations were mailed to students' home addresses and instructions for accessing the online platform were e-mailed to students shortly after the initial invitation. Towson University also called students in the treatment group. At the end of the summer all students in the treatment and control groups were offered the option to retake the placement test, and if their score improved enough they could place into a higher level math course.

Students in the treatment group had access to the online system for four to six weeks, depending on the length of the program at their institution. The online system was accompanied by a series of communications from facilitators who tracked students' progress and sent e-mail "nudges" two to three times per week to encourage them to engage with the online system. These nudges were informed by research on the kinds of messages that have been shown to influence student behavior (Martinez, 2014). For example, facilitators sent messages recognizing positive effort and informing students when they put in less effort than their peers.[8] Table 1 summarizes components of the intervention provided to the treatment and control groups at each institution.

[6]The administrators at Towson University were concerned we would not be able to detect significant impacts of the online platform if only 50% of students were assigned to the treatment because they expected that many students would not utilize the online platform and the eligible population was relatively small.
[7]Copies of the invitation letters are available upon request.
[8]Examples of these messages are available upon request.

Table 2. Student characteristics by institution.

	UMBC			Towson University			Bowie State University		
	Control	Treatment	Difference	Control	Treatment	Difference	Control	Treatment	Difference
SAT math	512	506	−6	420	384	−36	563	542	−21
SAT verbal	532	531	0	435	385	−50	589	578	−11
SAT math missing	17%	15%	−0.03	9%	41%	0.32	19%	16%	−0.03
SAT verbal missing	17%	15%	−0.03	9%	41%	0.32	19%	16%	−0.03
HS GPA	3.48	3.45	−0.03				3.62	3.67	0.05
HS GPA missing	3%	2%	−0.09				12%	14%	0.02
White	68%	61%	−0.07	0%	0%	0.00	34%	43%	0.09
Black	16%	20%	0.04	88%	89%	0.01	25%	38%	0.01
Hispanic	7%	6%	−0.01	3%	2%	−0.02	14%	5%	−0.09
Asian	2%	4%	0.03	0%	4%	0.04	9%	12%	0.03
Other race	7%	8%	0.01	8%	6%	−0.03	18%	2%	−0.16
Race missing	3%	2%	−0.09	9%	41%	0.32	3%	0%	−0.03
Female	67%	68%	0.02	53%	68%	0.15*	59%	57%	0.0
Age	18.2	18.2	0.02	17.8	18.0	0.19	18.7	18.1	−0.6
Age missing	3%	2%	−0.09	9%	41%	0.32	0%	0%	0.0
Inc <$50,000	24%	25%	0.02	45%	65%	−0.02			
Inc $50,000 to $100,000	26%	25%	−0.01	38%	22%	−.20			
Inc >$100,000	50%	49%	0.01	18%	14%	−0.08			
Income missing	17%	14%	−0.03	14%	43%	0.30			
Pell eligible							34%	36%	0.02
Pell status unknown							14%	19%	0.05
Initial placement score	12.5	12.5	0.0	61.3	64.6	3.4	18.7	19.1	0.4
F test	0.82			3.11			1.61		
p value	0.66			0.00			0.08		
Observations	345	352		65	90		58	58	

Notes. For the difference between treatment and control $^{*}p < 10\%$, $^{**}p < 5\%$, and $^{***}p < 1\%$.
The *F* test is of the null hypothesis that all coefficients are 0 in a regression of treatment assignment on the demographic variables. We would expect all coefficients to be 0 if the treatment and control groups are balanced.

Outcomes of interest included whether or not students chose to retake the placement test and their scores on the first and, if applicable, second test administrations. In addition, we observed whether students who were invited to participate in the program enrolled in math-related courses during their first year in college, whether, on average, they enrolled in higher level math courses, their course grades, and whether they passed those courses.

There were two notable differences in study design across sites:

1. Bowie State University's institutional review board required that students give consent before random assignment. Eligible students were first invited to participate in the study, and then those who opted to participate were randomly assigned to treatment and control groups.[9] Students in the control group were given access to a Blackboard website with static content and received fewer communications. Students in the treatment group, as at other sites, had access to MFL and received frequent nudges from the facilitator. In contrast, at UMBC, eligible students were given the opportunity to opt *out* of the study, and only 1% chose to do so. At Towson University, all eligible students were randomly assigned into treatment and control groups.[10] Control group

[9]We realize this requirement may have impacted the type of students involved in our study. Later we look at the results for only UMBC and Towson University to see if our results differ.

[10]It was agreed with Towson University that if any students (or their parents) in the control group contacted the institution requesting access to the program, they would be given access and excluded from the study. No such instances occurred.

Table 3. UMBC student utilization of MFL by initial placement test score.

Placement score*	Course placement	Indicated intention to use MFL	Logged onto MFL	Total N in treatment group
0 to 7	Developmental Math	73%	46%	37
8 to 9	Intermediate Algebra	64%	43%	28
10 to 13	Mathematical Ideas or Transition to Algebra for Applications	49%	34%	119
14 to 16	Finite Math, Math for Science, Statistics, or Biostatistics	56%	35%	168
Total		56%	36%	352

*Maximum possible score is 25.

students at these two institutions received no further communications other than the invitation to retake the placement test.

2. The three sites had somewhat different eligibility requirements: UMBC included students who placed into developmental math and into some 100-level courses. (Table 3 describes how many students initially placed into each course.) Many of these students needed to take pre-calculus or statistics for their degrees, so they had an incentive to improve placement scores to place directly into those subjects. Towson University invited students who placed into developmental math but below pre-calculus to participate in the study. Students who had already signed up to attend an intensive in-person summer bridge program were excluded. Bowie State University's primary focus was students who placed into college algebra, but the list of invitees included some students who placed into remedial math and pre-calculus.

We place the greatest weight on the results from UMBC given that it had the largest sample and cleanest implementation. The procedure for randomly assigning students was slightly different at Towson University and Bowie State University, and there are some concerns with missing data and small samples at these institutions.

Pearson's MyFoundationsLab

Each of the three campuses used Pearson's MyFoundationsLab, a mastery-based system for assessing and remediating college readiness skills in mathematics, reading, and writing with personalized learning plans and interactive learning activities. Based on an initial assessment called the PathBuilder, MFL creates an individual "learning path" consisting of modules that are identified as areas of weakness. Each module then begins with an assessment to identify specific topics (finer-grained than modules) on which a student needs to work. There are 243 topics in total.

Modules consist of instructional resources (tutorials, videos, and worked examples), practice problems, quizzes and posttests. The program is self-paced, meaning that students can work through topics and assessments on their own schedules. The system includes an instructor dashboard that provides information about students' scores on assessments in the system, the number of attempts they make, their progress in the course by topic and unit, and engagement metrics such as last log-in time and date. MFL is designed to work in a wide variety of contexts, including face-to-face and online-only programs. The campuses used only the math components.

This product was selected in part because it is instrumented with the Knewton "adaptive learning engine," which is intended to diagnose students' skill gaps at a granular level and prescribe specific activities to address individual gaps in conceptual understanding or skills. Investigating the impacts associated with adaptive technologies was a high priority given the lack of such evidence available when this study launched in 2013. However, Towson University was unable to use the Knewton component of the platform because the program coordinators opted to integrate MFL with their placement test instead.

Costs

Costs associated with the online intervention were relatively low. Institutions needed to appoint a program coordinator to determine which students should be eligible and issue invitations to students at the appropriate time (late spring or early summer). A facilitator at each institution had to provide students with access to MFL, answer questions, and issue the prescribed set of "nudges" over a period of four to six weeks. The coordinator also bore responsibility for enabling students to retake the placement test and making sure that students were aware of this option. Finally, advisors needed to be on hand to help students who improved their placement scores to enroll in higher level math courses.

Pearson waived the licensing fee for the students in the study, but under normal circumstances, students (or institutions, if they so choose) would incur a license fee to use MFL. This fee is estimated at $33 for 10 weeks of access. It is worth considering how students might have responded if they had borne this cost—some students may have opted not to participate, but it is also possible that students who paid the license fee might be more inclined to spend time in the program to make it worth the monetary cost. With conservative assumptions that a facilitator costs $4,000 and 200 students are enrolled in the program, the cost per student would be just over $50 for the facilitator. Combining this cost with the license costs yields an estimate of $83—considerably lower than the average per-student cost of $1,319 for the campus-based programs in Texas (Barnett et al., 2012).

Data

Data collected included initial placement test scores, placement scores for those who retook the test at the end of the summer, Pearson logs of who accessed the platform, transcript data for students' first year of college, and demographic variables. Average student characteristics by institution and treatment/control group are provided in Table 2. A substantial portion of the sample is minority (approximately half) and/or low income (31% have family incomes less than $50,000 or are Pell eligible), and the average age is 18. SAT scores for UMBC and Bowie State University are fairly high compared to students in other studies examining remedial math, and 89% of students at UMBC placed into college-level math (Table 3). Our sample is also younger than some other studies, especially those focused on community college students (Logue et al., 2016).

At UMBC and Bowie State University the treatment and control groups are balanced on student characteristics and missing data (i.e., we cannot reject at the 5% level the null of no difference across baseline characteristics). However, the imbalance at Towson University is concerning. Students in the treatment group are much more likely to be missing demographic data, and the institution was unable to clarify why this was the case. This motivates us to place less emphasis on the results at Towson University relative to the other institutions.

The placement tests used at the three institutions are the MAA Maplesoft (UMBC), Accuplacer (Towson University), and a locally developed exam (Bowie State University). We used student transcripts to determine whether students enrolled in college-level math-related courses, their grades and pass rates in math-related courses, and their overall GPAs.

Online surveys were administered to students in the treatment and control groups at UMBC. Students in the control group were asked about what, if any, work they did to improve their math skills over the summer and why they did or did not retake the placement test. The survey for the treatment group asked questions about why students did or did not choose to participate or retake the placement exam, what they felt they accomplished, and how they would rate the MyFoundationsLab materials. One hundred fifty-eight students in the treatment group took the survey and 178 students in the control group took it, for response rates of 45% and 52%, respectively.[11] Finally, we interviewed program facilitators to qualitatively assess the success of the programs.

Methodology

Random assignment enables us to estimate the causal impact of being assigned to the treatment group by comparing students in the treatment and control groups. No students switched between the two groups, and less than 1% of students opted out of the study after being assigned to a group. We are interested in both the use of the platform when it is offered for free and the impact it has on math skills. We estimate the impact of being offered free access to MFL (and receiving e-mail reminders) using the following specification:

$$Y_i = \beta_0 + \beta 1 * TREAT_i + \beta 2 * X_i + \varepsilon_i$$

Y_i represents the outcome variables, including retaking the placement test, final placement test score, math courses taken, and grades in first-year courses. TREAT is a dummy variable indicating whether the student was assigned to the control or treatment group, X_i is a vector of demographic variables used as controls, and ε_i is the error term. We are interested in β_1, which estimates the effect of being in the treatment group. This is an "intent to treat" effect in that not all students assigned to treatment participated in the intervention.

We estimate the treatment effect separately for each institution because of the differences in intervention design across the institutions. In addition, the three placement tests have different scales so it is difficult to compare the magnitude of placement score changes across institutions. We focus on the results from UMBC because Bowie State University's randomization process only included students who agreed to participate in the study and Towson University's higher rate of missing data for the treatment group is concerning. UMBC also had the largest sample of students.

We check the robustness of the estimates to alternative specifications by estimating regressions with control variables and probit models instead of linear probability models for the binary outcomes. Our main results are similar to those from both of these alternative specifications.

[11] Survey respondents were similar to nonrespondents on all measures except that respondents were more likely to be female (79% vs 56%).

Table 4. UMBC learning outcomes.

		Without covariates		With covariates	
	Control mean	Treatment effect	*SE*	Treatment effect	*SE*
Retook placement test	0.10	0.13***	(0.03)	0.13***	(0.03)
Max (pretest, posttest)	13.06	0.68**	(0.28)	0.80***	(0.26)
GPA, all graded courses	2.78	0.03	(0.06)	0.03	(0.05)
GPA, math courses	2.38	−0.02	(0.08)	−0.01	(0.08)
Took any math course	0.84	0.01	(0.03)	0.00	(0.02)
Passed any math course	0.75	−0.00	(0.03)	−0.01	(0.03)
Start above algebra	0.77	0.02	(0.03)	0.02	(0.03)
Pass above algebra	0.69	0.00	(0.04)	0.00	(0.03)
Start 200 level+	0.14	−0.02	(0.03)	−0.01	(0.03)
Pass 200 level+	0.13	−0.03	(0.02)	−0.02	(0.02)
Took any STEM course	0.91	−0.03	(0.02)	−0.04*	(0.02)
Passed any STEM course	0.84	−0.04	(0.03)	−0.04	(0.03)
Number of students		697		697	

Notes. Robust standard errors are reported.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.
Missing data is set to the mean, and dummy variables are included to indicate individuals missing data for a particular variable. Covariates include gender, age, SAT math, SAT verbal, HS GPA, race (Black, Hispanic, Asian, White, or other), and income (less than \$50,000, \$50,000−\$100,000, or more than \$100,000). The reported control mean is the constant in the regression with no covariates.

Results

We focus on the results at UMBC because of the aforementioned concerns with the study designs at Towson University and Bowie State University. Results for Towson University and Bowie State University are in Appendix B.

At UMBC we found considerable interest among students in participating in the program. The total study sample was 697 students, with 352 randomly assigned to the treatment group and 345 to the control group.[12] Fifty-six percent of students who were invited to participate in the intervention indicated that they intended to participate in the program, and of those, 65% logged into MFL at least once, resulting in 36% of invited students logging on at some point. Students with lower placement scores were more likely to log onto the platform, although the majority of eligible students had relatively high placement scores. Table 3 provides a breakdown of participation by initial placement test score.

Survey results indicate that among students who were invited to use MFL but chose not to register for the program, the most common reason for not registering was that students were satisfied with their placement test scores (38%). The two other common answers were that students did not want to work on their math skills over the summer (21%) and they did not think an online program would help improve their skills (19%).[13] Among students who did register, the main reason for doing so was to improve their score on the placement test (58%).[14]

Table 4 describes student learning outcomes by treatment and control groups. Assignment to the treatment group translated into significantly higher placement test retake rates,

[12] No students changed groups, and less than 1% opted out of the study. By opting out they prevented us from collecting data on them.
[13] Fifty-two students responded to this question. The other options were that they were unable to register because of technical problems, they did not have reliable access to the platform, and "Other."
[14] $N = 80$

Table 5. Detail on students who retook placement test at UMBC.

	Control	Treatment
Retake rate	10%	23%
Did not retake		
Pretest score	12.7	12.6
Retook		
Pretest score	10.6	12
Posttest score	15.1	17
Max (pre, post)	15.8	17.4
Change (post–pre)	4.5	5

with 23% of treatment group students opting to retake the placement test, compared to 10% for the control group. Students in the treatment group improved their score used for placement purposes (i.e., the maximum of their scores), on average, by 1.3 out of 25 possible points on the exam (from 12.5 to 13.7), compared to 0.5 for control group students (from 12.5 to 13.1). (Students who did not retake the placement test or who scored lower on the retake are assigned a score change of zero.) The difference in post-intervention placement scores between the treatment and control groups is significant at the 0.01 level.

However, the larger increase in scores among the treatment group may have been partly the result of the higher retake rate. If we look only at students who retook the test and include both score increases and decreases (an exploratory analysis that is not purely tied to the randomization given the impact of treatment assignment on test retaking probability), we find an average score gain of 5 points in the treatment group and 4.5 points in the control group (Table 5).

This is consistent with the lack of impacts on first-year course performance we find when comparing percentages of students who took any math course and passed any math course during the first year of college (Table 4). Eighty-four percent of students in the control group took a math course compared to 85% of students in the treatment group, and 75% of students in the control group passed any math course, while 74% of students in the treatment group did so. We also find small and generally statistically insignificant negative impacts on students taking and passing STEM courses.

We find null effects on the level of math course taken, which indicates that the increases in placement scores may not have been large enough to move students to the next cutoff score. We find a similar lack of differences when we look at GPAs in all graded courses and in math courses during this period. In sum, we find that being offered access to MFL increased the likelihood that students retook the placement test and increased their scores, but it did not necessarily increase enrollment or performance in math courses (college-level or remedial).

We focus on the results without controlling for demographic characteristics because 28% of students at UMBC are missing data for at least one of the control variables.[15] Adding control variables does not substantially change the results (as can be seen in Table 4), as would be expected given random assignment.[16] In addition, probit regressions for the binary outcomes yield similar results to those estimated using OLS (Table A1 in Appendix A).

[15] SAT and income are the most common missing variables for UMBC. Income data is only available for students who filed for financial aid.

[16] In these regressions we include dummy variables to indicate if a student is missing data for a given variable and, in the case of continuous variables, we replace the missing data with the mean value for that variable.

Finally, we estimate treatment effects by students' initial placement scores. We may expect students who start at different levels of math competency to be differentially impacted by access to MFL. Table A2 shows that we obtain the largest treatment effects for students with placement scores of 8 or 9, which would place them into algebra. However, we do not place too much weight on this finding given the small size of most of these subgroups and the resulting imprecision in the estimates. We also note that this finding is not robust to the inclusion of baseline covariates.

Although the majority of students offered access to MFL at UMBC had placement scores that placed them into college-level math, we do not find strong evidence that the results would be substantially different if a similar intervention were provided to students with lower initial placement scores. Our estimates, while imprecise, do not suggest larger results for either students at the top or bottom of the distribution of initial placement scores. If anything, they suggest that the students in the middle may benefit the most. A potential explanation for the lack of clear variation in treatment effects by baseline placement score is that the adaptive nature of MFL enabled all students to receive instruction that matched their skill level.

Detailed descriptions of the results from Towson University and Bowie State University, along with the caveats, are included in Appendix B. Overall, the findings from Towson University were roughly similar to those at UMBC, with the treatment increasing placement retake rates and scores but not enrollment in college-level math. At Bowie State University, students in the treatment group were significantly less likely to take the placement test and their average score gain was smaller than the average gain for students in the control group. This surprising result may be due to differences in the randomization procedure and materials offered to the control group, which are discussed in the appendix.

Discussion

This study found that online summer math programs using an adaptive learning technology platform could help students to raise their placement test scores at a relatively low cost to institutions and students. We estimate that the intervention would cost $83 per student if students or institutions paid for access to the MFL software, which is significantly less than estimated costs of many existing summer bridge programs. It is also much lower than the tuition costs of courses, though this must be balanced by the small impact it had on students placing into and passing college-level math courses.

Overall, students responded positively to the offer of the program, with roughly half the students who had access to MFL across the three sites logging into the system at least once. At two of the study sites, we found that those who received the intervention were more likely to retake the placement test and to raise their placement test scores, thus achieving significantly improved test scores, but we cannot be sure how much of the improvement in scores is due to actual learning vs. just retaking the test. In addition, we did not see evidence of performance gains in first-year math-related courses.[17]

[17] The notable exception is Bowie State University, where treatment group students were significantly less likely to retake the placement test. We believe this may be at least in part due to the randomization procedure, because students in the control group had opted to participate in the study and thus may not have been representative of the population at large. Another possible explanation is that control group students had access to static content on Blackboard (designed to be something of a placebo intervention), which may have had a larger effect on behavior than anticipated.

Our findings are most consistent with the research by Barnett et al. (2012) on summer bridge programs, indicating that these interventions can produce narrowly defined benefits for students (improved placement scores in our study and 5–9 percentage point increases in math pass rates in Barnett et al.) but that these advances do not translate into improved long-term academic performance. This is not particularly surprising, given the low-touch nature of the intervention compared to the more intensive traditional summer bridge programs. However, our findings are smaller in magnitude than those in Evans and Henry (2015), whose intervention produces 7 to 9 percentage point increases in college-level math placement. This may be due in part to our sample. We focus on students in four-year colleges whose math placement is higher than most students in the studies at community colleges.

Furthermore, survey results from UMBC suggest that the program may have had a limited impact because students spent relatively little time working with MFL. More than 50% of respondents reported spending between one and three hours per week on the program, and only 23% of respondents reported spending four hours or more per week on MFL.[18] This is not very much time considering that the program was only six weeks long and the recommended amount of time was 5 to 10 hours per week. This is consistent with the data from Pearson's usage logs, which indicate that the average student in the treatment group only completed 3 out of the 243 modules. Although this jumps to 7.7 when only looking at students who logged onto MFL, and the average student was recommended to complete 161 modules, most students still only completed a small percent of the recommended work.[19] It is unclear if the limited use of the online tool was due to the nature of the tool itself, the fact that students were expected to work independently over the summer months, or something else. Student surveys indicate that access to a computer and the Internet were not significant obstacles. How alternative tools or program setups might increase student engagement with math readiness software is fertile ground for future research.

The survey also indicates that satisfaction with one's initial placement score was the most common reason that students did not retake the placement test. The high number of students not wishing to improve their placement scores may be related to the fact that Institution A extended invitations to some students who placed into college-level math (though below pre-calculus). There were also some students who did not think they had learned enough to warrant retaking the placement test, but this was only 17% of all students in the treatment group (who responded to the survey). In addition, some students were unaware of the option or unable to retake the placement test during the given time (44% across both groups). Thus, in the future it may make sense to make this option available for a longer period of time and to improve communication to students.

Overall, students' ratings of the MFL materials were positive, indicating that the platform was easy to use, had good-quality instructional materials, was useful for reviewing and learning concepts, and was technically reliable. Seventy-three percent of respondents in the treatment group who participated in the program said they accomplished what they wanted to in the program. Among the 27% of students who did not accomplish what they wanted, the most common reason was that they did not have enough time because of personal

[18] $N = 105$.

[19] These results are pulled from the Pearson Usage logs. MFL determines which modules students should complete based on an initial assessment.

constraints. Given the flexibility of the program, it is hard to imagine a way in which it could be more accessible. However, this data point also suggests the need to create summer bridge programs that are more accessible to students than the current on-campus ones, given students' personal constraints over the summer. Given that the program was entirely optional, it is encouraging that 36% of invited students were willing to put effort into it.

It is possible that we would have found greater impacts on key outcomes with a different software product or with more effective messaging. Recent research in behavioral economics could be used to more effectively design nudges for students to engage with the program (Castleman, Page, & Schooley, 2014; Thaler & Sunstein, 2008). Program administrators may also be more effective in encouraging student participation or selecting appropriate modules in subsequent iterations of the program.

On balance, though, substantially improving students' math skills seems unlikely within the constraints of this format. We are more inclined to conclude that use of an adaptive learning product in an optional, online program can modestly mitigate one symptom of the college readiness problem—underperformance on placement tests—but is unlikely to address the underlying shortage of skills and knowledge of mathematics. Our results are broadly consistent with studies of other voluntary online courses, such as Massively Open Online Courses (MOOCs), which have extremely low completion rates (Jordan, 2014).

Still, it would be useful to conduct similar research using products with significantly different designs. For example, a system with fewer assessments and recommended modules might be more approachable for students working independently. It would also be worthwhile to dig further into the possibilities for using technology to reduce costs in blended summer programs. This program was very low touch, but one could imagine that more instructor communications or a few class meetings could increase student participation and learning.

None of the institutions are continuing the program in its current form. After the study, UMBC decided to tie student placement to intended major (instead of just placement test score) and changed placement cutscores for some courses. Faculty at UMBC are also considering a program that, in addition to testing and placing students, identifies knowledge gaps and sets up corresponding learning modules. They may require students to work through these before retaking the placement test. In addition, Towson University restructured student placement following the study, and its summer math readiness program is now focused on placing all students in credit-bearing math courses, regardless of their placement score.

As is evident from the institutions' next steps, these findings raise further questions about the predictive value of placement exams, given that improvements in math test scores did not lead to gains in math course success. Improvements in placement scores may simply reflect that students' scores will move substantially when they retake it, in ways that are unrelated to the development of their math knowledge. This reflection is important to consider when choosing how to use placement tests to assign students to developmental math. Further work is needed to understand the best methods for placing students into college math courses.

Funding

This research was generously supported by a grant from the Bill & Melinda Gates Foundation.

ARTICLE HISTORY

Received 9 August 2016
Revised 30 January 2017
Accepted 22 February 2017

References

Ackermann, S. P. (1991). The benefits of summer bridge programs for underrepresented and low transfer students. *Community/Junior College Quarterly of Research and Practice*, *15*(2), 211–224.

Alpert, W. T., Couch, K. A., & Harmon, O. R. (2016). A randomized assessment of online learning. *American Economic Review*, *106*(5), 378–382.

Bailey, T., Jeong, D. W., & Cho, S. W. (2010). Referral, enrollment, and completion in developmental education sequences in community colleges. *Economics of Education Review*, *29*(2), 255–270.

Barnett, E. A., Bork, R. H., Mayer, K., Pretlow, J., Wathington, H. D., & Weiss, M. J. (2012). *Bridging the gap: An impact study of eight developmental summer bridge programs in Texas* (NCPR Brief). Retrieved from http://files.eric.ed.gov/fulltext/ED539188.pdf

Belfield, C., & Crosta, P. (2012). *Predicting success in college: The importance of placement tests and high school transcripts* (CCRC Working Paper No. 42). New York, NY: Community College Research Center, Teachers College, Columbia University.

Bettinger, E. P., & Long, B. L. (2009). Addressing the needs of underprepared students in higher education: Does college remediation work? *Journal of Human Resources*, *44*(3), 736–771.

Boatman, A. (2012). *Evaluating institutional efforts to streamline postsecondary remediation: The causal effects of the Tennessee Developmental Course Redesign Initiative on early student academic success* (NCPR Working Paper). New York, NY: National Center for Postsecondary Research.

Bowen, W. G. (2013). *Higher education in the digital age*. Princeton, NJ: Princeton University Press.

Bowen, W. G., Chingos, M. M., Lack, K. A., & Nygren, T. I. (2014). Learning online at public universities: Evidence from a six-campus randomized trial. *Journal of Policy Analysis and Management*, *33* (1), 94–111.

Cabrera, N., Miner, D., Milem, J. (2011). Can a summer bridge program impact first-year persistence and performance?: A case study of the New Start Summer Program. *Research in Higher Education*, *54*(5), 481–498. doi:10.1007/s11162-013-9286-7

Calcagno, J. C., & Long, B. T. (2008). *The impact of postsecondary remediation using a regression discontinuity approach: Addressing endogenous sorting and noncompliance*. (NBER Working Paper No. w14194). Cambridge, MA: National Bureau of Economic Research.

Castleman, B. L., Page, L. C., & Schooley, K. (2014). The forgotten summer: Mitigating summer attrition among college-intending, low-income high school graduates. *Journal of Policy Analysis and Management*, *33*(2), 320–344.

Chattanooga State Community College. (2017). *Seamless alignment and integrated learning support*. Retrieved from https://www.chattanoogastate.edu/high-school/sails

Chen, X., Wu, J., & Tasoff, S. (2010). *Academic preparation for college in the high school senior class of 2003—04*. Washington, DC: U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics.

Chingos, M. M., Griffiths, R. J., Mulhern, C., & Spies, R. R. (2017). Interactive online learning on campus: Comparing students' outcomes in hybrid and traditional courses in the university system of Maryland. *The Journal of Higher Education*, *88*(2), 210–233.

Clotfelter, C. T., Ladd, H. F., Muschkin, C., & Vigdor, J. L. (2015). Developmental education in North Carolina community colleges. *Educational Evaluation and Policy Analysis*, *37*(3), 354–375.

Complete College America. (2011). *Time is the enemy: The surprising truth about why today's college students aren't graduating … and what needs to change*. Retrieved from http://completecollege.org/docs/Time_Is_the_Enemy.pdf

Complete College America. (2012). *Remediation: Higher education's bridge to nowhere*. Retrieved from http://www.completecollege.org/docs/CCA-Remediation-final.pdf

Evans, B. J., & Henry, G. (2015). *Self-paced remediation and math placement: A randomized field experiment in a community college*. Paper presented at the Society for Research on Educational Effectiveness Spring Conference, Washington, DC.

Fain, P. (2013, June 5). Remediation if you want it. *Inside Higher Ed*. Retrieved from \https://www.insidehighered.com/news/2013/06/05/florida-law-gives-students-and-colleges-flexibility-remediation

Figlio, D., Rush, M., & Yin, L. (2013). Is it live or is it Internet? Experimental estimates of the effects of online instruction on student learning. *Journal of Labor Economics*, *31*(4), 763–784.

Garcia, P. (1991). Summer bridge: Improving retention rates for underprepared students. *Journal of the Freshman Year Experience*, *3*(2), 91–105.

Greene, J. P., & Forster, G. (2003). *Public high school graduation and college readiness rates in the United States* (Education Working Paper No. 3). Retrieved from https://www.manhattan-institute.org/pdf/ewp_03.pdf

Hu, S., Park, T. J., Woods, C. S., Tandberg, D. D., Richard, K., & Hankerson, D. (2016). *Investigating developmental and college-level course enrollment and passing before and after Florida's developmental education reform* (REL 2017-203). Washington, DC: U.S. Department of Education, Institute of Education Sciences, National Center for Education Evaluation and Regional Assistance, Regional Educational Laboratory Southeast.

Jones, S., Sugar, T., Baumgardner, M., Raymond, D., Moore, W., Davidson, R., & Denham, K. (2012). *Remediation: Higher education's bridge to nowhere*. Retrieved from http://www.completecollege.org/docs/CCA-Remediation-final.pdf

Jordan, K. (2014). Initial trends in enrolment and completion of massive open online courses. *The International Review of Research in Open and Distributed Learning*, 15(1). Retrieved from http://www.irrodl.org/index.php/irrodl/article/download/1651/2813

Kokkelenberg, E. C., & Sinha, E. (2010). Who succeeds in STEM studies? An analysis of Binghamton University undergraduate students. *Economics of Education Review*, *29*(6), 935–946.

Logue, A. W., Watanabe-Rose, M., & Douglas, D. (2016). Should students assessed as needing remedial mathematics take college-level quantitative courses instead? A randomized controlled trial. *Educational Evaluation and Policy Analysis*, *38*(3), 578–598.

Long, B. T., & Boatman, A. (2013). The role of remedial and developmental courses in access and persistence. In L. W. Perna & A. P. Jones (Eds.), *The state of college access and completion: Improving college success for students from underrepresented groups* (pp. 77–95). New York, NY: Routledge.

Martinez, I. (2014). *The effects of nudges on students' effort and performance: Lessons from a MOOC* (EdPolicyWorks Working Paper). Retrieved from http://curry.virginia.edu/uploads/resourceLibrary/19_Martinez_Lessons_from_a_MOOC.pdf

Martorell, P., & McFarlin, L. J. (2011). Help or hindrance? The effects of college remediation on academic and labor market outcomes. *The Review of Economics and Statistics*, *93*(2), 436–454.

Means, B., Toyama, Y., Murphy, R., Bakia, M., & Jones, K. (2010). *Evaluation of evidence-based practices in online learning: A meta-analysis and review of online learning studies*. Retrieved from https://www2.ed.gov/rschstat/eval/tech/evidence-based-practices/finalreport.pdf

Myers, C., & Drevlow, S. (1982, March). *Summer bridge program: A dropout intervention program for minority and low-income students at the University of California, San Diego*. Paper presented at the annual meeting of the American Education Research Association, New York, NY.

National Center for Education Statistics. (2014). *Digest of Education Statistics for 2014. Table 311.4*. Retrieved from https://nces.ed.gov/pubs2016/2016006.pdf

Page, L. C., & Scott-Clayton, J. (2016). Improving college access in the United States: Barriers and policy responses. *Economics of Education Review*, *51*, 4–22.
Santa Rita, E., & Bacote, J. (1997). The benefits of college discovery prefreshman summer program for minority and low-income students. *College Student Journal*, *36*, 161–173.
Scott-Clayton, J., Crosta, P. M., & Belfield, C. (2014). Improving the targeting of treatment: Evidence from college remediation. *Education Evaluation and Policy Analysis*, *36*(3), 371–393.
Scott-Clayton, J., & Rodriguez, O. (2015). Development, discouragement, or diversion? New evidence on the effects of college remediation policy. *Education Finance and Policy*, *10*(1), 4–45.
Tan, N. (1985). *The SEEK Prefreshman Summer Program at Baruch College, 1984: A followup study*. New York: City University of New York, Bernard Baruch College, Department of Compensatory Programs.
Thaler, R. H., & Sunstein, C. R. (2008). *Nudge: Improving decisions about health, wealth, and happiness*. New Haven, CT: Yale University Press.
Van Dyken, M. J., Benson, L., & Gerard, P. (2015, June). *Persistence in engineering: Does initial mathematics course matter?* Paper presented at the 122nd American Society for Engineering Education Annual Conference and Exposition, Seattle, WA.
Xu, D., & Jaggars, S. S. (2013). The impact of online learning on students' course outcomes: Evidence from a large community and technical college system. *Economics of Education Review*, *37*, 47–56.

Appendix A: Tables With Robustness Checks

Table A1. Results with probit models.

	UMBC		Towson University		Bowie State University	
	Treatment effect	*SE*	Treatment effect	*SE*	Treatment effect	*SE*
Retook placement test	0.13***	(0.03)	0.24***	(0.06)	−0.25***	(0.08)
Took any math course	0.01	(0.03)	0.00	(0.03)	0.07	(0.06)
Passed any math course	0.00	(0.03)	0.03	(0.07)	0.07	(0.08)
Took any STEM course	−0.03	(0.02)	0.01	(0.03)		
Passed any STEM course	−0.04	(0.03)	0.01	(0.06)		
Number of students	697		155		116	

Notes. Marginal effects and Delta-method standard errors are reported.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level. Missing data is set to the mean and dummy variables are included to indicate individuals missing data for a particular variable.

Table A2. UMBC differences in learning outcomes by treatment and initial placement scores.

	Placement score (pretest): Without covariates														
	All students			0 to 7			8 to 9			10 to 13			14 to 16		
	Control mean	Treatment effect	*SE*	Control mean	Treatment effect	*SE*	Control mean	Treatment effect	*SE*	Control mean	Treatment effect	*SE*	Control mean	Treatment effect	*SE*
Retook placement test	0.10	0.13***	(0.03)	0.3	−0.00	(0.11)	0.12	0.20*	(0.11)	0.09	0.11**	(0.05)	0.08	0.15***	(0.04)
High placement score	13.06	0.68**	(0.28)	8.4	−0.02	(1.22)	9.00	2.07**	(0.98)	12.10	1.05***	(0.35)	15.353	0.43**	(0.19)
GPA, all graded courses	2.78	0.03	(0.06)	2.47	0.12	(0.22)	2.61	0.32*	(0.18)	2.78	0.11	(0.09)	2.87	−0.08	(0.08)
GPA, math courses	2.38	−0.02	(0.08)	1.7	−0.13	(0.28)	1.95	0.54**	(0.23)	2.31	0.06	(0.14)	2.61	−0.13	(0.12)
Passed any math course	0.75	−0.00	(0.03)	0.47	0.05	(0.12)	0.58	0.21*	(0.12)	0.81	−0.08	(0.05)	0.79	0.01	(0.04)
Started above algebra	0.77	0.02	(0.03)	0.37	0.09	(0.12)	0.42	0.18	(0.13)	0.84	−0.05	(0.05)	0.86	0.04	(0.04)
Passed above algebra	0.69	−0.00	(0.04)	0.33	−0.01	(0.12)	0.39	0.07	(0.13)	0.73	−0.02	(0.06)	0.79	0.01	(0.04)
Started 200 level	0.14	−0.02	(0.03)	0.00	0.03	(0.03)	0.06	0.01	(0.07)	0.13	−0.01	(0.04)	0.20	−0.04	(0.04)
Passed 200 level	0.13	−0.03	(0.02)	0.00	0.03	(0.03)	0.06	0.01	(0.07)	0.11	−0.01	(0.04)	0.17	−0.05	(0.04)
Number		697			67			61			234			335	

(Continued on next page)

Table A2. *(Continued)*

	Placement score (pretest): With covariates									
	All students		0 to 7		8 to 9		10 to 13		14 to 16	
	Treatment effect	*SE*	Treatment effect	*SE*	Treatment effect	*SE*	Treatment effect	*SE*	Treatment effect	*SE*
Retook placement test	0.13***	(0.03)	−0.07	(0.14)	0.14	(0.10)	0.10**	(0.05)	0.17***	(0.04)
High placement score	0.80***	(0.26)	−0.07	(1.50)	1.66	(1.10)	0.98***	(0.35)	0.51***	(0.18)
GPA, all graded courses	−0.07	(0.14)	−0.13	(0.24)	0.36**	(0.16)	0.16*	(0.09)	−0.06	(0.07)
GPA, math courses	−0.07	(1.50)	−0.54*	(0.32)	0.62**	(0.24)	0.10	(0.13)	−0.10	(0.11)
Passed any math course	−0.01	(0.03)	−0.14	(0.13)	0.25**	(0.10)	−0.08	(0.05)	0.01	(0.04)
Started above algebra	0.02	(0.03)	−0.03	(0.13)	0.16	(0.14)	−0.06	(0.05)	0.03	(0.03)
Passed above algebra	−0.00	(0.03)	−0.13	(0.11)	0.04	(0.15)	−0.02	(0.06)	0.01	(0.04)
Started 200 level	−0.01	(0.03)	0.02	(0.02)	−0.04	(0.08)	−0.02	(0.04)	−0.02	(0.04)
Passed 200 level	−0.02	(0.02)	0.02	(0.02)	−0.04	(0.08)	−0.02	(0.04)	−0.03	(0.04)
Number	697		67		61		234		335	

Notes. Robust standard errors are reported.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level. The reported treatment effects are from regressions with covariates. Covariates include gender, age, SAT math, SAT verbal, HS GPA, race (Black, Hispanic, Asian, White, or other), and income (less than \$50,000, \$50,000−\$100,000, or more than \$100,000). Missing data is set to the mean and dummy variables are included to indicate individuals missing data for a particular variable.

Appendix B: Results From Towson University and Bowie State University

At Towson University, 155 students were involved in the study: 90 in the treatment group and 65 in the control group.[20] Of those who were invited to participate, 49% acknowledged that they intended to utilize the software, and 82% of these students logged onto MFL at least once. As with UMBC, students who were invited to participate in the program were more likely to retake placement tests and to improve their scores. Thirty percent of treatment group students retook the placement test, compared to 6% of control group students, coinciding with significantly higher gains in placement test scores (see Table B1). Moreover, of students who did retake the placement test, those who had access to MFL did much better: these students raised their average scores by 8.2 points, while the very small number (4) of students in the control group who retook the placement test got lower scores by 5.3 points, on average. Looking at all students' maximum placement test scores, we see that the average treatment effect on score improvement is 6.4, which is significant at the 10% level (Table B2).[21] As at UMBC, students who received the intervention did not have better academic outcomes in math courses.

At Bowie State University, students were invited to participate in the study before they were assigned to treatment or control groups. Out of 543 eligible students, 116 consented to participate in the study (21%). These 116 students were then randomly divided into treatment and control groups. Out of 58 students in the treatment group, 44 logged into MFL at least once (76%). Gaining access to MFL did not, however, increase the likelihood that study participants retook the placement test. The share of treatment group students who opted to retake the placement test is significantly lower than that of the control group (36% compared to 62%). Moreover, Table B1 shows that the average score gain for students who had access to MFL was smaller than for those who did not, though this difference is not statistically significant (3.3 points for the control group vs. 1.8 points for the treatment group).[22]

Table B2 indicates that controlling for student demographic variables yields qualitatively similar results. However, as mentioned earlier, the substantial missing data for students in

Table B1. Detail on students who retook the placement test at Towson University and Bowie State University.

	Towson University		Bowie State University	
	Control	Treatment	Control	Treatment
Retake rate	6%	30%	62%	36%
Did not retake				
Pretest score	61.1	64.1	20.7	19.3
Retook				
Pretest score	63.8	65.9	17.4	18.6
Posttest score	58.5	74	22.6	23.2
Max (pre, post)	71	77.4	22.8	23.8
Change (post–pre)	−5.3	8.2	5.1	4.7

[20] The imbalance between the size of the treatment and control groups was the result of campus administrator preferences.

[21] We focus on this measure instead of the final pretest score because so few students in the control group retook the placement test and placements were determined by students' highest placement test score, not their most recent one.

[22] The average effect is smaller than what is reported in Table B2 because students who do not retake the placement test are recorded as having a score gain of 0.

Table B2. Treatment effect estimates, Towson University and Bowie State University.

	Towson University					Bowie State University				
		Without covariates		With covariates			Without covariates		With covariates	
	Control mean	Treatment effect	*SE*	Treatment effect	*SE*	Control mean	Treatment effect	*SE*	Treatment effect	*SE*
Retook placement test	0.06	0.24***	(0.06)	0.27***	(0.07)	0.62	−0.26***	(0.09)	−0.33***	(0.09)
Max (pretest, posttest)	61.71	6.37*	(3.76)	6.74*	(3.66)	22.03	−1.09	(1.10)	−1.40	(1.10)
GPA, all graded courses	2.18	0.09	(0.14)	−0.15	(0.16)	2.86	−0.03	(0.13)	−0.03	(0.14)
GPA, math courses	2.04	0.07	(0.19)	−0.20	(0.20)	2.34	−0.11	(0.22)	−0.37*	(0.22)
Took any math course	0.95	0.00	(0.03)	0.07	(0.04)	0.83	0.07	(0.06)	0.06	(0.07)
Passed any math course	0.79	0.03	(0.07)	0.11	(0.07)	0.72	0.07	(0.08)	0.04	(0.08)
Took any STEM course	0.95	0.01	(0.03)	0.07	(0.04)					
Passed any STEM course	0.85	0.01	(0.06)	0.09	(0.07)					
Number of students		155		155			116		116	

Notes. Robust standard errors are reported.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level. Missing data is set to the mean, and dummy variables are included to indicate individuals missing data for a particular variable. For Towson University, covariates include gender, age, SAT math, SAT verbal, race (Black, Hispanic, Asian, White, or other), and income (less than $50,000, $50,000–$100,000, or more than $100,000). For Bowie State University, covariates include gender, age, SAT math, SAT verbal, HS GPA, race (Black, Hispanic, Asian, White, or other), and an indicator if the student received a Pell Grant. The reported control mean is the constant in the regression with no covariates. Bowie State University did not provide information on STEM courses other than math courses.

the treatment group at Towson University is concerning. Probit regressions for the binary outcomes also confirm the results found using OLS (Table A1). The consistency of these results across multiple specifications adds weight to our initial finding that this intervention had no significant impacts on students' performance in their first-year courses. The consistency across UMBC and Towson University also suggests that the intervention encouraged students to retake the placement test, raising their scores.

One possible explanation for the difference in findings between Bowie State University and the other two institutions is the randomization procedure. At UMBC and Towson University, students were randomly assigned to treatment or control conditions without first obtaining their consent (though students at UMBC could opt out of the study). As a result, we were able to achieve a larger study population and produce results with greater external validity. At Bowie State University, on the other hand, students had to opt into the study before they could be assigned to treatment or control groups, and students were offered the opportunity to retake the placement test and keep the best score as an incentive for participation. It is therefore reasonable to hypothesize that students who opted into the study were more motivated than those who did not to improve their math skills or placement score, and thus are not representative of nonparticipants. There is evidence of this bias in data showing how many students actually logged into MFL—76% of the treatment cohort at Bowie State University compared with only 36% at UMBC and 40% at Towson University.

This hypothesis does not explain why control group students were much more likely to retake the placement test at Bowie State University and made slightly larger gains. It is possible that these students found alternate (and more effective) ways to prepare for the exam. The Institutional Review Board at Bowie State University required that students in the control group had access to static resources on their institution's learning management site. It is also possible that students score higher on a retake even when they do not prepare for it, and thus the larger retake rate among the control group led to a larger score increase (remembering that the score gain for students who do not retake the placement test is recorded as zero in our analysis in order to allow us to compare mean outcomes for all students in the treatment and control groups). Table B1 shows that if students who did not retake the placement test are removed from the analysis, we find that students in both treatment and control groups increased their scores by about 5 points (similar to what we found at UMBC). This suggests that MFL may have neither hurt nor helped students in the treatment group with respect to their math skills.