Should Students Assessed as Needing Remedial Mathematics Take
College-Level Quantitative Courses Instead?
A Randomized Controlled Trial

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Abstract

Many college students never take, or do not pass, required remedial mathematics courses theorized to increase college-level performance. Some colleges and states are therefore instituting policies allowing students to take college-level courses without first taking remedial courses. However, no experiments have compared the effectiveness of these approaches, and other data are mixed. We randomly assigned 907 students to (a) remedial elementary algebra, (b) that course with workshops, or (c) college-level statistics with workshops. Students assigned to statistics passed at a rate 16 percentage points higher than those assigned to algebra ($p < .001$), and subsequently accumulated more credits. A majority of enrolled statistics students passed. Policies allowing students to take college-level instead of remedial quantitative courses can increase student success.

Keywords: higher education, remediation, mathematics, randomized controlled trial
Should Students Assessed as Needing Remedial Mathematics Take College-Level Quantitative Courses Instead?

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Colleges in the United States assess a total of about 60% of their new freshmen as unprepared for college-level work (Grubb et al., 2011), most often in mathematics (Attewell, Lavin, Domina, & Levey, 2006). College policies usually require such students to complete remedial courses prior to taking college-level courses in the remedial courses’ disciplines, based on the purported theory that students need to pass the remedial courses in order to be able to pass the college-level courses. However, the percentage of students successfully completing remedial courses is low (Bailey, Jeong, & Cho, 2010). For example, at The City University of New York (CUNY) in fall 2014, 76% of new community college freshmen were assessed as needing remedial mathematics (CUNY, 2015a), and the pass rate in the highest-level remedial mathematics course across the community colleges was 38% (CUNY, 2015b). Further, at CUNY and nationally, many students, though assigned to remedial courses, wait to take them or never take them, delaying or preventing graduation (Bailey et al., 2010). It is therefore not surprising that students who enter college needing any remedial courses are less likely to graduate than are students who enter college with no such need (9% vs. 43% after three years at CUNY for students who enter in associate-degree programs; CUNY, 2014). Successful completion of mathematics remediation may be the single largest barrier to increasing graduation rates (Attewell et al., 2006; Complete College America, 2012).

Addressing the low pass rates in remedial mathematics courses could not only help overall graduation rates, it could also help close performance gaps. Students assessed as needing remediation are more likely to be members of underrepresented groups (Attewell et al., 2006). Therefore low mathematics remediation pass rates contribute to the lower college attainment rates of members of underrepresented groups.

Various solutions to the low remedial course pass rates have been proposed at CUNY and nationwide. One alternative is having students with remedial needs address them in the summer before entering college. Although there is research supporting this type of approach (Authors, 2014), a randomized controlled trial
found only modest positive effects in the first year following the summer program, and these positive effects did not persist to the second year (Barnett et al., 2012). Also, not all students can attend remedial courses the summer before college.

Another example is the CUNY Start program, in which new college students with multiple remedial needs postpone matriculation for one semester while engaging in full-time remediation. However, this program is only for students with severe remedial needs, not every student can devote an entire semester to remediation, and though its initial results are promising, there has not yet been an experiment evaluating it (Office of Academic Affairs, 2013).

The Carnegie Foundation for the Advancement of Teaching has promoted the use of Statway, which combines remedial mathematics with introductory statistics (Sowers, Strother, & Van Campen, 2013). A recent rigorous analysis supports Statway as increasing student success (Yamada, 2014). However, Statway can require a full academic year for credits for one college-level course, and requires students to know much of elementary algebra. Further, the effects on enrollment of students being assigned to such a course are unknown.

Alternatively, some practitioners have advocated streamlining the remedial math curriculum so that students learn only the remedial mathematics that they need for subsequent courses. However, only descriptive data are available for evaluating such approaches (Kalamkarian, Raufman, & Edgecombe, 2015).

As a form of streamlining, some colleges and states are instituting policies in which students assessed as needing remedial courses take college-level courses such as statistics instead, sometimes with additional academic support (see, e.g., Hern, 2012; Smith, 2015). Several theories have been suggested regarding why such approaches should be effective in increasing student success. First, at least some students assessed as needing remediation should perform satisfactorily in college-level courses because placement tests and other placement mechanisms are sometimes inaccurate, judging some students as needing remediation even though their skills are sufficient for college-level work (Scott-Clayton, Crosta, & Belfield, 2014). Second, assigning a student to a remedial course may decrease that student’s motivation due to college graduation being more distant, and/or because the student already had an unpleasant experience with this course in high school, and/or
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because of the stigma of being required to take a remedial course (see, e.g., Author, 1995; Bailey, 2009; Complete College America, 2011; Goldrick-Rab, 2007; Scott-Clayton & Rodriguez, 2012). Third, it has been proposed that students can pass college-level statistics more easily than remedial algebra because the former is less abstract and uses everyday examples (Burdman, 2013; Yamada, 2014).

There have been multiple attempts to compare the performance of students, assessed as needing remediation, who enroll first in remedial courses with the performance of students who enroll directly in college-level courses. Some of this research has used data obtained from naturally occurring variation in student course placement, and some has used quasi-experimental methods such as propensity score matching and regression discontinuity. Results have been mixed. Some studies have found that students assessed as needing remediation perform better in college-level courses if they first take remedial courses (e.g., Bettinger & Long, 2009; Moss, Yeaton, & Lloyd, 2014). Others have found that such students do just as well or better in completing college if they skip remediation (e.g., Boatman, 2012; Calcagno & Long, 2008; Clotfelter, Ladd, Muschkin, & Vigdor, 2015; Jaggars, Hodara, Cho, & Xu, 2015; Martorell & McFarlin, 2011). Still others have found both types of results (e.g., Melguizo, Bos, & Prather, 2011; Wolfle & Williams, 2014).

The term mainstreaming has been used to describe placing students assessed as needing remediation directly into a college-level course (see, e.g., Edgecombe, 2011; such students are not necessarily mixed within the classroom with other students, as occurs with mainstreaming in K-12 education). There have been several apparently successful programs for mainstreaming college students assessed as needing remediation, sometimes with additional instructional support (e.g., an English program at Community College Baltimore County, and a mathematics program at Austin Peay State University; Jones, 2014).

The concern with all of these studies is that, because none of them have used experimental methods (i.e., randomized controlled trials), there could have been uncontrolled, unmeasured differences in some variables across the groups of students exposed to different treatments (as in some propensity score matching studies), and/or the findings could be limited to a narrow range of students (as in some regression discontinuity studies). For example, student motivation, which is difficult to measure, may be affected by being labeled a remedial
SHOULD STUDENTS ASSESSED AS NEEDING REMEDIAL student, and so may vary across groups of students who are not randomly assigned to remedial and college-level courses. Such differences could help explain the inconsistent results across studies.

The purpose of the present research was therefore to use a randomized controlled trial to examine a promising approach for increasing student success: mainstreaming. The experiment compared academic performance (pass rates) in remedial elementary algebra with a college-level course (statistics) for students assessed as needing remedial elementary algebra. Most (56%) of the students who actually took the college-level course (statistics) passed that course, and students assigned to statistics passed at a rate that was 16 percentage points greater than students assigned to elementary algebra. Students do not first have to pass remedial mathematics in order to pass college-level statistics, and policies placing students assessed as needing remedial mathematics directly into college-level quantitative courses can increase student success.

**Design of Present Research**

For purposes of sample size and generalizability, we conducted the experiment at three CUNY community colleges (Colleges A, B, and C), one each in the boroughs of the Bronx, Manhattan, and Queens. At all three colleges, we randomly assigned students assessed as needing remedial elementary algebra to one of three fall 2013 course types: (a) traditional, remedial, noncredit, elementary algebra (Group EA), (b) that course with weekly workshops (Group EA-WS), or (c) college-level, credit-bearing statistics with weekly workshops similar to Group EA-WS’s (Group Stat-WS).

Additional academic support has been termed supplemental or corequisite instruction (Bueschel, 2009; Complete College America, 2012). The present experiment used it for three reasons: (1) Evidence suggests that such support tends to increase students’ grades (see, e.g., Bettinger & Baker, 2014; Bowles, McCoy, & Bates, 2008), (2) CUNY policy requires that students assessed as needing remediation be provided with an intervention addressing that need, and (3) the additional support helped allay concerns that placing students assessed as needing remedial elementary algebra directly into college-level statistics with no additional support would result in even lower pass rates than those for elementary algebra.

These three groups allowed us to examine: (1) the effects of adding workshops to elementary algebra by
should students assessed as needing remedial comparing groups ea and ea-ws (we could not assess the effects of adding workshops to statistics given that we could not offer statistics without workshops); (2) the effects of exposing students to statistics as opposed to elementary algebra, each with workshops (by comparing groups ea-ws and stat-ws); and (3) the effects of placing students into statistics with workshops as compared to a traditional remedial course (by comparing groups ea and stat-ws). we could also compare the performance of the three experimental groups with the performance of all students taking elementary algebra and statistics in fall 2012, allowing us to compare our students’ performance with typical norms.

we hypothesized that the ea group would pass at the typical elementary algebra rate (fall 2012, 37%), that the ea-ws group would pass at a higher rate due to the positive effects of the workshops, and that the stat-ws group would pass at a rate at least as high as the ea group, although lower than the typical rate for statistics (fall 2012, 69%; because the stat-ws students would be taking a college-level quantitative course without the assumed benefits of first taking elementary algebra, but with the benefits of the workshops and of being assigned to a college-level course).

**participant recruitment**

during the summer prior to the fall 2013 semester, all eligible students at each participating college were notified of the research via email and during in-person orientation sessions for new students. at the orientation sessions, potential participants were given a flyer and a consent form stating the requirements for study participation (appendices a and b contain the text of college a’s flyer and consent form): minimum age 18, first-time freshman, intending to major in certain disciplines (majors that did not require college algebra), and assessed as needing elementary algebra. participants could obtain a $40 metrocard for new york city public transportation if they were enrolled in their assigned research sections after the end of the course drop period (73% of participants retrieved them), and a $10 metrocard after the semester ended (35% retrieved them). we instructed recruiters to be neutral when describing the different treatment conditions to potential participants. during the experiment there were anecdotal reports that the stat-ws students felt fortunate to have been randomly assigned to that group.
A power analysis indicated that to detect a pass rate of 37% in the Stat-WS group (compared to the traditional rate of 69%) with 95% power would require at least 60 students per group. Given the possibility of significant attrition among the recruited students, and that we intended to conduct subgroup analyses, we aimed to recruit a total of approximately 900 students.

A total of 907 eligible students consented to the experiment. As soon as the consent form was signed, research personnel randomly assigned these students to one of the three course types (Groups EA, EA-WS, and Stat-WS) using random number tables created with MS Excel, and informed students of their assignments, including their course sections. Recruitment took place during the three months before the start of the semester. As of the official course census date (approximately two weeks after the start of the semester, the day after the end of the drop period), 717 of these consenting students were enrolled in their assigned research sections and were designated the experiment’s participants. Figure 1, Table 1, and Table 2 provide information about all of the students involved in the experiment.

Figure 1 shows the flow of target students through each stage of the experiment. There was an overall attrition rate of 21.0% (190 students) between when students were randomized and the semester’s course census date. As detailed in Table 3, attrition was significantly higher in Group EA-WS than in Groups EA or Stat-WS. A Tukey post-hoc test comparing attrition in Groups EA and Stat-WS was not significant, but tests comparing attrition between Group EA-WS with Groups EA and Stat-WS were significant \( p = .010 \) and \( p = .005 \), respectively. In contrast, there were no significant differences among the three groups in the percentages of students who withdrew during the semester. The relatively large attrition in Group EA-WS meant that we needed to consider the possibility that, although students were randomly assigned to Group EA-WS, the actual Group EA-WS participants did not constitute a random sample of those who consented. We therefore focused on comparisons between Groups EA and Stat-WS. However, note that, as indicated by Figure 1 and Table 3, the attrition among the EA-WS students (27.5%) was nevertheless less than the percentage of nonconsenting students who, although assigned to elementary algebra, did not take it (39.6%; because they never enrolled at CUNY, because their mathematics placement level changed, because they did not attend orientation, or because
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they avoided taking elementary algebra).

Of the 190 students who signed the consent form but who were not enrolled in their research sections on the fall 2013 census date ("noncompliers"), 110 were not enrolled in any college—CUNY or nonCUNY—that semester (National Student Clearinghouse data; an example of what has been called “summer melt,” Castleman & Page, 2014). Consistent with the attrition data reported earlier, the largest proportion of these 110 students consisted of students who had been randomly assigned to Group EA-WS (of 86 students randomized to that group but who did not start that course, 50 did not attend any college in the experiment’s semester).

A total of 34 noncompliers across the three groups enrolled in nonresearch sections of elementary algebra in the fall of the experiment. No student assigned to a research section attempted to attend a different research section. Although only research participants were supposed to enroll in research sections, five nonresearch students enrolled in research sections (four total in three EA-WS sections, and one in a Stat-WS section). We excluded these five students from all analyses.

Table 1 shows the variables for which we had data for both the 717 participants and the 190 noncompliers. There were no significant differences between these two groups except that, on average, noncompliers agreed to participate in the experiment significantly earlier than participants. These results are consistent with previous findings that students who agree early to participate in research are less likely to participate. Early-consenting students may be more likely to encounter work or other time conflicts with scheduled research (Authors, 2008).

To examine whether the students who participated in the treatments were representative of all students assessed as needing elementary algebra, we also compared participants with nonconsenters who took nonresearch sections of elementary algebra during the same semester as the experiment (60% of all nonconsenters). The two groups did not significantly differ on any of the variables for which we had data (age, placement test scores, gender, and high school GPA. However, they may have differed on other variables given that one group consented to be in an experiment, one which involved a class taught during the day, and the other did not.
Research personnel selected the course sections in which the participants would enroll and recruited the faculty. There were 12 instructors, four at each of the three colleges. The instructors had to be full-time, willing to teach two sections of elementary algebra and one of introductory statistics in fall 2013, and, preferably, have taught both subjects before (three of the 12 instructors had only taught elementary algebra before). In order to be able to assess instructor effects and to balance these effects across treatments, each instructor taught one section of each of the three course types: EA, EA-WS, and Stat-WS (Weiss, 2010). Thus there were 12 sections each of EA, EA-WS, and Stat-WS. This meant that the faculty had to be informed about the basic structure of the experiment, including during a 6-hour orientation session that they attended prior to the experiment (Appendix C provides an example of a faculty orientation agenda). The faculty were told that the researchers believed that “at least some students assessed as needing elementary algebra will successfully pass statistics without taking elementary algebra.” Faculty were not given the experiment’s research hypothesis and were never told that the researchers hoped that statistics would have at least the same pass rate as elementary algebra.

The faculty were actively involved in ensuring that the research was conducted properly. For example, the faculty at each college worked together to insure that all research sections of statistics at their college used the same syllabus (there was already a departmental common syllabus for elementary algebra at each college). Each instructor also met monthly with research personnel and weekly with the workshop leaders of that instructor’s two sections that included workshops. During the weekly sessions, the instructors gave their workshop leaders assignments and exercises for the participants to work on during the workshops and as homework. Research personnel told the instructors to teach and grade the research sections as they would ordinarily. Each instructor was paid $3,000 for his/her participation.

Research personnel recruited the workshop leaders. Qualifications included advanced undergraduate status at or recent graduation from CUNY, successful completion of the material to be covered in the leader’s workshops, a recommendation from a mathematics faculty member, and a satisfactory personal interview. A
total of 21 workshop leaders were selected for the 24 research sections that had associated weekly workshops (three workshop leaders each led the workshops for two sections). They were paid at the rate of $14 per hour. Before the experiment began, the workshop leaders met as a group with research personnel for 10 hours to learn about the experiment and to discuss how to conduct their workshops. During the experiment’s semester, the workshop leaders met monthly with research personnel and discussed together on social media their concerns and suggestions about conducting their workshops. Workshop leaders also attended their section’s regular class meetings.

Section size did not vary significantly by group (means and 95% CIs: Group EA 20.3 [17.5,23.2], Group EA-WS 18.9 [16.2,21.7], Group Stat-WS 20.5 [18.7,22.3], \( F[2,33] = 0.58, p = .56 \)). Elementary algebra sections and any associated workshops covered topics such as linear equations, exponents, polynomials, and quadratic equations (Appendix D provides a sample syllabus). Statistics sections and associated workshops covered topics such as probability, binomial probability distributions, normal distributions, confidence intervals, and hypothesis testing (Appendix E provides a sample syllabus). If students in statistics sections needed to review certain algebra concepts in order to understand a particular statistics topic, such as using variables in equations and different types of graphs, the workshop leader would cover that topic in the workshop. Course sections lasted three to six hours per week, depending on the college.

All workshops occurred weekly, lasted two hours each, and had the same structure: 10-15 minutes of reflection by students on what they had learned recently in class and what they had found difficult, then approximately 100 minutes of individual and group work on topics students had found difficult, and a final five minutes of reflection by students on the workshop’s activities and whether the students’ difficulties had been addressed. Research personnel informed all students enrolled in research sections with workshops that they were required to attend the workshops, and that if they missed more than three they would have to meet with the instructor. Only students in EA-WS and Stat-WS sections could attend those sections’ workshops.

At the end of the semester, EA and EA-WS participants took the required CUNY-wide elementary algebra final examination and received a final grade based on the CUNY-wide elementary algebra final grade
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rubric. Instructors graded their Stat-WS participants at their discretion using the common syllabus for that college. All outcomes other than a passing grade, including any type of withdrawal or a grade of incomplete, were categorized as not passing.

All participants who passed were exempt from any further remedial mathematics courses and were eligible to enroll in introductory, college-level, quantitative courses and, in the case of Stat-WS participants, to enroll in courses for which introductory statistics is the prerequisite. A passing grade in Statistics satisfied the quantitative category of the CUNY general education curriculum. Participants who did not pass had to enroll in traditional remedial elementary algebra and pass it before taking any college-level quantitative courses. Stat-WS participants were informed that if they did not pass, a failing grade would not be included in their GPAs.

To check course progress, research personnel observed three regular class meetings of each section, as well as at least three workshops for each section of Groups EA-WS and Stat-WS. Sections were one or two weeks behind the syllabus in 26% of the class meetings and 27% of the workshops observed. In such situations, research personnel reminded the relevant instructor or workshop leader to follow the syllabus as consistently as possible.

Participants completed a mathematics attitude survey at the semester’s start and end (based on Korey, 2000), and a student satisfaction survey at the semester’s end. These pencil-and-paper surveys primarily consisted of 7-point Likert scales. The mathematics attitude survey consisted of 17 questions covering the following four domains: perceived mathematical ability and confidence (“Ability”), interest and enjoyment in mathematics (“Interest”), the belief that mathematics contributes to personal growth (“Growth”), and the belief that mathematics contributes to career success and utility (“Utility”). The student satisfaction survey asked about a student’s activities during the semester, e.g. whether the student had gone for tutoring (available to all students independent of the experiment), and about a student’s satisfaction with those activities.

**Method of Analysis for Treatment Effects**

Given that the treatments were randomly assigned, simple comparisons of course outcomes for all 907 students randomized to the three groups can identify the relative treatment effects. Intent-to-treat (ITT) analysis
compares mean outcomes of groups as randomized, without regard to attrition and other forms of deviation from protocol, thus providing an unbiased estimate of treatment. We compared our two treatment groups, EA-WS and Stat-WS, with Group EA. We estimated the ITT effect using Equation 1:

\[
\ln \left( \frac{\hat{p}}{1-\hat{p}} \right)_i = \delta + \beta_1 \times \text{STATS}_i + \beta_2 \times \text{EAWORK}_i + \epsilon_i ,
\]

in which \( \ln \left( \frac{\hat{p}}{1-\hat{p}} \right)_i \) is the log odds of a positive outcome for student \( i \), \( \delta \) is the equation constant, STATS represents whether the student was randomized into group Stat-WS, EAWORK whether the student was randomized into group EA-WS, \( \beta_1 \) and \( \beta_2 \) are coefficients, and \( \epsilon_i \) is an error term. The outcome of interest is whether the student passed elementary algebra (or statistics if the student was assigned to statistics and participated).

We also fit a model that included a set of fixed effects (algebra placement test score, gender, high school GPA, number of days to consent, and controls for missing values). This vector of covariates is represented by \( X \) in Equation 2:

\[
\ln \left( \frac{\hat{p}}{1-\hat{p}} \right)_i = \delta + \beta_1 \times \text{STATS}_i + \beta_2 \times \text{EAWORK}_i + bX_i + \epsilon_i ,
\]

with terms defined as in Equation 1 plus addition of the coefficient \( b \). We did not include the prealgebra (arithmetic) placement score as a covariate because it did not add any explanatory power. We incorporated additional control variables in a subsequent analysis of the 717 participants (see Table 4), but among all students randomized we have only a limited set of covariates.

Given attrition varied by group, we also determined estimates of the effect of treatment on the treated (Treatment on Compliers, or TOC) by using Angrist, Imbens, and Rubin’s (1996) instrumental variables approach. Our design meets the assumptions necessary for this approach because (a) we randomized students into groups, (b) random assignment was highly correlated with receiving treatment, and (c) those assigned to the control group (Group EA) had no ability to enroll in a different group. Instrumental variables analysis has two steps: regressing random assignment on the actual receipt of the treatment, then using the predicted values from the first step in a second regression model predicting outcome variables (here, passing the assigned course). We
Results of Analysis for Treatment Effects

ITT and TOC

Table 5 reports the results using both ITT and both TOC models. The ITT estimates with no covariates (Equation 1) show that students in Group EA-WS were not significantly more likely to pass than those in Group EA ($p = .48$). Those in Group Stat-WS were significantly more likely to pass than those in Group EA by a margin of 16 percentage points, and than those in Group EA-WS by 13 percentage points. When we add covariates to the ITT equation (Equation 2), there is again no significant difference between groups EA and EA-WS ($p = .14$), but students in the Stat-WS group were significantly more likely to pass than EA students by 14 percentage points and than EA-WS students by 11 percentage points. TOC estimates show similar results.

Course Success Among Participants

Figure 2 shows the overall pass rates for each of the three groups of participants (EA, EA-WS, and Stat-WS) and compares them to the historical pass rates for these courses in fall 2012. The pass rate for Group EA-WS (44.9%), which was 5.6 percentage points higher than that of Group EA (39.3%), is also higher than that of students who took elementary algebra at the three colleges in fall 2012 (36.8%), and this difference is significant (95% CI for the difference in pass rates [1.7, 14.5], $t[5798] = 2.5, p = .013$). In contrast, the difference between the pass rate for Group EA and the pass rate for students who took elementary algebra in fall 2012 is not statistically significant (95% CI for the difference in pass rates [-3.7, 8.7], $t[5815] = 0.8, p = .42$). Group Stat-WS passed at a significantly lower rate (55.7%) than did students who took introductory statistics at the three colleges in fall 2012 (69.0%; 95% CI of the difference [-19.3,-7.3]; $t[4393] = 2.14, p < .001$). However, as demonstrated in Figure 2, if the Group Stat-WS sample is restricted to participants who received relatively high scores on the placement test, the mean pass rate (67.6%) is not significantly different from that of the previous year’s statistics students (69.0%; 95% CI of the difference [-10.3,7.6]; $t[4252] = 0.30, p = .762$). Colleges can place into statistics students just below the cut off for elementary algebra without any diminution in the typical statistics pass rate.
Having established a significant effect of the treatment using the ITT and TOC analyses, we utilized logistic regression to further investigate predictors of participants passing assigned courses. The main independent variable was a set of dummy variables indicating treatment status, with Group EA as the omitted reference group. For ease of interpretation, Table 4 reports the results of the logistic regression as average marginal effects rather than as odds-ratios. The largest effect size was that of the treatment—being placed in Group Stat-WS. As noted above, we find no significant difference between Group EA and Group EA-WS in the probability of passing ($p = .097$), but the difference between Groups EA-WS and Stat-WS is significant ($p = .031$), as is the difference between Groups EA and Stat-WS ($p < .001$). When controlling for all other variables, there is a significant difference of almost 17 percentage points between students in Group Stat-WS and Group EA. Being enrolled in Stat-WS increases students’ probability of passing more than the increase associated with a one-standard-deviation increase in the Compass algebra score. Some covariates showed significant effects: Students with higher algebra placement scores and higher high school GPAs were more likely to pass, and students whose first language was English were less likely to pass.

Given that the purpose of placement tests is to place students in courses in which they are most likely to be successful, the widespread use of these tests, and their significant cost (Rodríguez, Bowden, Belfield, & Scott-Clayton, 2015), we particularly examined the relationship between placement test score and passing. Table 4 shows that algebra placement test z-score is a strong predictor of passing with all participants combined. Figure 3 shows, for each group separately, the effect of these scores on passing. For this analysis we focused only on those participants who had placement scores, and controlled for the same covariates included in the logistic regression model reported above in Table 4. Given that the line for Group Stat-WS is higher than those for the other two groups in all cases, students with any placement test algebra score are more likely to pass if they enroll in introductory statistics with a weekly workshop as opposed to elementary algebra. It should also be noted that, in this sample, even students with average placement test scores have a better than 50% chance of passing statistics with a weekly workshop. Finally, the fact that the line for Group EA-WS is consistently higher than the line for Group EA supports the hypothesis that the workshops did help students to
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pass elementary algebra.

Additional Effects

Neither an instructor’s tenure status nor experience was significant in the logistic regression. To test further instructors’ impact on course outcomes, we used a mixed-effects logit regression model with instructor as the random effect. The results showed that instructor assignment affected students’ probability of passing. However, there was still an effect of treatment group, again indicating that, across classrooms and instructors, Stat-WS students were more likely to pass. A log-likelihood test comparing a standard logistic regression with the mixed-effects model showed the latter to fit the model significantly better, $\chi^2(1) = 8.01$, $p = .002$.

Being enrolled in Group Stat-WS may have particularly enhanced participants’ attitudes about mathematics. The three groups’ participants did not significantly differ on any of the four domains measured by the pre-course student mathematics attitude survey. However, comparing this survey’s pre- and post-course results among the 338 participants who completed both, Group Stat-WS participants showed significant increases by the end of the semester on the Interest, Growth, and Utility domains. In contrast, Groups EA and EA-WS participants showed increases only in the Interest domain (Table 6). However, conclusions should be made with caution given the relatively low response rates on these surveys, which were around 50%, and that, across all three treatments combined, the pass rate of participants who filled out both the pre- and the post-mathematics attitude surveys was 68%, as compared to 28% for other participants.

Two differences in student behavior in the three groups could help to explain the three groups’ pass rates differences. First, although there were no significant differences among the three groups in terms of reported use of the tutoring available to all students, the post-course student satisfaction survey showed that EA-WS and Stat-WS participants were more likely to participate in self-initiated study groups than were EA participants (95% CIs for the percentage of EA, EA-WS, and Stat-WS groups who engaged in a self-initiated study group: [33, 52], [52, 70], [60, 75], respectively; $F[2, 395] = 9.97$, $p < .001$). (Again, these results should be cautiously interpreted due to low survey response rates, around 50%.) Second, Stat-WS participants were more likely to attend their workshops (72.0%) than were EA-WS participants (65.0%; 95% CI for the difference in the
percentage of workshops attended by Stat-WS and EA-WS participants is $[-12.1, -1.8]$; $t(471) = -2.63, p = .009)$. These behavior differences may be course effects, not a priori causes of higher pass rates for Groups EA-WS and Stat-WS, indicating the positive motivational effects of being assigned to a college-level course.

Another possible reason for the higher pass rates in Group Stat-WS is that, despite randomization, the groups might have differed in terms of student characteristics that can affect passing. However, there is no compelling evidence that this occurred. Table 2 shows that only one variable, days to consent, differed significantly among the three groups. However, that variable showed no significant effect in the logistic regression (Table 4).

Still another possible reason for the higher pass rate in Group EA-WS than Group EA, and the highest pass rate in Group Stat-WS, is that, although each instructor taught one section of each course type, perhaps the faculty graded these courses differently. Given that elementary algebra and introductory statistics are qualitatively different courses, it is not possible to compare their grading directly. However, given that the experiment’s purpose was to compare student success rates in typical remedial mathematics and introductory statistics courses, a more useful question is whether the grading criteria used by the experiment’s instructors were similar to when these courses were usually taught. Eight pieces of evidence suggest that the results were due to group assignment, and not due to changes in the instructors’ grading practices.

First, all 12 of the instructors had taught elementary algebra before, nine had previously taught Introductory Statistics, and all were told by the researchers to teach as they usually did. Second, there were no significant relationships between participants passing and instructors’ tenure status, total years of experience, or experience teaching statistics (Table 4). Third, as shown by the results of the mixed-effects logistic regression and Table 4, the stronger effect size was for group assignment, not instructor. Fourth, all sections of elementary algebra across CUNY are standardized in terms of topics, a common final exam, and a common final grade rubric. For introductory statistics, the experiment’s instructors at each college taught from a common syllabus. Fifth, there were no significant differences in percentage passing by college (Table 4). Sixth, despite the use of a randomized controlled trial with monetary incentives for participation, as described previously, the pass rate
for Group EA was similar to that of students who took elementary algebra one year earlier, and the pass rate of Group Stat-WS was lower than that of students who took statistics one year earlier (students who were either exempt from remedial mathematics, or who had previously passed remedial mathematics and were in at least their second semester at CUNY). Seventh, as indicated earlier, the higher pass rates of Stat-WS students are consistent with indications that these students were more motivated.

Eighth, the Stats-WS students’ enhanced academic success lasted beyond the semester of the experiment (beyond the grading of the experiment’s instructors), as evidenced by the Stat-WS students’ greater credit accumulation rates. At the end of the experiment’s semester (fall 2013), the mean total accumulated credits was 5.9 [5.3, 6.5] for the EA participants and 8.3 [7.6, 8.9] for the Stat-WS participants. One year after the end of the experiment, the Stat-WS participants had increased their mean total accumulated credit advantage from 2.4 to 4.0 in comparison to the EA participants (mean values of 17.7 [16.0, 19.4] for EA vs. 21.7 [19.8, 23.6] for Stat-WS at the end of fall 2014). ITT and TOC tests, both with and without covariates, are significant ($p < .001$; Table 7). A higher percentage of the Stat-WS participants were enrolled (66%) than the EA participants (62%) in fall 2014, but this difference is not significant.

**Discussion and Policy Implications**

The results showed that the Stat-WS students passed statistics at, not the hypothesized same rate as the elementary algebra students, but at a significantly higher rate than did the EA and EA-WS students. The higher pass rate for statistics with additional support as compared to elementary algebra was robust across multiple types of analyses, colleges and instructors. Students assessed as needing elementary algebra do not first need to pass that course in order to pass a college-level quantitative course, at least not if that college-level course is introductory statistics with weekly workshops. These findings are inconsistent with the purported theory underlying many colleges’ policies requiring students to pass remedial courses prior to taking college-level courses. Instead, the results support state and college policies, instituted to increase college graduation rates, that allow, or require, placement of students assessed as needing remedial mathematics instead into college-
SHOULD STUDENTS ASSESSED AS NEEDING REMEDIAL level quantitative courses. Further, our results demonstrate that such policies can help to decrease performance gaps: our student sample was diverse and our findings did not differ by race.

This statistics course pass-rate advantage exists for students with a wide variety of placement test scores. Whether their placement test scores accurately reflect their abilities or not, students placed into statistics with workshops rather than elementary algebra should be more likely to pass a college-level quantitative course and complete college. Completion rates may be even higher should a college accept passing introductory statistics as satisfaction of its quantitative requirement for an undergraduate degree (as did CUNY in this experiment). Follow-up data for the present experiment suggest that the credit-accumulation advantage of the Stat-WS students is not only lasting, but growing since the experiment’s semester.

Our data are consistent with some of the theories regarding why placement into college-level courses can enhance student performance. For example, the evidence supports the theory that the STAT-WS students would be more motivated than the other students. The higher workshop attendance rate, the self-reported higher study-group rate, and the greater increase in positive attitudes towards mathematics among the Stat-WS participants all support this theory.

Our data suggest that students whose placement is elementary algebra will not only be more likely to pass but will have a more positive attitude toward mathematics if they first take statistics than if they first take elementary algebra. Thus taking statistics first might be appropriate for students intending to become STEM majors, not just other majors. Taking statistics first might encourage a student to remain, as well as to become, a STEM major.

Most participants in our EA and EA-WS groups ended the semester having spent 3 hours (or more) per week of course time, and the resulting tuition and financial aid, with no resulting progress towards their degrees, and still needing to pass a remedial course, but most of the participants in our Group Stat-WS ended the semester with three credits towards their degrees and satisfaction of their college’s general education quantitative requirement. Thus students in Group Stat-WS who passed were two courses closer to their degrees than were students in any of the groups who failed. Such degree progress contributes to what has been termed
SHOULD STUDENTS ASSESSED AS NEEDING REMEDIAL
academic momentum (Adelman, 2006; Attewell, Heil, & Reisel, 2012), in addition to decreasing time-to-
degree (Bowen, Chingos, & McPherson, 2009), both of which have been described as critical to college
completion. The increased total cumulated credits advantage of the Stat-WS compared to the EA participants
one year after the experiment was over supports the hypothesis that mainstreaming increased the long-term
academic momentum of the Stat-WS students.

Other findings from this experiment provide some guidance regarding the usefulness of workshops. The
evidence regarding whether the workshops helped the elementary algebra students pass is mixed. The ITT and
TOC comparisons of Groups EA and EA-WS were not significant (Table 4). Neither was the EA vs. EA-WS
comparison among participants ($p = .097$; Table 5). However, the EA-WS participants consistently passed at a
higher rate than the EA participants (Figures 2 and 3). Further, in accordance with predictions, the EA
participants passed at a rate similar to that of elementary algebra students one year earlier, but the EA-WS
participants passed at a significantly higher rate than did elementary algebra students one year earlier.

Prior research and the present Group EA vs. Group EA-WS comparisons only suggest that the
workshops improved the statistics pass rates. Due to CUNY policy participants could not be placed into
statistics without workshops. Therefore it is not possible to determine from the present experiment how much
of the greater pass rate by the Stat-WS students was due to the workshops and how much was due to the
students being enrolled in a college-level statistics course.

We also do not know what the pass rate would have been had our participants been randomly assigned
to college algebra with workshops. A direct comparison of student performance in such a course vs. statistics
with workshops could help determine whether the effect of mainstreaming with workshops is course specific.

An unanticipated and concerning finding of our experiment is that there was differential attrition for the
three groups (see Table 3), suggesting that assigning students to a time-consuming remedial course (here Group
EA-WS) may discourage them from attending college altogether. This possibility should be examined in future
research. This finding also indicates that, when comparing performance in different courses, researchers should
carefully examine the pre-course attrition rates, and not just performance during the courses. Note that Group
EA-WS students were required to attend class five-six hours per week just to address their remedial need, in comparison to three-four hours for Group EA, and five-six hours for Group Stat-WS to address their remedial need but to also receive three college-level general education credits. Thus Group EA-WS was most distant in time from the goal of graduation, and thus was probably least motivated to attend the experiment’s assigned course and possibly also college.

This experiment demonstrates that mainstreaming can positively affect the academic progress of many thousands of college students. One way to compare the effectiveness of traditional mathematics remediation and the type of treatment undergone by the Stat-WS participants is to compare the possibility of students completing their first college-level quantitative course (statistics) within two semesters by each path. At CUNY in fall 2012 alone, 7,675 new students were initially assessed with a placement of remedial elementary algebra. In order to complete statistics within two semesters of entry, such students would have to pass elementary algebra in the fall (the actual pass rate is 37%), return in the spring (the overall retention rate for freshmen from fall to spring is 84%), and then take and pass statistics in the spring (CUNY statistics students who have previously passed elementary algebra have a 68% pass rate). Even if we were to assume that all fall students who pass elementary algebra are retained for the spring, and that all such students take statistics that spring semester, the probability of completing statistics within two semesters for these students is therefore only .37 times .68, i.e., .25. In contrast, 56% of the Stat-WS participants passed statistics in their first semester, and of those who did not, more might pass it in their second semester if permitted to attempt it again. This suggests that, of students entering CUNY just in fall 2012, at least 2,379 more students would pass statistics by the end of their second semester at CUNY if they took a statistics-with-workshops quantitative path rather than the traditional elementary algebra path (4,298 vs. 1,919 students completing statistics within two semesters).

Although the results of the present experiment show that students assigned to remedial mathematics can progress more quickly towards their degrees if they instead take introductory statistics with workshops, degree progression is not the only consideration in setting remediation policy. The participants in Group Stat-WS were
only taught elementary algebra material to the extent that such material was needed to understand the
statistics material. Whether students should be graduating from college having learned statistics but without
having learned all of elementary algebra is one of the many decisions that a college must make regarding which
particular areas of knowledge should be required for a college degree. Views can differ as to which quantitative
subjects a college graduate should know. However, it is clear that passing elementary algebra is not necessary
in order to pass introductory statistics with weekly workshops. Therefore if a college or state deems
introductory statistics to be necessary for a degree, it does not necessarily need to also require the usually
precollege (i.e., remedial) course of elementary algebra. For a college or state to require all students to pass
elementary algebra first, in addition to completing the credits needed for their degrees, can be an extra cost for
students, colleges, and taxpayers, funds that could be spent on other college courses and programs. That extra
cost, as well as educational goals, should be taken into account when higher education policy decisions are
made (Bowen & Tobin, 2015). College communities, and our society, must decide whether the extra cost is
worth the results.
Colleges usually designated a student as needing elementary algebra if that student lacked both a mathematics SAT score of at least 500, and a New York State mathematics Regents score of at least 80 along with passing grades in three units of high school college preparatory mathematics, but had received a passing score (35 or higher) on the prealgebra portion of the CUNY mathematics placement examination (the ACT Compass test) along with a nonpassing score (less than 40) on the elementary algebra portion.

We used the `ivprobit` command in the Stata software package to compute the TOC estimates.

The pass rates among the groups of participants (Figure 2) are higher than those in the ITT and TOC estimates (Table 5) because consenting students who did not participate in the experiment generally left college or did not take elementary algebra (Figure 1), and thus were coded as not passing.

Average marginal effects were obtained using the `margins, dydx` command in the Stata 13 software package.

Mixed effects models are alternatively known as Hierarchical Linear Models (HLM). We also conducted logistic regression with instructor fixed effects; individual instructors were not significantly associated with students’ likelihood of passing.


Authors. (2008).

Authors. (2014).


Table 1

*Means and 95% Confidence Intervals of Characteristics of Participants and Noncompliers*

<table>
<thead>
<tr>
<th>Student Characteristic</th>
<th>Participants</th>
<th>Noncompliers</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age (years)</td>
<td>21.0[20.7,21.4]</td>
<td>21.6[20.9,22.4]</td>
</tr>
<tr>
<td>Age missing</td>
<td>.00[.00,.00]</td>
<td>.18[.12,.23]</td>
</tr>
<tr>
<td>Compass z-score (algebra)</td>
<td>-.003[-.073,.067]</td>
<td>.011[-.077,.100]</td>
</tr>
<tr>
<td>Compass score missing</td>
<td>.080[.060,.099]</td>
<td>.66[.59,.73]</td>
</tr>
<tr>
<td>Days to consent</td>
<td>77.1[75.5,78.7]</td>
<td>69.3[65.2,73.4]*</td>
</tr>
<tr>
<td>First language (English)</td>
<td>.56[.52,.60]</td>
<td>.57[.52,.61]</td>
</tr>
<tr>
<td>First language missing</td>
<td>.00[.00,.00]</td>
<td>.58[.51,.65]</td>
</tr>
<tr>
<td>Gender (female)</td>
<td>.54[.50,.58]</td>
<td>.57[.50,.64]</td>
</tr>
<tr>
<td>Gender missing</td>
<td>.00[.00,.00]</td>
<td>.053[.021,.085]</td>
</tr>
<tr>
<td>High school GPA z-score</td>
<td>-.022[-.095,.050]</td>
<td>.084[-.063,.232]</td>
</tr>
<tr>
<td>High school GPA z-score missing</td>
<td>.31[.28,.35]</td>
<td>.35[.28,.42]</td>
</tr>
<tr>
<td>Race (underrepresented)</td>
<td>.87[.84,.89]</td>
<td>.84[.80,.87]</td>
</tr>
<tr>
<td>Race missing</td>
<td>.00[.00,.00]</td>
<td>.61[.54,.68]</td>
</tr>
</tbody>
</table>

* Participants and noncompliers significantly different, *p* < .05.
### Table 2

**Means and 95% Confidence Intervals of Characteristics of the Three Groups of Participants**

**Group Means [and 95% CIs]**

<table>
<thead>
<tr>
<th>Student Characteristic</th>
<th>EA</th>
<th>EA-WS</th>
<th>Stat-WS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age missing</td>
<td>.04[.02,.06]</td>
<td>.05[.03,.08]</td>
<td>.02[.002,.03]</td>
</tr>
<tr>
<td>Compass z-score (algebra)</td>
<td>-.002[-.10,.10]</td>
<td>-.05[-.15,.05]</td>
<td>.06[-.05,.16]</td>
</tr>
<tr>
<td>Compass score missing</td>
<td>.18[.14,.23]</td>
<td>.22[.17,.26]</td>
<td>.20[.16,.25]</td>
</tr>
<tr>
<td>Days to consent</td>
<td>77.0[74.0,79.5]</td>
<td>77.2[74.6,79.7]</td>
<td>72.2[69.4,75.0]*</td>
</tr>
<tr>
<td>First language (English)</td>
<td>.56[.51,.62]</td>
<td>.56[.51,.61]</td>
<td>.56[.50,.61]</td>
</tr>
<tr>
<td>First language missing</td>
<td>.13[.09,.17]</td>
<td>.16[.12,.20]</td>
<td>.07[.04,.10]</td>
</tr>
<tr>
<td>Gender (female)</td>
<td>.51[.46,.57]</td>
<td>.58[.52,.63]</td>
<td>.55[.49,.61]</td>
</tr>
<tr>
<td>Gender missing</td>
<td>.02[.006,.04]</td>
<td>.003[-.003,.01]</td>
<td>.007[-.003,.02]</td>
</tr>
<tr>
<td>High school GPA z-score</td>
<td>.07[-.04,.18]</td>
<td>-.06[-.18,.05]</td>
<td>-.002[-.12,.11]</td>
</tr>
<tr>
<td>High school GPA missing</td>
<td>.33[.28,.38]</td>
<td>.33[.28,.39]</td>
<td>.30[.25,.35]</td>
</tr>
<tr>
<td>Instructor has taught statistics</td>
<td>.77[.73,.82]</td>
<td>.76[.72,.80]</td>
<td>.77[.73,.82]</td>
</tr>
<tr>
<td>Instructor has tenure</td>
<td>.37[.32,.42]</td>
<td>.38[.34,.43]</td>
<td>.42[.37,.47]</td>
</tr>
<tr>
<td>Race (underrepresented)</td>
<td>.87[.83,.90]</td>
<td>.88[.85,.91]</td>
<td>.84[.80,.88]</td>
</tr>
<tr>
<td>Race missing</td>
<td>.13[.09,.17]</td>
<td>.16[.12,.20]</td>
<td>.09[.06,.12]</td>
</tr>
</tbody>
</table>

* Significantly different from Groups EA and EA-WS, $p < .05$. 
Table 3

Attrition Following Random Assignment, and Withdrawal During the Semester, in Each Group

<table>
<thead>
<tr>
<th>Group</th>
<th>Attrition*</th>
<th>Withdrawal**</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean % [95% CI]</td>
<td>Mean % [95% CI]</td>
</tr>
<tr>
<td>EA</td>
<td>17.9 [13.5,22.2]</td>
<td>15.1 [10.5,19.7]</td>
</tr>
<tr>
<td>EA-WS</td>
<td>27.5 [22.5,32.5]</td>
<td>16.7 [11.7,21.6]</td>
</tr>
<tr>
<td>Stat-WS</td>
<td>17.2 [12.9,21.5]</td>
<td>15.6 [10.6,19.6]</td>
</tr>
</tbody>
</table>

Note. EA = elementary algebra, EA-WS = elementary algebra with workshops, Stat-WS = introductory statistics with workshops.

* $F(2, 904) = 6.23, p = .002$

** $F(2, 702) = 0.14, p = .870$
Table 4

*Logistic Regression Model Predicting Participants Passing Class*

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean marginal effect [95% CIs]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment Status (ref: Group EA)</td>
<td></td>
</tr>
<tr>
<td>Group EA-WS</td>
<td>.072[-.012,.156]</td>
</tr>
<tr>
<td>Group Stat-WS</td>
<td>.166[.083,.249]**</td>
</tr>
<tr>
<td>College (ref: College B)</td>
<td></td>
</tr>
<tr>
<td>College A</td>
<td>.015[-.084,.113]</td>
</tr>
<tr>
<td>College C</td>
<td>.015[-.073,.102]</td>
</tr>
<tr>
<td>Age (years)</td>
<td>.002[-.005,.009]</td>
</tr>
<tr>
<td>Compass z-score (algebra)</td>
<td>.126[.092,.160]**</td>
</tr>
<tr>
<td>Compass score missing</td>
<td>.024[-.100,.148]</td>
</tr>
<tr>
<td>Days to consent</td>
<td>-.001[-.003,.000]</td>
</tr>
<tr>
<td>First language (English)</td>
<td>-.087[-.157,-.018]*</td>
</tr>
<tr>
<td>Gender (female)</td>
<td>.012[-.058,.081]</td>
</tr>
<tr>
<td>High school GPA z-score</td>
<td>.076[.040,.112]**</td>
</tr>
<tr>
<td>Instructor experience (years)</td>
<td>.000[-.005,.006]</td>
</tr>
<tr>
<td>Instructor has taught statistics</td>
<td>-.024[-.119,.071]</td>
</tr>
<tr>
<td>Instructor has tenure</td>
<td>.026[-.061,.113]</td>
</tr>
<tr>
<td>Race (underrepresented)</td>
<td>-.069[-.174,.037]</td>
</tr>
</tbody>
</table>

* * p < .05. ** p < .001.*
Table 5

*Estimates of Treatment Effects on Passing Class*

<table>
<thead>
<tr>
<th></th>
<th>No Covariates</th>
<th></th>
<th>With Covariates</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ITT</td>
<td>TOC</td>
<td>n</td>
<td>ITT</td>
<td>TOC</td>
</tr>
<tr>
<td>Group Means</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>EA</td>
<td>.340</td>
<td>.347</td>
<td>297</td>
<td>.304</td>
<td>.316</td>
</tr>
<tr>
<td>EA-WS</td>
<td>.367</td>
<td>.359</td>
<td>313</td>
<td>.367</td>
<td>.362</td>
</tr>
<tr>
<td>Stat-WS</td>
<td>.498</td>
<td>.528</td>
<td>297</td>
<td>.497</td>
<td>.531</td>
</tr>
<tr>
<td>Treatment effects</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>EA-WS vs. EA</td>
<td>.027</td>
<td>.012</td>
<td>610</td>
<td>.034</td>
<td>.028</td>
</tr>
<tr>
<td>[.049,.10]</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Stat-WS vs. EA</td>
<td>.16**</td>
<td>.18**</td>
<td>594</td>
<td>.14**</td>
<td>.17**</td>
</tr>
<tr>
<td>[.080,.24]</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Stat-WS vs. EA</td>
<td>.13*</td>
<td>.15*</td>
<td>610</td>
<td>.11*</td>
<td>.13*</td>
</tr>
<tr>
<td>[.053,.21]</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*Note.* For a list of covariates see text. ITT = Intent to treat. TOC = Treatment on compliers. 95% CIs are shown in brackets. The Ns decrease for the analyses with covariates because of missing data for noncompliers who did not enroll at CUNY in fall 2013, and use of a dichotomous dependent variable.

* p < .01

**p < .001
Table 6

*Changes in Participant Mathematics Attitudes as Reported in Pre- and Post-Course Surveys*

<table>
<thead>
<tr>
<th>Group</th>
<th>Measure</th>
<th>Ability</th>
<th>Interest</th>
<th>Growth</th>
<th>Utility</th>
</tr>
</thead>
<tbody>
<tr>
<td>EA</td>
<td>Pre-Survey</td>
<td>M=18.5</td>
<td>M=14.1</td>
<td>M=15.1</td>
<td>M=14.3</td>
</tr>
<tr>
<td></td>
<td>[95% CI]</td>
<td>[17.3,19.7]</td>
<td>[13.1,15.1]</td>
<td>[14.2,16.0]</td>
<td>[13.5,15.1]</td>
</tr>
<tr>
<td></td>
<td>Post-Survey</td>
<td>M=19.1</td>
<td>M=15.5</td>
<td>M=14.8</td>
<td>M=14.6</td>
</tr>
<tr>
<td></td>
<td>[95% CI]</td>
<td>[17.8,20.3]</td>
<td>[14.5,16.5]</td>
<td>[13.7,15.8]</td>
<td>[13.7,15.4]</td>
</tr>
<tr>
<td></td>
<td>t(df)</td>
<td>0.93(105)</td>
<td>2.82(105)***</td>
<td>-0.74(105)</td>
<td>0.66(105)</td>
</tr>
<tr>
<td>EA-WS</td>
<td>Pre-Survey</td>
<td>M=17.5</td>
<td>M=13.6</td>
<td>M=15.4</td>
<td>M=14.3</td>
</tr>
<tr>
<td></td>
<td>[95% CI]</td>
<td>[16.4,18.7]</td>
<td>[12.6,14.6]</td>
<td>[14.6,16.1]</td>
<td>[13.5,15.2]</td>
</tr>
<tr>
<td></td>
<td>Post-Survey</td>
<td>M=17.3</td>
<td>M=15.2</td>
<td>M=15.2</td>
<td>M=14.3</td>
</tr>
<tr>
<td></td>
<td>[95% CI]</td>
<td>[16.1,18.4]</td>
<td>[14.2,16.1]</td>
<td>[14.2,16.2]</td>
<td>[13.5,15.2]</td>
</tr>
<tr>
<td></td>
<td>t(df)</td>
<td>-0.47(105)</td>
<td>3.14(105)***</td>
<td>-0.27(105)</td>
<td>0.019</td>
</tr>
<tr>
<td>Stat-WS</td>
<td>Pre-Survey</td>
<td>M=18.4</td>
<td>M=13.4</td>
<td>M=14.9</td>
<td>M=13.3</td>
</tr>
<tr>
<td></td>
<td>[95% CI]</td>
<td>[17.4,19.4]</td>
<td>[12.6,14.3]</td>
<td>[14.1,15.7]</td>
<td>[12.6,14.1]</td>
</tr>
<tr>
<td></td>
<td>Post-Survey</td>
<td>M=18.6</td>
<td>M=15.3</td>
<td>M=15.9</td>
<td>M=14.3</td>
</tr>
<tr>
<td></td>
<td>[95% CI]</td>
<td>[17.5,19.7]</td>
<td>[14.4,16.2]</td>
<td>[15.0,16.8]</td>
<td>[13.5,15.2]</td>
</tr>
<tr>
<td></td>
<td>t(df)</td>
<td>0.37(125)</td>
<td>4.10(125)***</td>
<td>2.14(125)*</td>
<td>2.42(125)**</td>
</tr>
</tbody>
</table>
### Table 7

*Estimates of Treatment Effects on Total Credits Cumulated from the Start of Fall 2013 Until the End of Fall 2014 (One Year Following the End of the Experiment)*

<table>
<thead>
<tr>
<th>Group Means</th>
<th>No Covariates</th>
<th></th>
<th>With Covariates</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ITT</td>
<td>TOC</td>
<td>n</td>
<td>ITT</td>
</tr>
<tr>
<td>EA</td>
<td>15.8</td>
<td>15.8</td>
<td>297</td>
<td>15.5</td>
</tr>
<tr>
<td>EA-WS</td>
<td>14.4</td>
<td>14.4</td>
<td>313</td>
<td>14.7</td>
</tr>
<tr>
<td>Stat-WS</td>
<td>20.5</td>
<td>21.4</td>
<td>297</td>
<td>20.2</td>
</tr>
</tbody>
</table>

Treatment effects

- EA-WS vs. EA  
  -1.4  
  [-3.5,0.8]  

- Stat-WS vs. EA  
  4.7*  
  [2.3,7.0]  

- Stat-WS vs. EA-WS  
  6.0*  
  [3.7,8.4]  

*Note. For a list of covariates see text. ITT = Intent to treat. TOC = Treatment on compliers. 95% CIs are shown in brackets.*

* p < .001
Figure 1. Flow of target students through the recruitment, random assignment, and treatment stages of the experiment (* = includes those who took another mathematics/quantitative course at CUNY, stayed at CUNY but did not take any mathematics/quantitative course, registered at nonCUNY colleges or universities, or did not register anywhere; EA = remedial, noncredit-bearing, elementary algebra course; WS = supplementary, corequisite, workshop; Stat = college-level, credit-bearing, introductory statistics course).
Figure 2. Course pass rates. The first three bars in the section labeled Fall 2013 (research sections) show the pass rates for the three types of courses: elementary algebra (EA), elementary algebra plus a weekly workshop (EA-WS), and introductory statistics plus a weekly workshop (Stat-WS). The bar in the first section shows the comparison elementary algebra pass rate of students at the three colleges at which the research was conducted, but one year prior to the research (Fall 2012, instead of Fall 2013). The fourth bar in the middle section shows the pass rate of students in the Stat-WS group whose scores on the Compass (placement) examination were relatively high (≥ 43 on the prealgebra, i.e. arithmetic, section of the Compass, and ≥ 19 on the algebra section). The last (fifth) bar shows the comparison statistics pass rate of students at the three colleges at which the research was conducted, but one year prior to the research.
Figure 3. Probability of passing as a function of Compass algebra z-score with covariates. Data are shown separately for each of the three groups of participants.