Early Intervention in College Classes and Improved Student Outcomes

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Abstract

This research investigates the effectiveness of an early academic intervention in Principles of Economics courses at a large public university. After the end of the fourth week of classes, students who fell below a 70% threshold on a performance measure, or had an attendance rate below 75%, were referred to the university's Student Success Center for additional academic support. A referral consisted of students being given optional assistance in course specific skills through tutoring, as well as training in general skills like time management and study skills. Using a regression discontinuity framework at the referral threshold, we find that the performance intervention improved student scores on common questions on the final exam by 6.5 to 7.5 percentage points for students at or near the performance threshold. The gains are particularly large for students who entered college with below average math placement scores. These results indicate that low-cost light-touch interventions may significantly affect student academic performance.

Key words: college education, student academic performance, intervention, economic education

JEL codes: A2, I2

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1. Introduction

Student progression and retention rates are of primary concern to college administrators and state legislators. Most college ranking systems factor in four and six-year graduation rates as part of the metric towards measuring the quality of the university and student success. Furthermore, from a financial standpoint, students who attend but do not complete college have relatively little additional earnings over students who finish high school but do not attend college. These students, however, will often have an additional financial burden from tuition payments and student loans[1]. From a political perspective, more state funding of public universities and financial aid is increasingly being made contingent on students making timely progression towards their degree. For these reasons, colleges and universities have a strong interest in ensuring that students successfully progress towards their degree. Progression in the first-year college courses are particularly important for retention and graduation as many students, particularly first generation college students, make the transition from high school to college. According to a U.S. News Report, one in three freshmen fail to continue on to their sophomore year[2].

In this article, we investigate the effectiveness of an early academic intervention on student performance in core economics courses at the University of South Carolina. In an initiative to help increase the success and retention rates of students enrolled in Principles of Microeconomics and Principles of Macroeconomics, students with poor performance and excessive absences early in the semester were referred to the university's Student Success Center (SSC). These students were then aided in receiving additional tutoring as well as training on

[1] https://www.bls.gov/opub/ted/2015/median-weekly-earnings-by-education-gender-race-and-ethnicity-in-2014.htm
[2] https://www.usnews.com/best-colleges/rankings/national-universities/freshmen-least-most-likely-return

general student success skills, such as time management and study skills. Since students were only referred if they fell below certain cutoffs, we estimate the effect of the intervention on future performance within a regression discontinuity framework.

Using data from the referrals, we compare student performance and attendance at the time of the referral to the student's performance at the end of the semester to investigate if students who fell just below the referral criteria did significantly better by the end of the semester than students who fell just above the cut-off point. We use as a measure of student performance at the end of the semester the percentage grade of twenty questions on the final exam common to all sections of the course. Our main results show that for students at or near the referral performance threshold, the intervention increased performance on the common questions by 6.5 to 7.5 percentage points. This result indicates that there is a strong effect from the referral. Further, these gains are largest for the students who are less prepared for college, as measured through initial college math placement test scores.

Our work builds on the established literature in a few dimensions. First, we provide evidence that an early intervention can affect student outcomes. Second, we provide evidence that a "nudge" can meaningfully change student behavior. Third, we find the impact of the intervention to be heterogeneous across individuals. Finally, our results show that students who are the least prepared for college, as measured by their performance on a college math placement test, benefit the most from the intervention.

The findings in this article help university administrators understand the importance of early intervention on student success, and thus allocate university funding in a more effective manner. If colleges are able to identify the students who most need study help and provide effective intervention programs, then implications for state allocation on school financing arise (Breneman, Abraham Jr. and Hoxby, 1998).

The paper will proceed as follows: the second section describes the background literature, the third describes the data, the fourth defines our empirical approach, the fifth presents our main results, the sixth extends our analysis and presents results from falsification tests, and then we conclude.

2. Background

Developmental services and development education in colleges and universities are designed to help students who are underprepared for college classes, or do not possess the necessary academic skills to succeed in college. Development services include student support services such as mentoring, tutoring, advising, skills workshops, and early warning academic monitoring. These services have increasingly been used in higher education to help students achieve success in college. While a few studies have attempted to determine the effectiveness of development services on student learning and retention, evidence on the effectiveness of these programs remains scarce with mixed results[3].

Interventions

Interventions within the semester generally fall into two categories: providing students information about their standing in the course and providing students with nudges, or requiring

[3] Development education can also consist of placement of students into remedial courses. See Calcagno and Long (2008) and Bettiner and Long (2009) for evidence on the effectiveness of remedial courses.

them to use, additional support services[4]. Two articles, Chen and Okediji (2014) and Smith, et. al. (2018) have found that simply reminding students of their standing in the course can improve student outcomes. The former employs a regression discontinuity design similar to this study and finds a gain of 13 percentage points on the final exam grades. The latter uses a randomized trial where students are reminded of their current grade on their homework assignments. This intervention improved homework performance by four percentage points. A third article, Dobkin, Gil, and Marion (2010), uses an intervention on the attendance dimension for students who score below a threshold and finds that an intervention to increase student attendance improves performance.

Nudges and Support Services

In a systematic review of the existing literature on nudges in education, Damgaard and Nielsen (2018), conclude that "few interventions produce positive effects for everyone and some nudges even have negative effects." In the specific context of nudging students to use services there are a pair of articles that suggest that these increase student use of services, but do not necessarily improve performance. Pugatch and Wilson (2018) employ a randomized experiment advertising peer tutoring services to students via a postcard and find an increase in attendance of about 7%. However, they find no change on performance. A similar study at a Community College finds that a "light-touch" intervention, whereby an individual visited math classes a few times during the semester to inform students about available educational support services, increased students'

[4] Moss and Yeaton (2015) find that warning letters can improve student achievement between semesters.

use of tutoring services and reduced math class withdrawal rates, but had no effect on overall pass rates (Butcher and Visher 2013).[5]

There are also a number of studies suggesting that peer tutoring or coaching can improve student outcomes. The main difficulty of these studies is accounting for the selection bias in which students take up the peer tutoring service. Munley, Garvey and McConnell (2010) use differential take-up rates for student athletes to resolve the selection problem and find that students who participate in peer-tutoring have prior GPAs of 0.10 points higher than those who do not. Another study to determine the impact of a peer-tutoring program on preventing academic failure and dropouts among first-year students was conducted at the University of Granada in Spain (Arco-Tirado, Fernández-Martín and Fernández-Balboa 2011). By comparing the performance of 50 freshmen in a treatment group who were paired up with 41 peer mentors, and 50 freshmen in a control group who did not get any tutoring, the study finds a positive impact of peer mentoring on students' performance rates. However, the participants in the study were volunteers who would have been more open towards learning, and thus more likely to benefit from the tutoring program. Only ten economics students were included in the treatment group, with the rest of the students majoring in Civil Engineering, Pharmacy, and Chemical Engineering. These results of the positive effects of development services on college performance and persistence are similar to those of Bettinger and Long (2009) and Bettinger and Baker (2014). Parkinson (2009) also finds a similar positive impact of peer tutoring on student grades in Chemistry and Calculus, while Paloyo, Rogan and Siminski (2016) find that one hour of Peer Assisted Study Sessions at an Australian University leads to a 0.065 standard deviation

[5] However, this intervention did increase the math pass rates of part-time students.

increase in grades, but this estimate was not statistically significant.[6] Another study in this literature by Angrist, Lang, and Kouropoulos (2009) evaluates the effectiveness of academic support series by devising an experiment that offers some students access to a center that coaches first-year students in study skills, offers other students financial incentives for good grades, and some a combination of the two. They find that female students had a high take-up rate for the study skills tutoring and tended to do better at the end of their first year. They attribute this result to the effectiveness of the intervention.

Heterogeneous Impacts

Past studies on the impact of peer support on minority students have shown that minorities tend to benefit from peer support. An investigation of the effectiveness of a tracking program in a large urban school district on high achievers found significant effects that are concentrated among black and Hispanic participants, with minorities gaining 0.5 standard deviation units in fourth-grade reading and math scores with persistent gains through sixth grade (Card and Guiliano 2016). Furthermore, peer support matters in the retention, persistence, and success among college students of color pursuing degrees in STEM fields (Palmer, Maramba and Dancy 2011). This result is likely due to participants being able to master the course content easier when lessons were reinforced in peer groups as these groups foster safe and engaging. A recent study estimating the effect of participation in the American Economic Association Summer Program (AEASP) finds that the program might directly have increased minority participation in the economics profession and the Ph.D.s awarded to minorities in economics by US universities (Becker, Rouse and Mingyu 2016).

[6] See Dawson, et. al (2014) for a review of the effectiveness of supplemental instruction.

While our study resembles some of the previous articles mentioned above, we add to these literatures in several respects. First, we find evidence that an intervention early in the semester can be effective at improving performance. Second, that an intervention in the form of providing information about the availability of services can be effective. Finally, we find evidence that it is the students who are relatively unprepared for college that benefit the most from the intervention.

3. Data

The primary data for this analysis comes from academic and university records[7] of students enrolled in full semester sections of Principles of Microeconomics and Principles of Macroeconomics taught by PhD holding faculty[8] during the Spring 2017 semester at the University of South Carolina. As part of an undergraduate "excellence initiative," faculty tracked student attendance and administered assessments of quizzes and/or homework assignments during the first four weeks of classes. At the end of four weeks, any student who fell below a 70% threshold on performance on class quizzes and homework assignments and/or 75% on class participation was referred to the university's SSC for additional academic support.[9] Students were informed in the syllabi of their respective classes regarding the existence of the early intervention procedure. Once referred, the students were contacted by the SSC to set up one-on-one meetings with a Peer Success Consultant to explore study and time management strategies, create action plans, and access helpful support services such as tutoring and skills workshops[10].

[7] We thank Dr. Nancy Buchan and Brian Shelton for their help in obtaining data on student characteristics. We also thank all economics instructors who aided us in collecting data on student performance.
[8] We remove sections taught by non-PhD holding faculty because the four-week performance scores do not reliably predict future performance.
[9] The undergraduate excellence initiative and the referral cutoffs were implemented prior to the design of this study.
[10] Among all business school classes that made referrals, 42% of students who were referred made a visit to the SSC. Furthermore, more than 2/3rds of those visits were to see peer tutors.

At the end of the semester, all instructors administered final exams with a subset of 20 questions being chosen as common across the courses (Microeconomics and Macroeconomics, obviously, having separate questions). These questions focus on basic economic concepts such as the circular flow of income, the Production Possibilities Frontier, markets, equilibrium and other fundamental topics.

Scores on these questions were then computed for each student and matched to their performance scores and attendance at four weeks. We choose to use the score on the common questions for three reasons. First, as part of the initiative, the scores for all students on these questions were already collected by instructors for these courses. Second, these scores have some advantages over using the final percentage grade in a course. Since the final percentage score is an accumulation of performance and participation grades throughout the semester, it includes some weight on performance prior to the intervention. Because we do not know when students actually use the resources provided by the SSC, we cannot say at what point in the semester these grades would represent pre-treatment and post-treatment. Further, the weight that grades pre-treatment had on the final exam varies across instructors. Finally, the common questions measure the student's knowledge on core economic concepts, which are taught across all sections of the course. Thus, if there is any instructor specific material that the tutors, for example, might not know as well, this will not affect our ability to measure the effect of the intervention.[11]

These performance measures were then matched to personal characteristics from a database of all students in the business school. Of these characteristics, we use race, gender and the student's

[11] See Sankaran, Al-Bahrani and Williams (2018) for a discussion of common questions versus course performance.

score on the college math placement exam.[12] Students in the principles-level classes are largely divided between the university's Business School and College of Arts and Sciences. Since we only have demographic information for students enrolled in the Business School, we limit our primary analysis to students in the Business School[13]. However, our results are robust to the inclusion of Arts and Sciences students in an analysis without covariates. Further, for reasons we explain in the following section, we also restrict our analysis to students who were *not* referred based on the attendance measure. This procedure leaves us with 640 students in the dataset used for our primary analysis. Table 1 reports summary statistics for the whole sample and the sample of students near the discontinuity.

[Table 1]

Of these students, 18% fell below the performance cutoff at four weeks. The average performance score was 82%, while the average on the common final questions was 78%. Females make up 40% of the population, while just over 3% is African American. The average math placement score is 15.4 out of 26, which is slightly higher than the cutoff for a recommended placement in calculus rather than college algebra (a score of 14).

4. Empirical Approach

The difficulty in evaluating the effect of the intervention on student outcomes is that the students who receive the treatment are also likely to do worse in the course. To identify the causal relationship, we adopt a sharp regression discontinuity design. Specifically, we take advantage of

[12] Students took one of two possible placement exams prior to beginning their freshman year: a college algebra or calculus exam. The math score we use is the college algebra score. If a student only took the calculus placement exam, we transform that score to a college algebra score. We do this via a linear mapping based on the recommended placement cutoffs for each test in developmental algebra and calculus.
[13] Analyzing a dataset of only Business School students also enables us to use a more homogeneous sample, since Business School students follow a more structured program of study and are subject to higher admission standards.

the intervention assignment rule whereby students who fall below a 70% on the performance measure receive the treatment. This procedure allows us to compare the students who were just above the threshold to the students who fall just below the threshold. This approach is complicated by the fact that students were also treated if their attendance rate was less than 75% for the first four weeks of class. For students who were referred for attendance, there should be no discontinuity at the performance threshold as they were treated regardless. To get around this issue, we restrict our analysis only to students who were *not* referred for attendance. Instead, we use the students who were referred for attendance as part of a falsification test. We choose to focus on the performance threshold instead of the attendance threshold because of the lack of continuity in measuring attendance after a fixed number of classes, i.e. students have either missed zero classes, one class, etc. Further, a student who missed 75% of a course that met on a Tuesday and Thursday would have missed two classes, whereas a student enrolled in a Monday, Wednesday, Friday course would have missed three. Finally, a very small number of students were referred for attendance who were *not* also below the performance threshold. Thus, our results should be interpreted as the effect of the treatment on performance, conditional on the student attending more than 75% of classes in the first four weeks.

Empirically, we estimate the following model:

$$Score_{ij} = \alpha + \gamma Ref_i + f_r(P_i - 0.7) + f_l(P_i - 0.7)Ref_i + \beta X_i + \mu_j + \varepsilon_i$$

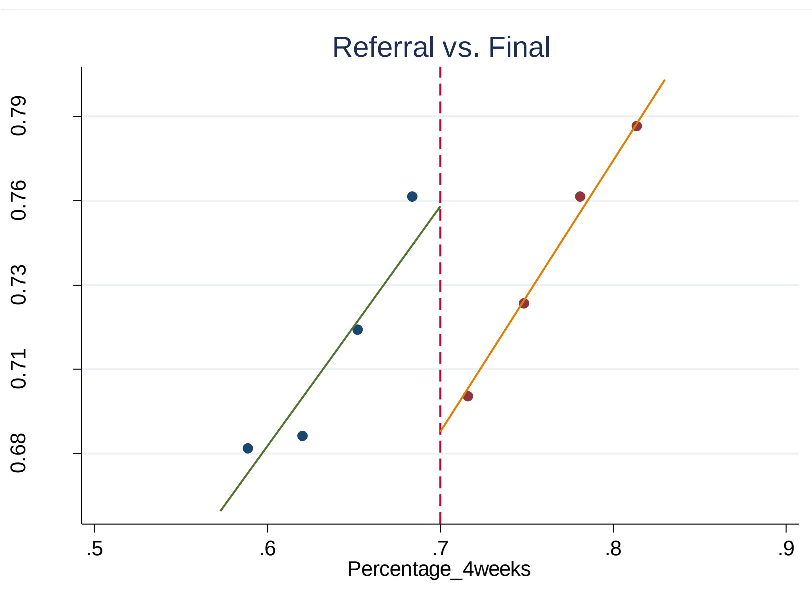
where $Score_{ij}$ is the percent of common questions on the final that student *i* answered correctly, Ref_i is a dummy variable where a 1 indicates the student received the treatment, f_r and f_l are polynomials that are functions of the difference between the student's performance at the time of referral (P_i) and the referral cutoff (0.70), which is estimated for right and left sides of the cutoff respectively, X_i is a vector of individual characteristics, μ_j is an instructor fixed effect and ε_i is a residual term. In this set-up, our treatment variable of interest is γ.

We estimate the model in two ways: first we estimate the model with 2nd, 3rd and 4th degree polynomials using the entire sample. Second, we estimate a local linear model with robust bandwidth selection (Calonico, Cattaneo and Farrell, 2017).[14]

5. Results

Before presenting the results of our estimation, in order to provide some *prima facie* evidence that the treatment has some effect, we first graph the unconditional relationship between the score on the common final questions against the four-week performance measure for students with a score within one standard deviation of the referral cutoff at four weeks.[15] The dashed line represents the threshold for treatment and the solid lines the linear fit on either side of the cutoff.

Figure 1



[14] The results are nearly identical for the robust bandwidth local linear model with and without covariates.
[15] Using alternative ranges of performance yield similar figures.

We can see a monotonic increase in performance on the final up until the cutoff for the threshold, at which point the average score drops before increasing again beyond the cutoff. This is consistent with what we would expect if the treatment has a positive effect.

[Table 2]

Table 2 presents our main results. In panel A, we report the effect of the treatment variable where we estimate global polynomials of the 2^{nd}, 3^{rd} and 4^{th} order. The treatment variable is largely consistent across specifications, but the standard errors rise with the order of the polynomial. The magnitude is between 6.7 and 10 percentage points or 1.3-2 additional questions correct out of 20. Since we are controlling for performance at four weeks, the coefficients on the covariates should be interpreted as the effect relative to the predicted score on the common final given their performance at four weeks. Because some of our other regressors are almost certainly good predictors of performance at four weeks, their direct effect on the final performance of students will be attenuated in our regression specification. Nevertheless, scores on the college placement math test are positively correlated with higher than otherwise predicted performance, where a standard deviation increase in math score suggests a 1.2 percentage point higher than otherwise predicted score on the common final questions. Females do slightly worse than predicted based on their performance at four weeks.

Panel B, reports the results from local linear regressions around the cutoff. The robust bandwidth is 11.8 percentage points around the cutoff (Calonico, Cattaneo and Farrell, 2017). We report the results for the robust bandwidth and also for 3/2s and 2/3rds of the robust bandwidth. The estimates are similar to that of the global polynomial. The treatment effect is estimated to be between 5.4 and 7.6 percentage points, although in the smallest bandwidth the standard errors are larger, and the effect is not statistically significant due to the relatively low numbers of observations. Once we restrict ourselves to students just around the cutoff, the math placement test score, no longer has any further association with the score on the common questions (beyond its association with performance at four weeks). This result is not surprising, as we would expect students near either side of the cutoff to be similar in ability.

Again, females do worse than predicted by 2 to 4 percentage points. This difference by gender is surprising, as one would expect students near the cutoff to be very similar to each other. One possible explanation for the coefficient on female is that the performance score at four weeks includes some non-exam performance measures. If females do systematically better on those measures, but not any better on the common questions, then the model would expect females to be doing better than they actually do on the final questions. That is to say, if performance at four weeks is a noisier signal of performance on the common questions for females and females do better on the four-week performance measure, then this effect could explain the coefficient. This is a plausible explanation since the four-week performance is based mostly on quizzes and homework assignments, some of which are to be completed outside of class. Indeed, the data does suggest this is possible as female students do have higher four-week performance scores on average than males, but slightly lower scores on the common questions. Even though the African American coefficient is insignificant and negative, we are unable to draw any firm conclusions since the number of African American students in the dataset is very small.

6. Discussion

Results by Math Placement Scores

Given the literature suggests that nudges may have very heterogenous effects (Allcott, 2011) and that groups constrained by some behavior barrier are more likely to benefit from nudges

(Damgaard and Nielsen, 2018), we also look at the effect of our treatment on students based on their college math placement score.[16] We group them into students who scored above average on the placement test and those who scored below average. This score provides some measure of the students' abilities – particularly analytical skills necessary for principles-level economics – when they began college.

[Table 3]

Table 3 reports the results from the local linear regression for each group. The treatment effect for the students below average is 17.4%, while just 2.1% on the above average students. This suggests that it is the students who are least prepared when entering college that see the largest benefits.

Identification

It is possible that the threshold of 70% in the regression discontinuity design we employ might, in itself, have some effect on student effort. For example, if students who scored just below a 70 decided to study hard, while those just above a 70 did not, then the treatment variable could spuriously attribute this behavior to the intervention. Indeed, 70% was chosen because it is the grade needed to receive a C in the class, which is necessary for all students in the Economics and Business majors. Hence, we may have reason to believe that students just below the cutoff will study harder than students above the cutoff even without an intervention. In this section we attempt to perform two falsification exercises to see if we were to see a similar effect around a

[16] A natural question given previous work is whether there are differences by race. Unfortunately, we do not have enough minority students in our sample to reliably report results by race.

performance score of 70 when there is no change in treatment for students with scores below 70 and those above 70.

In the first exercise, we consider students during this semester who were treated regardless of whether they fell below the performance cutoff: the students who were below the attendance threshold. We perform the same analysis as in the previous section. However, for this group there should be no jump at the performance cutoff of 70.

[Table 4]

Table 4 reports the results of this exercise. While the sample size is limited, there does not appear to be any systematic pattern as the coefficient on the treatment variable changes signs and magnitude across specifications.

In our second exercise, we use a group of students that were never treated: students from a previous semester.[17] We use data from instructors who taught in both the Spring 2016 and Spring 2017 semester. Since there were no referrals or common questions on the final exam in the 2016 semester, we use the score on the final exam as the outcome variable. We construct a performance score after four weeks to use as the assignment of our "treatment." Due to lacking data on the demographic characteristics and math test scores for these students, we perform the analysis without covariates.

[Table 5]

[17] There are a number of limitations to using data from prior semesters in addition to the lack of data on student characteristics. Most notably, the referral initiative required instructors to track attendance for at least four weeks. This change in tracking attendance confounds efforts to perform a difference in difference approach across semesters.

The results are reported in Table 5. Overall the effect is small and statistically insignificant. There does not appear to be any evidence that students just below the 70% threshold did any better than those just above the 70% threshold in previous semesters.

Besides the possibility that the threshold of 70% has some effect independent of the intervention, there is a potential for student manipulation of the treatment. Since information regarding the referral process is provided to students via the syllabus, it is possible that students who were near the performance cutoff tried harder to avoid being referred. Thus, the students right above the cut-off might be worse on average than if assigned randomly. As long as any difference in ability between students just above the threshold and those just below the threshold is explained by the college placement math scores, then this should not weaken our identification. For this to weaken our identification, it must be the case that a) students are aware of the policy b) are willing to study harder to avoid referral and c) are capable of affecting the four-week performance score at the margin. We think this is unlikely as there is little reason to believe that the intervention is particularly costly to students who do not wish to participate in the intervention, as participation was completely voluntary. Furthermore, the authors are skeptical that information provided on the syllabus implies that students are aware of the policy four weeks into the course.[18]

Finally, there is also the possibility of a selection bias in the data arising from students withdrawing from the course. If receiving the referral increased the likelihood of a student near the cutoff of withdrawing from the course and the students who withdrew were on average worse than those that did not (with a similar performance score at four weeks), then this could upwardly bias our estimate of the treatment. While, we cannot say much about how the students

[18] The authors will gladly provide numerous anecdotal accounts that support this assertion upon request.

who withdrew would have done on the final exam, we do know that withdraws are relatively rare, only 2.6% of all students enrolled in these courses. Further, because we must exclude the students who had an attendance score below 75% at four weeks, only 1.3% of the students (9 students in total) that would have been included in our analysis withdrew from the course. Of these students, less than half (4 students) would have been in the discontinuity sample.

Interpretation of Results

The interpretation of our main result is that intervention can improve the performance of students who are initially struggling in performance, but are attending the classes. This is a subset of students that has demonstrated, on at least some dimension, that they are more motivated to perform and/or might face fewer outside obstacles (i.e. work, family responsibilities) to class attendance. It might well be the students who are not attending classes might most benefit from the intervention. Though we are unable to estimate the effectiveness of the intervention among these students, we do find strong evidence that the intervention does help the performance of students who are attending class.

7. Conclusions

In this paper we consider the effectiveness of an early semester intervention for students falling below a 70% performance threshold after the first month of classes. The intervention consisted of a soft nudge to inform students about their standing in class and offer them support services through course-specific tutoring as well as general student success skills (such as study skills and time management). We exploit the nature of the intervention threshold by performing a regression discontinuity analysis. While employing demographic and math placement score controls, we analyze the impact of the intervention on student performance on a set of questions from the final exam using data from Spring 2017. Then, we proceed with falsification tests by conducting similar analyses using students from a previous semester before the policy was implemented. Finally, we investigate which students benefit the most from the intervention.

We find that students who were just below the performance threshold, i.e. those who received an intervention notice, performed 6.5 to 7.5 percentage points better on a set of questions on the final exam than students who were just above the threshold. We also find that the students who are least prepared when entering college, measured through placement scores on the math exam which students take before their first year of classes, benefit the most from the intervention. Students who had above average math placement scores increased their grade by 2.1% whereas students with below average math placement scores experienced a grade increase of 17.4% as a result of the intervention. The College Board shows that high school seniors from lower income families and those with less educated parents tend to score lower on the SAT math scores[19]. Taken together, this implies that the intervention benefits the students who need it the most. This intervention increases the awareness of students who might not have otherwise been aware of the resources available on campus for study help and life skills.

While this particular study focused on principles-level Economics classes, there is no reason to believe that the procedure could not work in other types of introductory-level classes. Given the obvious interest universities have in student performance, this soft-nudge intervention is potentially a relatively low-cost method to achieve tangible results. For the reasons that we noted at the beginning of this article, the benefits could be substantial.

[19] https://reports.collegeboard.org/pdf/total-group-2016.pdf

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Table 1: Descriptive Variables

	Whole Sample		Discontinuity Sample	
	Mean	SD	Mean	SD
Referred Performance	0.18	0.38	0.42	0.49
Common Final Questions	0.78	0.12	0.74	0.13
Performance (4 weeks)	0.82	0.13	0.72	0.07
Female	0.4	0.49	0.35	0.48
African American	0.03	0.17	0.04	0.19
Math Score	15.4	4.7	14.7	4.4
N	640		228	

Table 2

Panel A: Global Polynomial Results

Polynomial Degree	2nd	3rd	4th
Treatment	0.067**	0.099**	0.068
	(0.029)	(0.043)	(0.058)
Math Score	0.0025***	0.0024**	0.0024**
	(.001)	(.001)	(.001)
African American	-0.03	-0.03	-0.03
	(0.025)	(0.025)	(0.026)
Female	-0.024***	-0.023***	-0.023**
	(0.009)	(0.009)	(0.009)
Number of Observations	640	640	640

Panel B: Local Linear Results

Bandwidth	Robust	1.5X	0.67X
Treatment	0.076**	0.054**	0.067
	(0.034)	(0.027)	(0.044)
Math Score	0.001	0.002	0.001
	(0.002)	(0.001)	(0.002)
African American	-0.07*	-0.048	-0.07
	(0.040)	(0.033)	(0.045)
Female	-0.03*	-0.024*	-0.04*
	(0.167)	(0.013)	(0.022)
Number of Observations	228	351	148

Notes: All specifications include instructor fixed effects. Standard errors are in brackets. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.10 levels respectively.

Table 3

Results by Math Placement Test

	Below Average	Above Average
Treatment	0.174***	0.021
	(0.044)	(0.043)
Math Score	-0.006	0.010**
	(.004)	(0.004)
African American	-0.055	-0.043
	(0.089)	(0.031)
Female	-0.017	-0.05**
	(0.024)	(0.024)
Number of Observations	97	131

Notes: All specifications include instructor fixed effects. Standard errors are in brackets. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.10 levels respectively.

Table 4

Panel A: Global Polynomial Results

Polynomial Degree	2nd	3rd	4th
Treatment	0.088	-0.089	-0.146
	(0.06)	(0.119)	(0.19)
Math Score	0.000	0.000	0.000
	(.002)	(.003)	(.003)
African American	-0.06	-0.08	-0.095
	(0.071)	(0.068)	(0.071)
Female	-0.035	-0.035	-0.034
	(0.036)	(0.035)	(0.035)
Number of Observations	96	96	96

Panel B: Local Linear Results

Bandwidth	Robust	1.5X	0.67X
Treatment	0.004	0.036	-0.06
	(0.068)	(0.063)	(0.089)
Math Score	-0.000	0.004	-0.003
	(0.003)	(0.003)	(0.003)
African American	-0.037	-0.11	0.004
	(0.084)	(0.096)	(0.08)
Female	-0.001	0.003	-0.001
	(0.047)	(0.040)	(0.052)
Number of Observations	41	58	29

Notes: All specifications include instructor fixed effects. Standard errors are in brackets. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.10 levels respectively.

Table 5

Panel A: Global Polynomial Results

Polynomial Degree	2nd	3rd	4th
Treatment	-0.025	0.013	0.031
	(0.026)	(0.039)	(0.06)
Number of Observations	386	386	386

Panel B: Local Linear Results

Bandwidth	Robust	1.5X	0.67X
Treatment	0.010	0.003	0.006
	(0.037)	(0.029)	(0.049)
Number of Observations	135	188	90

Notes: All specifications include instructor fixed effects. Standard errors are in brackets. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.10 levels respectively.