A Test of Enhancing Learning in Economics through Nudges

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I. Introduction

Sunstein and Thaler (2008, p. 6) define a nudge as "any aspect of the choice architecture that alters people's behavior in a predictable way without forbidding any options or significantly changing their economic incentives." Behavioral economics (e.g., Sunstein and Thaler (2008), Kahneman (2011)) has demonstrated the important role that nudges can play in altering individual choices. Since Sunstein and Thaler (2008) there has been an explosion of research in education on the applicability of behavioral-economics interventions to improve learning outcomes.

A number of studies use nudges in university economics education and have reported mixed results. To highlight a few: Smith et al. (2018) report on the positive results of an experiment in three online economics classes in providing an informational nudge to the students. Carrell et al. (2015) use a series of informational nudges in a large introductory-level microeconomics class that serves as well as an encouragement to visit the instructor's office hours; they find positive results in grades as well as other measures of student effort.

Oreopoulos and Petronijevic (2019) provide a multi-pronged and in the end pessimistic reading of the usefulness of nudges in economics education. Over a five-year period, the authors in a series of controlled experiments with random assignment tested the effectiveness of nudges to improve student goal-setting and student mind-set. Their conclusion: "Our findings indicate that, at least for large four-year colleges like UofT, none of the interventions we test can generate a significant improvement in student grades or persistence."

In one of the component studies from which this conclusion is drawn, Oreopoulos et al. (2019) report on the use of emailed nudges to encourage greater studying on the part of their students. Those students receiving nudges did in fact spend significantly more time studying but had no different graded outcomes than those students who did not receive the nudges. The authors conclude that the nudges were successful, but that in the absence of guided intervention on “how to study” the students did not benefit from their added time.

Damgaard and Nielsen (2018) summarize the literature on nudges in education and provide useful context. They offer three warnings about the use of nudges in an educational setting.

First, nudges that are targeted on passive decision-making (e.g. deadline reminders) can have broad and long-term positive impact. However, they will be less effective (or ineffective) if used to affect a choice that involves active decision making (e.g. a nudge that encourages students to attend an information session or any session that offers assistance in boosting skills). Second, interventions that target active decision making have widely different results across implementations. Third, few interventions produce positive effects for everyone. Some interventions are more effective with high-SES students, for example, while other interventions are more effective with low-SES students. A badly targeted intervention can increase the existing inequality in educational outcomes.

In this paper we investigate the effectiveness of nudges and of more intensive support programs for a group of students enrolled in two introductory economics courses on the Chapel Hill campus of the University of North Carolina. We use emailed nudges to affect the passive decision-making of our students in reminding them of the availability of two academic support programs on our campus. We then test (a) the impact of the nudges in encouraging greater use of those programs and (b) the impact on academic performance (as measured by grades) of use of those support programs. In this way we will investigate the non-result of Oreopoulos et al. (2019) in the context of a more targeted support program for our students. Furthermore, we test to see if there are different effects on first-year students, women, and students of color.

II. Study Design and Data.

Our initial sample is the 478 students enrolled in two introductory Economics courses in the fall semester of 2019 at the University of North Carolina at Chapel Hill: a section of Economics 101 (Principles of Economics, 395 students) and a section of Economics 111 (Economics of Innovation and Entrepreneurship, 83 students). We obtained informed consent from 199 of the students. For each consenting student i we have the grades from two midterm examinations and one final examination (g_{i1}, g_{i2}, g_{iF}): these examinations represent 75 and 85 percent of the course grade for the two courses, respectively. We also have the student’s SAT or ACT score (S_{iS}) as an indicator of innate academic ability.[1] Table 1 reports the characteristics of students in the two sections. There is a clear bias in informed consent towards those who performed well in the course.

[Insert Table 1 Here]

During the fall semester the Economics Department offered two substantial supports (i.e. to assist students in their courses). The EconAid Center (EAC) provides walk-in peer tutoring for all students during the day and in early evening. We (Balaban and Conway) also offered five hour-long Learning Strategies Seminars (LS) during the semester. These two supports provide the type of informational nudge, or boost, that Damgaard and Nielsen (2018) discuss in their active decision-making interventions. The availability of these supports was advertised to all students in these two courses. We also sent out emails once per week to a randomly assigned half of the students to remind them of the availability of the EAC. We separately sent out emails the day before each LS to the remaining randomly assigned half of the students in these two classes.[2] These two groups of students receiving our email nudges are the treatment groups for the passive reminder nudges. Participation in the two informational nudges was low.[3]

Our statistical design takes the following form. We hypothesize a "grade production function":

$$g_{ij} = \alpha_j + \alpha_{101i} + \beta_E\, EAC_{ij} + \beta_L\, LS_{ij} + \gamma_S\, P_{ij} + \varepsilon_{ij}$$

g_{ij} is the grade for individual i on examination j. α_j is the mean score for the ECON 111 section and ($\alpha_j + \alpha_{101i}$) is the mean score for the ECON 101 section on examination j, conditional on innate ability and participation in supports. EAC_{ij} and LS_{ij} are metrics of participation by student i in the supports prior to examination j.[4] β_E and β_L

[1] We have the ACT scores for 132 students in the sample and the SAT scores for 112 students in the sample: some students reported results from both tests. As a summary measure from each exam we use the ACT Composite score (on a scale from 1 to 36) and for different entering cohorts either the SAT Highest Total for Reading, Writing and Math (on a scale from 200 to 1600), the Total Score (on a scale from 400 to 1600) or the Total Math/Reading/Writing Score (on a scale from 400 to 1600). To create a single measure per student we take the ACT score as the base. For students with only the ACT score or with both scores we use the ACT score. For students with only the SAT score, we convert the SAT score to the ACT score using a concordance created by ACT and SAT. (https://collegereadiness.collegeboard.org/pdf/guide-2018-act-sat-concordance.pdf). We interpolated fractional ACT values to distinguish adjacent but not identical SAT scores. SAT or ACT scores were not available for five students who transferred in to UNC from community colleges.

We initially collected high-school GPA scores for our students as well, but there were 20 students for whom these were not available. Given that it proved to be highly correlated with S_{is}, we have decided to use just our measure of SAT/ACT scores.

[2] In the pooled sections, 239 students - 95 of whom were from the informed consent group (40%) - received the email nudge regarding the availability of the EAC and 239 students - 103 of whom were from the informed consent group (43%) - received the email nudge regarding the LS. All students were randomly assigned to one of these two groups before we knew who would consent to participate in the study.

[3] The values are available from the authors upon request.

[4] We have two measures of participation in the EconAid Center (i.e., EAC_i) collected through the use of the online app My Digital

measure the average impacts of participation in these supports on grade j. γ_S measures the marginal impacts of prior education success on grade j. The unobserved (to the econometrician) characteristics of student i contribute to the random error ε_{ij}.

P_{ij} can be thought of as the "prior" of the individual's graded performance before examination j.[5] We anticipate that this prior will be important in determining the individual's participation in the learning supports. P_{ij} also serves as an evolving indicator of the individual's ability to perform on graded examinations, and so is included as a determinant of g_{ij} in the grade production function.

Previous academic success is one possible explanation of participation in EAC, as proxied by P_{ij} with marginal effect μ_P and of LS with marginal effect φ_P. Another is the random nudge provided: N_{EACi} for the EAC opportunity and N_{LSi} for the LS opportunity.[6]

We extend this system to test for differential effects by subgroup by including binary variables D_k reflecting students in identified subgroups: D_{Ci} for individuals of color, D_{Fi} for females, and D_{FYi} for first-year students.[7] Unobserved characteristics of the individual's choice environment are captured in the random errors υ_{Ei} and υ_{Li}.

We conjecture that we will see larger gains for women and students of color through the encouragement effect of these supports, and of these nudges. The gender gap in undergraduate economics has been an enduring concern within academic programs, leading to a randomized controlled trial entitled Undergraduate Women in Economics (UWE) sponsored by the AEA.[8] In a separate research project, Bayer and Wilcox (2019) investigated the disparity of choice of Economics major for both women and people of color when compared to Caucasian males. The disparities suggest that the choice environment for the

Hand. The first measure is the number of visits to the EconAid Center for assistance. The second is the number of minutes spent in total during those visits. (My Digital Hand records when a tutoring session begins and when it ends as reported by the tutor.) Results reported here use the number of visits by individual i prior to examination j as EAC_{ij}. For LS_{ij} we record the number of seminars the student participated in prior to the examination in question. The maximum number for the first midterm was two, the maximum number for the second midterm is four and the maximum number for the final examination is five.

[5] Our specification is:

$P_{i1} = 100*(S_{iS} - S_{LS})/(S_{HS} - S_{LS})$

$P_{i2} = 50*(S_{iS}-S_{LS})/(S_{HS}-S_{LS})+50*(g_{i1}-g_{L1})/(g_{H1}-g_{L1})$

$P_{i3} = 33*(S_{iS}-S_{LS})/(S_{HS} - S_{LS})+33*(g_{i1}-g_{L1})/(g_{H1}-g_{L1})+33*(g_{i2}-g_{L2})/(g_{H2}-g_{L2})$

where S_{is} is individual i's SAT score, S_{LS} and S_{HS} are the lowest and highest SAT scores among the individuals in the sample, g_{ij} is the grade of individual i on examination j, and g_{Lj} and g_{Hj} are the lowest and highest grades on examination j

We have given this prior fixed and equal weights on previous examination results. An extension to explore is estimation of those weights within this model. We will investigate that going forward.

[6] We chose in this research design to code the nudge as a binary variable – either the student received the stream of email nudges or not. It is conceivable that nudges have a "wearing-down" effect, and that the cumulative number of nudges could be important in the incentive to participate. We will examine that possibility in future work.

[7] We identify first-year status using the University assessment of program year. In determining females and people or color, we use our own observation. People of color in the definition we use here include Black, African-American, Hispanic and American Indian students. The excluded group is the group of Caucasian males in their second or higher year on campus.

[8] This trial was chaired by Claudia Goldin of Harvard University; information is available at https://scholar.harvard.edu/goldin/UWE.

three groups are quite different. The importance of individual encouragement to female students in economics is emphasized in the study of the relative success of female graduate students in Economics by Boustan and Langan (2019).

We investigate the differential effect of both nudges and substantive supports through the following estimating equations:

(1) $g_{ij} = \alpha+\alpha_1+\alpha_2 + \alpha_{101i} + (\beta_E+\Sigma_k\theta_{Ek}*D_k)\ EAC_{ij} + (\beta_L + \Sigma_k\theta_{Lk}*D_{ki})\ LS_{ij} + \gamma_P\ P_{ij} + \varepsilon_{ij}$

(2) $EAC_i = \alpha_E+\alpha_{E1}+\alpha_{E2}+\alpha_{E101}+\Sigma_k\rho_{Ek}*D_{ki}\ N_{EACki}+\mu_P\ P_{ij} + \upsilon_{Ei}$

(3) $LS_{ij} = \alpha_L+\alpha_{L1}+\alpha_{L2}+\alpha_{L101i}+\Sigma_k\ \rho_{Lk}*D_{ki}\ N_{LSki} + \varphi_P\ P_{ij} + \upsilon_{Li}$

Our hypothesis tests of active nudges will use the statistical estimates of β_E and β_L to test for the impact of these informational supports in improving student performance. If significant, they will indicate that the support in question is significantly improving the student graded performance even after controlling for the student's prior academic training and success in graded events. The statistical estimates of ρ_{Ek} and ρ_{Lk} provide a test of the impact of low-cost passive nudges on participation in these substantive supports. If positive and significant, they indicate that such nudges can be used effectively to improve student participation in these support activities. θ_{mk} is the indicator of marginal significance of learning support activity m on grade performance by sub-group k.

We stack these equations so that we have three observations per student: midterm 1, midterm 2 and final examination. α_1 and α_2 are coefficients associated with midterm 1, and similarly for midterm 2. The intercept thus is the conditional mean of the final examination.

III. Estimation Results

Table 2 reports the results of our test of the joint hypothesis that passive nudges are persuasive in bringing individuals to learning supports and that the learning supports boost grade performance.

[Insert Table 2 Here]

We find in our estimates of equation (1) that the contributions of θ_{Ek} and θ_{Lk} to the grade production function are insignificantly different from zero for each subgroup k, and so we drop them from this specification.[9] Grades on the first examination were significantly larger than those on the final examination. The grades for ECON 101 are significantly lower than those in ECON 111.[10]

The coefficients corresponding to the effects of the active nudges are insignificantly

[9] These results are not reported here but are available from the authors.

[10] The ECON 101 grades were "curved" upwards at the end of the semester, while those of Econ 111 were not.

different from zero, with positive sign (increasing grades) for the LS intervention and with negative sign (reduced grades) for the EAC intervention.

The two participation equations (EAC and LS) indicate significant impact of nudges for subgroups of the students.[11] We observe that first-year and female individuals were significantly more likely to visit the EconAid Center than their benchmark (older non-minority male) classmates after receiving the email nudges.[12] By contrast, these groups of students were all less likely (though insignificantly so) to respond to a nudge to participate in the LS seminars relative to benchmark classmates.

IV. Conclusions

The behavioral-economics literature on nudges has offered promise for low-cost and yet effective interventions to improve learning outcomes. Published research has reported significant positive effects of nudges on student performance, but recent work by Oreopoulos and co-authors drew negative conclusions about the possibility of such low-cost yet effective interventions. In our investigation we draw conclusions on both active and passive nudges.

The active nudges – informational boosts – in our research design are the LS seminars and the availability of the EconAid Center. Participation in these had insignificant marginal effects on grade performance after controlling for prior academic success. Passive nudges – emailed reminders of the availability of these support services – showed a positive but insignificant aggregate effect in encouraging participation. However, when we introduced the possibility of differing effects by subgroup (females, first-year students, individuals of color), we found that the nudges had differing effects: they were significantly effective in encouraging these students in these subgroups to visit the EAC but were ineffective in encouraging participation in the LS seminars. As Damgaard and Nielsen (2018) suggest, it is important that we track the impact of our "nudging" efforts because they can have different effects on subgroups of importance to us (female, first-year, individuals of color).

This research is limited by several factors: First, the low consent rate, especially by individuals of color. It is possible that our count of this group of students was low because

[11] The results when nudges are aggregated over all students are positive but insignificant on average. These results are available on request.

[12] Individuals of color also had a large positive coefficient, but the small number in sample led to large standard error and rejection of significance.

we used our personal observation to identify people of color. Second, the low participation rate in the Learning Strategies seminars may have impacted the results. These seminars, which were taught by the students' instructors, were designed to enhance students' learning skills and connect concepts across chapters, whereas the EAC tutoring sessions helped students understand individual concepts. However, there were many more opportunities to visit EAC since it is available 5 days a week whereas the LS seminars were only offered 5 times over the course of the semester. Even though we recorded the sessions and made them available to all students to watch at their convenience, we did not track who watched them. Future work will make efforts to avoid these shortcomings.

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TABLE 1: CHARACTERISTICS OF THE ECON 101 AND ECON 111 STUDENT SAMPLES

	Econ 101		Econ 111		Pooled Sample
	Total class	Informed Consent group	Total class	Informed Consent group	Informed Consent group
Enrolled (number)	395	158	83	41	199
First-year students (percent)	45	49	37	36	46
Female (percent)	52	58	47	54	57
People of color (percent)	11	5	18	15	7
Mean, MT1	77.3	80.7	82.7	85.8	81.1
Mean, MT 2	77.4	80.5	81.1	85.2	80.6
Mean, Final examination	74.8	77.8	79.2	82.7	79.1

TABLE 2: ESTIMATION INCORPORATING SUB-GROUP VARIATION

Equation		Coefficient	Standard Error	t statistic	Probability > t
g_{ij}					
	Constant	**65.48**	2.13	30.69	0.00
	P_{ij}	**0.27**	0.02	11.22	0.00
	α_{101i}	**-5.99**	1.59	3.76	0.00
	α_1	**3.27**	1.51	2.16	0.03
	α_2	2.22	1.33	1.67	0.09
	EAC_{ij}	-1.78	1.08	1.65	0.10
	LS_{ij}	10.94	5.94	1.84	0.07
EAC_{ij}					
	Constant	0.52	0.31	1.70	0.08
	P_{ij}	-0.003	0.004	0.78	0.42
	α_{E1}	**-0.57**	0.17	3.31	0.00
	α_{E2}	**-0.60**	0.17	3.53	0.00
	$D_{Ci}*N_{EACi}$	0.67	0.40	1.67	0.14
	$D_{Fi}*N_{EACi}$	**0.55**	0.16	3.37	0.00
	$D_{FYi}*N_{EACi}$	**0.40**	0.17	2.35	0.01
	α_{101i}	**0.47**	0.17	2.70	0.01
LS_{ij}					
	Constant	0.13	0.10	1.27	0.20
	P_{ij}	-0.00	0.00	0.09	0.93
	α_{L1}	**-0.17**	0.06	2.96	0.00
	α_{L2}	-0.10	0.06	1.68	0.09
	$D_{Ci}*N_{LSi}$	-0.10	0.12	0.85	0.40
	$D_{Fi}*N_{LSi}$	-0.10	0.05	1.91	0.06
	$D_{FYi}*N_{LSi}$	0.01	0.05	0.17	0.86
	α_{101i}	**0.19**	0.06	3.33	0.00

	Observations	Parameters	RMSE	F(2, 56x)	Probability
g_{ij}	569	6	10.65	29.19	0.00
EAC_{ij}	569	7	1.67	7.32	0.00
LS_{ij}	569	7	0.56	3.23	0.00

These coefficients were estimated in a system estimation of equations (1), (2) and (3). Zero correlation between errors of the first equation and the remaining two is imposed, while correlation between errors of EAC_{ij} and LS_{ij} is allowed. Coefficients significant at the 95 percent level of confidence are in boldface.

TABLE XX: STUDENT PARTICIPATION IN INFORMATIONAL BOOSTS

		Total Participants	Total from the Informed Consent group
Learning Strategies Seminars	First LS seminar	12	4
	Second LS seminar	20	12
	Third LS seminar	13	7
	Fourth LS seminar	15	8
	Fifth LS seminar	33	18
Attendance at EconAid Center	Prior to First Midterm	67	36
	Between first and second midterm	67	32
	Between second midterm and the final exam	37	18

TABLE XXX: AGGREGATED-MODEL ESTIMATION

Equation		Coefficient	Standard Error	t statistic	Probability > t
g_{ij}					
	Constant	56.72	7.97	7.12	0.00
	P_{ij}	0.33	0.06	5.70	0.00
	α_{101i}	-9.17	4.40	2.08	0.04
	α_1	7.53	4.99	1.51	0.13
	α_2	7.79	5.29	1.47	0.14
	EAC_{ij}	9.22	8.34	1.10	0.27
	LS_{ij}	--	--	--	--
EAC_{ij}					
	Constant	0.82	0.31	2.66	0.01
	P_{ij}	-0.005	0.004	1.42	0.16
	α_{E1}	-0.58	0.17	3.21	0.00
	α_{E2}	-0.60	0.17	3.42	0.00
	N_{EACi}	0.18	0.14	1.25	0.21
	α_{101i}	0.46	0.18	2.64	0.01
LS_{ij}					
	Constant	0.03	0.10	0.32	0.75
	P_{ij}	0.00	0.00	0.16	0.87
	α_{L1}	-0.17	0.06	2.96	0.00
	α_{L2}	-0.09	0.06	1.66	0.10
	N_{LSi}	0.07	0.05	1.48	0.14
	α_{101i}	0.20	0.06	3.41	0.00
	Observations	Parameters	RMSE	F(2, 57x)	Probability
g_{ij}	575	5	17.60	12.61	0.00
EAC_{ij}	575	5	1.71	5.11	0.00
LS_{ij}	575	5	0.56	4.41	0.00

These coefficients were estimated in a system estimation (3SLS) of equations (1), (2) and (3). Zero correlation between errors of the first equation and the remaining two is imposed, while correlation between errors of EAC_{ij} and LS_{ij} is allowed.