Pugatch, Todd; Schroeder, Elizabeth

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Promoting Female Interest in Economics: Limits to Nudges

Todd Pugatch and Elizabeth Schroeder
Oregon State University
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Abstract: Why is the proportion of women who study Economics so low? This study assesses whether students respond to messages about majoring in Economics, and whether this response varies by student gender. We conducted an experiment among more than 2,000 students enrolled in Economics Principles courses, with interventions proceeding in two phases. In the first phase, randomly assigned students received a message with basic information about the Economics major, or the basic message combined with an emphasis on the rewarding careers or financial returns associated with the major. A control group received no such messages. In the second phase, all students receiving a grade of B- or better received a message after the course ended encouraging them to major in Economics. For a randomly chosen subset of these students, the message also encouraged them to persist in Economics even if their grade was disappointing. The basic message increased the proportion of male students majoring in Economics by 2 percentage points, equivalent to the control mean. We find no significant effects for female students. Extrapolating to the full sample, the basic message would nearly double the male/female ratio among Economics majors. Our results suggest the limits of light-touch interventions to promote diversity in Economics.

Keywords: college major choice; gender gap in Economics; higher education; nudges; randomized control trial

JEL codes: I21, I23

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1 Introduction

Differences in earnings across graduates of different disciplines rival, and in some cases exceed, the difference in earnings between college and high school graduates (Arcidiacono 2004; Altonji, Blom, and Meghir 2012; Altonji, Kahn, and Speer 2016; Altonji, Arcidiacono, and Maurel 2016). As in many STEM (Science, Technology, Engineering, Mathematics) fields, an Economics degree offers high future salaries but the female share of graduates is low, contributing to workplace inequality (Dynan and Rouse 1997; Siegfried 2018; Altonji, Blom, and Meghir 2012). The number and composition of undergraduate students who major in Economics is also important to the Economics profession. The well-documented underrepresentation of women in the Economics profession (Bayer and Rouse 2016; Lundberg and Stearns 2018) may have consequences for the questions studied by economists. There is also evidence that the policy recommendations of economists vary by gender (May, McGarvey, and Whaples 2014), suggesting implications for economic policy. The scarcity of women in the field begins with undergraduate Economics majors.

One channel that impacts major choice is information. In choosing a college major, students form beliefs about the earnings and utility they expect to receive from potential majors (Stinebrickner and Stinebrickner 2013; Zafar 2013), and revise these beliefs in response to new information (Wiswall and Zafar 2014; 2015). Moreover, women appear to be more sensitive than men to grades in their introductory Economics courses (Rask and Tiefenthaler 2008; Avilova and Goldin 2018).

We designed a randomized control trial to test whether students respond to messages about majoring in Economics, and whether this response varies by student gender. The experiment included more than 2,000 students enrolled in Economics Principles courses at Oregon State University. Interventions proceeded in two phases. In the first phase, we randomly assigned students to receive messages emphasizing the rewarding careers or financial returns associated with the Economics major. The rewarding careers message took two forms, a video produced for wide distribution by the American Economic Association (we henceforth refer to this message as "AEA video□) or a local version featuring current and recent Economics students at the university (henceforth, "OSU video□), allowing us to test for role model effects. The financial returns message (hereafter referred to as "earnings information□) contrasted salaries for Economics graduates and those from other majors. We compare these groups to students receiving no email ("control□) and to a group receiving a "placebo" message with basic information about the major.

In the second phase, all students receiving a grade of B- or better received a message after the course ended encouraging them to major in Economics. For a randomly chosen subset of these students, the message included an additional "resilience" message acknowledging that Economics can be difficult, describing the benefits of a "growth mindset,□ and encouraging students to persist in Economics even if their grade was disappointing (e.g., if they expected an A but received a B). Our analysis follows a pre-

registered plan (Pugatch and Schroeder 2019), with analysis not specified in this plan explicitly labelled as exploratory.

We find that email messages increased the probability that a student went on to major in Economics. Although effect sizes were less than 2 percentage points, the magnitudes rival the control group proportion majoring in Economics. Moreover, because the outcome is measured by administrative data collected in the academic year following treatment, the effects represent a durable change in revealed preference.

All effects were driven by male students, however. We find no effects among female students, even when considering a continuous measure of interest in the major. Null effects for female students persist across several subsamples and specifications. Among the full sample of students, we find no effects of any message on intentions to minor in Economics. We also find no effects of the Phase Two intervention.

Using our results on the differential effects of the intervention among male and female students, we conduct a thought experiment. How would the male/female ratio of Economics majors change if sending the most effective message to all Principles students became departmental policy? In this scenario, the male/female ratio would rise from 1.4 to 2.7, an increase of 96%. For students earning a B- or better, the implied increase is 166%. Even under a more conservative scenario which averages over the effects of all message types, the male/female ratio would rise by 54%. Our results should therefore sound a note of caution about the potential for simple nudges to exacerbate inequalities within Economics.

We find several surprising results when comparing effects of different messages. In the full sample, the varying treatment messages were no more effective at increasing Economics majors than the placebo email. This result may be explained by low engagement with the experimental email messages, as most students opened the emails but did not click on the associated links. When the outcome is self-reported likelihood of majoring in Economics on a 0-100 scale, the earnings information and AEA video messages *reduced* likelihood to major by 2.6 and 2.2 percentage points, respectively. These negative effects were driven by male students on the intensive margin, however, suggesting that resistance to our marketing messages was concentrated among students unlikely to become Economics majors in any case.

We contribute to the burgeoning literature on promoting interest in undergraduate Economics, particularly among women and other groups underrepresented in the field. The large scale and negligible marginal cost of our experiment—the interventions consisted of a single email in each phase—help to understand the frontier of informational nudges to promote undergraduate Economics. We complement Bayer, Bhanot, and Lozano (2019), who test similar messages among incoming students at liberal arts colleges, by studying introductory Economics students at a less-selective public university. Both studies find positive effects of around 1 to 3 percentage points, with no significant effects for female students. Unlike Bayer, Bhanot, and Lozano (2019), however, we find positive, precisely estimated effects for male students, whereas their effects are statistically distinguishable

from zero only in the full sample. Together, these studies highlight the promise and limitations of light-touch interventions promoting undergraduate Economics.. We extend their results to encompass the possibility that informational nudges may increase inequalities within Economics.

Our work takes inspiration from the Undergraduate Women in Economics (UWE) challenge (Avilova and Goldin 2018), though it is not formally a part of that project. We build on important recent UWE contributions by Li (2018) and Porter and Serra (2019), using similar interventions but overcoming some limitations. Li (2018) tests various combinations of information, nudges, and mentoring for Economics students. She finds that information combined with a nudge increased the probability of majoring in Economics for female students whose grades were above the median. The information referred to career prospects and salaries in Economics, as well as the grade distribution in the class; the nudge was an encouraging message mentioning the student's high performance in the class. Because female students with grades above the median received all treatments, the effects of these interventions could not be identified separately. By contrast, our study was designed to separately identify the effects of encouragement, salary, and career information.

In another experiment, Porter and Serra (2019) find that female students were significantly more likely to major in Economics when a female role model visited their Principles class. The role models were two alumnae of the institution and spoke to students about how Economics helped in their careers. By contrast, our experiment presents role models in the form of video links sent via email, which has lower marginal cost and can scale more easily to reach larger numbers of students.[1] Our experiment also varies whether role models are from the students' institution or elsewhere, in order to test whether a role model's background matters to the student response.

Why did the interventions in Li (2018) and Porter and Serra (2019) increase female interest in Economics, whereas those in Bayer, Bhanot, and Lozano (2019) and our study did not? While the former experiments included personal interaction with students—via class presentations, mentoring, or role model visits—the latter included only impersonal, electronic communication. Greater engagement may therefore be required to increase female interest in Economics.

If more direct engagement with female students is key, college and university Economics departments face a dual challenge to increase gender diversity. First, programs to generate this engagement are relatively new, with the most effective program elements still unknown. These programs must overcome not only a skewed gender ratio in favor of men, but also a less inclusive environment for women and underrepresented minorities (Stevenson and Zlotnik 2018; Paredes, Paserman, and Pino 2020; Bayer, Bhanot, et al. 2020). Second,

[1] Additionally, because instructors in Porter and Serra (2019) hosted alumni role models in their classes, the instructors may have changed their subsequent behaviour in unobserved ways to reinforce the treatment, leading to an overestimate of the role model effect. The instructors in our experiment were blinded to student treatment status.

scaling programs to reach more students, particularly at large research institutions like Oregon State University, remains a challenge. Shifting the content of introductory courses (e.g., Bayer et al. 2020; Benjamin, Cohen, and Hamilton 2020; Bowles and Carlin 2020) may offer more promise than targeted messaging alone.

Finally, our work also contributes to a broader discussion in the literature on how to increase diversity in Economics (Bayer and Rouse 2016; Buckles 2019; Lundberg and Stearns 2019). In addition to UWE and related efforts to promote diversity in undergraduate Economics (Bayer, Bhanot, et al. 2020; Buchanan and Deyo 2020), other recent contributions have focused on graduate students (Boustan and Langan 2019) and faculty (Ginther et al. 2020).

2 Research Design

2.1 Context

The study took place at Oregon State University (OSU), which enrolls more than 25,000 students and is the largest university in the state. While female college students outnumber male students nationally, at OSU the reverse is true. In 2018-2019, the academic year of the study, the university's main campus awarded bachelor's degrees to 4,141 students, 48 percent of whom were female. Of these, 83 Economics degrees were awarded, 18 of them to female students (22%). Avilova and Goldin (2018) define a "conversion rate," which gives the ratio of male to female Economics majors at a school scaled by the ratio of male to female bachelor's degree recipients at that school overall.[2] Nationally, the conversion rate averaged 2.9 over the period of 2011-2015 (Avilova and Goldin 2018). At OSU's main campus, where our study was conducted, the conversion rate for the academic year ending in 2019 was 3.1, indicating a slightly higher ratio of male students, even correcting for the fact that OSU has a greater proportion of male students overall.

OSU operates on a quarter system, with three 10-week terms during the regular school year. Two Economics Principles courses are offered: Introduction to Microeconomics and Introduction to Macroeconomics. While the Economics major is relatively small, as is common among land-grant universities such as OSU, the Principles classes fulfill course requirements for 40 other majors, 15 minors and one certificate program. The largest group of these students (49% of our sample) comes from OSU's College of Business, followed by the College of Engineering (26%). The sample includes eight sections of Introduction to Microeconomics, four of which were taught by a female instructor, and five sections of Introduction to Macroeconomics, none of which were taught by a female instructor. A majority of Principles students take one course or the other, but students who take both may take them in either order, and occasionally take them simultaneously.

[2] The conversion rate is computed as [(Male Economics BAs/All Male BAs)/(Female Economics BAs/All Female BAs)].

Oregon State students declare an intended major upon admission. Undecided students may choose "University Exploratory Studies," which assigns them to an academic advisor and other services before they choose a disciplinary major. Additionally, international and transfer students may choose similar exploratory programs as their provisional major until they are ready to declare a disciplinary major. In our sample, 8% of students fall into these provisional categories at baseline. Students who want to switch to Economics from another major do not need departmental approval, but must schedule an appointment with an academic advisor lasting at least half an hour. There is no additional required paperwork and the appointments are easy to get, but they do present a cost to changing majors. These institutional features may generate path dependence in major choice compared to institutions that treat "undecided" as the default major for entering students.

2.2 Experiment

2.2.1 Phase One

The experiment took place in two phases. In Phase One, all students registered in Economics Principles courses on the main Corvallis campus were invited to participate in the study. We randomly assigned Phase One participants to the following groups:

1. *Control*: no encouragement message
2. *Placebo*: encouragement message based on description of Economics major on departmental website.
3. *Earnings information*: placebo content, plus information on earnings of Economics graduates one and fifteen years after graduation.
4. *AEA video*: placebo content, plus link to American Economic Association video "A career in Economics…it's much more than you think."
5. *OSU video*: placebo content, plus link to video testimonials by current Economics students and alumni of Oregon State University.

Messages were sent once, in Week 8 of the 10-week course, from the email account of the student's instructor.[3] The experiment was repeated in each of the three quarters (fall, winter, and spring) of the 2018-2019 academic year. All emails had the same subject, "ECON [201/202]: Consider majoring in Economics!" Figure A1 shows each message. We assigned treatment at the individual student level, stratifying by course section and class year (freshman/sophomore/other). Within strata, students had an equal probability of being assigned to each group. The total number of students in each group differed due to uneven strata sizes. Since the same student may take both introductory courses in the same

[3] In both phases, we (the researchers) sent the messages. Instructors and the Economics Academic Advisor were unaware of the treatment status of individual students.

term, or repeat the same course in multiple terms, it is possible to be assigned to a treatment group more than once. In addition to presenting results using student course enrollment as the unit of analysis, we therefore also include results that account for repeated observations from the same student.

2.2.2 Phase Two

In Phase Two, students who participated in Phase One and earned a grade of at least B- or above were enrolled in the study. We randomly assigned these students to receive one of two emails:

1. *Control*: message of congratulations on their course performance, with encouragement to major in Economics.
2. *Treatment*: control message content, plus "resilience" message acknowledging that Economics can be difficult and encouraging adoption of a growth mindset.

Phase Two messages were sent once, at the beginning of the academic term following the Phase One course, from the email account of the Economics Academic Advisor. Both messages had the subject, "ECON invite." Figure A2 shows each message. Treatment assignment stratified by course section, class year (freshman/sophomore/other), and course grade.

2.3 Statistical Power

We estimated statistical power to detect treatment effects as part of an analysis plan completed prior to obtaining data from the experiment (Pugatch and Schroeder 2019). Our power calculations assumed a sample size of 3,000 students, test size of 5%, power of 80%, and study arms of equal size. We calculated minimum detectable effects (MDEs) for comparisons of a single treatment arm versus the control group for the outcome of majoring in Economics (binary and self-reported 0-100 likelihood).[4] Under these assumptions, the experiment had 80% power to detect increases of 4.1 percentage points in Economics majors (binary) and on 6.2 percentage points on the likelihood of majoring in Economics (0-100). Although these magnitudes are relatively large, the power calculations were arguably conservative. They did not consider increased precision from including baseline outcomes. Moreover, pooling all encouragement emails in Phase One nearly halved the MDE for majoring in Economics (3.3 percentage points).

[4] In Phase One, we expected 3,000 participants spread evenly across five groups, yielding an average group size of 600. In Phase Two, we expected 40% of Phase One participants to qualify for the study, yielding a sample of 1,200, or two groups of 600 each. We assumed baseline means of 5% and 15% for the binary and continuous measures of majoring in Economics, respectively, based on past sections of introductory Economics courses. Supplemental Appendix Figures 1-2 plot statistical power against MDEs.

2.4 Data

We collected data on study participants from baseline and endline surveys, and from administrative sources. The baseline survey was conducted in the first two weeks of each term. The endline survey was conducted in Weeks 9-10 (recall that messages were sent in Week 8 of the 10-week term). To incentivize responses, students earned course credit for completing the surveys. Surveys included questions about perceptions of the Economics major and the likelihood of majoring or minoring in Economics. Administrative data on experimental take-up include whether students opened treatment emails, clicked on links within those emails, or scheduled appointments with the Economics Academic Advisor. The university registrar provided data on student demographics and grades, as well as student major. We use an indicator of whether a student was an Economics major in Winter 2020 as our main outcome of interest.[5] Estimates for this outcome therefore represent the effects of the treatments two to four terms later, ensuring that students had sufficient time to reflect on the information and take the necessary administrative steps. For other outcomes—minoring in Economics (0/1) and the likelihood of majoring or minoring in Economics (0-100)—we rely on stated preferences from the endline survey.[6] Table A1 presents a list of variables from these data sources used in our analysis.

3 Methodology

3.1 Main analysis

Our main method for analyzing the results of the experiment is the ordinary least squares (OLS) regression. For Phase One, the main regression specification is:

$$y_{is} = \alpha_0 + \alpha_1 placebo_{is} + \alpha_2 earnings_{is} + \alpha_3 AEAvideo_{is} + \alpha_4 OSUvideo_{is} + \theta y_{0is} + \gamma_s + \varepsilon_{is} \quad (1)$$

In equation (1), i indexes students; s indexes strata; y is an outcome of interest, such as majoring in Economics; $placebo, earnings, AEAvideo$ and $OSUvideo$ are indicators for belonging to these treatment groups (the control group is the omitted category); y_0 is the baseline outcome; γ is a strata dummy; and ε is an error term. The inclusion of strata dummies isolates the random variation in treatment status within strata. The inclusion of the baseline outcome adjusts for any differences among baseline outcomes between treatment groups and makes estimates more precise. We estimate heteroscedasticity-robust

[5] We did not observe an administrative major in Winter 2020 for 48 students in our final sample. For these students, the outcome is an indicator for being an Economics major in the last term we observed the student, provided this was at least one term later than when the student was in the experiment.

[6] The registrar reports academic minors only upon graduation, which would lead to a high rate of missing data for our sample.

standard errors. Our coefficients of interest are α_1 through α_4, which measure the difference in outcomes between each treatment arm and the control group.

For Phase Two, the main regression specification is:

$$y_{is} = \beta_0 + \beta_1 T_{is} + \theta y_{0is} + \gamma_s + PhaseOne_{is}\alpha + \varepsilon_{is} \quad (2)$$

where T is an indicator for assignment to the Phase Two treatment group; $PhaseOne_{is}$ is a vector of Phase One treatment assignment indicators; and all else is as in equation (1). Our coefficient of interest is β_1, which measures the difference in outcomes between the treatment and control groups within the Phase Two sample (conditional on Phase One treatment).

The coefficients of interest in equations (1) and (2) all represent intent-to-treat (ITT) effects. Our outcome variables of interest for both phases are the choice to major or minor in Economics, and enrollment in an Economics course after the term of the experiment. Additionally, for Phase One we analyze the likelihood of majoring or minoring in Economics, on a 0-100 scale, from the Phase One endline survey.

We test the null hypotheses that each coefficient of interest (α_1 through α_4 in equation [1], and β_1 in equation [2]) equals zero. For equation (1), we test the messages jointly as well as all pairwise comparisons of messages, as specified in the analysis plan.

3.2 Heterogeneous Effects

We test for all treatment effects specified in Section 3.1 in the full sample and separately for male and female students. We also test for Phase One treatment effects (i.e., equation [1]) within the subsample of students who earn a B- or above. Finally, we test for Phase Two treatment effects (i.e., equation [2]) separately for students whose grade was in the "A" category (A- or above), students whose grade was in the "B" category (B-, B, or B+), and students who received a "disappointing" grade, where disappointment is defined as a student who expected an A grade but received a grade of B+ or below. Each of these subgroups appeared in the analysis plan.

3.3 Mechanisms

We estimate equation (1) using several potential mechanisms from the Phase One endline survey as dependent variables. These potential mechanisms are separate indicators for the biggest appeal of the Economics major: fun to study, future income, and rewarding career. We test for these mechanisms in the full sample and separately for male and female students. Each of these mechanisms appeared in the analysis plan.

4 Results

4.1 Baseline balance and attrition

Table 1 presents sample sizes, separately for Phase One (Panel A) and Phase Two (Panel B). Over the three terms of the study, enrollment in Economics Principles courses totaled 2,679. Of those, 2,277 participated in the study, or 85 percent.[7] Among participants, 803 were female, or 35 percent. Introductory microeconomics accounted for 1,420 participants, with the remainder in introductory macroeconomics. Phase Two had 981 participants, as earning a B- or better was an additional condition. The participation rate and female proportion in Phase Two were similar to Phase One.

The interventions were intended to encourage students to major or minor in Economics. Before assessing their success, in Table A2 we consider the overall frequency of transitions in or out of the Economics major and minor. Among study participants, 71 students majored in Economics by Winter 2020, or 3.2% of the sample (Panel A). Of these, 33 students—46% of Economics majors—were not Economics majors at baseline. By contrast, only 6 students switched out of Economics. Although these numbers are small in absolute terms, they suggest enrollment in Economics Principles courses often precedes entry to the major. However, the likelihood of switching out of the major was higher among female than male students. Students switching into Economics tended to come from Business and related fields, or were previously undecided (Table A3). Political Science was the most popular destination for students switching out of Economics.

A similar number of students switched into the Economics minor as into the major (Panel B). However, 44 students switched out of the minor, against 34 who switched in.[8] Transition rates in and out of the minor are similar between male and female students.

Next, we check balance across treatment assignment by comparing observable student-level characteristics recorded at baseline. Following the analysis plan, the observable characteristics are female, first generation college student, race, high school GPA, OSU GPA, expected course grade, intention to major in Economics (0/1), intention to minor in Economics (0/1), likelihood of majoring in Economics (0-100), and likelihood of minoring in Economics (0-100). We run an ordinary least squares (OLS) regression of each baseline characteristic on indicator variables for assignment to each treatment arm and a full set of strata dummies. In other words, we run equations (1) and (2), replacing the left-hand side outcome with a baseline characteristic and omitting the baseline outcome from the right-hand side.[9] For each equation, we conduct an F-test that all of the regression coefficients on the treatment indicators equal zero.

[7] To participate, students had to be at least 18 years old, complete the baseline survey, and provide consent.

[8] Of the 44 students who switched out of the Economics minor, only 3 switched into the Economics major.

[9] In these balance checks, we also excluded the Phase One treatment assignment dummies from equation (2), as all baseline characteristics were determined prior to treatment assignment.

Table 2 presents results. At baseline, the Phase One control group was 32% female, 57% white, and 14% first generation, with an entering high school grade point average (GPA) of 3.46 and Oregon State GPA of 3.05.[10] Most students (94%) expected to earn an A or B in the course. Three percent and five percent of students said they intended to major or minor in Economics, respectively. When asked to state the likelihood of majoring or minoring in Economics on a 100-point scale, however, the average values were 17.5 and 26.2, suggesting openness to choose Economics exceeding the binary measures. Phase Two students, i.e., those who earned at least a B-, had broadly similar characteristics, though their baseline GPAs and expectations of earning an A were unsurprisingly higher. Their intentions to major and minor in Economics were also similar to Phase One, suggesting that high performers in Economics were initially no more likely to choose the discipline.

Most characteristics were balanced across treatment arms, although we observe a few statistically significant differences in both phases. We ascribe these imbalances to bad luck, as randomization fell under our control as researchers. The most notable difference is the overrepresentation of female students in the treatment group for both Phases. Disaggregating results by gender will circumvent this difference when interpreting results.

The final row of Table 2 reports endline survey completion rates. Failure to complete the endline constitutes survey attrition. Endline completion rates were high, ranging from 85-89% across Phase One treatment arms and 94-95% in Phase Two. Treatment status did not significantly predict attrition. By definition, there is no attrition in the administrative data.[11]

4.2 Take-up and knowledge

A necessary condition for the intervention to influence perceptions or intentions to major in Economics is for treated students to read the encouragement messages. Table 3 presents measures of take-up. Most treated students opened the email, with Phase One opening rates of 64-71% across treatment arms. All emails included a link to schedule an appointment with the Economics Academic Advisor. The AEA and OSU video treatments also had links to the respective videos. Click-through rates were low, however, reaching a maximum of 2% in the earnings information arm. Nonetheless, these rates were jointly significantly greater than the control group, which had a zero click-through rate by definition. The Phase One treatment therefore succeeded in calling student attention to the Economics major beyond the status quo represented by the control group.

Appointments with the Economics Academic Advisor reached similar rates as email clicks, suggesting the clicks translated into appointments, although in this case the difference with the control group was not statistically significant. We do not observe walk-in appointments or other interactions between students and the advisor.

[10] Oregon State GPA is missing for students in their first term at the university.

[11] The administrative data fails to report post-study major for 39 students, however, because the term in which they took Principles is the last term we observe them enrolled at the university.

Self-reported rates of video viewership were higher in the treatment arms than the control group, as expected, though the difference is significant only for the AEA video. However, the viewership results are puzzling for several reasons. First, viewership rates range from 19-23%, far exceeding click-through rates. Second, viewership rates are similar across all treatment groups, even if no video link was included in the message. Finally, viewership rates for the AEA video are similar to the OSU video within the AEA video treatment arm, and vice versa. We attribute these anomalies to weaknesses in the survey design. Problems inherent to self-reported data are well known. The question wording may have generated additional confusion. The endline survey asked, "Before taking this survey, had you seen the video [video name and link]?" Students may have thought that viewing the video was a course requirement they were asked to complete, or remembered seeing a link to a video, even if they did not watch it. Students may also have been exposed to the videos through channels other than our intervention. One instructor in the study regularly shows the AEA video in class, for instance. The OSU video may have generated interest beyond its treatment arm because it featured current students, and spillovers from treatment groups may have occurred if students shared or watched the videos together.

The final rows of Table 3 show rates of correct responses to questions about the median salaries of Economics majors one and fifteen years after graduation. This information was provided in the earnings information treatment. For both questions, correct response rates are similar across all groups. Moreover, the earnings information group failed to have the highest correct response rate for either question. It is possible that information about future earnings for Economics graduates was already well known, that the information provided in the treatment was not sufficiently salient, or some combination of these.

Overall, students responded modestly in the short run to the Phase One treatments. Most opened the messages, but other immediate changes in behavior and knowledge intended by the messages largely failed to materialize. These results suggest potential weak links in the causal chain between the additional information included in the treatment messages and changes in intentions to study Economics.

In Phase Two, rates of opening emails were similar to Phase One, but clicking links and advisor appointments were more common. This greater apparent interest in Economics may reflect the better course performance of Phase Two participants. Surprisingly, the rate of clicked links was significantly higher among Phase Two control students, suggesting lower receptiveness to the resilience message than we hoped.[12]

[12] We do not report other measures of take-up and knowledge in Table 2 because these were measured before Phase Two treatment occurred.

4.3 Outcomes

4.3.1 Phase One: Majoring in Economics

Table A4 shows various points in the distribution of the change in self-reported likelihood (0-100) of majoring in Economics between baseline and endline. Note that the median change is zero across all experimental groups, for both the full sample and separately for male and female students. Moreover, for female students the 25th and 75th percentile of changes are also zero across all groups. When combined with the data on transitions in Table A2, these results suggest strong path dependence in preferences for the Economics major among study participants. The supplemental appendix provides histograms of these distributions. These descriptive statistics do not substitute for our formal analysis, however.

The results of estimating equation (1) appear in Table 4. In the full sample of students, the placebo had a significant effect, increasing students' likelihood of declaring an Economics major by 1.9 percentage points, significant at 5 percent (column 1). This effect was driven by male students, whose likelihood of majoring in Economics increased by 2.5 percentage points after receipt of the placebo, also significant at 5 percent (column 2). The earnings information had a weakly significant effect in the full sample, with a coefficient of 1.5 percentage points (column 1). These effects are similar in magnitude to the control mean; around two percent of control group students were Economics majors by the following winter term, with a slightly higher figure of 2.7 percent for female students. Moreover, because the outcome is measured by administrative data collected in the academic year following treatment, these positive effects represent a durable change in revealed preference.

None of the treatments had a significant effect on majoring in Economics for female students (column 3). While this could in part be due to the smaller sample size, note that the effect of the placebo for male students is weakened when female students are added to the sample. Over all three samples, the coefficients on the treatments are not statistically different from the coefficient on the placebo, and the four emails are not jointly significant. Taken together, these results provide some evidence that an email message from a course instructor can successfully encourage male students, at least, to major in Economics. The treatment emails were in general no more effective than the placebo, which is unsurprising given the low student engagement with the video messages in particular.

The regressions in Table 4, columns (4)-(6) show results for students who earned a B- or higher in the course. The placebo continues to have a significant effect, increasing the likelihood of becoming an Economics major by 4 percentage points (column 4). This effect is again driven by male students, whose likelihood of majoring in Economics increased by 6 percentage points, significant at one percent (column 5). Earnings information is weakly significant in the male and full samples, and the OSU video has a weakly significant effect for male students, with coefficients in the range of 3 to 4 percentage points (columns 4-5).

These larger effect sizes for better-performing students compared to the full sample are consistent with similar findings by Li (2018). In contrast to her results, however, we fail to find effects for better-performing female students.

One concern about the results in columns (1)-(6) is that students who took more than one Economics Principles course during the academic year, either because they took both Micro and Macro, or because they repeated a class, will appear in the data more than once. These students may also have been treated more than once. To address this issue, we repeat the analyses above, including each student only once and using a measure of the intensity of the treatments. The treatment variables, instead of being dummies, now give the number of times a student was exposed to a given treatment. For example, a student who took introductory microeconomics and then introductory macroeconomics may have received one placebo email and one AEA-video email, or received the OSU video twice. The baseline controls are those reported in the baseline survey the first time the student took a Principles class; the endline survey outcomes are from the last time a student was observed. These regressions also control for whether a student has taken a previous course as part of the study. There are 1,883 unique students in this dataset.[13] Although this specification did not appear in our analysis plan, we think it is a logical extension to the main specification given the high proportion of students appearing multiple times in the study.

Results from these regressions are presented in Table 4, columns (7)-(12). These results are qualitatively similar to those using the full sample of observations. Weakly significant positive effects on majoring in Economics continue to appear in the male subsample and full sample for the placebo, with similar magnitudes (columns 7-8). In the female subsample, no treatment increases the probability of majoring in Economics, but the OSU video now has a negative effect, reducing the likelihood by 1.7 percentage points, significant at 5 percent (column 9). Results for students who earned a B- or higher in their most recent principles course are similar, with the effects of the placebo on majoring in Economics again strengthening for male students and the full sample. The negative effect of the OSU video for female students is no longer significant, suggesting that any risk of driving female students away from the major through outreach efforts may be concentrated among students with lower grades.[14]

[13] Removing duplicates drops 332 instances of a student appearing a second time; 22 instances of a student appearing a third time; and one instance of a student appearing a fourth time. Together, these dropped observations represent 16% of the original sample size of 2,238. Of the 332 students who appear more than once, 236 took both macro and micro without repeating either; 78 took one course and repeated it one or more times; and 18 took both courses and repeated one. For students who participated in a pilot version of this study in Spring 2018, the number of times they received a treatment includes the pilot emails.

[14] Tests for differential effects of the treatments on students from underrepresented minority groups produced null results, including repeating the Phase One regressions for majoring in Economics on this subsample, as well interacting an underrepresented-minority dummy with an indicator for having received any treatment. These tests were limited by small sample sizes and the fact that only three students identified by the university as underrepresented minorities (American Indian or Alaska Native; Black or African American; Hispanic; and Native Hawaiian or Pacific Islander) switched into the Economics major over the course of the study.

Table 5 shows results for a continuous, self-reported measure of the likelihood to major in Economics at the time of the endline survey. Here, the treatments had effects in the opposite direction as for the binary measure from administrative data. On a scale of zero to 100, the average likelihood of majoring in Economics reported by students in the control group at the end of the term was around 19, slightly higher for male students, and about 17 for female students. Earnings information reduced this likelihood by 2.6, significant at 5%. The AEA video reduced the likelihood by 2.2, significant at 10%. Both effects were driven by male students. The coefficients were not significant in the female subsample, but for male students the effect of earnings information and the AEA video were negative and relatively large; both are -4.0 and significant at 5%. In the male subgroup, the treatments were jointly significant, and the earnings information and AEA video coefficients were significantly different from the placebo.

Overall, we find that a simple nudge—a single email during a 10-week course—can increase the probability of majoring in Economics by the following academic year. Effects are driven by male students, with no statistically significant effects for female students. We also find negative effects of some messages on a continuous measure of interest in the Economics major, also driven by male students. We attempt to reconcile these seemingly contradictory results in the subsection on robustness checks.

4.3.2 Phase One: Minoring in Economics

Table 6 presents results for minoring in Economics. This outcome is not reported in administrative data. Instead, we rely on self-reports from the endline survey, both binary (i.e., answering "Economics" to the question, "What is your intended minor?") and continuous (likelihood on 0-100 scale). None of the treatments significantly affected the binary measure of minoring in Economics. For female students earning a B- or better, earnings information increased the continuous likelihood (0-100) of minoring in Economics by 7.3 points, significant at 10% (column 12). This result suggests that, although better-performing female students do not consider future earnings sufficient reason to major in Economics, it may make them more open to minoring in Economics.

4.3.3 Phase Two

Table 7 presents the results from Phase Two of the interventions. Here, we estimate equation (2), where the treatment is the addition of a "resilience" message to the email sent to all students earning a B- or higher in a Principles class, encouraging them to major in Economics. We do not find any impact of the resilience message on majoring in Economics. For students who took a Principles class in Fall 2018, we are additionally able to see whether the student took a subsequent Economics course in the next two terms, but do not find any impact of the treatment on this outcome, either. These results persist whether a student received an A or B grade, and for students whose grade was disappointing (i.e.,

students who expected an A but earned a lower grade), who we expected would respond most to the resilience treatment. [15]

We ascribe these null results to lack of significant differentiation between the treatment and control messages. The treatment message differed from the control only in a single paragraph of text which could be easily overlooked by busy students. Students may also have paid less attention to emails that came from an academic advisor they had not met, as opposed to their own instructors. Similar rates of opening emails and clicking links in Phase Two suggest no meaningful difference in engagement with the messages between treatment and control.

4.3.4 Robustness checks

One concern with our results, particularly for majoring in Economics in response to Phase One treatments, is that they are potentially spurious. When testing such a large combination of outcomes and treatments, some statistically significant results should appear by chance. Indeed, when correcting for multiple hypotheses to control the false discovery rate (FDR), none of the corresponding q-values fall below conventional significance thresholds.[16] This correction is potentially severe, however, given the number of hypotheses we test (132 in each of Tables 4-6, for instance).

To improve power and reduce the number of hypotheses tested, in Table A5 we bundle all treatments into a single indicator.[17] We find that assignment to any email increases the probability of majoring in Economics by 1.3 percentage points, significant at 10% (Panel A, column 1). As before, the effect is driven by male students (column 2). We also find a 1.7-point decline in the continuous measure of majoring in Economics, again driven by male students (columns 4-5). We find no significant effects for minoring in Economics. Panel B repeats the exercise, this time using an indicator for opening the treatment email, instrumented by random assignment. These specifications measure the average treatment effect on the treated (ATT), where treatment refers to opening the email. Effect sizes increase in magnitude, with the pattern of significance identical to Panel A.

Across several samples and specifications, we therefore find the same pattern of results. Random assignment to an informational email increases the share of students majoring in Economics, particularly for male students and the basic "placebo" email. Yet these same male students react negatively, particularly to more nuanced messages, when stating their

[15] We similarly obtain null results when repeating the Phase Two regressions using the intensity-of-treatment variables and removing duplicates from the sample. Results in supplemental appendix.

[16] The q-value is the analogue of the conventional p-value when controlling the FDR at level q. In other words, a q-value of 0.05 means the null hypothesis would be rejected only when permitting the FDR to be no less than 5%. We calculate sharpened q-values for Tables 4-6 using the procedure of Benjamini, Krieger, and Yekutieli (2006), as implemented by Anderson (2008). Results available upon request.

[17] The analysis in Table A5 is exploratory.

preferences on a continuous scale. Null results for female students when bundling treatments into a single indicator suggests lack of power does not explain these results.

What explains the seemingly contradictory response of male students to the intervention? Before exploring mechanisms in greater detail in the next subsection, we first check whether differences in outcome measurement help resolve the puzzle. Our two measures of majoring in Economics differ in three dimensions: 1) scale (binary versus continuous), 2) source (administrative versus self-reported), and 3) timing (after versus during the course). We explore these differences in Table A6. Panel A reprises the main results from Tables 4-5, columns (1)-(3) for reference. Panel B of Table A6, columns (1)-(3) uses binary intention to major in Economics from the endline survey as the outcome, allowing us to match the timing and self-reporting of the continuous measure. Panel B, columns (4)-(6) recode the continuous measure as binary, using a threshold of 90% as the indicator for majoring in Economics. For both outcomes, we find no statistically significant treatment effects. The timing and source of the data therefore cannot explain the opposing signs we find in Panel A. Instead, the negative response found using the continuous measure represents effects on the intensive, rather than extensive margin. The (male) students who disliked the messages were unlikely to major in Economics even in the absence of the treatment.

Finally, in Table A6, Panel C, we alter the timing of the administrative outcome to be the first observation after the course concludes. This outcome therefore captures revealed preference for the Economics major in the short term, as opposed to the longer-term measure in our main estimates. Here we again find no statistically significant effect. Differences in columns (1)-(3) among Panels A-C therefore demonstrate that the treatment changed behavior only in the long run.

A remaining puzzle is why we find no durable effects for female students across specifications. A potential explanation lies in Table A4, Panel C. Across all treatment arms, at least half of female students report no change in their likelihood of majoring in Economics, even when allowed to state their preference on a 0-100 scale. If female students' prior beliefs about Economics are particularly impervious to change, it is unsurprising that a single email message will not change their behavior.

4.4 Mechanisms

To explore channels through which the treatment variables could be affecting students' choices, we estimate equation (1) using potential mechanisms from the Phase One endline survey as dependent variables. Results are presented in Table 8. The AEA video has a negative and significant effect on male students reporting that the biggest appeal of the Economics major is that it is fun to study, reducing the likelihood of this response by six percentage points. This is a potential explanation for the result that the AEA video decreased male students' self-reported likelihood of majoring in Economics. The AEA video had a weakly significant effect of the same magnitude increasing all students' likelihood of

reporting that the biggest appeal of Economics is future income. Among female students, the AEA video decreased the likelihood of selecting "a rewarding career" as the biggest draw of Economics by eleven percentage points. Earnings information increased the proportion of female students reporting "future income" as the biggest appeal of Economics by 10 percentage points. This result aligns with prior expectations, though we know from other results that it was insufficient to increase majoring or minoring in Economics among female students. None of the other treatments had significant effects on the mechanism responses. Additionally, no treatment had an impact on whether a student took a subsequent Economics course (columns 10-12), suggesting this was not the channel through which the treatment increased Economics majors.[18]

4.5 Discussion

Our main results in Table 4 demonstrate a positive effect of the placebo on majoring in Economics, driven by male students. Suppose Oregon State University adopted the placebo message as Economics Department policy. How would the policy change the number of Economics majors and the male/female ratio?

Table 9 presents results from this counterfactual exercise. The "control" scenario presents the status quo, by extrapolating the control group proportion majoring in Economics by Winter 2020 (bottom of Table 4) to the entire study population. In this scenario for the full sample, 29 male students and 21 female students would be Economics majors, for a male/female ratio of 1.4 (Table 9, Panel A).[19] The "placebo" scenario adjusts the proportions according to the gender-specific point estimates for this intervention (Table 4, columns 2-3). In this scenario, the implied male/female ratio rises to 2.7. This is an increase of 96% over the control scenario, a stunning change.

Panel B of Table 9 repeats the exercise for the subsample of students earning a B- or better. The increase in the male/female ratio under the placebo scenario is even more dramatic in this case, given the wide discrepancy in point estimates for these better-performing students (Table 4, column 5-6). The male/female ratio projects to 4.4, an increase of 166% over the control scenario. Panel C uses the "intensity sample" from Table 4, columns (8)-(9), i.e., one observation per student. Here, the male/female ratio in the control scenario rises to 2.3. In the placebo scenario, the ratio rises a further 58%, to 3.7.

A potential objection to this counterfactual exercise is that the placebo yields the highest point estimates and thus generates the largest effects, which we could not have known in advance.[20] Yet even when using point estimates for the bundled treatment (Table

[18] We obtain similar results when repeating this exercise using the intensity-of-treatment variables and removing duplicates from the sample. Results in supplemental appendix.

[19] This ratio falls well below the male/female ratio in Economics degrees awarded at Oregon State during the same year, 3.6 (Section 2.1). The discrepancy suggests further differential selection into the major by gender beyond Principles courses.

[20] The placebo estimates are also the most precise among treatment coefficients. Additionally, the placebo would be the preferred policy if the department's goal is to maximize

A5, columns 2-3), the male/female ratio rises to 2.1, or a 54% increase over the control scenario. Regardless of how we define the treatment, this exercise suffers from the well-known limitations of such counterfactual exercises in partial equilibrium. Nor does it account for further changes to the gender ratio that occur between the Principles courses and graduation. Our results nevertheless consistently suggest that a simple nudge can exacerbate the gender gap in the Economics major.

5 Conclusions

We find evidence that sending students information about Economics via email has the potential to significantly impact their choice of major. A single email containing basic information about the Economics major, as well as an email adding salary information to this basic message, had significant positive impacts on male students' declaration of an Economics major two to four terms after the intervention. These increases were around 2 percentage points, small in absolute terms but comparable in size to the control mean. Yet for a continuous measure of interest in Economics, male students reacted negatively to salary information and to a message featuring the rewarding careers and diversity in the Economics profession. This potentially puzzling result stems from different reactions of male students at the intensive and extensive margins of interest in Economics. Marginal students with sufficient interest in the field changed their major in response to the messages, while those unlikely to major in any case reacted negatively.

We did not find similar effects for female students. The bulk of female students maintained their initial interest in Economics throughout the course, regardless of treatment assignment, despite entering with a similar, if not slightly higher, level of interest to their male classmates. Extrapolating our estimates to the full sample, the placebo message would nearly double the male/female ratio among Economics majors.

Why does our simple intervention increase Economics majors among male students, but not female? One potential explanation is that a higher level of engagement is required to attract female students to Economics. Informational nudges, such as in our study and in Bayer, Bhanot, and Lozano (2019), can increase student interest in Economics. But neither our intervention nor theirs increased interest in Economics among the subsample of female students. By contrast, the interventions studied by Li (2018), which included mentoring of potential Economics students, and Porter and Serra (2019), in which female role models visited Economics courses, featured deeper engagement with students. These latter studies increased female interest in Economics.

Our results suggest limits to informational nudges to promote interest in Economics among women and other groups underrepresented in the field. Simple nudges could even exacerbate existing inequalities. But in identifying these limitations, our work also suggests

the total number of majors. Oregon State University pays piece rates to departments for student credit hours and degrees awarded, providing strong incentives to attract as many students as possible.

avenues for future research. An important remaining question is how departments can promote the major at greater scale in a way that reaches all students. One approach that scales relatively easily is changing the content of introductory courses, and future work may determine whether these changes complement or substitute for marketing efforts to diversify the student population. Pursuing these questions will help to better understand how to promote diversity in Economics.

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7 Tables

Table 1: Sample sizes

	Fall 2018	Winter 2019	Spring 2019	Total
	(1)	(2)	(3)	(4)
Panel A: Phase 1				
enrolled	878	803	998	2,679
participated	765	673	839	2,277
participation rate	0.87	0.84	0.84	0.85
participants by sex:				
male	499	424	551	1,474
female	266	249	288	803
female proportion	0.35	0.37	0.34	0.35
participants by course:				
microeconomics	518	394	508	1,420
macroeconomics	247	279	331	857
Panel B: Phase 2				
B- or better	335	349	440	1,124
participated	301	302	378	981
participation rate	0.90	0.87	0.86	0.87
participants by sex:				
male	200	194	252	646
female	101	108	126	335
female proportion	0.34	0.36	0.33	0.34
participants by course:				
microeconomics	223	188	239	650
macroeconomics	78	114	139	331

Table shows sample sizes by study phase. Participated means student was at least 18 years old, consented to participate in study, and completed baseline survey.

Table 2: Baseline balance

	Phase One (all students, within term)						Phase Two (B- or above, after term)		
	control	placebo email	AEA video	earnings information	OSU video	F-test (p-value)	control	treatment	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
female	0.32	0.41	0.36	0.35	0.33	0.05	0.30	0.38	0.00
	[0.47]	[0.49]	[0.48]	[0.48]	[0.47]		[0.46]	[0.49]	
white	0.57	0.58	0.59	0.58	0.61	0.84	0.65	0.65	0.95
	[0.49]	[0.49]	[0.49]	[0.49]	[0.49]		[0.48]	[0.48]	
Asian	0.08	0.08	0.06	0.06	0.07	0.67	0.07	0.05	0.08
	[0.27]	[0.27]	[0.24]	[0.24]	[0.26]		[0.26]	[0.21]	
Hispanic	0.10	0.08	0.09	0.10	0.07	0.41	0.07	0.06	0.49
	[0.30]	[0.27]	[0.28]	[0.30]	[0.25]		[0.26]	[0.23]	
1st generation	0.14	0.08	0.09	0.09	0.12	0.03	0.09	0.07	0.37
	[0.35]	[0.27]	[0.29]	[0.29]	[0.32]		[0.29]	[0.26]	
High school GPA	3.46	3.50	3.45	3.49	3.46	0.20	3.61	3.63	0.40
	[0.40]	[0.37]	[0.42]	[0.37]	[0.42]		[0.39]	[0.40]	
Oregon State GPA	3.05	3.05	3.07	3.10	3.05	0.52	3.35	3.40	0.11
	[0.56]	[0.56]	[0.56]	[0.52]	[0.58]		[0.46]	[0.51]	
expected grade: A	0.48	0.49	0.48	0.54	0.52	0.22	0.66	0.71	0.05
	[0.50]	[0.50]	[0.50]	[0.50]	[0.50]		[0.47]	[0.45]	
expected grade: B	0.46	0.45	0.47	0.41	0.42	0.22	0.32	0.27	0.09
	[0.50]	[0.50]	[0.50]	[0.49]	[0.49]		[0.47]	[0.44]	
intends to major in Economics	0.03	0.02	0.03	0.04	0.05	0.18	0.04	0.05	0.64
	[0.17]	[0.14]	[0.18]	[0.19]	[0.21]		[0.20]	[0.22]	
intends to minor in Economics	0.05	0.03	0.04	0.06	0.03	0.28	0.04	0.05	0.41
	[0.22]	[0.18]	[0.20]	[0.24]	[0.18]		[0.20]	[0.22]	
likelihood of majoring in Economics (0-100)	17.45	17.25	19.16	20.12	18.07	0.38	18.53	18.17	0.66
	[24.40]	[24.34]	[26.13]	[26.09]	[25.27]		[26.00]	[26.09]	
likelihood of minoring in Economics (0-100)	26.23	25.68	27.18	27.37	26.22	0.87	25.96	27.41	0.45
	[26.91]	[27.18]	[27.11]	[28.03]	[25.85]		[25.88]	[27.65]	
completed endline survey	0.87	0.85	0.86	0.89	0.87	0.51	0.94	0.95	0.92
	[0.34]	[0.36]	[0.35]	[0.31]	[0.34]		[0.23]	[0.23]	
N	456	455	455	460	451	2,277	487	494	981

Table shows mean of baseline characteristics, by experimental phase and study arm. Standard deviations in brackets. Columns (6) and (9) report p-values of joint test of treatment dummies on baseline characteristic, controlling for strata dummies.

Table 3: Take-up and knowledge

	Phase One (all students, within term)						Phase Two (B- or above, after term)		
	control	placebo email	earnings information	AEA video	OSU video	F-test (p-value)	control	treatment	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
opened encouragement email	0.00	0.64	0.71	0.68	0.67	0.00	0.69	0.74	0.13
	[0.00]	[0.48]	[0.45]	[0.47]	[0.47]		[0.463]	[0.44]	
clicked link in email	0.00	0.00	0.02	0.00	0.01	0.01	0.06	0.03	0.06
	[0.00]	[0.05]	[0.13]	[0.00]	[0.09]		[0.23]	[0.18]	
made appointment with Economics advisor	0.00	0.01	0.01	0.01	0.01	0.18			
	[0.05]	[0.10]	[0.10]	[0.10]	[0.09]				
viewed AEA video (self-report)	0.16	0.19	0.21	0.23	0.23	0.06	N/A	N/A	
	[0.37]	[0.39]	[0.41]	[0.42]	[0.42]				
viewed OSU video (self-report)	0.17	0.20	0.22	0.23	0.21	0.19	N/A	N/A	
	[0.37]	[0.40]	[0.41]	[0.42]	[0.41]				
1st year salary range correct	0.23	0.22	0.21	0.25	0.27	0.17	N/A	N/A	
	[0.42]	[0.41]	[0.41]	[0.43]	[0.45]				
15th year salary range correct	0.17	0.23	0.21	0.23	0.20	0.31	N/A	N/A	
	[0.38]	[0.42]	[0.41]	[0.42]	[0.40]				
N	456	455	460	455	451		487	495	

Table shows mean of listed characteristic in each treatment arm for each phase of experiment (standard deviations in brackets). Sample in Phase One is all students who consented to participate in study. Sample in Phase Two is all study participants receiving B- or above in Phase One. Data on Phase 2 opened emails and links clicked for Fall 2018 & Winter 2019 are aggregates by treatment status, not linked to microdata. Phase 2 data for advisor appointments unavailable for Spring 2019. Columns (6) & (9) report p-value of F-test of joint hypothesis that all treatment arms predict characteristic. p-values adjust for stratification of treatment.

Table 4: Phase One, Major in Economics (binary)

Outcome	Major in Economics (binary)											
Specification	Analysis plan						Intensity of treatment (exploratory)					
Sample				B- or above						B- or above		
	all	male	female	all	male	female	all	male	female	all	male	female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
placebo email	0.019**	0.025**	0.004	0.044**	0.063***	0.011	0.015*	0.020*	0.001	0.037**	0.055**	0.011
	(0.009)	(0.013)	(0.012)	(0.018)	(0.023)	(0.031)	(0.008)	(0.011)	(0.011)	(0.018)	(0.023)	(0.027)
earnings information	0.015*	0.013	0.012	0.033*	0.038*	0.022	0.006	0.006	-0.001	0.002	-0.003	-0.003
	(0.009)	(0.011)	(0.017)	(0.018)	(0.020)	(0.038)	(0.007)	(0.009)	(0.013)	(0.013)	(0.014)	(0.031)
AEA video	0.011	0.011	0.005	0.026	0.030	0.006	0.008	0.010	-0.001	0.019	0.023	0.006
	(0.008)	(0.010)	(0.013)	(0.017)	(0.020)	(0.033)	(0.007)	(0.008)	(0.015)	(0.016)	(0.016)	(0.041)
OSU video	0.005	0.010	-0.010	0.010	0.028*	-0.032	-0.001	0.007	-0.017**	0.002	0.025	-0.031
	(0.008)	(0.010)	(0.012)	(0.015)	(0.017)	(0.028)	(0.007)	(0.009)	(0.008)	(0.013)	(0.016)	(0.021)
N	2,238	1,448	790	1,003	665	338	1,883	1,213	670	864	572	292
Control mean	0.023	0.020	0.027	0.037	0.030	0.052	0.027	0.029	0.023	0.042	0.044	0.039
p-values of null hypothesis:												
all treatments=0	0.16	0.37	0.42	0.08	0.05	0.26	0.18	0.36	0.10	0.21	0.14	0.28
placebo=earnings	0.67	0.34	0.66	0.59	0.33	0.77	0.40	0.30	0.89	0.07	0.02	0.69
placebo=AEA video	0.37	0.24	0.94	0.38	0.21	0.89	0.46	0.34	0.89	0.39	0.19	0.91
placebo=OSU video	0.09	0.20	0.16	0.07	0.14	0.11	0.04	0.21	0.08	0.04	0.15	0.10
earnings=AEA video	0.66	0.78	0.71	0.74	0.73	0.71	0.86	0.79	0.99	0.40	0.27	0.84
earnings=OSU video	0.23	0.72	0.15	0.21	0.64	0.09	0.39	0.95	0.20	0.98	0.14	0.30
AEA video=OSU video	0.39	0.93	0.18	0.37	0.95	0.19	0.22	0.78	0.24	0.31	0.90	0.27

Table reports coefficients of regressions of dummy for majoring in Economics on treatment status. Sample is all students who consented to participate in study. Outcome is dummy for majoring in Economics, from administrative data in Winter 2020 or most recent available. Specifications for "intensity of treatment," columns (7)-(12), remove duplicate observations of student and keep only last term observed. Explanatory variables in intensity of treatment specifications represent number of times exposed to treatment. All regressions include strata dummies and baseline outcome. Robust standard errors in parenthesis. * significant at 10%, ** significant at 5%, *** significant at 1%

Table 5: Phase One, Major in Economics (continuous)

Outcome	Major in Economics (self-reported likelihood, 0-100)											
Specification	Analysis plan						Intensity of treatment (exploratory)					
Sample				B- or above						B- or above		
	all	male	female	all	male	female	all	male	female	all	male	female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
placebo email	-0.5	-0.1	-0.9	0.0	0.8	0.5	0.5	0.5	1.0	1.0	2.7	-1.1
	(1.3)	(1.6)	(2.1)	(1.8)	(2.1)	(3.3)	(1.1)	(1.4)	(1.9)	(1.6)	(2.0)	(3.1)
earnings information	-2.6**	-4.0***	0.2	-1.0	-2.1	3.0	-1.4	-2.2*	0.5	0.3	-0.4	0.7
	(1.2)	(1.5)	(2.1)	(1.8)	(2.2)	(3.4)	(1.0)	(1.3)	(1.8)	(1.6)	(1.9)	(3.3)
AEA video	-2.2*	-4.0***	1.0	-2.6	-4.0*	2.0	-1.6*	-2.5**	0.3	-1.1	-1.0	-1.6
	(1.3)	(1.5)	(2.2)	(1.9)	(2.2)	(3.3)	(1.0)	(1.2)	(1.7)	(1.6)	(1.9)	(3.2)
OSU video	-1.5	-2.1	-0.7	-2.4	-3.0	-0.4	-0.9	-1.5	0.4	-1.9	-1.8	0.3
	(1.3)	(1.5)	(2.3)	(1.8)	(2.0)	(3.4)	(1.2)	(1.4)	(2.1)	(1.7)	(1.9)	(3.3)
N	1,964	1,271	693	947	623	324	1,661	1,073	588	812	534	278
Control mean	18.6	19.5	16.7	20.1	20.8	18.5	17.1	18.3	14.9	17.8	18.0	17.5
p-values of null hypothesis:												
all treatments=0	0.18	0.01	0.92	0.41	0.12	0.85	0.22	0.07	0.99	0.61	0.35	0.96
placebo=earnings	0.09	0.01	0.59	0.59	0.18	0.45	0.12	0.07	0.81	0.69	0.19	0.62
placebo=AEA video	0.18	0.02	0.38	0.15	0.02	0.66	0.07	0.05	0.74	0.26	0.11	0.88
placebo=OSU video	0.45	0.20	0.91	0.17	0.06	0.80	0.29	0.19	0.80	0.13	0.04	0.71
earnings=AEA video	0.75	0.97	0.71	0.37	0.40	0.77	0.83	0.87	0.92	0.50	0.80	0.52
earnings=OSU video	0.36	0.20	0.69	0.42	0.66	0.33	0.69	0.65	0.97	0.27	0.53	0.92
AEA video=OSU video	0.55	0.21	0.47	0.92	0.63	0.51	0.51	0.49	0.95	0.67	0.69	0.61

Table reports coefficients of regressions of self-reported likelihood of majoring in Economics on treatment status. Sample is all students who consented to participate in study. Outcome is self-reported likelihood of majoring in Economics, 0-100 scale, from endline survey. Specifications for "intensity of treatment," columns (7)-(12), remove duplicate observations of student and keep only last term observed. Explanatory variables in intensity of treatment specifications represent number of times exposed to treatment. All regressions include strata dummies and baseline outcome. Robust standard errors in parenthesis. * significant at 10%, ** significant at 5%, *** significant at 1%

Table 6: Phase One, Minor in Economics

Outcome	Minor in Economics											
	self-reported (0/1)						self-reported likelihood (0-100)					
Sample				B- or above						B- or above		
	all	male	female	all	male	female	all	male	female	all	male	female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
placebo email	0.001	0.001	0.002	0.006	0.006	0.000	-1.0	0.2	-1.7	0.8	-0.1	4.3
	(0.011)	(0.016)	(0.012)	(0.018)	(0.023)	(0.027)	(1.6)	(2.0)	(2.7)	(2.4)	(2.8)	(4.3)
earnings information	0.019	0.018	0.017	0.028	0.028	0.027	-0.7	-1.0	-0.6	0.8	-1.7	7.3*
	(0.013)	(0.017)	(0.016)	(0.018)	(0.025)	(0.027)	(1.5)	(1.8)	(2.6)	(2.3)	(2.8)	(4.4)
AEA video	0.000	-0.013	0.022	-0.004	-0.008	-0.001	-1.7	-2.6	0.2	-0.6	-2.0	5.6
	(0.011)	(0.014)	(0.019)	(0.016)	(0.020)	(0.027)	(1.5)	(1.8)	(2.7)	(2.3)	(2.7)	(4.2)
OSU video	-0.004	-0.013	0.009	-0.003	-0.018	0.006	-2.1	-2.6	-1.6	-2.5	-3.9	0.7
	(0.010)	(0.014)	(0.013)	(0.014)	(0.019)	(0.022)	(1.4)	(1.7)	(2.6)	(2.2)	(2.4)	(4.2)
N	1,976	1,278	698	953	626	327	1,971	1,274	697	951	624	327
Control mean	0.033	0.041	0.016	0.027	0.031	0.018	26.0	27.3	23.1	27.6	29.7	22.8
p-values of null hypothesis:												
all treatments=0	0.45	0.32	0.67	0.46	0.46	0.84	0.64	0.37	0.92	0.51	0.50	0.29
placebo=earnings	0.17	0.36	0.32	0.29	0.42	0.32	0.88	0.56	0.66	1.00	0.59	0.45
placebo=AEA video	0.93	0.35	0.30	0.59	0.55	0.98	0.65	0.16	0.47	0.56	0.52	0.74
placebo=OSU video	0.63	0.35	0.63	0.63	0.34	0.83	0.48	0.17	0.98	0.14	0.16	0.36
earnings=AEA video	0.15	0.07	0.81	0.09	0.16	0.38	0.52	0.38	0.77	0.54	0.91	0.68
earnings=OSU video	0.06	0.06	0.62	0.09	0.07	0.40	0.36	0.39	0.68	0.12	0.41	0.07
AEA video=OSU video	0.70	0.98	0.49	0.94	0.63	0.82	0.80	0.96	0.49	0.39	0.45	0.21

Table reports coefficients of regressions of self-reported likelihood of minoring in Economics on treatment status. Sample is all students who consented to participate in study. Outcome is self-reported likelihood of minoring in Economics, binary for columns (1)-(6), continuous 0-100 in columns (7)-(12), from endline survey. All regressions include strata dummies and baseline outcome. Robust standard errors in parenthesis. * significant at 10%, ** significant at 5%, *** significant at 1%

Table 7: Phase Two

Outcome	Major in Economics			Took subsequent Economics course		
Sample	all	male	female	all	male	female
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: all grades						
treatment	0.000	0.003	0.005	-0.029	-0.038	0.013
	(0.012)	(0.015)	(0.026)	(0.050)	(0.066)	(0.120)
N	974	644	330	301	200	101
Control mean	0.058	0.064	0.042	0.316	0.336	0.262
Panel B: A students only						
treatment	-0.012	0.001	-0.012	-0.015	-0.058	0.338
	(0.023)	(0.033)	(0.055)	(0.080)	(0.115)	(0.207)
N	368	242	126	128	85	43
Control mean	0.098	0.118	0.054	0.297	0.340	0.176
Panel C: B students only						
treatment	0.009	0.015	0.006	-0.069	-0.044	-0.085
	(0.012)	(0.014)	(0.026)	(0.066)	(0.089)	(0.144)
N	600	399	201	170	113	57
Control mean	0.034	0.033	0.035	0.341	0.344	0.333
Panel D: "disappointed" students only						
treatment	-0.001	0.003	-0.005	-0.067	-0.005	-0.047
	(0.016)	(0.018)	(0.081)	(0.108)	(0.137)	(0.248)
N	354	237	117	100	68	32
Control mean	0.043	0.043	0.044	0.289	0.294	0.273

Sample in Panel A is students receiving B- or above in Economics Principles. Sample in Panel B is students receiving at least A- in Economics Principles. Sample in Panel C is students receiving B+, B, or B- in Economics Principles. Sample in Panel D is "disappointed" students, defined as students who expected an A grade in course but received a B+ or below. Sample for major in Economics (columns 1-3) is Academic Year 2018-2019. Sample for took subsequent Economics course (columns 4-6) is Fall 2018. All regressions include strata dummies and baseline outcome. Baseline outcome for major in Economics (columns 1-3) is dummy for Economics major at the beginning of the term. Baseline outcome for took subsequent Economics course (columns 4-6) is dummy for taking Economics course for university credit prior to course taken for this study. Outcomes from administrative data. Robust standard errors in parenthesis. * significant at 10%, ** significant at 5%, *** significant at 1%

Table 8: Mechanisms: perceptions of Economics major (Phase One endline survey)

Outcome	Biggest appeal of Economics major:									Took subsequent Economics course		
	fun to study			future income			rewarding career					
Sample	all	male	female	all	male	female	all	male	female	all	male	female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
placebo email	-0.01	-0.01	0.00	0.02	0.05	-0.02	-0.03	-0.01	-0.07	-0.03	0.00	-0.09
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.03)	(0.03)	(0.05)	(0.04)	(0.05)	(0.06)
earnings information	0.00	0.01	-0.01	0.03	0.01	0.10*	-0.01	0.01	-0.05	-0.03	0.00	-0.08
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.057)	(0.03)	(0.03)	(0.05)	(0.04)	(0.05)	(0.06)
AEA video	-0.03	-0.06**	0.05	0.06*	0.06	0.04	-0.04	-0.01	-0.11**	-0.01	-0.02	0.00
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.06)	(0.03)	(0.03)	(0.05)	(0.04)	(0.05)	(0.06)
OSU video	-0.01	-0.01	-0.03	0.02	0.03	0.01	0.00	0.03	-0.05	-0.05	-0.06	-0.04
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.06)	(0.03)	(0.03)	(0.05)	(0.04)	(0.05)	(0.07)
N	1,976	1,278	698	1,976	1,278	698	1,976	1,278	698	1,438	923	515
Control mean	0.16	0.18	0.11	0.36	0.35	0.39	0.19	0.16	0.25	0.35	0.35	0.35
p-values of null hypothesis:												
all treatments=0	0.77	0.09	0.21	0.56	0.51	0.31	0.36	0.63	0.23	0.68	0.65	0.47
placebo=earnings	0.58	0.55	0.79	0.71	0.33	0.04	0.40	0.46	0.66	0.92	0.95	0.91
placebo=AEA video	0.55	0.06	0.14	0.31	0.74	0.29	0.59	0.87	0.28	0.57	0.71	0.16
placebo=OSU video	0.94	0.96	0.37	0.99	0.64	0.61	0.24	0.15	0.78	0.58	0.23	0.50
earnings=AEA video	0.24	0.01	0.07	0.53	0.19	0.35	0.17	0.56	0.14	0.64	0.65	0.22
earnings=OSU video	0.51	0.50	0.49	0.70	0.60	0.18	0.73	0.50	0.89	0.51	0.19	0.58
AEA video=OSU video	0.59	0.05	0.02	0.31	0.42	0.64	0.09	0.21	0.21	0.26	0.40	0.54

Table reports coefficients of regressions of indicated outcome on treatment status. Sample is all study participants who completed endline survey (columns 1-9) or all study participants in Fall 2018 and Winter 2019 (columns 10-12). All regressions include strata dummies and baseline outcome. Baseline outcome for taking subsequent Economics course (columns 10-12) is dummy for taking Economics course for university credit prior to course in this study. Robust standard errors in parentheses.

Table 9: Economics majors, counterfactual exercises

	male			female			projected
	base	major		base	major		male/female
	population	proportion	projected	population	proportion	projected	ratio
scenario	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: all students							
control	1,448	0.020	29	790	0.027	21	1.4
placebo	1,448	0.045	65	790	0.031	24	2.7
Panel B: B- or better							
control	1,003	0.030	30	338	0.052	18	1.7
placebo	1,003	0.093	93	338	0.063	21	4.4
Panel C: intensity sample							
control	1,213	0.029	35	670	0.023	15	2.3
placebo	1,213	0.049	59	670	0.024	16	3.7

Table shows projected proportions and numbers of Economics majors under scenarios listed in first column. "Control" scenario based on proportions majoring in Economics among control group. "Placebo" scenario based on proportions majoring in Economics in placebo email group. Base population refers to sample size within the study population. Column (7) shows projected male/female ratio among Economics majors, i.e., column (3)/column (6).

Figure A1(a): Phase One: placebo

ECON 201: Consider majoring in Economics!

Jon Chesbro <jon.chesbro@oregonstate.edu>
Mon 5/20/2019 12:00 PM
To: Schroeder, Elizabeth <Liz.Schroeder@oregonstate.edu>

Having trouble reading this? To view this email as a web page, click here.

Hi Liz,

I hope you have enjoyed learning about Economics this term. As you plan your future studies, **I encourage you to consider majoring or minoring in Economics**. In addition to a traditional Economics degree, Oregon State University's Economics Program offers options in Managerial Economics; Law, Economics and Policy; and Mathematical Economics. Economics training provides excellent preparation for graduate work in Economics, Public Policy, Law, and Business. OSU Economics graduates also use their degrees to work professionally in the public (federal, state and local government) and private (banking, consulting, retail, and corporate) sectors.

If you are interested in majoring or minoring in Economics or would like to learn more, please make an **appointment** with Laura Relyea, the Economics Academic Advisor.

Sincerely,
Jon Chesbro
Instructor, Economics

You may update your profile here.
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Figure A1(b): Phase One: earnings information

ECON 201: Consider majoring in Economics!

Camille Nelson <camille.nelson@oregonstate.edu>
Mon 5/20/2019 12:00 PM
To: Schroeder, Elizabeth <Liz.Schroeder@oregonstate.edu>

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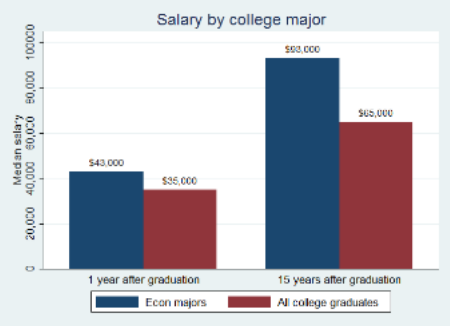
Hi Liz,

I hope you have enjoyed learning about Economics this term. As you plan your future studies, **I encourage you to consider majoring or minoring in Economics.** In addition to a traditional Economics degree, Oregon State University's Economics Program offers options in Managerial Economics; Law, Economics and Policy; and Mathematical Economics. Economics training provides excellent preparation for graduate work in Economics, Public Policy, Law, and Business. OSU Economics graduates also use their degrees to work professionally in the public (federal, state and local government) and private (banking, consulting, retail, and corporate) sectors.

Majoring in Economics can be a smart career decision. Average earnings for economics majors are higher than the overall average for college graduates, both at the start of their careers and throughout their lives.



Source: hamiltonproject.org/charts/career_earnings_by_college_major/

If you are interested in majoring or minoring in Economics or would like to learn more, please **make an appointment with Laura Relyea, the Economics Academic Advisor.**

Sincerely,
Camille Nelson
Senior Instructor, Economics

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Figure A1(c): Phase One: AEA video

ECON 202: Consider majoring in Economics!

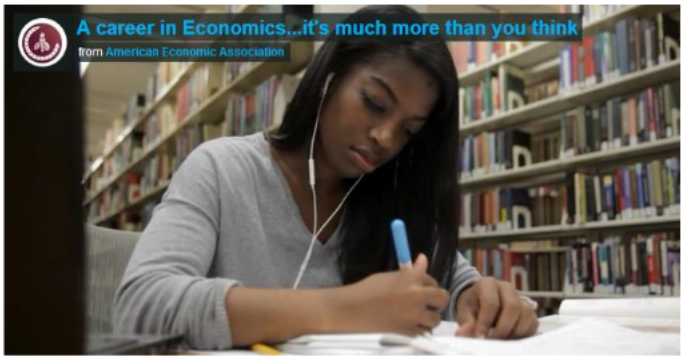
Mike Nelson <mike.nelson@oregonstate.edu>
Mon 5/20/2019 12:00 PM
To: Schroeder, Elizabeth <Liz.Schroeder@oregonstate.edu>

Having trouble reading this? To view this email as a web page, click here.

Hi Liz,

I hope you have enjoyed learning about Economics this term. As you plan your future studies, **I encourage you to consider majoring or minoring in Economics**. In addition to a traditional Economics degree, Oregon State University's Economics Program offers options in Managerial Economics; Law, Economics and Policy; and Mathematical Economics. Economics training provides excellent preparation for graduate work in Economics, Public Policy, Law, and Business. OSU Economics graduates also use their degrees to work professionally in the public (federal, state and local government) and private (banking, consulting, retail, and corporate) sectors.

Learn more in this video about careers in Economics...it's much more than you think!



If you are interested in majoring or minoring in Economics or would like to learn more, **please make an appointment with Laura Relyea, the Economics Academic Advisor.**

Sincerely,
Mike Nelson
Senior Instructor, Economics

You may update your profile here.
This email was sent by: Oregon State University, Printing & Mailing Services,4700 SW Research Way, Corvallis, OR 97333

Figure A1(d): Phase One: OSU video

ECON 201: Consider majoring in Economics!

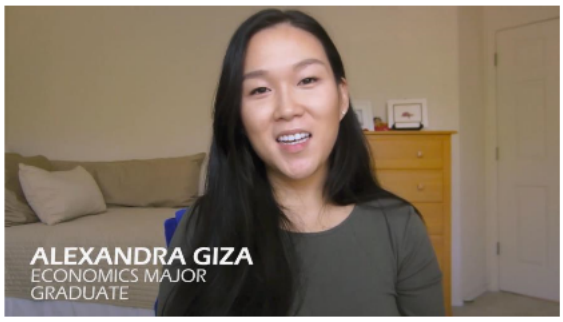
Camille Nelson <camille.nelson@oregonstate.edu>
Mon 5/20/2019 12:00 PM
To: Schroeder, Elizabeth <Liz.Schroeder@oregonstate.edu>

Having trouble reading this? To view this email as a web page, click here.

Hi Liz,

I hope you have enjoyed learning about Economics this term. As you plan your future studies, **I encourage you to consider majoring or minoring in Economics**. In addition to a traditional Economics degree, Oregon State University's Economics Program offers options in Managerial Economics; Law, Economics and Policy; and Mathematical Economics. Economics training provides excellent preparation for graduate work in Economics, Public Policy, Law, and Business. OSU Economics graduates also use their degrees to work professionally in the public (federal, state and local government) and private (banking, consulting, retail, and corporate) sectors.

Learn more in this video about what it's like to major in Economics at Oregon State!



If you are interested in majoring or minoring in Economics or would like to learn more, **please make an appointment with Laura Relyea, the Economics Academic Advisor.**

Sincerely,
Camille Nelson
Senior Instructor, Economics

You may update your profile here.
This email was sent by: Oregon State University, Printing & Mailing Services,4700 SW Research Way, Corvallis, OR 97333

Figure A2(a): Phase Two: control

ECON invite

Laura Relyea <laura.relyea@oregonstate.edu>
Tue 1/8/2019 12:01 PM
To: Schroeder, Elizabeth <Liz.Schroeder@oregonstate.edu>

Having trouble reading this? To view this email as a web page, click here.

Hi Liz,

Your principles of economics instructor indicated that you performed well in a recent class-congratulations! As a result, the economics faculty would like to encourage you to major in Economics.

We have four choices for you: the regular economics major, and options in managerial economics, mathematical economics, and law, economics and policy. We also offer an economics minor to complement your current major.

Why Economics?

- A Career in Economics...it's much more than you think.
- Look at what economists do.
- OSU economics graduates go on to:
  - public sector: federal, state, and local government
  - private sector: banking, consulting, retail, and corporate sectors.
  - advanced study: economics, public policy, law, and business.
- Career earnings - what economists can expect to earn in different fields

Please make an appointment with me if you would like to learn more about our program. To set up a conversation, click here or on the yellow "schedule your appointment" post-it-note below. I look forward to sharing the opportunities in economics with you!

Best,
Laura

Laura Relyea, Academic Advisor
School of Public Policy | Economics Program
Oregon State University | Bexell Hall, 418E
Laura.Relyea@oregonstate.edu
P: (541) 737-2369 | **F:** (541) 737-2289
http://liberalarts.oregonstate.edu/spp/econ/

Students please include your name and OSU ID# on all inquiries.

You may update your profile here.
This email was sent by: Oregon State University, Printing & Mailing Services,4700 SW Research Way, Corvallis, OR 97333

Figure A2(b): Phase Two: treatment

ECON invite

Laura Relyea <laura.relyea@oregonstate.edu>
Tue 1/8/2019 12:00 PM
To: Schroeder, Elizabeth Liz.Schroeder@oregonstate.edu

Having trouble reading this? To view this email as a web page, click here.

Hi Liz,

Your principles of economics instructor indicated that you performed well in a recent class-congratulations! As a result, the economics faculty would like to encourage you to major in Economics.

Even if your grade was not as high as you had hoped, we encourage you to take additional Economics courses. Economics can be a challenging subject. Your Economics ability can grow with dedication and hard work. Adopting this "growth mindset" can help you continue to improve and succeed as you take more advanced courses.

We have four choices for you: the regular economics major, and options in managerial economics, mathematical economics, and law, economics and policy. We also offer an economics minor to complement your current major.

Why Economics?

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  - public sector: federal, state, and local government
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Students please include your name and OSU ID# on all inquiries.

Table A1: Variables

Variable	Source
Treatment assignment	
Phase One	
control group	administrative
placebo email	administrative
earnings information	administrative
AEA video	administrative
OSU video	administrative
Phase Two	
control group	administrative
treatment group	administrative
Baseline characteristics	
female	administrative
white	administrative
Asian	administrative
Hispanic	administrative
first generation	administrative
high school GPA	administrative
Oregon State University GPA	administrative
expected course grade	baseline survey
intends to major in Economics	baseline survey
intends to minor in Economics	baseline survey
likelihood of majoring in Economics (0-100)	baseline survey
likelihood of minoring in Economics (0-100)	baseline survey
Take-up	
opened encouragement email	administrative
clicked link in email	administrative
made appointment with Economics advisor	administrative
completed endline survey	endline survey
viewed AEA video	endline survey
1st year salary range correct	endline survey
15th year salary range correct	endline survey
Outcomes	
major in Economics	administrative
intends to minor in Economics	endline survey
likelihood of majoring in Economics (0-100)	endline survey
likelihood of minoring in Economics (0-100)	endline survey
took subsequent Economics course	administrative

Table A2: Transition matrix for Economics major and minor

	endline								
sample	all			male			female		
	no	yes	attrit	no	yes	attrit	no	yes	attrit
baseline	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Economics major									
no	2,158	33	38	1,393	24	26	765	9	12
	96.8%	1.5%	1.7%	96.5%	1.7%	1.8%	97.3%	1.1%	1.5%
yes	6	41	1	2	29	0	4	12	1
	12.5%	85.4%	2.1%	6.5%	93.5%	0.0%	23.5%	70.6%	5.9%
total	2,164	74	39	1,395	53	26	769	21	13
	95.0%	3.2%	1.7%	94.6%	3.6%	1.8%	95.8%	2.6%	1.6%
Panel B: Economics minor									
no	1,863	34	282	1,193	26	183	670	8	99
	85.5%	1.6%	12.9%	85.1%	1.9%	13.1%	86.2%	1.0%	12.7%
yes	44	35	19	33	26	13	11	9	6
	44.9%	35.7%	19.4%	45.8%	36.1%	18.1%	42.3%	34.6%	23.1%
total	1,907	69	301	1,226	52	196	681	17	105
	83.8%	3.0%	13.2%	83.2%	3.5%	13.3%	84.8%	2.1%	13.1%

Cells show count of students. Sample is all participants in study, 2018-2019. Panel A shows transitions in/out of Economics major based on major at beginning of term of enrollment and most recent record through Winter 2020, from administrative data. Panel B shows transitions in/out of Economics minor based on responses to baseline and endline survey. Values under counts are row percentage, within full/male/female sample.

Table A3: Switches between Economics and other disciplines

	total	male	female
	(1)	(2)	(3)
Panel A: Switches into Economics from:			
Business/finance/marketing	11	9	2
Undecided	10	6	4
Mathematics	4	4	0
Engineering/Computer Science	3	3	0
Other	3	1	2
Political Science	2	1	1
Panel B: Switches into Economics minor from (major):			
Business/finance/marketing	17	12	5
Undecided	8	8	0
Engineering/Computer Science	5	5	0
Economics	2	0	2
Other	1	1	0
Political Science	1	0	1
Panel C: Switches out of Economics major to:			
Political Science	2	0	2
Political Science and Public Policy	2	0	2
Psychology	2	2	0
Panel D: Switches out of Economics minor from (major):			
Business/finance/marketing	18	15	3
Undecided	13	10	3
Engineering/Computer Science	7	7	0
Economics	2	1	1
Other	2	0	2
Political Science	2	0	2

Cells show count of students switching in/out of Economics major and minor. Each row is a previous major (for switches into Economics) or new major (for switches out of Economics). Uses administrative data on majors and endline survey data for minors. "Undecided" includes University Exploratory Studies and similar programs.

Table A4: Transition matrix for intentions major/minor in Economics

	percentile				
treatment	10	25	50	75	90
Panel A: full sample					
control	-20	0	0	5	25
placebo email	-20	0	0	0	20
AEA video	-20	-5	0	0	15
earnings information	-25	-10	0	0	16
OSU video	-20	-5	0	5	20
Panel B: male students					
control	-20	0	0	10	27
placebo email	-20	0	0	5	20
AEA video	-20	-5	0	0	10
earnings information	-30	-10	0	0	18
OSU video	-20	-5	0	5	20
Panel C: female students					
control	-20	0	0	0	10
placebo email	-20	0	0	0	15
AEA video	-19	0	0	0	25
earnings information	-20	0	0	0	15
OSU video	-30	0	0	0	20

Table shows distribution of change in self-reported likelihood (0-100) of majoring in Economics between baseline and endline survey.

Table A5: Phase One, bundled treatment (exploratory)

Outcome	Major in Economics						Minor in Economics					
	administrative (0/1)			self-reported likelihood (0-100)			self-reported (0/1)			self-reported likelihood (0-100)		
Sample	all	male	female	all	male	female	all	male	female	all	male	female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: OLS												
treatment	0.013	0.015	0.003	-1.7	-2.7	-0.1	0.004	-0.001	0.012	-1.4	-1.6	-1.0
	(0.007)*	(0.009)*	(0.010)	(1.0)*	(1.2)**	(1.7)	(0.009)	(0.012)	(0.011)	(1.2)	(1.4)	(2.1)
Panel B: IV												
opened email	0.019	0.023	0.004	-2.5	-4.1	-0.2	0.006	-0.002	0.016	-2.0	-2.4	-1.2
	(0.010)*	(0.013)*	(0.014)	(1.4)*	(1.9)**	(2.2)	(0.013)	(0.018)	(0.013)	(1.7)	(2.1)	(2.6)
N	2,238	1,448	790	1,964	1,271	693	1,976	1,278	698	1,971	1,274	697
Control mean	0.022	0.020	0.027	18.6	19.5	16.7	0.033	0.041	0.016	26.0	27.3	23.1
1st stage F-stat	3,336.5	1,720.7	1,389.1	3,048.1	1,525.7	1,322.4	3,111.6	1,587.0	1,306.2	3,089.6	1,559.8	1,285.8

Table reports coefficients of regressions of indicated outcome on treatment status. Sample is all students who consented to participate in study. Outcome in columns (1)-(3) is dummy for Economics as most recent major recorded in administrative data, through Winter 2020. All other outcomes from endline survey. In Panel A, treatment is dummy for assignment to any Phase One email. IV regressions in Panel B report coefficient on dummy for opened email, using treatment assignment as instrument. All regressions include strata dummies and baseline outcome. Robust standard errors in parenthesis.

Table A6: Outcomes, Phase One (alternative outcomes)

Outcome				Major in Economics		
Sample	all	male	female	all	male	female
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: main estimates	administrative, long-term			endline survey, continuous (0-100)		
placebo email	0.019	0.025	0.004	-0.5	-0.1	-0.9
	(0.009)**	(0.013)**	(0.012)	(1.3)	(1.6)	(2.1)
earnings information	0.015	0.013	0.012	-2.6	-4.0	0.2
	(0.009)*	(0.011)	(0.017)	(1.2)**	(1.5)***	(2.1)
AEA video	0.011	0.011	0.005	-2.2	-4.0	1.0
	(0.008)	(0.010)	(0.013)	(1.3)*	(1.5)***	(2.2)
OSU video	0.005	0.010	-0.010	-1.5	-2.1	-0.7
	(0.008)	(0.010)	(0.012)	(1.3)	(1.5)	(2.3)
N	2,238	1,448	790	1,964	1,271	693
Control mean	0.022	0.020	0.027	18.6	19.5	16.7
Panel B: endline survey		binary		1(likelihood>=90%)		
placebo email	-0.004	0.006	-0.026	0.007	0.012	0.002
	(0.009)	(0.011)	(0.017)	(0.011)	(0.015)	(0.019)
earnings information	-0.009	-0.008	-0.011	-0.005	-0.009	0.007
	(0.008)	(0.008)	(0.019)	(0.011)	(0.013)	(0.020)
AEA video	0.003	0.001	0.001	0.005	-0.007	0.031
	(0.010)	(0.012)	(0.019)	(0.011)	(0.013)	(0.021)
OSU video	-0.001	0.008	-0.023	0.005	0.006	0.000
	(0.009)	(0.011)	(0.017)	(0.011)	(0.012)	(0.021)
N	1,976	1,278	698	1,964	1,271	693
Control mean	0.035	0.030	0.047	0.028	0.026	0.031
Panel C	administrative, short-term					
placebo email	0.002	-0.003	0.008			
	(0.006)	(0.008)	(0.008)			
earnings information	0.004	-0.003	0.016			
	(0.006)	(0.008)	(0.011)			
AEA video	0.009	0.007	0.010			
	(0.007)	(0.010)	(0.010)			
OSU video	-0.004	-0.004	-0.005			
	(0.005)	(0.008)	(0.006)			
N	2,238	1,448	790			
Control mean	0.027	0.027	0.027			

Table reports coefficients of regressions of indicated outcome on treatment status. Administrative, long-term refers to most recent major recorded in administrative data, through Winter 2020. Administrative, short-term refers to earliest major recorded in administrative data after course ended. Outcomes from endline survey are self-reported intention to major. Sample is all students who consented to participate in study. All regressions include strata dummies and baseline outcome. Robust standard errors in parenthesis.