Financial Literacy and Academic Outcomes

Daniel A. Brent
Douglas H. Wrenn*

April 18, 2022

Abstract

Outstanding student debt has almost doubled in the past decade to over $1.7 trillion. Delayed graduation contributes to the student debt burden by increasing tuition payments without any additional wage premium. Behavioral theory suggests that young adults are prone to biases limiting their ability to connect short-run actions with long-run outcomes. We attempt to correct these biases using a low-touch financial literacy intervention within a randomized encouragement design at a large public university. We randomly invite and incentivize first-year students to participate in an online tutorial connecting short-run academic success to long-run debt obligations and examine differences in academic outcomes. The intervention increases credits earned and GPA: treated students earn 0.55 more credits per semester and GPAs increase by 0.08. We also find substantially larger effects for underrepresented minorities and less-prepared students. The intervention puts these at-risk students on track to graduate on time, where they would otherwise fall behind. Applying a simply benefit-cost formula, our results suggest that spending one dollar incentivizing financial literacy in this context leads to tuition savings of $115.

JEL CODES: D14, G53, I21, I23
KEYWORDS: financial literacy, academic outcomes, randomized encouragement

*Daniel A. Brent: Agricultural Economics, Sociology, and Education, Pennsylvania State University; 114 Ferguson Building, University Park, PA 16802; dab320@psu.edu and Douglas H. Wrenn: Agricultural Economics, Sociology, and Education, Pennsylvania State University; 115 Ferguson Building, University Park, PA 16802; dhw121@psu.edu. Both authors acknowledge financial support from Penn State's College of Agricultural Sciences. The views expressed herein are those of the authors and do not necessarily reflect the views of Penn State.

1 Introduction

U.S. student debt was $1.7 trillion in the fourth quarter of 2021, an increase of $800 billion over the previous decade (FRED, 2022). Tuition increases, low savings and interest rates, and the lure of loan forgiveness are widely discussed as contributing factors to rising debt burdens. One factor receiving less attention is delayed graduation. A 2019 report by the National Center for Education Statistics found that only 41% of full-time students earned their degrees in four years, and only 59% graduated in six years (de Brey et al., 2019). Given that savings and borrowing decisions are based on four-year tuition schedules, slow academic progress and delayed graduation will likely lead to increased borrowing and debt.

Improving academic outcomes increases welfare beyond the financial benefits of on-time graduation leading researchers and policymakers to design programs to meet a variety of academic objectives. Interventions that increase the salience or reduce the complexity of college applications and financial aid programs have strong effects on applications, admissions, retention, and academic performance (Bettinger et al., 2012; Hoxby et al., 2013; Castleman and Page, 2016; Page and Scott-Clayton, 2016; Dynarski et al., 2021). However, these interventions rely on underlying public or private programs such as subsidized loans, grants, and merit and need-based scholarships that are costly to scale (Oreopoulos and Petronijevic, 2019). Consequently, researchers have embraced "low-touch" behavioral interventions that target behavioral failures such as improving study habits and time management (Lavecchia et al., 2016). While these interventions are low-cost and easy to scale, results have been mixed. Most show limited ability to impact behavior and academic outcomes, and those

that do typically show limited persistence (Oreopoulos and Petronijevic, 2019; Oreopoulos et al., 2022).

In this paper, we develop a low-touch behavioral intervention that combines the financial incentives inherent in loans, grants, and scholarships with the low-touch, low-cost nature of recent behavioral interventions. Specifically, we develop a financial literacy tutorial that directly links short-run success in the classroom with the financial costs of delayed graduation. Recent experimental work demonstrates the ability of financial literacy education to influence behavior (Kaiser et al., 2021). Our goal is to improve academic outcomes and on-time graduation rates by connecting short-run decisions about academic effort with the long-run financial benefits of on-time graduation.

We embed our intervention in a randomized controlled trial where we randomly invite and incentivize students to participate in our tutorial. Our experiment is conducted at Pennsylvania State University's (Penn State) main campus using first-year students enrolled in the Fall of 2020 (N=7,599).[1] From this sample, we randomly select a subset (N=2,226) to receive one of three types of encouragement (nudges): an email-only invitation (N=1,114) or an email invitation coupled with one of two financial incentives of $10 (N=558) or $20 (N=554).[2] We evaluate the academic impact of our intervention by linking invitations and participation in the tutorial to student academic records for Fall 2020 and Spring 2021. Our primary outcomes are credits earned and GPA that serve as key measures for students' pro-

[1] We exclude adult learners and international students given that they are likely to respond to different incentives than traditional first-year students. International students are likely to have very different borrowing constraints, and adult learners are likely to take longer to graduate given they were going back to school while continuing to work.

[2] The final sizes for each treatment group were determined based on power calculations and our experimental budget. During the experiment, each student receives an email with a personalized link to our tutorial, which is designed and delivered online using Qualtrics' survey tool.

gression towards graduation.

Our results show that our financial literacy intervention has a positive and statistically significant impact on both credits earned and GPA. Students taking part in the tutorial earn 0.55 more credits per semester, relative to a baseline average of 14.72, and raise their GPAs by 0.08 relative to a baseline average of 3.41. Over the course of the first year, the intervention increases the average number of credits earned by 0.88. Converting our main results on credits and GPA to effect sizes, the tutorial increases credits earned and GPA by 0.164 and 0.145 standard deviations, respectively. These effect sizes are consistent with results from other experimental work examining the impact that improvements in financial literacy education have on downstream financial behaviors (Kaiser et al., 2021).

Within our experimental design, we stratify based on several variables related to student characteristics including underrepresented minority status, first-generation status, in-state tuition status, and the quartiles of the distribution of Penn State's first-year academic evaluation index. The academic evaluation index is a measure Penn State uses to assess incoming students' academic preparation based on high school grades and standardized test scores. There is significant heterogeneity in treatment effects by race and academic preparedness. Underrepresented minority students earn an additional 2.37 credits over the course of their first year relative to a control group mean of 28.4. At a rate of 28.4 credits earned per year, the control group is projected to be six credits short of graduating on time. Our intervention eliminates this shortfall, placing treated minority students on track to graduate in four years. We find similar results for less academically prepared students - those below the median of the evaluation index. It is intuitive that our intervention primarily affects students at risk of delayed graduation. The tutorial specifically describes the financial consequences of the

decision to drop or fail a class. Our intervention is unlikely to affect students who are not at risk of dropping or failing classes since the primary information is not relevant and they have few margins for adjustment.

We also find that our intervention improves persistence and retention. Using enrollment data for Fall 2021, we find that drop-out rates for students invited to take part in our tutorial are 1.8 percentage points lower than the control group average of 6.6%. This is a 27% reduction in drop-outs over the first year. Based on student numbers in the original sample, this translates into an additional 137 students returning to school that may not have otherwise.

We make two primary contributions to the literature. First, we demonstrate a new low-touch intervention in higher education that has moderate-sized effects on important student outcomes. Our intervention differs from studies that increase the salience or reduce the cost and/or complexity of accessing government funding (Bettinger et al., 2012; Hoxby et al., 2013; Castleman and Page, 2016; Page and Scott-Clayton, 2016; Dynarski et al., 2021). These programs only target specific eligible populations, and each additional participant requires substantial public investment. These programs are likely worthy investments, but they are not free. Our intervention targets all domestic non-adult students and therefore is broadly applicable to students not eligible for need- or merit-based aid. Our intervention also contrasts with other low-touch interventions that do not have an impact on student behavior. We establish a new mechanism - increasing the salience of personal financial costs - to improve academic effort and performance.

We also contribute to the research on the impact of financial education (Kaiser et al., 2021) by demonstrating that financial literacy affects academic behavior in higher education. While research shows financial literacy has large impacts on a range of outcomes (Lusardi

and Mitchell, 2014) establishing casual effects is still challenging. Field experiments isolate the causal effect of interest (Harrison and List, 2004; List, 2011). The tutorial and the results highlight that academic behavior in college is an important financial behavior that can affect long-term financial well-being. Related research finds "debt letters" improve academic outcomes but do not affect student debt in an observational setting (Stoddard et al., 2017), and have no effect on debt in a randomized experiment (Darolia and Harper, 2018). A key distinction is that prior research targeted students with excessive debt or at risk of delinquency, whereas we target all students in their first year when they still have margins of adjustment to reduce their final debt burden. We also present students with a clear pathway to avoid accumulating debt in the first place.

Delayed graduation is the cumulative consequence of a series of short-run academic outcomes. These short-run outcomes are impacted by many factors including how much a student works, how many courses they attempt and pass each semester, study habits, social distractions, procrastination, and poor time management (Arum and Roksa, 2014; Oreopoulos et al., 2022).[3] Recent behavioral research demonstrates that children and young adults are subject to biases, especially related to intertemporal optimization, that limit their ability to trade off current and future benefits and costs (Alan and Ertac, 2018; Lavecchia et al., 2016; Giedd et al., 2012). In our context, this may lead to sub-optimal academic effort if the benefits of that effort - on-time graduation and reduced debt - occur many years in the future, and the costs - increased study time - occur in the present and are more salient.

Academics have designed and analyzed a host of interventions to improve academic out-

[3] Kolodner (2017) discusses six reasons students may not graduate on time. One reason - what she terms the 12-credit fallacy - is driven by how colleges and lending agencies define a full-time load, which is 12 credits per semester. If followed strictly, a student will require five years, or two extra semesters, to graduate based on a 120-credit graduation requirement.

comes ranging from college applications and admissions (Bettinger et al., 2012; Hoxby et al., 2013; Dynarski et al., 2021), retention (Castleman and Page, 2016), and study habits and performance in courses (Oreopoulos and Petronijevic, 2019; Dobronyi et al., 2019; Clark et al., 2020; Oreopoulos et al., 2022). Concurrently, there has been increased interest in financial literacy and its impact on financial knowledge and behavior (Lusardi and Mitchell, 2014). Kaiser et al. (2021) document that randomly providing financial education impacts a diverse set of behaviors including patience in the classroom (Alan and Ertac, 2018), saving and budgeting (Bruhn et al., 2016), purchasing insurance (Cole et al., 2013), and retirement investments (Duflo and Saez, 2003). Our research bridges the gap between the literature examining interventions designed to improve post-secondary academic outcomes and the literature examining how financial literacy education impacts financial behavior.[4]

Existing research aimed at designing low-touch interventions to improve study habits and academic outcomes has found mixed results (Oreopoulos and Petronijevic, 2019). In many cases, carefully designed interventions have little or no impact on student effort and academic outcomes (Fryer, 2016; Dobronyi et al., 2019; Oreopoulos et al., 2022). Conversely, interventions that increase the salience or reduce the complexity of college applications and financial aid find strong effects on applications, admissions, and retention (Bettinger et al., 2012; Hoxby et al., 2013; Castleman and Page, 2016; Dynarski et al., 2021). Similarly, large state financial aid programs have a positive effect on academic outcomes (Scott-Clayton, 2011; Castleman and Long, 2016).

One reason why students may not respond to low-touch interventions is the failure to con-

[4] Both sets of literature have foundations in behavioral failures related to intertemporal optimization, specifically present bias (O'Donoghue and Rabin, 1999, 2001; Augenblick et al., 2015).

nect study habits and grades with long-term uncertain outcomes, which exacerbates present biases and procrastination (O'Donoghue and Rabin, 1999). Our intervention connects tangible short-run decisions to future financial costs, similar to research showing that imagining one's future self can increase patience and lower discount rates (Alan and Ertac, 2018). We find effect sizes similar to those evaluating expensive government financial aid policies (Scott-Clayton, 2011; Castleman and Long, 2016).[5]

Our results have important policy implications. First, because Penn State is a large, flagship public university with a diverse student body similar to many other universities in the United States, our results are likely externally valid for other institutions in the U.S. Second, our financial literacy intervention provides a low-cost mechanism to improve academic outcomes. A back of the envelope calculation based on our research budget and standard tuition at Penn State shows that our intervention saves over $115 in tuition for every dollar spent on incentives, which is a large, positive return on investment. Finally, our results for minority students and those with less academic preparation show that our intervention addresses a important social need - to help less-prepared students succeed in higher education (Beattie et al., 2019).

[5] Our research is similar to research looking at the impact of goal-setting on academic outcomes (Clark et al., 2020).

2 Data and Research Design

2.1 Student Data

Our study focuses on the population of 8,460 first-year students accepted and enrolled at Penn State's main campus in the Fall of 2020. We obtain information on the students including IDs, race, gender, tuition residency, incoming high school evaluation index, and whether the student was first-generation through a collaboration with Penn State's Office of Planning, Assessment, and Institutional Research.

We focus on first-year students for several reasons. First, they provide a clear starting point from which to evaluate the short- and long-run impacts of our intervention on a variety of measures of academic success. Observing students from their first semester forward allows us to control for natural attrition (drop-outs) in the population and separately identify the impact that our intervention has on academic success and student retention. Second, students in their first year of college have more latitude to change their schedules and academic behavior in response to our financial literacy intervention - i.e., students further along in their careers may have already made decisions preventing them from graduating on time. Third, while this paper focuses on the impact of our financial literacy intervention on academic outcomes (GPA and credits earned) in the short-run we also have interest in its impacts on college persistence and retention. If our intervention leads to improvements in short-run academic outcomes, such as GPA and credits earned per semester, then it may also lead to improvements in persistence and retention.[6] And finally, our research budget constrained us

[6] While not the main focus of our paper, we show preliminary results (in Section 5) that our intervention has impacted retention with students in the treatment group being 1.8% more likely to be enrolled in the

to select only a subset of students on which to conduct the experiment.

We create an inclusion criteria to the first-year population data to arrive at our experimental sample. We first remove all international students (N=761). Our intervention highlights the connection between academic effort, student debt and borrowing; international students are likely to have different borrowing needs and restrictions and thus respond differently compared to domestic students. Next, we remove all adult learners and nursing students (N=48) since they likely have different academic goals and tuition structures than traditional students. Finally, we remove any students (N=50) missing their incoming evaluation index. Penn State assigns each student a science and non-science evaluation index created using incoming grades and college admission test scores. We use Penn State's first-year evaluation index as a dimension in the stratification during randomization and as a variable to evaluate treatment effect heterogeneity. Our final experimental data set consists of 7,599 first-year students entering Penn State in the Fall of 2020.

We track academic responses to treatment by linking our experimental sample with student academic records using student IDs. Using Penn State's iTwo web interface, we pull academic records for each student for the fall and spring semesters of the 2020-2021 academic year. We collect data on total classes, credits attempted, classes dropped, failed and passed, credits earned, GPA, and information on individual course grades. The final data set is a two-semester panel of academic outcomes with individual outcomes serving as dependent variables in our empirical models.

Summary statistics for our academic outcome variables are shown in Table 1. The last two variables in the table are indicators for the share of students in the data that were of-

Fall semester of 2021. This is roughly a 30% reduction in attribute compared to the control group.

fered and participated in the tutorial. The average number of classes taken (5.36) and passed (5.06) as well as the total number of credits attempted (15.67) puts the average student on pace to graduate in four years assuming a standard 120-credit-hour program. However, students *earn* fewer than 15 credits per semester, which will require the average student to stay longer than four years to graduate.[7] The mean values mask a great deal of heterogeneity with roughly 16% of the sample earning 12 credits or less implying that they would need to stay an extra year to graduate. The summary statistics show averages for students in their first year of college. If rates of credit accumulation fall as students progress, as some anecdotal evidence suggests, then the average graduation time will increase as progress slows. This is the type of behavior that our financial literacy intervention is designed to target.

2.2 Experimental Design

Our experiment is based on a randomized encouragement design. Since we cannot force students to participate in our tutorial, we randomly nudge them to participate using *offers* of varying intensity.[8] We divide the experimental sample of first-year students (7,599) into treatment (2,226) and control (5,373) groups. We then split the treatment group into an email-only group (1,114 or 50%) that received only an email prompt to participate and two financial incentive groups that received emails and one of two financial incentives (offers): \$10 (558 or 25%) or \$20 (554 or 25%).[9] All payments for the experiment were made using

[7] Approximately 75% of students in our experimental sample do not have any credits (transfer or AP) at the beginning of their first year.

[8] As noted, our research design in based on the idea of randomized encouragement, which entails randomly offering a person a chance to participate in treatment. In the remainder of the paper, we use the terms offer, prompt, and encouragement synonymously to signify students that received the treatment offer.

[9] In the experiment, we offered two different dollar amounts, but in the remainder of the paper we refer to the offering of either amount as the "incentive" which denotes receiving an email and a monetary offer. The remaining students only received the email.

Penn State's *LionCash* system, which is a campus payment system that can be used to purchase most items on campus and around town.

We vary the intensity of our offers for two reasons. First, by providing different intensities we produce variation in the uptake probability, which we use in our instrumental variables (IV) models to identify the average treatment effect on the treated. Second, we are interested in tracing out demand responses to the different levels of intensity to be used in providing recommendations to the university and other policy makers in scaling up and extending our financial literacy intervention.

The final counts of students in each treatment group were determined based on projections about the take-up rates among students offered treatment and the $10,000 budget we faced in running our experiment.[10] Specifically, we developed priors on the expected take-up rates for the monetary incentive groups and worked backwards to maximize our budget and total numbers of treated students.[11] During randomization we stratified invitations across four variables: whether the student was eligible for in-state tuition, an indicator for whether the student was an under-represented minority (non-White and non-Asian), whether the student was a first-generation college student, and by the quartile of the distribution of Penn State's evaluation index.

The actual randomization process entailed creating two invitation "waves" to ensure that we could in fact pay all participants who received an incentive offer. The first wave (N=1,200)

[10] The funding for this project came from two sources. First, we received two $2,500 grants from the College of Agricultural Sciences. And second, we received a research grant of $5,000 from the department of Agricultural Economics, Sociology, and Education.

[11] Our prior on the take-up rate for the monetary incentive was 50%. As we show later, this turned out to be an overestimation. Actual take-up rates for the email incentive were around 14%, and for the monetary incentives they were around 34%. We also find no statistical difference in take-up rates between the $10 and $20 incentives.

was sent on December 9th, 2020. This was the Wednesday of the last week of classes for the Fall 2020 semester and a week before the beginning of final exams. The second wave (N=1,026) went out on January 11th, 2021, which was a week before the start of classes for the Spring 2021 semester. Based on low response rates in the first two waves, we sent out a third wave of emails on January 18th, 2021 using contact information for all students that had not responded during the first two waves. In each wave students were given one week to complete the tutorial and reminders emails were sent three days and one day before the tutorial closed. The actual tutorial (Section 3) was designed and implemented online using Qualtrics' survey tool. Students received personalized links to the tutorial in the emails they were sent. An example of the email that was sent to each student is shown in the Appendix, Figure A1.

Summary statistics for student characteristics are shown in Table 2. The table shows tests of balance for all variables across the treatment and control groups. A review of the table shows that we achieve good balance on all variables with the exception of the female indicator which is significant at the 10% level. Even though we have fewer females in the invitation group the means are still very similar (50% compared to 53%). We also find that the p-value for the F-test that all of the variables jointly predict treatment is insignificant.

3 Financial Literacy Intervention

We designed our treatment drawing on insights from the literature in behavioral economics and psychology and from the growing literature on financial literacy (Lusardi and Mitchell, 2014; Lavecchia et al., 2016; Kaiser et al., 2021). Economic models of rational human cap-

ital investment predict that individuals make short- and long-run trade-offs, *ex-ante*, to maximize lifetime welfare subject to resource constraints (Becker, 1962). Evidence from behavioral economics, however, shows that people face behavioral and cognitive biases that limit the predictions of this model. These biases are particularly strong among children and young adults, which heightens interest among researchers and policymakers focused on educational outcomes and policy (Alan and Ertac, 2018; Lavecchia et al., 2016; Giedd et al., 2012).

Present bias (limits on intertemporal decision-making), lack of patience, and anchoring impact the ability to connect the consequences of short-run decisions to future outcomes. This inattention is particularly salient in higher education where students regularly make decisions - how many credits to attempt, how to sequence courses, whether to drop a class, and how much to study - that accumulate over their careers to impact long-run outcomes. Cognitive biases may translate into poor financial outcomes if students who fall behind because of sub-optimal short-run decisions are forced to borrow more than expected to complete their degrees, assuming they complete them at all.

Our hypothesis is that by reframing existing financial literacy resources available to students to highlight the connection between short-run academics and long-run finances we can impact graduation rates and potentially debt through changes in academic effort. To this end, we examine how our intervention impacts GPA and how many credits students earn per semester (Credits). We focus on these two outcomes because they proxy for: (1) how well a student is performing academically (GPA) and (2) how quickly he or she is progressing towards graduation (Credits).

The signs on the coefficients for grades and credits are somewhat of an open question. For

credits earned per semester, if the effect of our intervention is to speed up graduation rates, then we expect the coefficient to be positive as this indicates that students are accumulating more credits each semester and approaching the total needed to graduate faster. The sign for GPA is less certain. Students can increase their credits and performance (GPA) at the same time by increasing academic effort. Alternatively, they can avoid dropping difficult classes where they expect to earn a poor grade in order to pass and receive credit. If students perform worse in these classes that would have otherwise been dropped then the increase in credits will come at the expense of a lower GPA. Thus, the sign of GPA does not have a clear prediction.

Our financial literacy tutorial is structured in four parts: (1) questions pertaining to each student's financial situation; (2) questions soliciting baseline financial literacy knowledge; (3) information on the benefits and costs of college and how short-run decisions may impact long-run outcomes; and (4) information about existing financial literacy resources at Penn State. The tutorial takes approximately 10 minutes to complete and is delivered online.[12] We track student responses to each question as well as clicks and time information to track how long students spend on each question and section. In Part (3) that provides instead of elicits information we force students to spend 15 seconds on each page before progressing to encourage them to carefully read all the material.

In Part (1) we ask students a series of questions about their expected levels of debt, whether their family helps them pay for college, and whether they work. In Part (2) we collect information on baseline financial knowledge by asking four financial literacy questions. Three of the questions are standard and taken from Lusardi and Mitchell (2014). The

[12] The Qualtrics tutorial is shown in Figure A2 in the Appendix.

fourth we added to gain a deeper knowledge of baseline financial literacy. In Part (3) we present our main behavioral intervention on the financial implications of staying on schedule and graduating on time, and in Part (4) we link each student to the existing resources on financial literacy available at Penn State.[13] The reason we design our own intervention (Part 3), as opposed to linking to existing resources at Penn State, is because most of the existing resources focus on late- or post-college financial decision-making such as investing in 401Ks, signing up for insurance, and paying down debt, whereas the purpose of our research project is to reduce debt and borrowing in the first place. So, we redesign some of the existing financial literacy resources and pull information from other places within the university to emphasize how short-run activities impact long-run finances.

Resources on average debt and income after graduation and on income levels necessary to comfortably manage various debts levels, under standard repayment periods and interest rates, are available to students at Penn State. However, these resources are located in disparate locations making it hard, if not impossible, for students to find and connect them and internalize the information. Our tutorial is designed to reduce this ambiguity. Specifically, we pull together the resources and repackage them in a short, easy-to-understand format.

The main tutorial (Part (3)) presents a scenario where a student is considering dropping a class that may impact how long they need to graduate. We highlight the financial costs in terms of upfront payments as well as amortized monthly payments if the dropped class requires the student to take an extra semester to graduate and borrow at average levels to finish. We also highlight the benefits of a college degree including average starting salaries

[13] A link to Penn State's financial literacy resources in the Sokolov-Miller Family Financial and Life Skills Center can be found here: https://financialliteracy.psu.edu/

noting the wide dispersion based on majors. The tutorial is presented in two versions depending on whether the student is a Pennsylvania resident or not. Recent research with elementary school students using similar methods found that a low-touch educational intervention that improved students' ability to imagine their future selves improved intertemporal decision-making (Alan and Ertac, 2018). We provide a similar low-touch intervention - we do not explicitly tell student what to do - designed to achieve similar results with the long-run benefit of reducing debt burdens.

4 Empirical Strategy

We use two regression models to estimate two types of treatment effects. The first is a linear regression model, which we use to recover the intent to treat (ITT) or reduced-form effect. The estimating equation is given by

$$Y_{it} = \alpha + \pi_1 Email_{it} + \pi_2 Incentive_{it} + \varepsilon_{it}, \tag{1}$$

where Y_{it} is an academic outcome for student i in semester t, $Email_{it}$ and $Incentive_{it}$ are indicators for whether student i received an email or monetary offer in semester t, and ε_{it} is error term. The offer variables are time-varying because some students received invitation emails in the Fall 2020 semester and others received the invitation in Spring 2021. A student who receives an email in the Spring 2021 semester is not considered as invited in the Fall 2020 semester. Our main outcomes of interest are credits earned and GPA. The π coefficients measure the effect of random assignment into the email or incentive group on academic

outcomes. The coefficients will underestimate the average treatment effect on the treated (ATT) because not everyone in the email and incentive groups participates in the financial literacy tutorial.

We use a two-stage-least-squares model (2SLS) to estimate the local average treatment effect (LATE). Our estimating equations for the 2SLS model are:

$$Treat_{it} = \alpha + \delta_1 Email_{it} + \delta_2 Incentive_{it} + \phi_{it}, \tag{2a}$$

$$Y_{it} = \alpha + \beta \widehat{Treat}_{it} + \nu_{it} \tag{2b}$$

Equation 2a is the first-stage participation equation measuring the effect of random assignment into the email or incentive group on a student's probability of completing the tutorial ($Treat_{it}$). Identification relies on the random assignment and joint significance of the δ coefficients. Equation 2b is the structural equation. $\widehat{Treat}_{it}$ is instrumented using the random assignment of the email and monetary incentives, and β estimates the LATE which is the treatment effect for those induced into taking part in our tutorial after receiving one of the two types of invitations.

Traditionally, 2SLS estimators in randomized encouragement designs recover the LATE, which may be different than the ATT. In our experimental design only students who receive the encouragement can participate in the treatment so we have one-sided noncompliance. Therefore, we have no *defiers* or *always-takers* so the LATE is equivalent to the ATT.[14] The ATT can also be written as the ratio of π (ITT) over δ (first-stage IV), which recognizes that the ATT is the average effect of the random encouragement scaled by the impact of encour-

[14] A defier is treated if-and-only-if they are not encouraged, and an always-taker is treated regardless of whether they are encouraged.

agement on the probability of receiving treatment. In the main results, we present models with and without semester fixed effects. All models use robust standard errors clustered at the student level.

5 Results

5.1 Participation Rates and Responses to Financial Literacy Tutorial

We begin by examining the impact that the email and monetary offers had in encouraging participation in our financial literacy tutorial. Table 3 presents results for three linear probability models regressing participation on different types of encouragement. All models are estimated using our experimental sample of 7,599 students in the Spring 2021 semester after all students received their invitations and completed the tutorial.

Model (1) includes indicators for whether the student received the email-only offer (Email) or one of the two financial offers (Incentive). Both offers induce participation with the email increasing participation by 14.5% and the financial incentives increasing it by 34.2%. The financial offers are more than two times as effective at inducing participation relative to the email-only offer. Thus, it appears that our randomized encourage succeeded in inducing participation and that there is significant variation across the offer types.

Models (2) and (3) break the financial offers into separate groups. Our intention in making different money offers was to induce further variation in participation rates and produce a demand curve of responsiveness to use in making post-study recommendations. The re-

sults, however, indicate that student don't distinguish between the $10 or $20 offers. Model (2) shows similar coefficients for the different financial offers, and model (3) confirms that the two financial offers have an economically and statistically indistinguishable effect on participation. While we are not certain why students viewed the different financial offers the same, at the very least the results hint at deficiencies in understanding implied wage rates. Specifically, assuming it takes 10 minutes to complete the tutorial the fact that students are indifferent between $10 and $20 offers implies they are indifferent between earning $60 and $120 per hour. Based on the results in Table 3, in the remainder of the paper we combine the two financial offers into a single variable called "Incentive".

Table A1 provides summary statistics and balance tests for survey questions by participants based on whether the student offered the email or incentive offers. The p-values are from t-tests of equality of means. Looking across both groups, we observe that: (1) students spent roughly 7.5 minutes taking the tutorial; (2) 50% of all students have loans; (3) average expected debt is around $20,000; and (4) 83% of students are getting some type of family assistance. We also find that baseline financial literacy is quite low. For both groups, students answered slightly more than two of the four questions correctly.[15]

5.2 Main Results for the Effect of the Financial Literacy Tutorial on Academic Outcomes

Our main results are shown in Tables 4 and 5. Panel A. of Table 4 displays ITT results and panel B. shows ATT (2SLS) results. Models (1) and (3) examine the impact of treatment on credits earned per semester and (2) and (4) look at the impact on GPA. The dependent

[15] See Figure A2 in the Appendix for the actual questions.

variables are listed above the columns. Models (1) and (2) are estimated without semester fixed effects and models (3) and (4) include them. The model with fixed effects is more appropriate because the academic outcomes and the invitation variables are correlated with semester indicators. Specifically, more students were invited by the Spring 2021 semester and there were also better academic outcomes in the Spring across the whole sample, so we prefer models with semester fixed effect and include them whenever evaluating outcomes in both semesters.[16] The last row of the Tables 4 displays sample means for each outcome variable to provide context for the magnitude of the treatment effects.

For the ITT results, we find a limited effect for the email offer with small and statistically insignificant effects in the model with semester fixed effects. The effect of the Email offer on credits is roughly half the size of the Incentive offer and the Email offer slightly decreases GPAs. The financial incentives (Incentive) offers generate larger and statistically significant effects for both credits earned and GPA. Assignment to the Incentive group increases credits earned per semester by 0.170 in the model with semester fixed effects. The Incentive group's GPA is 0.034 points higher than the control group.

For the ATT models (Panel B.), we find positive and significant results in all specifications. In our preferred model, with semester fixed effects, participating in the tutorial increases the number of credits earned per semester by 0.55 and increases the GPA by almost 0.08 points. The GPA results are similar to previous studies examining at the short-run (one-year) impact of need-based assistance on academic success. Scott-Clayton (2011), in a regression discontinuity (RD) analysis of West Virginia's PROMISE program, finds that

[16] In the control group, the average number of credits earned was 14.47 in fall 2020 and 14.98 in spring 2021. The control group average GPA was 3.39 in the Fall and 3.43 in the spring. Only students in wave 1 (N=1,200) were invited in the fall, whereas all 2,226 students were invited by spring.

students in the grant program increased their first-year cumulative GPAs by 0.16 points, which is similar to our results. Their GPA estimate falls within confidence bounds for the GPA results in ATT model with fixed effects.

In addition to presenting our results in unit values, we convert them to effect sizes, which are shown in Table A2 in the Appendix. Examining effect sizes allows us to place our results in the broader literature on treatment effects and connect it with other experimental work in the area of financial literacy. In our preferred ITT specification, with semester fixed effects, the Email and Incentive offers increase the number of credits earned by 0.022 and 0.051 standard deviations, respectively, with the latter significant at the 10% level. For GPA, the effect sizes are -0.041 and 0.062 for the Email and Incentive interventions with the coefficient on Incentive significant at the 5% level. The ATT results show that participation in the tutorial increases credits earned by 0.164 standard deviations and GPA by 0.145 standard deviations.

From the perspective of standard treatment effects models, our effect size estimates are on the small side. However, they are economically meaningful in terms of changes in academic outcomes, especially in terms of credits earned. More specifically, our effect size estimates fit squarely within the range of estimates from previous work using Randomized Control Trails to study the impact of financial literacy. Kaiser et al. (2021), in a recent meta-analysis of this RCT literature, find that the average effect size in studies looking at the impact of financial literacy education on financial behaviors (64 studies and 458 estimates) is 0.094 standard deviations. Our estimates are consistent, and in many cases, larger than these results.

Table 5 extends our analysis of credits earned to look at the impact of our intervention on cumulative credits by semester within the first year. The results show a smaller and in-

significant impact during the fall semester. This is likely because only half of the treatment group was invited in the fall semester, and it was already late in the semester which limited their ability to change behavior. The cumulative impact in the spring semester is larger and statistically significant at the 10% level. For the ATT model for spring semester (Panel B., column 2), we find that participating in the financial literacy tutorial increased average credits earned by 0.883 over the course of the year compared to those that did not take part in the tutorial. Compared to the control group mean of 29.63, the results suggest that the intervention pushes the average student above the threshold of 15 credits per semester needed to graduate in four years. Specifically, our ATT results suggest participates ended their first year with roughly 30.5 credits hours compared to 29.63 in the control group.

Our cumulative credits results are also consistent with results found in other recent papers analyzing the impact educational interventions on academic outcomes. Castleman and Long (2016), in an RD analysis of Florida's Student Access Grant program, found that students who were eligible for the grant accumulated 1.1 more credits by the end of their first year. In another study using a similar RD research design, Scott-Clayton (2011) analyzed the WV PROMISE program and found that students accumulated 2.1 more credits. Given that both of these programs involved a substantial financial investment, we find our results based on a low-touch financial literacy intervention particularly compelling.

We also study the effect of our financial literacy tutorial on student retention (re-enrollment the following academic year). We did not initially consider retention as an outcome of interest when generating our pre-analysis plan with the Office of Institutional Research. However, there is attrition in our dataset. Specifically, 213 out of the 7,599 students who began in Fall 2020 dropped out by the end of the Spring 2021 semester. Since some of those students

were invited to the tutorial, we assess the effect of our intervention on student retention one year later.

To do this, we collected enrollment data for the Fall 2021 semester and compared it to our original data for Fall 2020 to evaluate the effect of our random treatment assignment on student retention. Retention results are recovered using a linear probability model that estimates the probability of dropping out conditional on being enrolled in Fall 2020. The results from this model are presented in Table 6. Column (1) shows there are no differential dropout rates for the Email and Incentive groups in Spring 2021. However, by Fall 2021 both the Email and Incentive groups are less likely to drop out compared to the control. The magnitudes are economically meaningful; we find that dropout rates fell by 2.0 and 1.6 percentage points for the Email and Incentive group, respectively, compared to a control group mean of 6.6%. This represents an average reduction of close to 27% year-over-year; it also represents an additional 137 students returning to school. While the retention results are preliminary - we do not know students induced to stay enrolled due to treatment will end up graduating - they suggest another powerful outcome of our financial literacy tutorial.

5.3 Heterogeneity and Robustness

Heterogeneity by Student Characteristics

Our initial experimental design planned to examine heterogeneity by the four variables we stratified the sample over: underrepresented minorities (non-White and non-Asian), first-generation colleges students, those paying in-state tuition, and incoming student evaluation score. We first show how these variables affect participation in treatment conditional on in-

vitation, and baseline academic outcomes in the control group in Table 7. Underrepresented minority students are more likely to participate, but only if they are incentivized. Neither first-generation nor in-state status has any impact on participation. There is positive selection into treatment based on the quartiles of the evaluation index; more prepared students are more likely to participate in the tutorial. The positive selection by evaluation index is more pronounced in the Incentive group relative to the Email group.

We also see that these variables are highly influential on academic outcomes in the control group. Underrepresented minority students have lower GPAs and earn fewer credits per semester, as do first-generation students. In-state students earn fewer credits per semester and have similar GPAs. Students with higher evaluation index scores have higher GPAs and earn more credits per semester, and the effect is monotonically increasing in the quartiles. These baseline conditions are important when interpreting the heterogeneous treatment effects.

We analyze heterogeneity in treatment effects by splitting our data into sub-samples based on each of the four variables described above. To reduce the number of comparison groups, we pool the first and second quartiles of the evaluation index into one sub-sample (Lower Eval.) and the third and fourth quartiles into another sub-sample (Upper Eval.). We focus on the cumulative credits earned by the end of Spring 2021 semester as our primary outcome of interest as this is the most relevant for assessing the potential for our intervention to impact on-time graduation; it is also the most relevant outcome for comparing our results with previous interventions designed to impact academic success.

Table 8 presents both the ITT and ATT results for each sub-sample. Columns (1) and (2) present heterogeneous treatment effects by minority status. We find no effect in either model

for non-minority students. The results for minority students, however, are quite striking. For the ITT model, we find no effect for the email offer, but minority students offered the incentive earn an additional 1.07 credits across their first academic year compared to the control group. From the ATT results, we find that underrepresented minority students who take the tutorial earn an additional 2.37 credits during their first year. Minority students in the control group earn only 28.4 credits in the first year. Extrapolating forward, this would leave those students roughly 6.4 credits, or one-half a semester, short of graduating in four years. If our treatment effects persist beyond the first year our tutorial would put these students back on track for on-time graduation. A similar pattern is found when examining heterogeneity based on the evaluation score in columns (7) and (8). Students with below-median evaluation indices are much more responsive to treatment, but only when incentivized to take the tutorial. The magnitudes are striking because the Lower Eval. sub-sample is larger than the Minority sub-sample and the effect in the Upper Eval. sub-sample is even smaller than the Non-Minority sub-sample. We find no other strong patterns of heterogeneity based on first-generation or in-state tuition status.

In order to interpret the heterogeneous treatment effects it is helpful to consider the baseline effects along with the information presented. The control group average number of credits earned in the first year for the Non-Minority and Upper Eval. sub-samples are 29.9 and 30.6 credits, respectively. These students are generally on track to graduate on-time so information about the financial costs of delayed graduation may not impact their behavior. Students who at higher risk of dropping and failing classes are more likely to act on the information. This helps explain why the Minority and Lower Eval. sub-samples are more responsive; they are at risk of not graduating in four years. One exception to this explanation

is the effect for first-generation students who earn fewer credits and have lower GPAs than the Minority and Lower Eval. sub-samples. Therefore, being at risk of delayed graduation may be a necessary but not sufficient condition for acting on the information in the tutorial.

To further understand why underrepresented minority and less prepared students are so much more responsive to the tutorial, we show the differences in the survey data by underrepresented minority status and evaluation scores in Tables A3 and A5 in the Appendix. Underrepresented minority students spend more time on the survey, are more likely to take out loans, have higher expected debt, and have lower levels of financial literacy than non-minority students. Having more debt and lower baseline levels of financial literacy may represent both the motivation and opportunity to change behavior. However, students with lower evaluation scores have similar levels of debt and other variables collected in the tutorial, but much lower levels of financial literacy compared to students with higher evaluation scores. The survey data broken down by first-generation status is similar to the breakdown by minority status. First generation students participating in the tutorial have higher debt levels, less family help, are more likely to work and have lower levels of financial literacy.

Explaining the Mechanism

Next, we present results that help disentangle how students earn more credits by examining the number of classes passed and either dropped or failed in Table 9. We pool dropped and failed classes because in both scenarios the student will not earn credits towards graduation. The Incentive offer increases the number of classes passed by 0.83 and reduces the number of classes dropped or failed by 0.048. Students taking the tutorial pass an additional 0.277 classes per semester and drop or fail 0.152 fewer classes. The difference between classes

dropped or failed and classes passed shows another interesting mechanism; taking a higher load. We do not observe classes that are dropped in the first two weeks during the add/drop period, but it appears that students participating in the tutorial take and pass more classes. This may help combat the 12-credit fallacy; 12 credits per semester is a full load from a tuition and student loan or scholarship perspective, but it will not allow a student to graduate in four years.

Heterogeneity across Instruments

It is clear from the ITT results above that students respond differently to the Incentive and Email offers. Not surprisingly, the Incentive offer induces greater participation in the tutorial but also attracts different types of students, as shown in Table 7. In Table 10, we show different ATTs for each type of invitation offer and expand the outcomes to highlight different response mechanisms. We present our baseline results (Table 4, columns (3) and (4)) in the first two columns in Panel A, and in columns (3) and (4) we show results for the number of classes dropped and failed. For the overall sample, the tutorial seems to increase credits earned and GPA primarily through failing fewer classes. In Panel B. we remove all students who received a financial incentive offer to isolate the LATE for students with the Email offer and Panel C. removes all students with an Email offer to estimate the LATE for incentivized offers. The effect of credits earned is the same in both panels. Although the ITT is larger for the Incentive group it is scaled by a smaller factor since the participation rate in the Incentive group is lower.[17] The LATE for the Incentive group is much more precise due to the greater participation rate - the standard error falls as the first stage coefficient

[17] Recall that the LATE $= \frac{\text{ITT}}{\text{First Stage}}$; a lower first stage coefficient will scale up the ITT by a larger factor.

rises. Without the Incentive offer, we would not have found a statistically significant effect on credits earned.

There is also heterogeneity across offer types in *how* the students earn more credits. The average GPA of incentivized participants increases whereas non-incentivized student GPAs fall. This can be explained by examining the final two columns. Incentivized participants primarily earn more credits by failing fewer classes, which will raise the GPA since failing a class results in the lowest possible grade. Conversely, non-incentivized participants earn more credits by dropping fewer classes, which can lower GPA if grades in courses that would have been dropped are lower than average grades. The results highlight an important element in our experimental design that is not always present in low-touch interventions - providing a financial incentive for the students to participate. At least part of the different LATEs can be explained by compositional differences in the groups. The Incentive group attracts more underrepresented minority students who are more responsive to treatment. However, the Incentive group also attracts more students with high evaluation index scores who are less responsive to treatment. Therefore, even students that are observationally equivalent may have differential responses based on how they were invited to participate in the tutorial.

Randomization Inference and Multiple Hypothesis Testing

Lastly, based on the advice of Young (2019); List et al. (2019), we also use randomization inference to test our hypotheses. Randomization inference, developed by Fisher (1935), uses the randomization design as opposed to asymptotic theory to generate inference. This allows for testing exact null hypotheses by permuting the treatment and considering the potential outcomes under different treatment assignments. Then the observed values can be compared

to the distribution of null effects under the placebo treatment assignments. The exact p-values are simply the probability of observing more extreme values than those observed in the actual treatment assignment. This is similar to bootstrap inference except that it exploits the randomization procedure instead of more general resampling. We also follow Young (2019)'s advice of correcting for multiple hypothesis testing based on the approach of Romano and Wolf (2005).

The exact p-values from the randomization inference and those correcting for multiple hypothesis testing are presented in Tables A6 and A7. These tables correct for multiple hypothesis testing on different outcomes and sub-samples for the ITT results. The exact p-values are similar to the asymptotic p-values for the overall ITT results in Table 4 with the exception of cumulative credits. Correcting for multiple hypothesis test does increase the p-values for the incentive ITTs. The sub-sample analysis is not as robust to randomization inference and multiple hypothesis testing. The underrepresented minority has p-values above 0.10 in both the exact p-values and adjusted p-values. The Lower Eval. sub-sample is still statistically significant in both exact and adjusted p-values.

6 Conclusion

Collective student loans currently sit at $1.7 trillion with over 45 million Americans owing some amount of money for education. Before the COVID19 pandemic 1 in 10 people were delinquent or in default on those loans. Student debt creates a financial burden for many people and has the potential to strain the overall economy. Student debt has become a polarizing political topic with many advocating for varying degrees of debt forgiveness.

From an economic perspective taking on debt to finance an investment in human capital is still worthwhile for many students. However, taking more time to graduate will often increase debt burden without providing additional benefits. We target on-time graduation as a mechanism to reduce student debt by embedding a financial literacy tutorial highlighting the financial costs of delayed graduation in a randomized experiment to evaluate the impact of the tutorial on academic outcomes. Our experiment takes place in a large state university that is typical of many students' university experiences.

The effect on short-run academic outcomes, the inputs to on-time graduation, are very promising. Students taking the tutorial earn higher GPAs and more credits. The magnitudes are economically significant, and more pronounced for historically disadvantaged groups. Underrepresented minorities are three times as responsive as non-minority students and earn an additional 2.37 credits per year. Students below the median in their incoming evaluation index are also more responsive to treatment.

The results are compelling given the low-touch nature of our intervention. The tutorial is distributed online, takes less than 10 minutes, and we use relatively modest incentives ($10 and $20) to encourage participation. Therefore, we are hopeful that the results are scalable. To consider how the intervention may pass a benefit cost-analysis we examine the total number of additional credits earned due to the intervention. If we assume that 12 credits will lead to an extra semester, then we saved treated students almost $670,000 in tuition with a return on investment of over $115 of reduced tuition for every dollar spent on incentives. This assumes that the results do not persist at all beyond the first year.

There are still many open questions to examine. The most critical is determining if the short-run improvements in academic outcomes translate into increases in on-time graduation

and decreases in student debt. It is also important to see how the intervention works on different cohorts (e.g. second or third year students). While Penn State is a typical flagship state university it is important to consider the external validity at other types of institutions such as less selective public or private universities. Since we also highlight more general topics of financial literacy there may be other benefits due to the increased interest in financial literacy.

References

Alan, Sule and Seda Ertac, "Fostering patience in the classroom: Results from randomized educational intervention," *Journal of Political Economy*, 2018, *126* (5), 1865–1911.

Arum, Richard and Jospia Roksa, *Aspiring adults adrift*, Chicago, IL: University of Chicago Press, 2014.

Augenblick, Ned, Muriel Niederle, and Charles Sprenger, "Working over time: Dynamic inconsistency in real effort tasks," *The Quarterly Journal of Economics*, 2015, *130* (3), 1067–1115.

Beattie, Graham, Jean-William P. Laliberte, Catherine Michaud-Leclerc, and Philip Oreopoulos, "What sets college thrivers and divers apart? A contrast in study habits, attitudes, and mental health," *Economics Letters*, 2019, *178*, 50–50.

Becker, Gary S., "Investment in human capital: A theoretical analysis," *Journal of Political Economy*, 1962, *70* (2), 9–49.

Bettinger, Eric P., Bridget Terry Long, Philip Oreopoulos, and Lisa Sanbonmatsu, "The role of application assistance and information in college decisions: Results from the H and R block FAFSA experiment," *Quarterly Journal of Economics*, 2012, *127* (3), 1205–1242.

Bruhn, Miriam, Luciana de Souza Leão, Arianna Legovini, Rogelio Marchetti, and Bilal Zia, "The impact of high school financial education: Evidence from a large-scale evaluation in Brazil," *American Economic Journal: Applied Economics*, 2016, *8* (4), 256–95.

Castleman, Benjamin L. and Bridget Terry Long, "Looking beyond enrollment: The causal effect of need-based grants on college access, persistence, and graduation," *Journal of Labor Economics*, 2016, *34* (4), 1023–1073.

_ **and Lindsay C. Page**, "Freshman year financial aid nudges: An experiment to increase FAFSA renewal and college persistence," *Journal of Human Resources*, 2016, *51* (2), 389–415.

Clark, Damon, David Gill, Victoria Prowse, and Mark Rush, "Using goals to motivate college students: Theory and evidence from field experiments," *Review of Economics and Statistics*, 2020, *102*, 648–663.

Cole, Shawn, Xavier Giné, Jeremy Tobacman, Petia Topalova, Robert Townsend, and James Vickery, "Barriers to household risk management: Evidence from India," *American Economic Journal: Applied Economics*, 2013, *5* (1), 104–35.

Darolia, Rajeev and Casandra Harper, "Information use and attention deferment in college student loan decisions: Evidence from a debt letter experiment," *Educational Evaluation of Policy Analysis*, 2018, *40* (1), 129–150.

de Brey, Cristobal, Lauren Musu, Joel McFarland, Wilkinson-Flicker, Anlan Zhang, Claire Branstetter, and Xiaolei Wang, "Status and trends in the education of racial and ethnic groups 2018," Technical Report NCES 2019-038, National Center for Education Statistics, U.S. Department of Education 2019.

Dobronyi, Christopher R., Philip Oreopoulos, and Petronijevic, "Goal setting, academic reminders, and college success: A large-scale field experiment," *Journal of Research on Educational Effectiveness*, 2019, *12*, 38–66.

Duflo, Esther and Emmanuel Saez, "The role of information and social interactions in retirement plan decisions: Evidence from a randomized experiment," *The Quarterly journal of economics*, 2003, *118* (3), 815–842.

Dynarski, Susan, C.J. Libassi, Katherine Michelmore, and Stephanie Owen, "Closing the gap: The effect of reducing complexity and uncertainty in college pricing on the choices of low-income students," *American Economic Review*, 2021, *111* (6), 1721–1756.

Fisher, R.A., *The design of Experiments*, Chicago, IL: Oliver & Boyd, 1935.

FRED, "Student Loans Owned and Securitized," Technical Report, Board of Governors of the Federal Reserve System 2022. accessed March 22, 2022.

Fryer, Roland G., "Information, non-financial incentives, and student achievement: Evidence from a text messaging experiment," *Journal of Public Economics*, 2016, *144*, 109–121.

Giedd, Jay N., Michael Stockman, Catherine Weddle, Maria Liverpool, Gregory L. Wallace, Nancy Reitano Lee, Francois Lalonde, and Rhloshel Lenroot, "Anatomic magnetic resonance imaging of the developing child and adolescent brain," in Valerie F. Reyna, Sandra B. Chapman, Michael R. Dougherty, and Jere Confrey, eds., *The Adolescent Brain: Learning, Reasoning, and Decision Making*, American Psychological Association, 2012.

Harrison, Glenn W and John A List, "Field experiments," *Journal of Economic literature*, 2004, *42* (4), 1009–1055.

Hoxby, Caroline, Sarah Turner et al., "Expanding college opportunities for high-achieving, low income students," *Stanford Institute for Economic Policy Research Discussion Paper*, 2013, *12* (014), 7.

Kaiser, Tim, Annamaria Lusardi, Lukas Menkhoff, and Carly Urban, "Financial education affects financial knowledge and downstream behavior," *Journal of Financial Economics*, 2021.

Kolodner, Meredith, "6 reasons you may not graduate on time," *New York Times*, 2017.

Lavecchia, Adam, Heidi Liu, and Philip Oreopoulos, "Behavioral economics of education: Progress and possibilities," in Eric A. Hanushek, Stephen J. Machin, and Ludger Woessmann, eds., *Handbook of the Economics of Education*, Vol. 5, Elsevier, 2016, pp. 1–74.

List, John A, "Why economists should conduct field experiments and 14 tips for pulling one off," *Journal of Economic perspectives*, 2011, *25* (3), 3–16.

_ , Azeem M Shaikh, and Yang Xu, "Multiple hypothesis testing in experimental economics," *Experimental Economics*, 2019, *22* (4), 773–793.

Lusardi, Annamaria and Olivia S. Mitchell, "The economic importance of financial literacy: Theory and evidence," *Journal of Economic Literature*, 2014, *52* (1), 5–44.

O'Donoghue, Ted and Matthew Rabin, "Doing it now or later," *American economic review*, 1999, *89* (1), 103–124.

_ and _ , "Choice and procrastination," *The Quarterly Journal of Economics*, 2001, *116* (1), 121–160.

Oreopoulos, Philip and Uros Petronijevic, "The remarkable unresponsiveness of college students to nudge and what we can learn from it," *NBER Working Paper Series*, 2019, *26059.*

_ , Richard W. Patterson, Uros Petronijevic, and Nolan G. Pope, "Low-touch attempts to improve time management among traditional and online college students," *Journal of Human Resources*, 2022, *57* (1), 1–43.

Page, Lindsay and Judith Scott-Clayton, "Improving college access in the United State: Barriers and policy responses," *Economics of Education Review*, 2016, *51*, 4–22.

Romano, Joseph P and Michael Wolf, "Exact and approximate stepdown methods for multiple hypothesis testing," *Journal of the American Statistical Association*, 2005, *100* (469), 94–108.

Scott-Clayton, Judith, "On money and motivation: A quasi-experimental analysis of financial incentives for college achievement," *Journal of Human Resources*, 2011, *46* (3), 614–646.

Stoddard, Christiana, Carly Urban, and Maximilian Schmeiser, "Can targeted information affect academic performance and borrowing behavior for college students? Evidence from administrative data," *Economics of Education Review*, 2017, *56*, 95–109.

Young, Alwyn, "Channeling Fisher: Randomization tests and the statistical insignificance of seemingly significant experimental results," *The Quarterly Journal of Economics*, 2019, *134* (2), 557–598.

Tables

Table 1: Summary Statistics for Academic Outcome Data

	Mean	SD	N
Classes			
Total	5.36	0.92	14985
Passed	5.06	1.21	14985
Failed	0.14	0.60	14985
Dropped	0.07	0.37	14985
Credits			
Attempted	15.67	2.36	14985
Earned	14.76	3.35	14985
Grades			
GPA	3.41	0.54	14754
Treatment			
% Invited	0.29	0.46	14985
% Completed	0.05	0.22	14985

Notes: The summary statistics are for academic outcome variables for students included in our experimental sample. The data are pooled across fall and spring semesters for the 2020-2021 academic year.

Table 2: Balance Table of Student Characteristics by Treatment Status

	Invitation (N=2226)		No Invitation (N=5373)		
	Mean	SD	Mean	SD	p-value
Minority	0.19	0.39	0.19	0.39	0.92
First-Gen	0.14	0.35	0.14	0.35	0.89
Evaluation	3.26	0.26	3.25	0.27	0.79
In-State	0.62	0.49	0.62	0.49	0.94
First-Time	0.74	0.44	0.75	0.43	0.24
Female	0.50	0.50	0.53	0.50	0.06
F-test p-value		0.4551			

Notes: The columns show the means and standard deviations for the sample partitioned by the random invitation assignment. Invitation pools the two monetary offers and email-only offer. The p-values are from t-tests of equality of means. The F-test is from a test of joint significance from a regression of treatment status on all variables listed.

Table 3: Participation Rates in Financial Literacy Tutorial by Offer Type

	(1)	(2)	(3)
Email	0.145***	0.145***	0.145***
	(0.011)	(0.011)	(0.011)
Incentive	0.342***		0.333***
	(0.014)		(0.020)
$10		0.333***	
		(0.020)	
$20		0.352***	0.019
		(0.020)	(0.029)
Observations	7,599	7,599	7,599

Notes: The outcome variable in each model is an indicator for whether a student accepted the offer to participate in the financial literacy tutorial by the end of the spring 2021 semester. Each column show results from regressing this indicator variable on a set variables indicating whether the student was assigned the email incentive or one of the two monetary incentives. The Incentive variable is the combined effect of both monetary incentives. The final counts for participation are 159, 184, and 193 for the email, $10, and $20 offers, respectively. *p<0.1; **p<0.05; ***p<0.01

Table 4: The Effect of the Financial Literacy Tutorial on Credits Earned and GPA

	Credits (1)	GPA (2)	Credits (3)	GPA (4)
A. Intent to Treat				
Email	0.172*	-0.014	0.075	-0.022
	(0.093)	(0.016)	(0.094)	(0.016)
Incentive	0.269***	0.042***	0.170*	0.034**
	(0.091)	(0.015)	(0.092)	(0.015)
B. Treatment Effect on the Treated				
Treatment	0.907***	0.107**	0.550*	0.079*
	(0.279)	(0.045)	(0.285)	(0.046)
F-statistic	2,141.4	2,140.6	2,058.4	2,057.7
Fixed Effects (Semester)	No		Yes	
Observations	14,985	14,754	14,985	14,754
Control Group Mean	14.72	3.41	14.72	3.41

Notes: The table presents the main treatment effects of the financial literacy tutorial on academic outcomes. The dependent variables in each model are listed above the columns. Credits is the total credits earned in each semester and GPA is term grade point average. The academic outcomes in the data are for fall and spring semesters of the 2020-2021 academic year. Part A. presents ITT results and Part B. presents ATT results. The Email and Incentive variables are used as instruments in the IV estimation for the ATT models. Models (1) and (2) are estimated without semester fixed effects and models (3) and (4) include them. The Control Group Mean shows the mean values of the outcome variables for the control group. Robust standard errors are clustered at the student level and shown in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 5: The Effect of the Financial Literacy Tutorial on Cumulative Credits Earned

	Fall 2020 (1)	Spring 2021 (2)
A. Intent to Treat		
Email	0.038	0.066
	(0.143)	(0.171)
Incentive	0.188	0.320*
	(0.125)	(0.168)
B. Treatment Effect on the Treated		
Treatment	0.678	0.883*
	(0.466)	(0.475)
F-statistic	916.0	1,102.1
Observations	7,599	7,386
Control Group Mean	14.47	29.63

Notes: The outcome variable is the cumulative credits earned by the end of either fall 2020 or spring 2021. Panel A. shows the intent to treat effect and Panel B. shows the average treatment effect on the treated from an IV model using the email and incentive as instruments. The control group mean is presented in the final panel. All models include semester fixed effects. Robust standard errors are clustered at the student level. *p<0.1; **p<0.05; ***p<0.01

Table 6: The Effect of the Financial Literacy Tutorial on Dropping out of College

	Spring 2021 (1)	Fall 2021 (2)
A. Intent to Treat		
Email	0.005	-0.020***
	(0.006)	(0.007)
Incentive	-0.007	-0.016**
	(0.005)	(0.007)
Observations	7,599	7,599
Control Group Mean	0.028	0.066

Notes: The outcome variable in each model is an indicator for whether a student from our original sample - those 7,599 enrolled students in fall 2020 - was not in the sample (dropped out) in either spring 2021 or fall 2021. The control group means for each semester are presented in the bottom panel. Robust standard errors are presented in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 7: Participation and Academic Outcomes and Treatment Responses by Student Characteristics

	Treatment		Credits	GPA
	Email	Incentive		
	(1)	(2)	(3)	(4)
Minority	0.014	0.096**	-0.607***	-0.068***
	(0.028)	(0.038)	(0.116)	(0.018)
First Gen.	0.010	0.002	-0.685***	-0.167***
	(0.032)	(0.042)	(0.131)	(0.022)
In-State	0.019	0.019	-0.172**	-0.014
	(0.022)	(0.030)	(0.078)	(0.013)
Eval. Q2	0.051*	0.044	0.521***	0.093***
	(0.029)	(0.038)	(0.112)	(0.019)
Eval. Q3	0.030	0.122***	0.938***	0.226***
	(0.028)	(0.039)	(0.106)	(0.018)
Eval. Q4	0.066**	0.183***	1.50***	0.444***
	(0.030)	(0.040)	(0.112)	(0.016)
Observations	1,077	1,088	10,594	10,420

Notes: Columns (1) and (2) present the results from a linear probability model for completing the tutorial when provided the Email offer (column (1)) and the Incentive offer (column (2)). Columns (3) and (4) regress academic outcomes on student characteristics in the control group. All student characteristics are indicator variables. Each value is relative to a student that is non-minority, is not first-generation, pays out-of-state tuition, and is in the lowest quartile of the first-year evaluation index. Robust standard errors are clustered at the student level. *p<0.1; **p<0.05; ***p<0.01

Table 8: The Effect of the Financial Literacy Tutorial on Cumulative Credits Earned by Student Characteristics

	Non-Minority	Minority	Non-First-Gen.	First-Gen.	Out-of-State	In-State	Lower Eval.	Upper Eval.
	Spring 2021	Spring 2021	Spring 2021	Spring 2021	Spring 2021	Spring 2021	Spring 2021	Spring 2021
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Intent to Treat								
Email	0.098	-0.116	-0.042	0.775	-0.272	0.271	0.190	-0.046
	(0.170)	(0.539)	(0.178)	(0.554)	(0.260)	(0.225)	(0.245)	(0.233)
Incentive	0.155	1.07**	0.349**	0.196	0.381	0.282	0.669***	-0.035
	(0.177)	(0.450)	(0.172)	(0.549)	(0.261)	(0.218)	(0.231)	(0.239)
B. Treatment on the Treated								
Treatment	0.499	2.37**	0.894*	0.997	0.884	0.903	2.24***	-0.104
	(0.523)	(1.08)	(0.492)	(1.48)	(0.778)	(0.599)	(0.798)	(0.577)
F-statistic	832.0	275.5	942.7	159.7	395.3	708.7	421.1	704.5
Observations	6,008	1,378	6,383	1,003	2,801	4,585	3,706	3,680
Control Group Mean	29.9	28.4	29.8	28.2	29.6	29.6	28.7	30.6

Notes: The dependent variable is cumulative credits earned by the end of spring 2021 semester. Each columns represents a regression for the sub-sample of interest. Minority is defined as any non-Asian and non-White student. First generation (First-Gen.) is defined as any student without a parent with a college degree. In-State and Out-of-State are defined as a student paying in-state and out-of-state tuition. Upper Eval. is an indicator for students with first-year evaluation scores above the 50th percentile and Lower Eval. is an indicator for student with scores at or below the 50th percentile. Panel A. presents the intent-to-treat results and panel B. presents the average treatment effect on the treated in an IV model using email and incentive as instruments. The control group means for each of the sub-samples are presented in the bottom panel. Robust standard errors are presented in parentheses. *p<0.1; **p<0.05; ***p<0.01

Table 9: The Effect of the Financial Literacy Tutorial on Classes Dropped, Failed, and Passed

	Pass (1)	Drop/Fail (2)
A. Intent to Treat		
Email	0.048	-0.019
	(0.036)	(0.021)
Incentive	0.083**	-0.048***
	(0.035)	(0.018)
B. Treatment Effect on the Treated		
Treatment	0.277***	-0.152***
	(0.107)	(0.057)
F-statistic	2,058.4	2,058.4
Observations	14,985	14,985
Sample Mean	5.06	0.22

Notes: The outcome variables are the number of classes passed (Pass) and dropped or failed (Drop/Fail). Panel A. shows the intent-to-treat effect and Panel B. shows the average treatment effect on the treated from an IV model using the email and incentive as instruments. The control group mean is presented in the final panel. All models include semester fixed effects. Robust standard errors are clustered at the student level. *p<0.1; **p<0.05; ***p<0.01

Table 10: Heterogeneity in Treatment Effects by Different Instruments

	Credits (1)	GPA (2)	# Drop (3)	# Fail (4)
A. All Treatments (ATT)				
Treatment	0.550*	0.079*	-0.035	-0.117**
	(0.285)	(0.046)	(0.032)	(0.046)
Observations	14,985	14,754	14,985	14,985
B. Only Email (LATE)				
Treatment	0.579	-0.172	-0.162**	0.018
	(0.738)	(0.129)	(0.071)	(0.145)
Observations	13,299	13,091	13,299	13,299
C. Only Incentive (LATE)				
Treatment	0.548*	0.109**	-0.023	-0.132***
	(0.292)	(0.047)	(0.033)	(0.045)
Observations	13,306	13,093	13,306	13,306
Control Sample Mean	14.72	3.41	0.08	0.15

Notes: The results show the IV models with academic outcomes for the number of credits earned (Credits), grade point average (GPA), classes dropped (# Drop), and classes failed (# Fail). Panel A. includes all observations in the experimental sample with LATE equal to the ATT. Panels B. drops all students with a financial incentive and panel C. drops all students with an email-only invitation. Panels B. and C. estimate the LATE for subgroups based on the offer type they receive. The control group mean for the outcome variables are in the bottom panel. All models include semester fixed effects. Robust standard errors are clustered at the student level. *p<0.1; **p<0.05; ***p<0.01

7 Appendix

Table A1: Balance Table of Student Responses to the Financial Literacy Tutorial by Treatment Group

	Incentive (N=377)		Email (N=159)		
	Mean	Std. Dev.	Mean	Std. Dev.	p-value
Duration	7.33	5.65	7.91	6.29	0.32
Loans	0.51	0.50	0.49	0.50	0.65
Debt	21525.20	19004.34	19308.18	18084.52	0.20
Family Help	0.84	0.36	0.82	0.38	0.58
Work	0.69	0.46	0.73	0.45	0.32
Fin. Lit.	2.24	1.14	2.14	1.23	0.38
Center	0.47	0.50	0.44	0.50	0.57
Click	0.21	0.41	0.15	0.36	0.11

Notes: The results are based on responses by students who were offered and participated, or were treated by the financial literacy tutorial. The Incentive columns are the number of takers out of 1,115 that were offered one of the monetary treatments ($10 or $20). The Email columns are the number of takers out of 1,114 that were offered the email-only treatment. The results are for statistical balance between the two treatment groups. p-values are from t-tests of coefficient significance of each variable in a regression of the Incentive indicator on all variables shown. The Fin. Lit. variable is the number of correct responses out of the four multiple choice questions. All other variables are in the units displayed.

Table A2: The Effect of Financial Literacy Tutorial on Academic Outcomes with Standardization

	Credits (1)	GPA (2)	Credits (3)	GPA (4)
A. Intent to Treat				
Email	0.051*	-0.025	0.022	-0.041
	(0.028)	(0.030)	(0.028)	(0.030)
Incentive	0.080***	0.078***	0.051*	0.062**
	(0.027)	(0.027)	(0.027)	(0.028)
B. Treatment on the Treated				
Treatment	0.271***	0.198**	0.164*	0.145*
	(0.083)	(0.082)	(0.085)	(0.085)
F-statistic	2,141.4	2,140.6	2,058.4	2,057.7
Fixed Effects (Semester)	No		Yes	
Observations	14,985	14,754	14,985	14,754
Control Sample Mean	14.72	3.41	14.72	3.41
Control Sample Std. Dev.	3.41	0.55	3.41	0.55

Notes: Academic outcome dependent variables are listed above each column and are for fall and spring of the 2020-2021 academic year. All dependent variables are standardized (mean zero and standard deviation of one). Part A. presents ITT results and Part B. presents ATT results from an IV model with the Email and Incentive variables used as instruments. Robust standard errors are clustered at the student level. *p<0.1; **p<0.05; ***p<0.01

Table A3: Balance Table of Student Responses to Financial Literacy Tutorial by Minority Status

	Minority (N=123)		Non-Minority (N=413)		
	Mean	Std. Dev.	Mean	Std. Dev.	p-value
Duration	7.99	6.14	7.36	5.76	0.31
Loans	0.57	0.50	0.49	0.50	0.11
Debt	23699	19437	20024	18476	0.06
Family Help	0.80	0.40	0.85	0.36	0.19
Work	0.70	0.46	0.70	0.46	0.99
Fin. Lit.	1.96	1.17	2.28	1.15	0.01
Center	0.54	0.50	0.44	0.50	0.05
Click	0.26	0.44	0.17	0.38	0.04

Notes: The results are based on responses by students who participated in the financial literacy tutorial broken out by minority (non-Asian and non-White) status. The results are for statistical balance between the two groups. p-values are from t-tests of coefficient significant on each covariate in a regression of the Incentive indicator on all variables. The Fin. Lit. variable is the number of correct responses out of the four multiple choice questions.

Table A4: Balance Table of Student Responses to Financial Literacy Tutorial by First Gen. Status

	First-Gen. (N=80)		Non-First Gen. (N=456)		
	Mean	Std. Dev.	Mean	Std. Dev.	p-value
Duration	7.51	5.57	7.50	5.90	0.99
Loans	0.71	0.46	0.47	0.50	0.00
Debt	29938	18885	19276	18284	0.00
Family Help	0.61	0.49	0.88	0.33	0.00
Work	0.78	0.42	0.69	0.46	0.09
Fin. Lit.	1.98	1.04	2.25	1.18	0.04
Center	0.54	0.50	0.45	0.50	0.13
Click	0.26	0.44	0.18	0.38	0.11

Notes: The results are based on responses by students who participated in the financial literacy tutorial broken out by whether the student was a first-generation student or not. The results are for statistical balance between the two groups. p-values are from t-tests of coefficient significant on each covariate in a regression of the Incentive indicator on all variables. The Fin. Lit. variable is the number of correct responses out of the four multiple choice questions.

Table A5: Balance Table of Student Responses to Financial Literacy Tutorial by Evaluation Score

	Lower Eval. (N=231)		Upper Eval. (N=305)		
	Mean	Std. Dev.	Mean	Std. Dev.	p-value
Duration	7.59	6.29	7.44	5.49	0.77
Loans	0.52	0.50	0.50	0.50	0.58
Debt	22143	18981	19902	18540	0.17
Family Help	0.83	0.38	0.84	0.36	0.72
Work	0.70	0.46	0.70	0.46	0.94
Fin. Lit.	1.93	1.13	2.42	1.14	0.00
Center	0.47	0.50	0.45	0.50	0.60
Click	0.21	0.41	0.18	0.38	0.37

Notes: The results are based on responses by students who participated in the financial literacy tutorial broken out by whether the student is in the upper or lower half of the first-year evaluation score distribution. The results are for statistical balance between the two groups. p-values are from t-tests of coefficient significant on each covariate in a regression of the Incentive indicator on all variables. The Fin. Lit. variable is the number of correct responses out of the four multiple choice questions.

Table A6: Randomization Inference and Multiple Hypothesis Testing

Outcome	Variable	p-value RI	p-value Adj.	p-value
Credits	Email	0.43	0.42	0.56
GPA	Email	0.18	0.14	0.35
Cumulative Credits	Email	0.70	0.79	0.70
Credits	Incentive	0.06	0.07	0.18
GPA	Incentive	0.02	0.02	0.10
Cumulative Credits	Incentive	0.06	0.19	0.19

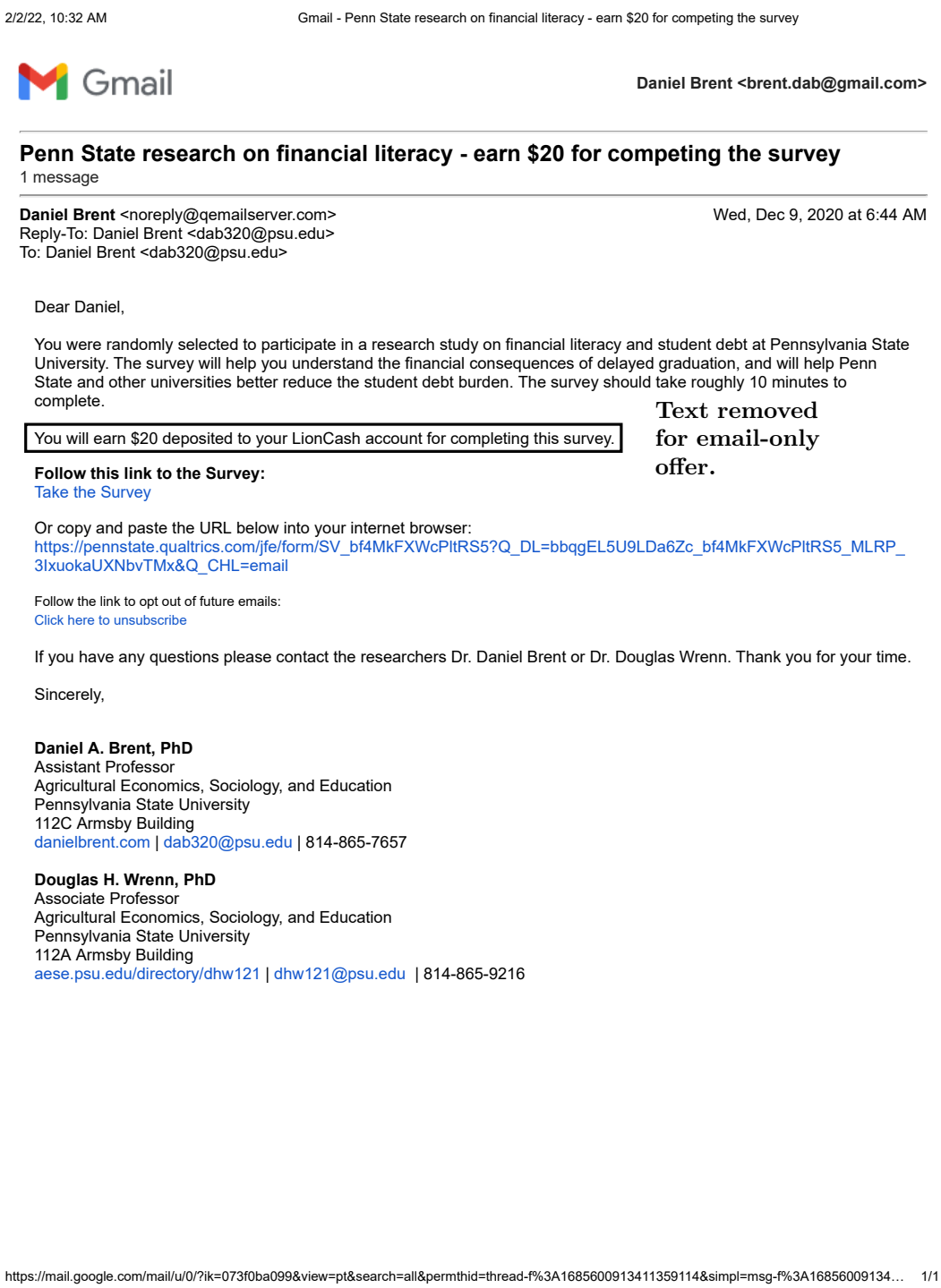
Notes: The table presents the asymptotic p-values presented in the regression tables (Tables 4 and 5), the p-values based on randomization inference (p-value RI), and randomization inference p-values adjusted for multiple hypothesis testing (p-value adj.). The Outcome column defines the outcome variable of interest and the Variable describes one of the two encouragements.

Table A7: Randomization Inference and Multiple Hypothesis Testing - Heterogeneity

Sub-Sample	Variable	p-value	p-value RI	p-value Adj.
Minority	Email	0.83	0.87	1.00
First-Gen.	Email	0.16	0.34	0.77
In-State	Email	0.23	0.39	0.84
Upper Eval.	Email	0.84	0.88	0.98
No Minority	Email	0.57	0.69	0.98
No First-Gen.	Email	0.81	0.87	1.00
Out-of-State	Email	0.30	0.48	0.91
Lower Eval.	Email	0.44	0.58	0.96
Minority	Incentive	0.02	0.13	0.19
First-Gen.	Incentive	0.72	0.81	1.00
In-State	Incentive	0.20	0.37	0.81
Upper Eval.	Incentive	0.88	0.92	0.89
No Minority	Incentive	0.38	0.53	0.95
No First-Gen.	Incentive	0.04	0.16	0.36
Out-of-State	Incentive	0.14	0.31	0.76
Lower Eval.	Incentive	0.00	0.06	0.05

Notes: The table presents the asymptotic p-values presented in the regression tables (p-value), the p-values based on randomization inference (p-value RI), and randomization inference p-values adjusted for multiple hypothesis testing (p-value adj.). The Sub-Sample column defines the sub-sample of interest and the variable describes one of the two encouragements.

Figure A1: Invitation to Qualtrics Survey



2/2/22, 10:32 AM Gmail - Penn State research on financial literacy - earn $20 for competing the survey

Gmail **Daniel Brent <brent.dab@gmail.com>**

Penn State research on financial literacy - earn $20 for competing the survey
1 message

Daniel Brent <noreply@qemailserver.com> Wed, Dec 9, 2020 at 6:44 AM
Reply-To: Daniel Brent <dab320@psu.edu>
To: Daniel Brent <dab320@psu.edu>

Dear Daniel,

You were randomly selected to participate in a research study on financial literacy and student debt at Pennsylvania State University. The survey will help you understand the financial consequences of delayed graduation, and will help Penn State and other universities better reduce the student debt burden. The survey should take roughly 10 minutes to complete.

You will earn $20 deposited to your LionCash account for completing this survey.

Text removed for email-only offer.

Follow this link to the Survey:
Take the Survey

Or copy and paste the URL below into your internet browser:
https://pennstate.qualtrics.com/jfe/form/SV_bf4MkFXWcPltRS5?Q_DL=bbqgEL5U9LDa6Zc_bf4MkFXWcPltRS5_MLRP_3IxuokaUXNbvTMx&Q_CHL=email

Follow the link to opt out of future emails:
Click here to unsubscribe

If you have any questions please contact the researchers Dr. Daniel Brent or Dr. Douglas Wrenn. Thank you for your time.

Sincerely,

Daniel A. Brent, PhD
Assistant Professor
Agricultural Economics, Sociology, and Education
Pennsylvania State University
112C Armsby Building
danielbrent.com | dab320@psu.edu | 814-865-7657

Douglas H. Wrenn, PhD
Associate Professor
Agricultural Economics, Sociology, and Education
Pennsylvania State University
112A Armsby Building
aese.psu.edu/directory/dhw121 | dhw121@psu.edu | 814-865-9216

https://mail.google.com/mail/u/0/?ik=073f0ba099&view=pt&search=all&permthid=thread-f%3A1685600913411359114&simpl=msg-f%3A16856009134… 1/1

Figure A2: Qualtrics Survey Instrument

Intro

Financial literacy is a broad topic focused on various personal financial topics such as debt management, investment strategies, budgeting, and major purchases such as cars and houses.

Financial literacy within higher education is critical since it has a major impact on future debt and earnings. This short survey is being conducted as a research project by economists at Penn State. The information supplements resources at the Sokolov-Miller Family Financial and Life Skills Center - https://financialliteracy.psu.edu/. There you will find a wealth of information on a variety of topics to help build your financial literacy.

This survey will focus on the **costs and benefits of on-time graduation**. We hope it will assist you in planning your academic schedule and coursework – deciding which classes to take and when and whether to drop a course – as well as becoming more efficient at allocating your time and effort between academic and non-academic priorities. We expect the survey to take 10-15 minutes.

Proceeding with the survey implies you consent to be part of a research project that will use your anonymized data. Your participation in this survey is voluntary and you may decide to stop at any time. You do not have to answer any questions that you do not want to answer. Your participation implies your voluntary consent to participate in the research.

If you have questions, complaints, or concerns about the research, you should contact Prof. Daniel Brent at dab320@psu.edu or Prof. Douglas Wrenn at dhw121@psu.edu. If you have questions regarding your rights as a research subject or concerns regarding your privacy, you may contact the Penn State Office for Research Protections at 814-865-1775.

Financial literacy is a broad topic focused on various personal financial topics such as debt management, investment strategies, budgeting, and major purchases such as cars and houses.

Financial literacy within higher education is critical since it has a major impact on future debt and earnings. This short survey is being conducted as a research project by economists at Penn State.

The information supplements resources at the Sokolov-Miller Family Financial and Life Skills Center - https://financialliteracy.psu.edu/. There you will find a wealth of information on a variety of topics to help build your financial literacy.

This survey will focus on the **costs and benefits of on-time graduation**. We hope it will assist you in planning your academic schedule and coursework – deciding which classes to take and when and whether to drop a course – as well as becoming more efficient at allocating your time and effort between academic and non-academic priorities. We expect the survey to take 10-15 minutes.

You will earn $10 for completing this survey. All questions must be answered. We will deposit $10 into your LionCash account within 1-2 weeks of completing the survey. Extra time may be needed for the deposit if the survey is completed close to PSU's winter break.

Proceeding with the survey implies you consent to be part of a research project that will use your anonymized data. Your participation in this survey is voluntary and you may decide to stop at any time. You do not have to answer any questions that you do not want to answer. Your participation implies your voluntary consent to participate in the research.

If you have questions, complaints, or concerns about the research, you should contact Prof. Daniel Brent at dab320@psu.edu or Prof. Douglas Wrenn at dhw121@psu.edu. If you have questions regarding your rights as a research subject or concerns regarding your privacy, you may contact the Penn State Office for Research Protections at 814-865-1775.

Financial literacy is a broad topic focused on various personal financial topics such as debt management, investment strategies, budgeting, and major purchases such as cars and houses.

Financial literacy within higher education is critical since it has a major impact on future debt and earnings. This short survey is being conducted as a research project by economists at Penn State.

The information supplements resources at the Sokolov-Miller Family Financial and Life Skills Center - https://financialliteracy.psu.edu/. There you will find a wealth of information on a variety of topics to help build your financial literacy.

This survey will focus on the **costs and benefits of on-time graduation**. We hope it will assist you in planning your academic schedule and coursework – deciding which classes to take and when and whether to drop a course – as well as becoming more efficient at allocating your time and effort between academic and non-academic priorities. We expect the survey to take 10-15 minutes.

You will earn $20 for completing this survey. All questions must be answered. We will deposit $20 into your LionCash account within 1-2 weeks of completing the survey. Extra time may be needed for the deposit if the survey is completed close to PSU's winter break.

Proceeding with the survey implies you consent to be part of a research project that will use your anonymized data. Your participation in this survey is voluntary and you may decide to stop at any time. You do not have to answer any questions that you do not want to answer. Your participation implies your voluntary consent to participate in the research.

If you have questions, complaints, or concerns about the research, you should contact Prof. Daniel Brent at dab320@psu.edu or Prof. Douglas Wrenn at dhw121@psu.edu. If you have questions regarding your rights as a research subject or concerns regarding your privacy, you may contact the Penn State Office for Research Protections at 814-865-1775.

The survey will contain four parts.

1. Questions about your financial situation
2. Questions about your financial literacy
3. Information about the costs of college and benefits
4. Financial literacy resources at Penn State

Financial Situation

Did you take out loans to finance your education?

- ○ Yes
- ○ No
- ○ Prefer not to answer

How much debt do you anticipate to have when you graduate?

- ○ Less than $10,000
- ○ $10,000-$20,000

- ○ $20,000-$30,000
- ○ $30,000-$40,000
- ○ $40,000-$50,000
- ○ More than $50,000
- ○ Prefer not to answer

Are you receiving financial assistance from your family in paying for college (including living expenses)?

- ○ Yes
- ○ No
- ○ Prefer not to answer

Are you working, or do you plan to work, while attending Penn State?

- ○ Yes
- ○ No
- ○ Do not know
- ○ Prefer not to answer

Financial Literacy Questions

Suppose you had $100 in a free savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow:

- ○ More than $102
- ○ Exactly $102
- ○ Less than $102
- ○ Do not know

Suppose that the interest rate on your free savings account was 1% per year and inflation was 2% per year. After 1 year, with the money in this account would you be able to buy:

- ○ More than today
- ○ Exactly the same as today
- ○ Less than today
- ○ Do not know

Do you think that the following statement is true or false? Buying a single company stock usually provides a safer return than a stock mutual fund.

- ○ True
- ○ False
- ○ Do not know

Approximately how much money would you have if you invested $1000 today for 30 years with a 7.2 % return?

- ○ $3,500
- ○ $8,000
- ○ $10,000
- ○ Do not know

college_finance

This section illustrates the financial implications of taking extra time to graduate.

There are many factors that influence on-time graduation.

The scenario we present in this survey is based on a hypothetical situation where a student drops a class, which forces them to borrow money to stay an extra semester.

We recognize that individual circumstances will differ. The goal of this section is not to direct you, but to provide you with the information and tools necessary to make informed decisions based on your own situation.

To ensure that you read all of the information, you can only move onto the next page after sufficient time has elapsed.

The table below shows how long it takes the average Penn State student to graduate.

Time to graduate	% of Students
4 years	62.5%
5 years	82.6%
6 years	84.9%

Based on these results, 20% of Penn State undergraduates spend at least one extra year in school.

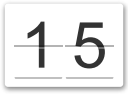
Suppose you are considering dropping a class, which would force you to stay an extra semester.

What might this decision cost you in the long run? What are the tradeoffs?

To personalize the information to your particular situation, please indicate whether you are paying in-state or out-of-state tuition.

- ○ I pay in-state tuition
- ○ I pay out-of-state tuition

college_finance_in



The Costs

Current Penn State estimates for annual tuition and fees for in-state students are:

- Tuition and Fees (in-state): **$18,450**
- Room and Board: **$11,884**

Since you will likely need to pay for food and lodging regardless of whether you are at Penn State, we will only include tuition and fees. The cost estimates are as follows:

- Tuition and fees for one semester= **$9,225**
- If you take out a student loan to pay for these costs the **typical student loan is repaid in 10 years at a 4.5%** interest rate.
- Interest payments = **$2,225**.
- Monthly payment for 10 years = **$95**
- That is an **extra $95 every month for 10 years** for dropping one class!

Note that interest rates change over time, and a higher rate will lead to a larger debt burden. If you take out loans for living expenses, like room and board, your debt will be even higher.

The Tradeoffs

We now know that delaying graduation even one semester can cost a lot. But, there may be benefits if having a higher GPA gets you a better job or into a better graduate school.

There are some rules of thumb about the necessary starting salary to comfortably repay student loans. (The typical debt of a Penn State graduate is $25,000.)

Penn State provides the following template which suggests that total annual payments on debt should be less than 8% of your starting salary.

Debt and Recommended Salaries

	Amount Borrowed	Estimated Monthly Payments	Recommended Annual Salaries
Average Penn State Undergrad	$25,000	$259.10	$38,865.00
Average Penn State Undergrad plus an extra semester	$34,225	$354.70	$53,205.00

From our scenario, dropping a class and staying an extra semester would add $9,225 in debt bringing you to **$34,000 in total debt** at graduation.

In order to comfortably pay for the initial ($25,000) in debt you would need a **starting salary of $39,000**, but with the new debt ($34,225) you would need a **starting salary of $53,000**.

The question is whether the higher GPA achieved by staying an extra semester produces an income high enough to cover the extra borrowing costs?

Keep in mind that this is just an example.

It is possible that the costs of delaying graduation could make sense financially - particularly if you changed your major from one with a low starting salary to one with a higher starting salary.

You can familiarize yourself with typical starting salaries by majors at this website in the "Field of Study" section:

Below is a histogram of starting salaries along with the average starting salary. Clearly there is a wide range of earnings depending on your major.

Was the information provided new to you?

- ○ Yes
- ○ Somewhat
- ○ No

Did you find the information provided in this section to be useful?

- ○ Yes
- ○ Maybe
- ○ No

college_finance_out

15

The Costs

Current Penn State estimates for annual tuition and fees for out-of-state students are:

- Tuition and Fees (in-state): **$35,984**
- Room and Board: **$11,884**

Since you will likely need to pay for food and lodging regardless of whether you are at Penn State, we will only include tuition and fees. The cost estimates are as follows:

- Tuition and fees for one semester= **$17,922**
- If you take out a student loan to pay for these costs the **typical student loan is repaid in 10 years at a 4.5%** interest rate.
- Interest payments = **$4,400**.
- Monthly payment for 10 years = **$186**
- That is an **extra $186 every month for 10 years** for dropping one class!

Note that interest rates change over time, and a higher rate will lead to a larger debt burden. If you take out loans for living expenses, like room and board, your debt will be even higher.

10

The Tradeoffs

We now know that delaying graduation even one semester can cost a lot. But, there may be benefits if having a higher GPA gets you a better job or into a better graduate school.

There are some rules of thumb about the necessary starting salary to comfortably repay student loans. (The typical debt of a Penn State graduate is $25,000.)

Penn State provides the following template which suggests that total annual payments on debt should be less than 8% of your starting salary.

Debt and Recommended Salaries

	Amount Borrowed	Estimated Monthly Payments	Recommended Annual Salaries
Average Penn State Undergrad	$25,000	$259.10	$38,865.00
Average Penn State Undergrad plus an extra semester	$42,992	$445.56	$66,834.00

From our scenario, dropping a class and staying an extra semester would add $17,922 in debt bringing you to **$43,000 in total debt** at graduation.

In order to comfortably pay for the initial ($25,000) in debt you would need a **starting salary of $39,000**, but with the new debt ($43,000) you would need a **starting salary of $66,000**.

The question is whether the higher GPA achieved by staying an extra semester produces an income high enough to cover the extra borrowing costs?

Keep in mind that this is just an example.

It is possible that the costs of delaying graduation could make sense financially - particularly if you changed your major from one with a low starting salary to one with a higher starting salary.

You can familiarize yourself with typical starting salaries by majors at this website in the "Field of Study" section:

Below is a histogram of starting salaries along with the average starting salary. Clearly there is a wide range of earnings depending on your major.

Was the information provided new to you?

- ○ Yes
- ○ Somewhat
- ○ No

Did you find the information provided in this section to be useful?

- ○ Yes
- ○ Maybe
- ○ No

Sokolov-Miller Center

We recognize there are many reasons for dropping or failing classes. This information is not intended to describe whether your decisions are right or wrong. Rather, we hope to inform you about the financial consequences of your decisions in college. There is a wealth of additional information including individual sessions available through the Sokolov-Miller Family Financial and Life Skills Center.

Click here to learn more about the Center.

Are you now likely to utilize the Sokolov-Miller Family Financial and Life Skills Center **online** resources?

- ○ Definitely yes
- ○ Probably yes
- ○ Might or might not
- ○ Probably not
- ○ Definitely not

Are you now likely to utilize the Sokolov-Miller Family Financial and Life Skills Center **in-person** resources?

- ○ Definitely yes
- ○ Probably yes
- ○ Might or might not
- ○ Probably not
- ○ Definitely not

You indicated that you are interested in the **online resources** from the Sokolov-Miller Family Financial and Life Skills Center.

Please click here to view the resources available online.

You indicated that you are interested in setting up an **individualized session** with the Sokolov-Miller Family Financial and Life Skills Center.

Please click here to set up an individualized session.

Comments

Thank you for your participation. Please provide any comments you may have below.