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Nudging study habits: A field experiment on peer tutoring in higher education☆

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ABSTRACT

More than two of every five students who enroll in college fail to graduate within six years. Peer tutoring offers one approach to improve learning outcomes in higher education. We conducted a randomized controlled experiment designed to increase take-up of university tutoring services. Brief, one-time messages increased tutoring take-up by seven percentage points, or 23% of the control group mean. Attendance at multiple tutoring sessions increased by nearly the same amount, suggesting substantial changes in study habits in response to a simple and inexpensive intervention. The intervention cost $3.32–$14.58 per additional tutoring hour, the lowest reported in the literature on peer tutoring experiments. We find little evidence of advertising-induced tutoring on learning outcomes.

1. Introduction

More than two out of every five students who enrolled in college in 2007 failed to graduate by 2013. Even at selective four-year institutions, more than one-third of students did not graduate in six years (National Center for Education Statistics, 2014).[1] Studying is a fundamental input for student success in college, yet many students study less than necessary to progress to graduation (Beattie, Lalibert, Michaud-Leclerc, & Oreopoulos, 2017). University students who procrastinate, as measured by self-reported cramming for exams (Beattie, Lalibert, & Oreopoulos, 2016) or small delays in course enrollment (Banerjee & Duflo, 2014; De Paola & Scoppa, 2015; Novarese & Di Giovinazzo, 2013), have worse academic outcomes. Stinebrickner and Stinebrickner (2008) and Lindo, Swensen, and Waddell (2012) found that exogenous increases in campus distractions (video games owned by a randomly assigned roommate and the success of the university football team, respectively) led students to study less and earn lower grades. Yet little experimental or quasi-experimental evidence exists on how to change study habits.

Peer tutoring offers one approach to change study habits and improve student outcomes in higher education. This paper evaluates a randomized experiment that advertised peer tutoring services to college students via postcard. The experiment varied the messages used to encourage students to attend tutoring, including framing tutoring as a positive social norm or offering small financial incentives to overcome resistance to attendance. We compare these messages to a benchmark postcard that only provided information about tutoring, and to a pure control group that received no advertising.

We find that advertising increased tutoring attendance by seven percentage points, or 23% of the control group mean. Moreover, the experiment increased attendance at multiple tutoring sessions by 6 percentage points, nearly the same magnitude as the effect on attendance at a single session. This finding suggests durable changes in study behavior for a simple and inexpensive intervention.

Comparing tutoring take-up across postcards, we find no significant differences across messages. At first glance, this finding suggests that students responded to the informational content of the advertisements. However, further exploration reveals similar responses to the postcards across class years, which is not entirely consistent with the informational mechanism, as we expect older students to be more aware of tutoring services prior to postcard receipt. We also find that students more prone to procrastination, as measured by delays in course

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[1] "Selective" refers to admissions rate between 25–49% . Figures for public institutions only.

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registration, respond as strongly to postcards as those less prone to procrastination. Tutoring take-up therefore appears unrelated to information alone or to students' propensity to procrastinate rather than study. Instead, the evidence suggests that simply making the existence of tutoring services salient induced more students to attend.

When using the random variation in postcard receipt as an instrument, we find no evidence that tutoring altered student grades. This finding is consistent with substitution of tutoring with an equally effective alternative form of study, or with substitution of study effort from untutored to tutored courses, leaving overall grades unchanged. Our null findings on grades are also consistent with the potential ineffectiveness of tutoring as a study strategy among the subpopulation of students induced to attend tutoring through the postcard. Nonetheless, our findings on increased attendance at multiple tutoring sessions suggests that students valued the service. Moreover, point estimates of the effect of tutoring on student grades are too imprecise to rule out positive effects.

Universities have employed a range of efforts to increase retention, including better targeting of financial aid, remedial courses, and increased advising. Between 1987 and 2008, expenditure on student services, of which tutoring is a part, grew at nearly double the rate of instructional expenditures across every higher education institutional category (Ehrenberg, 2012, p. 205). Peer tutoring offers at least two advantages relative to other student services. First, it is low cost. Because tutors are also students, they can be hired at the relatively low prevailing wage of student workers. Second, tutoring engages students in behavior directly intended to increase their academic performance. It can therefore complement other efforts, such as removing financial barriers or advising, intended to promote student success.

We make three main contributions to the literature on improving student outcomes in higher education. First, we demonstrate that a low-cost, one-time intervention to promote peer tutoring can meaningfully alter study behavior. Four prior studies (Angrist, Lang, & Oreopoulos, 2009; ideas42, 2015; Paloyo, Rogan, & Siminski, 2016; Parkinson, 2009)—conducted at an Irish university, a Canadian university, a community college in the United States, and an Australian university, respectively—have evaluated peer tutoring using an experimental design.[2] As in our work, each of these studies found that randomly encouraging students to attend peer tutoring sessions increased take-up.[3] Also as in our work, three of the four studies failed to find significant positive effects of tutoring on student performance.[4] We extend these findings to a new setting, bolstering the external validity of the experimental results. At $3.32–$14.58 per additional hour of tutoring, ours has the lowest reported costs among these experiments, yet it was sufficient to alter behavior.

Perhaps more importantly, we provide suggestive evidence that the channel through which tutoring attendance increased was not information alone or reduced procrastination, but the increased salience of tutoring availability. Our work therefore provides new evidence on a common way that colleges provide individualized academic support across the curriculum at low cost.

Second, we contribute to the broader literature applying the insights of behavioral economics to education (Koch, Nafziger, & Nielsen, 2015; Lavecchia, Liu, & Oreopoulos, 2014). Specifically, we provide evidence consistent with the presence of present bias and social stigma among students. Studying, either alone or with a tutor, has salient and immediate costs, with distant and uncertain future benefits. Students with a bias for present utility may therefore make suboptimal studying choices. Making the availability or benefits of tutoring more salient might counter present bias and increase investments.

Another behavioral explanation for suboptimal human capital investment is student concern about identity. Feelings of social exclusion can decrease utility (Akerlof & Kranton, 2002) and reduce cognitive performance (Baumeister, Twenge, & Nuss, 2002). If students place high value on perceived intellectual ability, then seeking assistance through tutoring could carry a stigma that leads to its underuse. On the other hand, interventions to increase a sense of belonging can improve academic performance (Walton & Cohen, 2007; 2011). One treatment arm of our study addresses stigma by framing tutoring as a strategy used by successful students.

Third, our work is part of a burgeoning literature on nudges—changes to the presentation of choices that do not meaningfully alter costs or benefits (Thaler & Sunstein, 2008)—in higher education (ideas42, 2016). These nudges include efforts to increase college applications, enrollment, or financial aid among potential college students currently enrolled in high school (Bettinger, Long, Oreopoulos, & Sanbonmatsu, 2012; Castleman & Page, 2015; Castleman, Page, & Schooley, 2014; Hoxby & Turner, 2013), as well as interventions to improve outcomes among students already enrolled (Angrist et al., 2009; ideas42, 2015; Smith, White, Kuzyk, & Tierney, 2017). Nudging students to attend peer tutoring can serve as a low-cost complement or alternative to remedial courses (Bettinger & Long, 2009; Calcagno & Long, 2008; De Paola & Scoppa, 2015; Martorell & McFarlin Jr, 2011; Moss & Yeaton, 2006; Scott-Clayton & Rodriguez, 2014) and student advising (Angrist et al., 2009; Bettinger & Baker, 2013; Ellis & Gershenson, 2016; Visher, Butcher, & Cerna, 2011) as a way to promote student retention and graduation. Our advertising devices are a variant of those used in Wilson, Frade, Rech, and Friedman (2016) and in Friedman and Wilson (2016), studies that examined how to increase household investment in another component of human capital production (preventive health inputs).

In the next section, we describe the research setting and experimental design. Section 3 describes the data and empirical methods. Section 4 presents results and Section 5 concludes.

2. Program description

2.1. Study setting

We conducted this experiment at Reed College, an elite liberal arts college in Portland, Oregon. Reed enrolls 1400 students and has a student-faculty ratio of nine to one.[5] It is highly selective, admitting 35 percent of applicants, with an average high school GPA of 3.9 and mean SAT score of 2060 (95th percentile on a scale of 2400) among admitted students. The student body is 54% female, with a racial and ethnic composition of 60% white, 10% Asian, 10% Hispanic, 5% black, 8% international, and the remaining 7% in other categories. The college offers 40 majors, of which the most popular categories are in mathematics and natural sciences (29%) and history and social sciences (23%). Despite the college's elite status, 20% of students receive Pell Grants, giving Reed a higher share of low-income students than many peer institutions (Burd, 2013). The six-year graduation rate is 79% , whereas many elite liberal arts colleges have six-year graduation rates above 90% (Grove, 2017; US News & World Report, 2016).

[2] Other studies have found positive effects of peer tutoring on student outcomes (Dawson, van der Meer, Skalicky, & Cowley, 2014; Munley, Garvey, & McConnell, 2010), but rely on observational data and may therefore be biased due to student self-selection into tutoring.

[3] Instead of providing randomized encouragement, Parkinson (2009) randomly assigned students to receive tutoring within sections of particular classes.

[4] The exception is Parkinson (2009), which studied a sample of 67 students at an Irish university assigned to tutoring in specific courses, rather than to general tutoring services, as in the other studies including ours. Parkinson (2009) also reclassified students who did not comply with treatment as members of the control group, introducing potential bias in the estimated grade effects. Angrist et al. (2009) studied a program that bundled peer advising and tutoring at a Canadian university. They found positive effects on student grades only when the intervention was combined with a large financial incentive requiring students to maintain high grades in exchange for a scholarship.

[5] All data in this section are from 2015 and made available by the Reed Office of Institutional Research, unless otherwise noted.

Fig. 1. Postcards for each treatment arm.
Note: "The DoJo" refers to the main campus tutoring center, located in the Dorothy Johansen House. The Paradox, referenced in postcards (c) and (d), is the campus coffee shop.

Reed offers a range of peer tutoring services that students may access free. Tutors are hired from a pool of advanced students who have been recommended by faculty members. A tutoring center with a dedicated working space is open 9am–11pm, seven days per week during the academic year. At the center, students can receive drop-in peer tutoring in the most popular courses in biology, chemistry, economics, and mathematics, as well as writing assignments in any discipline. Students can also make appointments with individual peer tutors for one-on-one sessions at the tutoring center or another location, though free sessions are limited to one hour per class, per week. The departments of biology, languages, mathematics, and physics also maintain their own satellite tutoring centers that follow the same arrangements as the main tutoring center. Overall, nearly 250 students are eligible tutors, with 35 regularly employed for drop-in tutoring at the main center. During the academic year of the experiment, the tutoring center served 348 students (25% of the student body), spread across 1707 tutoring visits. Of students who received tutoring at the center, 69% returned for a second visit and 51% visited three or more times.

2.2. Experimental design

We conducted our experiment during the spring semester of 2015. All Reed students were randomly selected to be in a pure control group or one of four treatment arms. Students studying abroad, on leave, or who enrolled after random assignment were excluded from the sample (explaining the discrepancy between Reed's total enrollment and the number of experimental participants). Each treatment consisted of a postcard placed in the student's on-campus mailbox at the beginning of the semester. The postcards, designed in consultation with tutoring center administrators, varied as follows (see Fig. 1 for images of the postcards):

1. *Information*. Provided information about the tutoring services offered at the center and its opening hours.
2. *Framing*. Provided information about tutoring, plus the message that "successful" students "know when to ask for help."
3. *Incentive ($5)*. Provided information about tutoring, plus offered $5 credit at the campus coffee shop if the student attended at least one hour of tutoring.[6]
4. *Incentive ($10)*. Provided information about tutoring, plus offered $10 credit at the campus coffee shop if the student attended at least one hour of tutoring.

In the financial incentives treatments, attendance at any type of tutoring (drop-in, individual, or satellite center) would allow students to redeem the postcard for the given amount at the coffee shop. Postcards were addressed to individual students and tutors checked that the student presenting the incentive postcard to a tutor was its intended recipient, minimizing risk of students sharing incentive postcards across study arms.

Postcards were mailed to students in early February 2015, during the second week of class that semester.[7] Campus mail remains a common form of communication at the college. Conversation with

[6] The campus coffee shop sells goods typically found in coffee shops and is located in the center of campus.

[7] We chose the second week of classes to distribute the postcards to ensure salience of their receipt. A disproportionate volume of campus mail is distributed to students during the first week of classes of each semester.

students in classes and in office hours indicates that the vast majority of students report checking their mail box every day.[8] All students in the campus mail system were automatically enrolled in the experiment, which includes virtually all students. Random assignment occurred within strata defined by student gender, class year, and academic division of their major.[9] We assigned 327 students to the control group (26%), 312 students to the information postcard (25%), 310 students to the framing postcard (25%), 159 students to the $5 incentive (13%), and 151 students to the $10 incentive (12%). More details on student characteristics and balance tests across study arms appear in the Data section.

The theory underlying the experimental design is student decision-making under uncertainty, in which tutoring is a human capital investment with uncertain benefits. When considering tutoring, a student weighs the costs of attending tutoring with its expected benefits. Costs include the opportunity cost of spending the time elsewhere, as well as a potential stigma associated with seeking academic help. The potential benefits include an enhanced understanding of a certain assignment or topic, the chance to learn amongst peers, and the associated academic outcomes that accompany each of these.

Each treatment attempted to overcome a different perceived constraint to student use of tutoring. If students were unaware of the presence of the tutoring center but would otherwise demand its services, then comparing the first treatment to the control group will measure the marginal value of this information. Alternately, students already aware of the tutoring center might be induced to attend because the postcard makes tutoring more salient in their decisions.

The second postcard framed this information by associating use of the tutoring center with student success and other positive attributes, such as resourcefulness and scholarly engagement. If tutoring carried a negative stigma—a particular concern on a campus of high-achieving students—then this framing should improve tutoring center usage relative to information alone.

The third and fourth treatments paired information with financial incentives. The financial incentive was modest and intended to overcome perceived transactions costs to attending tutoring. For instance, if some students were on the margin of choosing tutoring over an alternative activity, the financial incentive might induce them to attend. In this regard these treatments resemble nudges rather than changes in student income, in contrast to the gift certificates raffled by Paloyo et al. (2016), which had denominations of US$735 or US$3715, or the merit scholarships offered by Angrist et al. (2009), which were worth either US$1000 or US$5000, depending on student performance.

3. Data and methodology

During the semester of the experiment, tutoring attendance and final course grades among all students in the experiment were recorded in the existing administrative data collection system. All tutoring centers on campus kept records of student visits.[10] We also monitored redemptions of postcards at the coffee shop among students assigned the financial incentive treatments.

Our primary questions of interest are:

1. What was the effect of receiving a postcard on demand for tutoring?
2. Were some postcards more effective than others?
3. What was the effect of tutoring on grades?

Because we randomized the allocation of postcards, simple comparisons of mean outcomes such as tutoring attendance and grades across treatment groups should yield unbiased estimates of these effects. However, to improve precision of estimates and to mitigate any spurious correlations between observed characteristics and treatment assignment, we also use regression analysis.

To measure whether the intervention increased the demand for tutoring, we estimate the parameters of the following regression:

$$tutor_i = \beta_0 + \beta_1 info_i + \beta_2 framing_i + \beta_3 FiveDollars_i + \beta_4 TenDollars_i + \delta_s + \epsilon_i \quad (1)$$

where i indexes students; *tutor* is an indicator for tutoring attendance; *info, framing, FiveDollars*, and *TenDollars* are dummy variables for being assigned to the information, framing, $5 financial incentive, and $10 financial incentive treatment arms, respectively; δ_s is a stratum (gender-class year-division of major) fixed effect; and ϵ is an error term. Including strata fixed effects ensures that the variation in treatment status is random with respect to these characteristics. We also run specifications with additional control variables, such as baseline GPA, race, and international student status.

The coefficients β_1 through β_4 measure the effect of each type of postcard on tutoring attendance relative to students who did not receive any postcard, which is the omitted category. We also run variants of Eq. (1) in which we pool multiple treatment indicators into one variable. In one specification, we combine the financial incentive indicators, in order to check whether offering any financial incentive increases take-up. In another specification, we combine all the treatment dummies into a single indicator for receiving any postcard, to test whether these combinations of treatments have an effect:

$$tutor_i = \beta_0 + \beta_1 anypostcard_i + \delta_s + \epsilon_i \quad (2)$$

We also examine whether postcards affected whether students attended more than one tutoring session.

To measure the effect of the intervention on grades, we replace the outcome in Eq. (2) with student GPA in the semester of the experiment. The coefficient on the treatment dummy then measures the effect of receiving any postcard on grades regardless of tutoring attendance, or the intent-to-treat effect (ITT).

To measure whether tutoring altered grades, we use an instrumental variables strategy in which Eq. (2) is the first stage.[11] In the second stage, we regress grades on tutoring attendance, using the treatment indicator as an instrument:

$$GPA_i = \alpha + \gamma tutor_i + \delta_s + \epsilon_i \quad (3)$$

where *GPA* is student grades and all else is as in Eq. (2). The coefficient of interest is γ, which measures the local average treatment effect (LATE) of tutoring. In other words, γ is the effect of tutoring on students who attended tutoring because they received a postcard, but would not have attended otherwise. This coefficient will be positive if tutoring is a more effective form of studying than the student's alternative use of time. This assumption seems reasonable if this alternative use of time is socializing or a non-academic activity. However, if the postcard leads students to substitute tutoring for time spent studying independently, or to reduce subsequent study time, then the coefficient may be zero or even negative.

4. Results

4.1. Balance tests

We first check that randomization was successful in balancing the characteristics of students across study arms. Table 1 shows mean

[8] Even among students who reside off campus, the majority come to campus every day to attend class and to study in the library, including on days that they do not have classes.

[9] There are five academic divisions: Arts; History and Social Sciences; Literature and Languages; Math and Natural Sciences; and Philosophy, Religion, Psychology, and Linguistics. Interdisciplinary, ad-hoc, and undecided were combined in one group for purposes of the experiment.

[10] Unfortunately, we do not have records of the courses in which students received tutoring, preventing us from connecting tutoring to specific course grades.

[11] We also tried using Eq. (1) as the first stage, but the instruments were weaker, increasing the risk of biased estimates of the treatment effect.

Table 1
Descriptive statistics for baseline characteristics by study arm.

Study arm:	Control	Postcard				p-value
		Information	Framing	Five dollars	Ten dollars	
	(1)	(2)	(3)	(4)	(5)	(6)
Freshman	0.269	0.260	0.268	0.264	0.272	.985
	[0.025]	[0.025]	[0.025]	[0.035]	[0.036]	
Sophomore	0.239	0.253	0.245	0.258	0.258	.958
	[0.024]	[0.025]	[0.024]	[0.035]	[0.036]	
Junior	0.248	0.237	0.239	0.239	0.232	.977
	[0.024]	[0.024]	[0.024]	[0.034]	[0.034]	
Senior	0.245	0.250	0.248	0.239	0.238	.979
	[0.024]	[0.025]	[0.025]	[0.034]	[0.035]	
Female	0.541	0.548	0.539	0.541	0.550	.783
	[0.028]	[0.028]	[0.028]	[0.040]	[0.041]	
Asian	0.171	0.135	0.142	0.138	0.159	.775
	[0.021]	[0.019]	[0.020]	[0.027]	[0.030]	
Black	0.040	0.045	0.052	0.088	0.066	.299
	[0.011]	[0.012]	[0.013]	[0.023]	[0.020]	
Hispanic	0.125	0.109	0.139	0.069	0.079	.177
	[0.018]	[0.018]	[0.020]	[0.020]	[0.022]	
American Indian	0.031	0.016	0.042	0.050	0.020	.284
	[0.010]	[0.007]	[0.011]	[0.017]	[0.011]	
Pacific Islander	0.012	0.003	0.010	0.006	0.000	.659
	[0.006]	[0.003]	[0.006]	[0.006]	[0.000]	
White	0.752	0.747	0.787	0.767	0.781	.744
	[0.024]	[0.025]	[0.023]	[0.034]	[0.034]	
International	0.095	0.058	0.065	0.075	0.073	.568
	[0.016]	[0.013]	[0.014]	[0.021]	[0.021]	
GPA (baseline)	3.071	3.079	3.064	3.080	3.063	.338
	[0.029]	[0.032]	[0.032]	[0.039]	[0.047]	
Observations	327	312	310	159	151	

Notes: Standard deviations are in brackets. All variables measured at baseline. GPA [baseline] is pre-experiment grade point average. All other variables are indicator variables equal to one if true and zero otherwise. p-value is from joint test of orthogonality of all treatment arms.

characteristics of students in the control group (column (1)) and each treatment group (columns (2)–(5)). The *p*-value testing the orthogonality of all treatment arms for each characteristic appears in column (6). Indicators for treatment are not jointly statistically significant predictors of any of the characteristics considered, including class year, gender, race/ethnicity, and baseline GPA. We conclude that the randomization was successful. We also control for the characteristics listed in the table in our regression estimates to account for any spurious correlations and to improve precision.

4.2. Tutoring take-up

Fig. 2 shows unadjusted rates of tutoring take-up across treatment arms. Attendance in at least one tutoring session exceeds the control group mean of 29% in all treatment arms. Take-up among students receiving postcards varies in a narrow range from 34% (for the ten-dollar incentive) to 37% (for the information-only postcard). Attendance at multiple tutoring sessions also exceeds the control group mean in all treatment arms (24–25% among students receiving postcards vs. 18% in the control group).

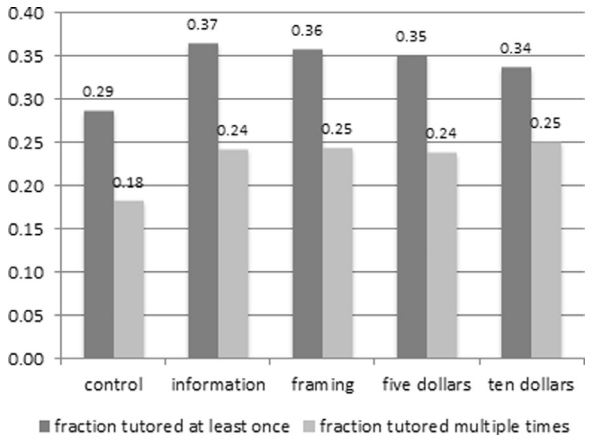

Fig. 2. Tutoring take-up by treatment arm.

Table 2 presents regression estimates of take-up, following Eq. (1). In column (1), we find that students receiving the information-only postcard were 7.9 percentage points more likely to attend tutoring. The coefficient is large relative to the control group mean of 29% and significant at the 5% level. The coefficient on the framing postcard is similar in magnitude, representing a 7.1 percentage-point increase in tutoring attendance, and is also significant at 5% . This result is consistent with our hypothesis that tutoring attendance carries a social stigma in this context, though it is surprising that the effect is not larger than the information-only postcard. Coefficients on the five- and ten-dollar incentive treatments are also positive, but not statistically distinguishable from zero. When comparing coefficients across treatment arms, we cannot reject that all treatments had an identical effect on tutoring take-up, as shown by the *p*-values from pairwise comparison of treatment arm coefficients at the bottom of column (1).

To check whether imprecision in the financial incentive treatment coefficients is due to lack of statistical power, in column (2) we pool the incentive treatments into a single indicator labeled “Money.” The coefficient on this pooled treatment shows a 5.3 percentage-point increase in tutoring attendance relative to the control group, but remains statistically indistinguishable from zero. Among students in the financial incentives treatments who attended a tutoring session, less than 10% redeemed their postcards at the campus coffee shop. These relatively low take-up and redemption rates suggest that students did not respond to the financial incentive itself, but to the increased prominence of tutoring in their decisions due to the postcard.

Column (3) pools all treatments together, as in Eq. 2. Receiving any postcard increased the likelihood of tutoring attendance by 6.7

Table 2
Effect of postcards on tutoring take-up.

Dependent variable:	Tutored (Y/N)					
	(1)	(2)	(3)	(4)	(5)	(6)
Information	0.079**	0.079**		0.088**	0.088**	
	(0.036)	(0.036)		(0.036)	(0.036)	
Framing	0.071**	0.071**		0.072**	0.072**	
	(0.036)	(0.036)		(0.035)	(0.035)	
Five dollars	0.059			0.066		
	(0.043)			(0.043)		
Ten dollars	0.046			0.055		
	(0.045)			(0.044)		
Money		0.053			0.061*	
		(0.035)			(0.035)	
Any postcard			0.067**			0.074***
			(0.028)			(0.028)
p-values on equality of coefficients						
information = framing	.834			.673		
information = five dollars	.672			.626		
information = ten dollars	.482			.472		
framing = five dollars	.803			.895		
framing = ten dollars	.592			.707		
five dollars = ten dollars	.795			.825		
Additional controls	NO	NO	NO	YES	YES	YES
Observations	1259	1259	1259	1259	1259	1259

Notes: Parameters estimated using ordinary least squares (OLS) regression. Robust standard errors are in parentheses. Tutored is an indicator variable equal to one if the student came for a tutoring session in the semester and zero otherwise. All specifications include indicator variables for randomization assignment strata defined by gender, class year, and academic division. "Additional controls" are GPA at baseline, Asian, Black, Hispanic, American Indian, Pacific Islander, and international student indicator variables.
*** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

Table 3
Effect of postcards on multiple tutoring.

Dependent variable:	Multiple tutoring sessions (Y/N)					
	(1)	(2)	(3)	(4)	(5)	(6)
Information	0.061*	0.061*		0.068**	0.068**	
	(0.032)	(0.032)		(0.032)	(0.032)	
Framing	0.062**	0.062**		0.064**	0.064**	
	(0.031)	(0.031)		(0.031)	(0.031)	
Five dollars	0.053			0.057		
	(0.039)			(0.039)		
Ten dollars	0.065			0.072*		
	(0.040)			(0.039)		
Money		0.059*			0.064**	
		(0.031)			(0.031)	
Any postcard			0.061**			0.065***
			(0.025)			(0.025)
p-values on equality of coefficients						
information = framing	.965			.894		
information = five dollars	.845			.789		
information = ten dollars	.927			.922		
framing = five dollars	.814			.872		
framing = ten dollars	.955			.833		
five dollars = ten dollars	.800			.745		
Additional controls	NO	NO	NO	YES	YES	YES
Observations	1259	1259	1259	1259	1259	1259

Notes: Parameters estimated using ordinary least squares (OLS) regression. Robust standard errors are in parentheses. Tutored is an indicator variable equal to one if the student came for a tutoring session in the semester and zero otherwise. All specifications include indicator variables for randomization assignment strata defined by gender, class year, and academic division. "Additional controls" are GPA at baseline, Asian, Black, Hispanic, American Indian, Pacific Islander, and international student indicator variables.
*** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

percentage points, or 23% of the control group mean, significant at 5% . The magnitude of this effect is similar to ideas42 (2015), which found that emails increased tutoring attendance from 5% to 7% , or 34% of the control group mean.

Columns (4)–(6) add a student's baseline GPA, race, and international student status to the specifications in the first three columns. Coefficients increase in magnitude and become more precise when adding these controls. The effects of the information and framing treatments grow to 8.8 and 7.2 percentage points, respectively. The effect of the pooled monetary incentive treatment is now 6.1 percentage points and significant at 10% (column (5)). As before, we cannot reject that all treatments had an identical effect on tutoring take-up. Pooling all treatments yields an increase in tutoring take-up of 7.4 percentage points, significant at 1% .

We draw two conclusions from Table 2. First, postcards were successful in attracting students to peer tutoring. This finding is consistent with students exhibiting present bias in study decisions, though alternative interpretations are possible. For instance, students could be experimenting with tutoring as an alternative study strategy with unknown payoffs. Second, none of the treatments were more successful than others in increasing tutoring attendance. This result is somewhat surprising, because the effectiveness of the postcard as a nudge might also suggest that altering the content of the nudge (in the form of the framing or incentive treatments) would further increase attendance, but this was not the case. We explore the role of information in tutoring take-up in more detail later in the paper.

Another dimension of take-up in response to the intervention is whether students attended multiple tutoring sessions. Although a nudge such as postcards might be successful in inducing students to attend tutoring once, it would be more surprising if such a simple intervention led to more persistent engagement with tutoring. To test this possibility, we re-run the regressions from Table 2 but redefine the outcome as an indicator for whether the student attended more than one tutoring session. We report results in Table 3.

Results for attendance at multiple tutoring sessions are similar to those for attending any tutoring session. Focusing on specifications with added controls in columns (4)–(6), we find positive and nearly identical coefficients on all treatment variables. The information, framing, and financial incentive arms led to increases of 6.8, 6.4, and 6.4 percentage points in attendance at multiple tutoring sessions, all significant at 5% (column (5)). Pooling all treatment arms (column (6)), the postcards increased attendance at multiple tutoring sessions by 6.5 percentage points, significant at 1 percent. As with attending any tutoring, for multiple tutoring sessions we cannot reject equality of coefficients across all treatment arms. Although the coefficient magnitudes for multiple tutoring sessions are somewhat smaller than for any tutoring attendance, they generally differ by less than two percentage points, indicating that the postcards not only induced students to attend tutoring, but also to continue attending after their initial visit.

Table 4
Effect of tutoring on term GPA.

Dependent variable:	Term GPA									
Specification:	2SLS								Reduced form (OLS)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Tutored	−0.586	−0.594	0.066	−0.003						
	(0.578)	(0.433)	(1.275)	(1.320)						
Tutored multiple times					−0.648	−0.666	0.085	−0.004		
					(0.638)	(0.486)	(1.640)	(1.595)		
Any postcard									−0.040	0.000
									(0.038)	(0.032)
First-stage F-statistic	5.75	7.10	0.48	0.46	6.23	7.36	0.38	0.40		
Additional controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Controls for term GPA in previous terms	NO	NO	YES	YES	NO	NO	YES	YES	NO	YES
Observations	1246	1246	746	746	1246	1246	746	746	1246	746

Notes: Parameters estimated using two-stage least squares (2SLS) or ordinary least squares (OLS) regression, as indicated. Robust standard errors are in parentheses. Term GPA is the grade point average for the semester in which we ran the experiment. All specifications include indicator variables for randomization assignment strata defined by gender, class year, and academic division. "Additional controls" are GPA at baseline, Asian, Black, Hispanic, American Indian, Pacific Islander, and international student indicator variables. "Controls for term GPA in previous terms" include semester specific GPAs for three previous semesters.
*** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

4.3. Effect of tutoring on grades

Tables 2 and 3 showed that postcards successfully encouraged students to attend tutoring sessions. Did tutoring improve grades? To answer this question, we use the Two-Stage Least Squares (2SLS) estimator to regress grade point average in the term of the experiment on an indicator for tutoring attendance, instrumenting for tutoring using an indicator for assignment to any of the treatment arms, as in Eq. (3). Table 4, column (1) shows results from the basic 2SLS specification without controls. The point estimate on the tutoring indicator is −0.586, suggesting that tutoring reduced grade point average by this amount (on the standard 0–4 scale) among students induced to attend tutoring due to postcards. The coefficient is not statistically different from zero, however. When adding controls for student demographics in column (2), the point estimate remains negative and becomes larger in magnitude, but again is not statistically distinguishable from zero.

A potential confounding factor in these regressions is that students selecting into tutoring due to the experiment may have been experiencing a downward trajectory in grades, and therefore would have earned lower grades even in the absence of tutoring. Although the regression in column (2) includes a student's baseline GPA, controlling for the level of GPA would not mitigate selection on trends. In columns (3) and (4), we include separate controls for GPA in each of the previous three semesters. Although this specification will account for confounding variation due to GPA trends, first-year students and others without three consecutive semesters of enrollment on campus (such as those who studied abroad) are dropped, reducing sample size.

With this caveat in mind, in column (3) we find that the effect of tutoring has flipped signs and is now positive and small, at 0.066. The point estimate remains imprecise. Adding controls in column (4) causes the coefficient to become negative again, but nearly zero (−0.003) and with a very large standard error.

A potential explanation for the apparent lack of effect of tutoring on grades observed thus far is that attending a single tutoring session is insufficient to influence outcomes. We therefore alter the explanatory variable to be an indicator for attendance at multiple tutoring sessions, which also increased in response to postcards (Table 3). The coefficients, reported in columns (5)–(8), retain the same pattern of signs as previously, and remain statistically insignificant.

A further caveat to the 2SLS results in Table 4 is the possibility of bias due to weak instruments. Across all specifications in columns (5)-(8), the largest first stage F-statistic is 7.36, below the threshold of 10 commonly used to determine instrument relevance (Stock & Yogo, 2002).[12] Columns (9) and (10) therefore report the reduced-form effect of receiving a postcard on grades, eliminating any bias from a weak first stage or the need to define the type of tutoring received. These reduced-form coefficients are much smaller than the 2SLS specifications, which is as expected since postcard receipt does not automatically result in tutoring attendance. The reduced-form coefficient is negative without controls, zero with controls, and imprecise in both cases. In sum, we find no evidence in Table 4 that using postcards to alter the propensity to attend tutoring affects grades.

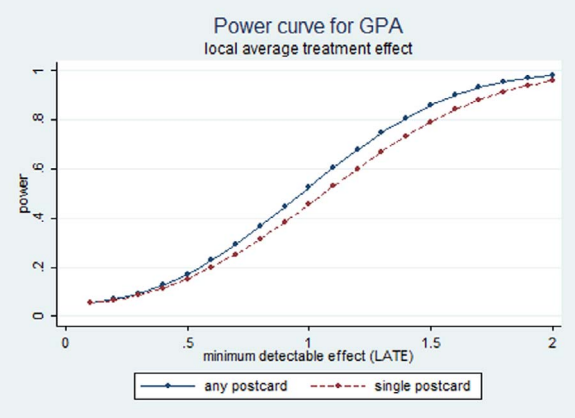

Fig. 3. Power to detect GPA effects.

Treatment effect estimates in Table 4 are imprecise across all specifications. A closer look at the statistical power of the experiment to detect grade effects reveals why. Although a 7-percentage point increase in tutoring take-up is meaningful, the absolute number of students affected on a small campus (about 90 students in our sample of 1259) is too limited to detect effects on grades. Even if the true local average treatment effect of tutoring is an increase of one GPA point (equivalent to moving from a C average to a B), the pooled postcard treatment would only have about 50% power to detect this effect (Fig. 3).[13] Power would be even less, about 45% , to detect this effect

[12] Constructing Anderson-Rubin confidence intervals, which are robust to weak instruments, leads to even wider confidence intervals than under standard asymptotics, leaving our conclusions unchanged.

[13] We calculate power for a 5% test size by using the observed sample size, take-up rate, and standard deviations of baseline GPA in the treatment and control groups.

Table 5
Effect of postcards on course withdrawal and academic warnings.

Specification	2SLS								Reduced form (OLS)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Course withdrawal										
Tutored	−0.004	−0.003	−0.024	−0.013						
	−0.048	−0.044	−0.078	−0.085						
Tutored multiple times					0.02	0.017	−0.187	−0.18		
					−0.058	−0.053	−0.221	−0.24		
Any postcard									0.001	0.004
									−0.004	−0.003
First stage F-statistic	1.53	1.84	0.67	0.54	1.52	1.79	0.21	0.16		
Observations	1259	1259	754	754	1259	1259	754	754	1259	754
Panel B: Academic warning										
Tutored	0.063	0.094	−0.171	−0.217						
	−0.245	−0.213	−0.325	−0.361						
Tutored multiple times					0.095	0.119	0.204	0.248		
					−0.283	−0.248	−0.674	−0.764		
Any postcard									0.006	−0.005
									−0.018	−0.021
First stage F-statistic	1.57	1.9	0.7	0.56	1.58	1.86	0.24	0.2		
Observations	1246	1246	746	746	1246	1246	746	746	1246	746
Additional controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Controls for GPA in previous terms	NO	NO	YES	YES	NO	NO	YES	YES	NO	YES

Notes: Parameters estimated using two-stage least squares (2SLS) or ordinary least squares (OLS) regression, as indicated. Robust standard errors are in parentheses. Course withdrawal (Panel A) is an indicator for withdrawing from at least one course during the term of the experiment. Academic Warning (Panel B) is an indicator for receiving at least one grade of D or lower in the term of the experiment. All specifications include indicator variables for randomization assignment strata defined by gender, class year, and academic division. "Additional controls" are GPA at baseline, Asian, Black, Hispanic, American Indian, Pacific Islander, and international student indicator variables. "Controls for term GPA in previous terms" include semester specific GPAs for three previous semesters.
*** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

from a single treatment arm. Achieving 80% power, a common benchmark in randomized control trials, would require a minimum detectable effect on GPA of around 1.4, which would be extraordinarily high for peer tutoring. In short, the experiment allows for precise measurement of changes in tutoring take-up, but is underpowered to detect reasonably-sized effects on grades, even when studying the universe of Reed students.

An additional limitation of the results in Table 4 is that they measure only whether the experiment affected average grades. Another potential effect of tutoring is that it helps students to avoid particularly bad academic outcomes, rather than altering the average outcome. For instance, tutoring might induce some students to persist in classes from which they would otherwise withdraw. Any effects on course withdrawal would alter the composition of courses that enter into average GPA between treated and control students, changing the interpretation of the average grade results. Course withdrawal is also an outcome of independent interest.

We check whether tutoring influenced course withdrawal in Table 5, Panel A, using the same specifications as Table 4. We define course withdrawal as an indicator for withdrawing from at least one course during the semester of the experiment. Estimates are imprecise and change signs across specifications. These results suggest that the effect of postcards on average grades is not confounded by course composition effects.

Another poor academic outcome that tutoring might influence is academic probation. Reed College issues academic warning letters to any student receiving a grade of D or below in any course. In the semester of the experiment, 9% of students received this warning. Table 5, Panel B examines whether tutoring altered the likelihood of receiving an academic warning. Again, we find no statistically significant coefficients, with signs alternating across specifications.[14] In sum, we fail to find evidence that tutoring influenced grades, either on average or for particularly bad outcomes.

4.4. Heterogeneity in tutoring take-up

The results in the previous subsection failed to detect an effect of tutoring on grades. Yet the experiment clearly influenced student study habits, as demonstrated in Tables 2 and 3. We explore tutoring take-up in greater detail in Table 6, splitting the sample by gender and class year to see if different groups of students respond differently to postcards. Limiting the sample to female students in column (1), we find that females who received a postcard were 6.1 percentage points more likely to attend tutoring, although the effect is not statistically significant at conventional levels. Male students responded at a higher rate, with a precisely estimated 8.7 percentage point increase in response to the postcards, despite their lower tutoring attendance in the control group (21% versus 35% for females). These results contrast with Angrist et al. (2009) and Paloyo et al. (2016), in which female students took up tutoring at higher rates than males, possibly because our liberal arts college setting differs from the large universities in those studies.[15] Nonetheless, we fail to reject the null hypothesis of equivalent take-up between male and female students, so we cannot conclude that there are gender differences. Future work might consider how the motivation to attend tutoring differs by gender, and tailor advertising messages accordingly.

In columns (3)–(6), we split the sample by class year. Students from all class years respond to postcards, with the exception of sophomores. This pattern may reflect changing demand for academic support as students progress through college. First-year students struggling to adjust to college coursework may be particularly receptive to nudges, but could have more ingrained study habits by sophomore year. Juniors and seniors have declared their majors and may again be susceptible to

[14] We have also checked whether tutoring altered other measures of academic performance besides average grades. We find no significant coefficients across all specifications in Tables 4 and 5 when defining the outcome as a dummy variable for whether a student's grades fall below the 25th percentile. We also find no significant treatment effects when running quantile regressions for term GPA on postcard receipt at the 10th, 25th, 50th, 75th, and 90th percentiles. Results are omitted for brevity but available upon request.

[15] The other tutoring RCTs (ideas42, 2015; Parkinson, 2009) do not discuss heterogeneity in take-up. None of the four prior tutoring RCTs report heterogeneity in take-up rates according to the other student characteristics we consider in this section.

Table 6
Heterogeneity in effect of postcards on tutoring take-up, by gender and class year.

Dependent variable:	Tutored (Y/N)							
Sample:	Female (1)	Male (2)	First year (3)	Sophomore (4)	Junior (5)	Senior (6)	Registered on first day (7)	Registered after first day (8)
Any postcard	0.061	0.087**	0.114*	–0.057	0.137***	0.110**	0.074**	0.070
	(0.041)	(0.039)	(0.061)	(0.062)	(0.052)	(0.045)	(0.037)	(0.062)
control group mean	0.35	0.21	0.42	0.40	0.19	0.14	0.22	0.28
p-values on equality of coefficients								
any postcard, female = any postcard, male	0.638							
any postcard, first year = any postcard, sophomore			0.042					
any postcard, first year = any postcard, junior			0.766					
any postcard, first year = any postcard, senior			0.956					
any postcard, sophomore = any postcard, junior			0.013					
any postcard, sophomore = any postcard, senior			0.025					
any postcard, junior = any postcard, senior			0.684					
any postcard, first day = any postcard, after first day							0.950	
Observations	684	575	335	313	302	309	627	297

Notes: Parameters estimated using ordinary least squares (OLS) regression. Robust standard errors are in parentheses. Tutored is an indicator variable equal to one if the student came for a tutoring session in the semester and zero otherwise. All specifications include indicator variables for randomization assignment strata defined by gender, class year, and academic division, as well as controls for GPA at baseline, Asian, Black, Hispanic, American Indian, Pacific Islander, and international student indicator variables. Control variable omitted if it defines the sample (e.g., regressions for male students do not include female indicator variable).
*** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level.

nudges towards academic support as their courses become more demanding.[16]

The pattern of take-up across class years also sheds light on the role of information in tutoring take-up. If students were responding only to the informational content of postcards, then we would expect first-year students to have the largest response, as they are least likely to be aware of tutoring availability. While first-year students do increase tutoring attendance by 11.4 percentage points in response to postcards, the effect is statistically indistinguishable from the effects for juniors and seniors, who are likely more aware of tutoring. Lower demand for tutoring among juniors and seniors makes these results more striking. Tutoring attendance in these class years is less than half that of first-year students in the control group. An alternative possibility, suggested by a referee, is that first-year students could be the most aware among class years if tutoring services were advertised during student orientation. Although we are unaware of this or other advertising targeting first-years, in either case we should expect to see a gradient in take-up across class years, which we do not.

This is not to deny that information played some role in take-up. Another possibility, also mentioned by a referee, is that younger students responded to information about tutoring availability, while older students responded to more specific information, such as tutoring center hours or that it was free of charge. Although information likely had some influence, the results suggest that information is not the only channel through which the experiment increased tutoring.

In columns (7) and (8), we split the sample by registration timing, building on previous studies of delays in course enrollment and academic outcomes (Banerjee & Duflo, 2014; De Paola & Scoppa, 2015; Novarese & Di Giovinazzo, 2013). In the spring semester of each year, continuing students are allowed to register for next year's courses. Using registration data from the spring before the experiment began, we split the sample of continuing students into those who registered for at least one class on the day that registration opened and those who registered after this day. Students who fail to register on the first available day, when the most popular courses reach capacity, might also procrastinate on other tasks.[17]

The results of this analysis suggest that both groups of students responded approximately the same to the postcard. Although the estimated effect is not statistically significant for students who registered after the first day that registration was allowed, we cannot reject the equality of coefficients between groups. The similarity in tutoring take-up between groups suggests that the effect of postcards was not due to a decrease in procrastination. This result also suggests that reducing present bias is not the most likely explanation for tutoring take-up, as students registering late are also most likely to exhibit present bias and respond via this channel. Instead, the results in Table 6 are consistent with increased salience as a key channel through which postcards affected tutoring.

4.5. Cost effectiveness analysis

How many additional tutoring hours resulted from the intervention, and at what cost? Conditional on attending any tutoring, students in the treatment group attended an average of 5.6 h of tutoring. If we assume that students induced into tutoring by the postcards attended this average amount, an increase in take-up of seven percentage points among 932 treated students implies an additional $0.07 \times 932 \times 5.6 = 365$ hours of tutoring as a result of the experiment.

The cost of the intervention includes postcard design, printing, and delivery; coffee shop gift card redemptions; tutoring center administrator time; and wages for peer tutors. If we assume that the college had to expand peer tutoring capacity to meet all additional demand, then we estimate the cost of the intervention at $5320, or $14.58 per additional hour of tutoring demand. If instead we assume that this additional demand was met by existing peer tutoring capacity (as is more likely), then we can subtract the tutoring wage of $11.25 from this

[16] We have verified that baseline characteristics are balanced between sophomores in the treatment and control groups, helping to rule out imbalanced randomization as an explanation for these results. Our interpretation of the take-up pattern across class years, though consistent with the evidence, is nonetheless speculative.

[17] Consistent with the existing economic literature on small delays in course enrollment and academic outcomes (Banerjee & Duflo, 2014; De Paola & Scoppa, 2015; Novarese & Di Giovinazzo, 2013), we find that registering after the first day is associated with lower GPA. We regressed GPA on an indicator variable for registering after the first day of registration and the full set of controls for randomization strata and other demographic characteristics. Conditional on these characteristics, we find that individuals who register after the first day of registration have GPAs that are 0.22 points lower (significant at the 1% level).

estimate, leaving a cost of $3.32 per additional hour.[18]

These costs compare favorably to other tutoring experiments, regardless of the measure used. Angrist et al. (2009) report costs of $302–$739 per treated participant, while Paloyo et al. (2016) report a cost of $30.72 per additional tutoring hour. Although Parkinson (2009) and ideas42 (2015) do not report program costs, they are likely low, as Parkinson (2009) relied on volunteer tutors, ideas42 (2015) was an email intervention, and neither experiment relied on financial incentives. On the other hand, Parkinson (2009) included students in just two courses, whereas our experiment included all students, and ideas42 (2015) likely required greater administrator time to manage its multiple treatment arms, repeated emails, and involvement of faculty. We conclude that our experiment led to changes in study habits at relatively low cost.

5. Conclusion

This paper reported results of an experiment that used a one-time advertisement to promote peer tutoring at Reed College. Students randomly chosen to receive a postcard about tutoring services were seven percentage points more likely to attend than the control group. Treated students were also 6 percentage points more likely to attend multiple tutoring sessions, suggesting durable changes in study habits. There were no statistically significant differences in tutoring take-up across different postcard messages. Common magnitudes of take-up across class years help to rule out a purely informational channel through which the treatment increased tutoring. Similarly, common take-up rates among students who did and did not register for class on the first available day help to rule out reductions in procrastination. Instead, increased salience of tutoring resulting from postcard receipt is consistent with these patterns, though the evidence remains suggestive.

Using this experimental variation in tutoring attendance, we fail to find any effect of tutoring on grades. One possibility is that students substituted tutoring for other study time. Students induced to attend tutoring may have felt less subsequent need to study on their own, leaving their grades unchanged. Data limitations provide another explanation. We are unable to connect student grades to the specific courses for which tutoring was received. Perhaps more importantly, the experiment is underpowered to detect reasonable effect sizes on average grades, even with individual randomization of all students enrolled at the college.

The intervention cost $3.32–$14.58 per additional tutoring hour, depending on whether we assume that additional tutoring demand was met through existing or new peer tutoring capacity. Regardless of what we assume, however, these are the lowest reported costs in the literature on peer tutoring experiments.

A shared finding across our study and the prior tutoring experiments in the literature (Angrist et al., 2009; ideas42, 2015; Paloyo et al., 2016; Parkinson, 2009) is that students respond to messages encouraging them to attend tutoring. Our failure to find significant effects of tutoring on grades also echoes the results of Angrist et al. (2009), ideas42 (2015), and Paloyo et al. (2016).[19] We extend these previous experiments, which took place at universities in four different countries, to a new setting, an elite liberal arts college. The similarities in experimental design and results with the community college studied in ideas42 (2015) are particularly striking, given the dissimilarity in institutional settings. Moreover, our low-tech approach using postcards sent via postal mail contrasts with the email- and text-based interventions of Angrist et al. (2009), ideas42 (2015) and Paloyo et al. (2016), suggesting that the communication medium is not central to outcomes.[20] Together, these experiments help to build an externally valid body of evidence demonstrating that university students change their study behavior when encouraged.

More broadly, this paper extends a literature dating at least to Leventhal, Singer, and Jones (1965) demonstrating that students change behavior in response to specific messages. In Leventhal et al. (1965), student take-up of vaccines was highest when public health pamphlets sent by mail contained information about the location of the vaccination clinic on campus. Because all study subjects were seniors, the authors interpret the results as a response to the salience of vaccination services, rather than to receipt of new information. Our intervention also directed students to take a specific action at a particular location, with similar increases in take-up. The increased take-up in the case of tutoring is perhaps more surprising, given the greater uncertainty about the quality and efficacy of treatment in comparison to vaccines. Regardless of differences in study setting, these and other studies of successful nudges show that universities can change student behavior at low cost. To improve outcomes, universities should ensure that they direct students to services with high potential for success.

Future tutoring interventions might include more directed efforts to influence study habits. For instance, planning prompts help convert intentions into action (Bird, Castleman, Goodman, & Lamberton, 2017; Rogers, Milkman, John, & Norton, 2015). Future versions of postcards might prompt students to plan to attend peer tutoring on a regular basis. Tutoring might also work best with complementary inputs, such as academic coaching (Bettinger & Baker, 2013; Oreopoulos & Petronijevic, 2016), peer advising (Ellis & Gershenson, 2016), or goal setting (Dobronyi, Oreopoulos, & Petronijevic, 2017). Future research might also expand the range of outcomes considered, for instance by surveying students on their subjective well-being. In short, the behavior change documented in this study in response to a low-cost intervention holds promise for improved student outcomes.

Appendix A. Cost effectiveness analysis: details

We include the following costs in our analysis:

1. Postcard design, printing, and delivery: $335
   - Based on printing cost of $0.20/postcard × 1000 postcards = $200, plus 1.5 days (12 h) of student worker time for design and delivery at $11.25/h = $135.
2. Coffee shop gift cards: $109
   - Based on take-up of 35% of 310 students in cash incentive arms, 10% card redemption rate, and assuming all redeemed cards valued at $10, then: $310 \times 0.35 \times 0.10 \times \$10 = \$109$. (The tutoring center was charged only for postcards redeemed, not postcards delivered.)
3. Administrator time: $766
   - Based on two days of tutoring center administrator time to set up and monitor the intervention, 261 working days per year, at approximate annual salary plus benefits of $100,000:[21] ($100,000/261) × 2 = $766.
4. Peer tutor labor: $4110
   - Based on seven percentage points increase in take-up among 932 treated students, 5.6 average tutoring hours per treated student (conditional on attendance), and $11.25 hourly tutor wage: $0.07 \times 932 \times 5.6 \times \$11.25 = \$4110$.

[18] See Appendix A for details of these calculations. Costs might increase in other settings where the redemption rate of gift cards might be higher.

[19] In contrast, the tutoring in Parkinson (2009) was targeted to particular courses, an approach that might be more effective at promoting increases in grades.

[20] Our approach also contrasts with the trend towards electronic communications in other higher education interventions (Castleman & Page, 2015; ideas42, 2016; Oreopoulos & Petronijevic, 2016).

[21] Salary and benefits are our own approximation, and do not necessarily reflect an administrator's actual compensation.

Total cost is therefore $5320. If additional hours of tutoring induced by the postcards came from excess capacity of existing peer tutors, then peer tutor labor has no marginal cost, reducing total cost to $1210.

To calculate the increase in tutoring hours induced by postcards, we assume each student taking up the offer attended the treatment group average (conditional on any tutoring attendance) of 5.6 h of tutoring. Based on seven percentage points increase in take-up among 932 treated students, then the intervention led to an increase of $0.07 \times 932 \times 5.6 = 365$ h of tutoring.

At a program cost of $5320, these additional 365 h of tutoring cost $14.58 per h. At a program cost of $1210, each additional hour of tutoring cost $3.32.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.econedurev.2017.11.003.

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