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ABSTRACT

Nudging Study Habits: A Field Experiment on Peer Tutoring in Higher Education*

More than two of every five students who enrolled in college in 2007 failed to graduate by 2013. Peer tutoring services offer one approach toward improving 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 7 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. We find little evidence of advertising-induced tutoring on learning outcomes.

JEL Classification: D83, I23

Keywords: peer tutoring, human capital investment, behavioral response to advertising, nudges, higher education

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NON-TECHNICAL SUMMARY

We ran an experiment that advertised peer tutoring to college students. Receiving a one-time message increased tutoring attendance by 7 percentage points. Attendance at multiple tutoring sessions increased by 6 percentage points. Results suggest that low-cost nudges can lead to important changes in study habits.

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 do not graduate in 6 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. University students who procrastinate, as measured by self-reported cramming for exams (Beattie et al. 2016) or small delays in course enrollment (Novarese and Di Giovinazzo 2013, Banerjee and Duflo 2014, De Paola and Scoppa 2015), have worse academic outcomes. Stinebrickner and Stinebrickner (2008) and Lindo et al. (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 7 percentage points, or 23% of the control group mean. We find no significant differences across messages, suggesting that simply making the existence of tutoring services salient induced more students to attend.

We also find similar response to the postcards across class years, helping to rule out

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

explanations of take-up based merely on information provision. 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 more durable changes in study behavior for a simple and inexpensive intervention. However, when using the random variation in postcard receipt as an instrument, we find no evidence that tutoring altered student grades, consistent with substitution of tutoring with an equally effective alternative use of time.

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.

Research on peer tutoring in higher education has found generally positive effects on student outcomes. Munley et al. (2010) found increased grades among Lehigh University students who attended peer tutoring. Dawson et al. (2014) reviewed 29 studies of supplemental instruction, a type of peer-led group study. Of the 7 quantitative studies reviewed, all found that participating students had higher grades. Like Munley et al. (2010), most of the included studies were observational and accounted for self-selection by modeling tutoring participation. These studies therefore require correct specification of the selection equation to produce unbiased estimates of the effect of tutoring, but none have random variation in tutoring exposure available to use as an exclusion restriction.

To our knowledge, four prior studies have evaluated peer tutoring using an experimental design. Parkinson (2009) found that students at an Irish university randomly assigned to tutoring in math and chemistry received higher exam grades. Angrist et al. (2009) studied a program that provided peer advising and structured study sessions to students at a Canadian university. Offering these services to randomly selected students led to take-up rates of 10-25%, depending on the participation measure used. Point estimates for the effect of the program on student grades were positive, but significant only when the intervention was combined with a large financial incentive requiring students to maintain high grades in exchange for a scholarship. Paloyo et al. (2016) randomly offered students at an Australian university the chance to win gift certificates for participating in peer tutoring. Like Angrist et al. (2009), they found increased take-up of tutoring due to the offer, but positive point estimates for student grades were not statistically significant.

In work closely related to ours, ideas42 (2015) conducted an experiment to promote tutoring at West Kentucky Community and Technical College. Students and faculty were randomly chosen to receive emails about the college's Tutoring Center. The emails increased the share of students attending tutoring, the number of tutoring sessions attended, and the rate at which faculty referred students to tutoring. Tutoring attendance did not increase grades, consistent with our findings.[2]

Our work builds on these studies but is unique in several respects. The Parkinson (2009) study had only 67 participants and covered two academic subjects, whereas our experiment includes more than 1,200 students and covers all subjects offered by the college. The program in Angrist et al. (2009) is a bundled intervention that includes mentoring, study skills training, and group study. This combination of interventions could alter student behavior

[2]In a separate experiment at State University of New York-Brockport, ideas42 (2016) chose a random subset of academically struggling students to receive regular emails promoting strategies for academic success, including tutoring. The intervention increased use of the tutoring center. The treatment decreased course withdrawals, but grade effects are not reported. This experiment is less comparable to ours because information about the tutoring center was bundled with other messages about improving study habits.

through many channels, whereas we are able to focus more precisely on the effect of academic peer tutoring. The program in Paloyo et al. (2016) relies on structured peer-led group study, while ours uses small-group or one-on-one tutoring tailored to students' specific needs. Most treatments tested by ideas42 (2015) included several emails sent to students during a semester, whereas our experiment used a single postcard. Moreover, the community college setting of the ideas42 (2015) experiment contrasts with the elite liberal arts college of our work. That similar experiments conducted in both settings increased take-up of tutoring helps to bolster the external validity of each study's findings.

Among these tutoring experiments, ours is the least expensive, consisting of a one-time message and a financial incentive of no more than $10, yet it was sufficient to alter behavior. Our work therefore provides new evidence on a common way that colleges provide individualized academic support across the curriculum at low cost.

We also contribute to the broader literature applying the insights of behavioral economics to education (Lavecchia et al. 2014, Koch et al. 2015). The prevalence of present bias among students (Solomon and Rothblum 1984, Steel 2007, Golsteyn et al. 2014) poses challenges for the standard model of human capital investment. 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 or tutoring choices. The studies cited earlier on student procrastination (Novarese and Di Giovinazzo 2013, Banerjee and Duflo 2014, De Paola and Scoppa 2015, Beattie et al. 2016) and distraction (Stinebrickner and Stinebrickner 2008) suggest that 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. Students can experience disutility if their behavior violates the norms of their social group (Akerlof and Kranton 2002). If students place high value on perceived intellectual ability, then seeking assistance through tutoring could carry a stigma that leads

to its underuse. Students might be particularly vulnerable to this stigma at an elite school with a small community, such as the college in this study. One treatment arm of our study addresses stigma by framing tutoring as a strategy used by successful students.

When present bias, stigma, and other behavioral explanations lead to suboptimal choices, low-cost interventions such as nudges—changes to the presentation of choices that do not meaningfully alter costs or benefits (Thaler and Sunstein 2008)—can be effective at increasing human capital investment. In education, providing information about returns to schooling can lead to increases in knowledge, aspirations, enrollment, and attainment (Nguyen 2008, Jensen 2010, Hoxby and Turner 2013, Oreopoulos and Dunn 2013, McGuigan et al. 2014, Dinkelman and Martinez A 2014), though this finding is not universal (Pekkala Kerr et al. 2015).

Our work is part of a burgeoning literature on nudges 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 et al. 2012, Hoxby and Turner 2013, Castleman et al. 2014, Castleman and Page 2015), as well as interventions to improve retention or increase learning among students already enrolled (Angrist et al. 2009, ideas42 2015, Smith and White 2016). This paper also relates to the literature on programs to promote college retention and progression, such as remedial courses (Moss and Yeaton 2006, Calcagno and Long 2008, Bettinger and Long 2009, Martorell and McFarlin Jr 2011, Scott-Clayton and Rodriguez 2014, De Paola and Scoppa 2015) and student advising (Angrist et al. 2009, Visher et al. 2011, Bettinger and Baker 2013, Ellis and Gershenson 2016). Peer tutoring can serve as a low-cost complement or alternative to such programs. Our advertising devices are a variant of those used in Wilson et al. (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 1,400 students and has a student-faculty ratio of 9:1.[3] It is highly selective, admitting 35% of applicants, with an average high school GPA of 3.9 and mean SAT score of 2,060 (95th percentile on a scale of 2,400) 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, many students are of modest backgrounds, with half of students receiving financial aid, of whom 45% have parental income less than $65,000. The six-year graduation rate is 79%, suggesting completion is more of a challenge than at peer institutions.

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, 7 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

[3]All data in this section are from 2015 and made available by Reed Office of Institutional Research. The only exception is 6-year graduation rate, which is from *U.S. News and World Report*.

physics also maintain their own satellite tutoring centers that follow the same arrangements as at 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 1,707 tutoring visits. Of students who received tutoring at the center, 69% returned for a second visit and 51% visited 3 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 Figure 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.[4]

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

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.[5] Campus mail remains a common form of communication at the college. Most students check their mailbox every day they are on campus, with most students on campus at least three days per week. 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.[6] 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 detail on student characteristics and balance tests between treated and control groups 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

[5]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.

[6]There are 5 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.

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 tutoring. 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$3,715, or the merit scholarships offered by Angrist et al. (2009), which were worth either US$1,000 or US$5,000, 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.[7] 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 unobserved 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:

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

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,

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

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 Equation (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:

$$tutor_i = \beta_0 + \beta_1 anypostcard_i + \delta_s + \epsilon_i \tag{2}$$

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 Equation (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 Equation (2) is the first stage.[8] In the second stage, we regress grades on tutoring attendance, using the treatment indicator as an instrument:

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

$$GPA_i = \alpha + \gamma tutor_i + \delta_s + \epsilon_i \tag{3}$$

where GPA is student grades and all else is as in Equation (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 who received a postcard and those who did not. Table 1 shows mean characteristics of students in the control group (column 1) and pooled treatment groups (column 2). The difference between groups and corresponding p-value testing the null hypothesis of no difference appear in columns (3)-(4), respectively. The experiment was well balanced by student class year, gender, race/ethnicity, and baseline GPA. Treated students were 3 percentage points less likely to be international students, significant at 10%. The presence of one significant difference at the 10% level is less than what we would expect across the 13 characteristics tested, however. 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

Figure 2 shows unadjusted differences in tutoring take-up, defined as attending at least one tutoring session during the semester of the experiment, across treatment arms. Tutoring attendance in each treatment arm exceeds the control group mean of 29%. 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).

Table 2 presents regression estimates of take-up, following Equation (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 significant at 10%. Coefficients on the five- and ten-dollar incentive treatments are also positive, but not statistically distinguishable from zero.

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 percent 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 occasioned by the postcard. When comparing coefficients across treatment arms, we cannot reject that all treatments had an identical effect on tutoring take-up.

Column (3) pools all treatments together, as in Equation 2. Receiving any postcard increased the likelihood of tutoring attendance by 6.7 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. 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). The effect of the monetary incentive is driven by the ten-dollar treatment, as might be expected (column 4). Pooling all treatment arms (column 6), the postcards increased attendance at multiple tutoring sessions by 6.5 percentage points, significant at 1%. Although the coefficient magnitudes 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.

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 Equation (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 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)-(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 (-.003) and with a very large standard error.

An 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 and Yogo 2002).[9] Columns (9)-(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

[9]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.

postcards to alter the propensity to attend tutoring affects grades.

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 1,259) is too limited to detect effects on grades. Even if the true local average treatment effect of tutoring 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 (Figure 3).[10] Power would be even less, about 45%, to detect this effect 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. To explore this possibility, in Table 5 we run the same specifications as Table 4, but redefine the outcome as a dummy variable for whether a student's grades fall below the 25th percentile. In all specifications, coefficients are positive, suggesting that such low grades are more likely in response to tutoring. The estimates are noisy, however, such that no coefficient is statistically distinguishable from zero.[11]

[10] 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.

[11] We obtain similar null results when redefining the outcome to be withdrawing from any course during the term, which often occurs because of poor academic performance.

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 take-up of tutoring, as demonstrated in Tables 2 and 3. We explore this 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 female students 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 more strongly, with a 8.7 percentage point increase in response to the postcards, significant at 5% level. The effectiveness of our postcard nudge is therefore largely driven by the response by male students.

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 the changing nature of student demand for academic support as they 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 nudges towards academic support as their courses become more demanding.

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 nearly identical (11.0 percentage points) and more precisely estimated for seniors, who are most likely to be aware of tutoring. 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 (Novarese and Di Giovinazzo 2013, Banerjee and Duflo 2014, De Paola and Scoppa 2015). In the spring semester of each year, continuing students are allowed to register for next year's courses. We use registration data to split the sample of continuing students into those who registered for at least one class on the first day that registration was allowed and those who registered for their first class after the first day that registration was allowed. Students who fail to register on the first available day, when the most popular courses reach capacity, might also procrastinate on other tasks.[12]

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, the sample size is much smaller than for students registering on the first day, which could explain the less precise estimate. The similarity in tutoring take-up between groups suggests that the effect of postcards was not due to a decrease in procrastination. Instead, the results in Table 6 suggest increased salience is the more likely channel through which postcards affected tutoring.

[12]Consistent with the existing economic literature on small delays in course enrollment and academic outcomes (Novarese and Di Giovinazzo 2013, Banerjee and Duflo 2014, De Paola and Scoppa 2015), we find that registering after the first day of registration 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).

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 7 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, suggesting that merely making tutoring more salient to students induced this response. 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.

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. Our study is not well-powered to detect grade effects, however.

A shared finding across our study and the prior tutoring experiments in the literature (Angrist et al. 2009, Parkinson 2009, ideas42 2015, Paloyo et al. 2016) 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).[13] 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 increased take-up of tutoring with the community college studied in ideas42 (2015) are particularly striking, given the dissimilarity in institutional settings.

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

Together, these experiments help to build an externally valid body of evidence demonstrating that university students change their behavior when studying becomes more salient.

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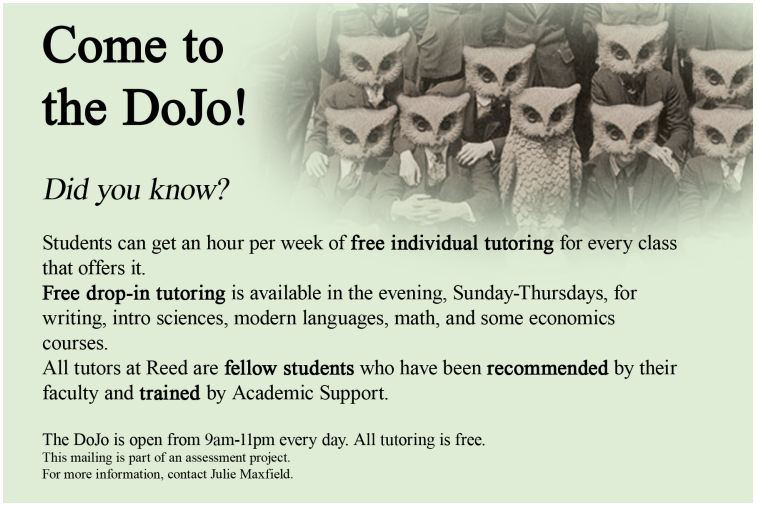
Figure 1: Postcards for each treatment arm



(a) Information

(b) Framing

(c) Incentive ($5)

(d) Incentive ($10)

Note: "The DoJo" refers to the main campus tutoring center, located in the Dorothy Johansen House. The Paradox, referenced in postcards (c)-(d), is the campus coffee shop.

Figure 2: Tutoring take-up by treatment arm

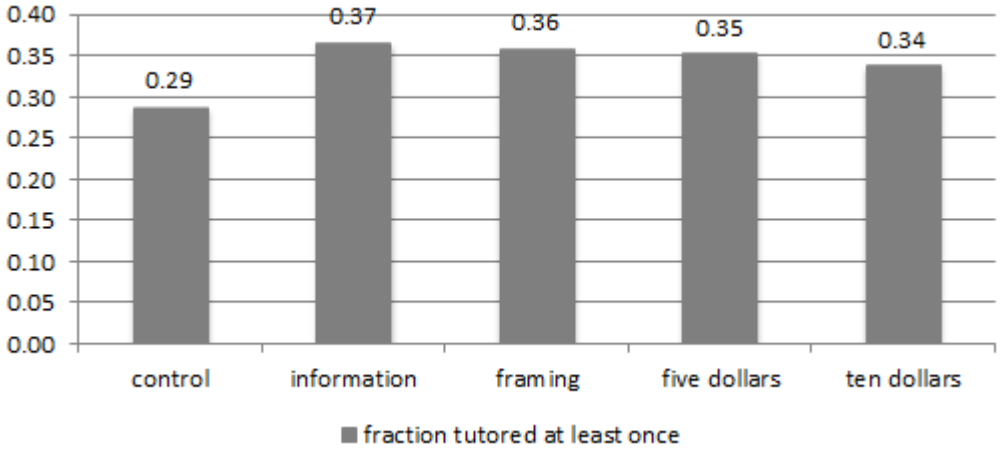


Figure 3: Power to detect GPA effects

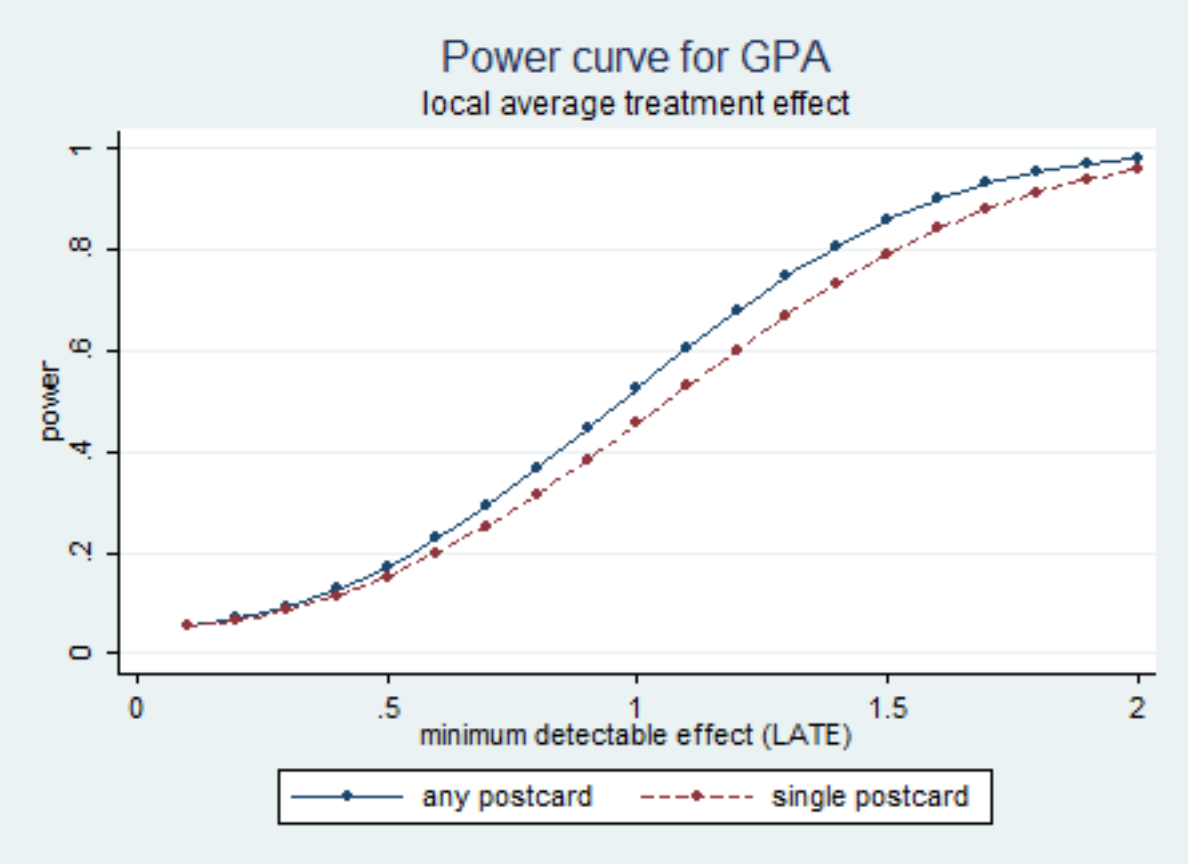


Table 1: Balance tests

Study arm:	Control	Assigned Postcard	Difference	P-value
	(1)	(2)	(3)	(4)
Freshman	0.269	0.265	0.004	0.886
	(0.025)	(0.014)		
Sophomore	0.239	0.252	-0.013	0.624
	(0.024)	(0.014)		
Junior	0.248	0.237	0.011	0.700
	(0.024)	(0.014)		
Senior	0.245	0.246	-0.001	0.969
	(0.024)	(0.014)		
Female	0.541	0.544	-0.003	0.933
	(0.028)	(0.016)		
Asian	0.171	0.142	0.029	0.196
	(0.021)	(0.011)		
Black	0.040	0.058	-0.018	0.208
	(0.011)	(0.008)		
Hispanic	0.125	0.107	0.018	0.373
	(0.018)	(0.010)		
American Indian	0.031	0.031	0.000	0.962
	(0.010)	(0.006)		
Pacific Islander	0.012	0.005	0.007	0.205
	(0.006)	(0.002)		
White	0.752	0.769	-0.017	0.533
	(0.024)	(0.014)		
International	0.095	0.065	0.030*	0.079
	(0.016)	(0.008)		
GPA (baseline)	3.071	3.071	0.000	0.983
	(0.029)	(0.018)		
Observations	327	932	1,259	

Notes: Standard deviations are in parentheses. 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.
*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

Table 2: 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)
Additional controls	NO	NO	NO	YES	YES	YES
Observations	1,259	1,259	1,259	1,259	1,259	1,259

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 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

Table 3: Take-up of multiple tutoring sessions

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)
Additional controls	NO	NO	NO	YES	YES	YES
Observations	1,259	1,259	1,259	1,259	1,259	1,259

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 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

Table 4: Effect of tutoring on term GPA

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	1,246	1,246	746	746	1,246	1,246	746	746	1,246	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 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

Table 5: Effect of Tutoring on Likelihood of Low Term GPA (25th percentile or below)

Table 5: Effect of Tutoring on Likelihood of Low Term GPA (25th percentile or below)

Dependent variable:	Below 25th Percentile Term GPA									
Specification:	2SLS								Reduced form (OLS)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Tutored	0.377	0.362	0.546	0.595						
	(0.398)	(0.318)	(1.295)	(1.371)						
Tutored multiple times					0.416	0.406	0.701	0.719		
					(0.439)	(0.356)	(1.727)	(1.695)		
Any postcard									0.026	0.014
									(0.027)	(0.027)
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	1,246	1,246	746	746	1,246	1,246	746	746	1,246	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 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

Table 6: Tutoring take-up by gender, class year, and registration delay

Dependent variable:	Tutored (Y/N)							
Sample:	female	male	first year	sophomore	junior	senior	registered on first day	registered after first day
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(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)
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 (i.e. regressions for male students do not include female indicator variable).

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.