Information and credible sanctions in curbing online cheating among undergraduates: a field experiment

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Abstract

The rapid increase in online instruction in higher education has heightened concerns about cheating. We use a randomized control design to test whether informing students that we can detect plagiarism reduces cheating. We further test whether informing students they have been caught cheating reduces subsequent cheating. We find informing students about our capability to detect plagiarism has little effect on cheating. Notifying students that they have been caught cheating and are on a watch list reduces subsequent cheating attempts by at least 65 percent depending on the class and sample. We test for peer effects but conclude we cannot credibly identify peer effects distinct from own-cheating propensities. **Keywords:** information, sanctions, cheating **JEL No.** 123, J24

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1 Introduction

Online college courses and degree programs have expanded dramatically over the past ten years. This growth heightened long-standing concerns about cheating and plagiarism given the ease with which material can be acquired and shared electronically. But there is also software that screens assignments for plagiarism, which increases the probability of detection. Whether the net result is an increase in cheating is not well-known (Watson and Sottile, 2010).

In this study we use experimental and non-experimental methods to ascertain the extent of cheating and assess efforts to deter it. The setting is a large public university in which undergraduates have to complete a learning module to develop their facility with Microsoft Excel. The software requires that students download a file, build a specific spreadsheet, and upload the file back into the software.¹ The software grades and annotates their errors. Students can correct their mistakes and resubmit the assignment two more times. Students have to complete between 3 to 4 projects over the course of the semester depending on the course. Unbeknownst to the students, the software embeds an identifying code into the spreadsheet. If students use another student's spreadsheet, but upload it under their name, the software will indicate to the instructor that the spreadsheet has been copied and identify both the lender and user of the plagiarized spreadsheet. Even if a student copies just part of another student's spreadsheet, the software will flag the spreadsheet as not the student's own work.

Since 2015, the syllabus for the module, under the sub-heating "Academic Integrity," has included an explicit statement that students should do their own work along with a reminder of the importance of Excel skills in the job market. Despite the exhortation, copied projects rose from 2.3 percent in the fall of 2016 to 14.9 percent in the fall of 2018 (Figure 1). We have no way of knowing whether students read the statement on academic integrity that faculty included with their course syllabus. We also don't know whether the rising rate of plagiarism was because students determined they could use other student's work without fear of detection and whether students shared this information with others.

¹Cengage's Skills Assessment Manager (SAMTM).

As a first step in devising a strategy to deter cheating, we designed an experiment to determine whether direct messages to students regarding our ability to detect cheating would curb it or whether we needed some form of sanction before students appreciated the seriousness of cheating. To address these questions we randomly divided students in each of the four courses into groups A and B (n = 3, 515). Each course had multiple sections. Students were unaware which group they were in. We first amended the course syllabus statement on academic integrity. We reminded students to submit their own work but we explicitly informed them that the software could detect any work copied from another student. Furthermore, the software could identify both the sender and the user of the plagiarized material. This information was available to both groups A and B via the course syllabus. One week before the first assignment was due, we sent an email to Group A reminding students to submit their own work and that the software could detect any work they copied from another spreadsheet. The email further stated that those caught cheating on the first assignment would be put on a watch list for subsequent assignments. Further violations of academic integrity would involve their course instructor for further disciplinary action. Group B received the same email one week before the second assignment. All students flagged for cheating in either of the two assignments were sent an email informing them that they were currently on a watch list for the rest of the semester's assignments. We used a difference-in-difference design to estimate the effect of the potential sanctions on plagiarizing subsequent projects among the subset of students flagged for cheating.

We find that warning students about the software's ability to detect cheating has a practically small and statistically insignificant effect on cheating rates. Flagging cheaters, however, and putting them at risk for sanctions lowers cheating by approximately 75 percent. An important question that we can not address effectively is the presence of spillover or peer effects (Sacerdote, 2001; Carrell et al., 2008, 2013). By definition, cheating involves interactions among students, a violation of the stable unit treatment value assumption (SUTVA). We explore models that use mean cheating rates in a section of a course on own cheating but conclude that we cannot separately identify own effects from peer effects. (Angrist, 2014).

Our study contributes to the literature on cheating among college students in several novel ways. First, we ascertain the extent of cheating with software. Most early reports of college cheating are based on student surveys, and with a few exceptions, are limited to a single institution or course. The extent of self-reported cheating of any kind can be as high as 82 percent (Bowers, 1964; McCabe and Trevino, 1997; McCabe et al., 2001b).² More serious forms of cheating such as overt plagiarism are roughly 20 percent across several surveys (McCabe et al., 2001b; Karlins et al., 1988; Dee and Jacob, 2012). More recent studies of cheating in the age of the internet use proprietary software such as Turnitin.com to evaluate the originality of students' written assignments (Ledwith and Rísquez, 2008; Walker, 2010; Dee and Jacob, 2012). Turnitin provides a similarity score. Researchers present scales of similarity or they create a binary indicator of plagiarism based on a specific threshold of similarity after careful screening for false positives. Dee and Jacobs reported that 3 percent of written assignments were plagiarized (Dee and Jacob, 2012). Turnitin and similar software, however, cannot readily detect contract cheating in which students pay others to do their work or obtain work from sources not in databases accessible by Turnitin (Rogerson, 2017).

Our study is novel in that we evaluate Excel spreadsheets instead of writing assignments. Any part of the spreadsheet that comes from another source is flagged by the software. Thus, there is no discretion as to the determination of cheating. Moreover, the system is largely, but not completely closed, as there is unlikely to be any relevant material on the internet specific to these projects that students can enter as their own. Students are encouraged to use Google for help with Excel as we consider that part of the learning process. A disadvantage is that the software must identity both the sender and user for us to flag the student for cheating. There are cases in which material in the spreadsheet is from an unidentified source. In this case we assume no cheating, which may be a false negative.

A second contribution is that we can test whether students who are informed that they used another student's work on their first assignment, are less likely to do so on subsequent assignments.

²This is based on a series of questions that ask if students had ever engaged in serious forms of cheating over the past year. While this is evidence of the pervasiveness with which cheating occurs it does not measure the frequency of cheating on any given assignment.

Two studies using Turnitin across multiple assignments find that students whose first assignment is flagged for similarity to material from other sources reduce their plagiarism (Ledwith and Rísquez, 2008; Walker, 2010). However, as noted before, Turnitin generates a similarity score. Thus, a reduction in plagiarism can mean a student whose first assignments matches 80-100 percent of the text from another source, but whose second assignment matches 35 percent of the text from another source can be classified as a reduction in cheating. In our study there is less ambiguity. Students who use all or any other part of another student's spreadsheet in their first assignment are warned of more serious sanctions if cheating is detected on subsequent assignments. The first email or warning is informational and devoid of any explicit sanction. If flagged for cheating on the first or second assignment, the second email demonstrates our ability to detect cheating and puts them at risk for disciplinary actions by their professor or school administration should the behavior continue.

A third advance of our study is the combined use of experimental and non-experimental methods with large samples. Most studies of student cheating rely on surveys of students (McCabe et al., 2001a; Teixeira and Rocha, 2010; Watson and Sottile, 2010; Power, 2009; Carrell et al., 2008). Others describe characteristics of students that plagiarize using software such as Turnitin (Walker, 2010). Another set of studies compares student performance on exams in proctored or un-proctored settings (Harmon and Lambrinos, 2008; Hollister and Berenson, 2009; Hylton et al., 2016). In one study the authors randomly assign 380 students in an online class to an exam proctored by a webcam (N=186) and the other half to un-proctored exam (N=186). There is no difference in students' scores on the exams between the treatment and control group (Hylton et al., 2016). In an impressive study of plagiarism, authors use experimental methods to analyze the writing assignment of 573 students and 1,256 writing assignments for plagiarism using the software Turnitin (Dee and Jacob, 2012). The setting is a select post-secondary institution. The research question is whether improving students' understanding of plagiarism and how to avoid it can lessen its prevalence. The treatment consists of a mandated online tutorial on plagiarism administered at the beginning of the course. The authors find that 3.3 percent of students' assignments in the control group are plagiarized but only one percent of assignments in the treatment group. The authors conclude that

the tutorial is effective because it educates students as to what constitutes plagiarism rather than increasing their perceived risk of being caught.

Our study is different from Dee and Jacob (2012) in that we focus on two aspects of deterrence: informational warnings regarding our ability to detect cheating and explicit demonstration of our ability to catch cheating. We use experimental methods to randomly assign a warning (information) about our ability to detect cheating. We contrast the effect of the warning with putting those caught cheating at risk for more serious disciplinary actions should cheating persist. We show the direct effect of sanctions on cheating in the semester under study, but we offer suggestive evidence of longer-term effects of lower cheating in subsequent semesters. By sanctioning students caught cheating, we reduce the uncertainty around the system's ability to detect cheating. Our results highlight the importance of actually changing risk estimates of academic dishonesty as opposed to simply supplying information.

2 Conceptual Motivation

Our simple conceptual framework is a slight modification of Dee and Jacob (2012). We start with the assumption that achieving a good grade in the Excel module will improve students' marketability (M) with direct returns of w. M is a function of how much effort (E) students exert, their ability (A) and whether or not they cheat on an assignment (CH). Cheating improves their marketability through a higher grade and a signal to an employer of a valued skill $\frac{\delta M}{\delta CH} > 0$. Cheating, however, increases the risk of being caught (π) and sanctioned (S). Warnings that the software can detect cheating (I) should also deter cheating $\frac{\delta pi}{\delta I} < 0$. Effort on assignments increases the opportunity cost of not putting the time and energy elsewhere c(E,A), but lessens the need to cheat. Students chose a level of CH and E to maximize their objective function

$$U = wM(E, A, CH) - c(E, A) - S\pi(E, I, CH).$$
 (1)

For any level of effort, students must balance the gains to cheating against the expected costs

 $(w \frac{\delta M}{\delta ch} - S \frac{\delta \pi}{\delta ch}).$

Our experiment manipulates I and S with the goal of deterring cheating. To be specific the warning that we can detect cheating increases I if it is held as credible, increasing a students expected π , and the sanction informing students they have been caught cheating raises the credibility of detection capabilities reducing the uncertainty around π for subsequent attempts. Knowledge of π may be shared and both these interventions may have effects outside of individual responses. Empty threats might be taken as positive reinforcement around cheating, with students assuming a low π . By contrast, sanctions increases the credibility of the system's ability to detect cheating and this information may spread. Unlike Dee and Jacob (2012), we assume that students know that sharing work constitutes cheating. As Dee and Jacob (2012) noted, we contend that students are less knowledgeable as to what constitutes plagiarism in writing assignments than explicitly asking for another student's work.

3 The Experiment

3.1 Software and Setup

The setting is a large public urban university in the spring of 2019.³ Students from four introductory classes in business, two in Accountancy, one in Finance and one in Management, are included in the study.⁴ Each course has multiple sections with an average of 1,000 students per course and roughly 80 students per section. Exceptions include two large online sections in the two Accountancy courses. All students enrolled in all four courses are included in the experiment.

Students in each course are required to build 3 to 4 Excel spreadsheets over the course of the semester from detailed instructions that are generated from Cengage's Skills Acquisition Manager or SAMTM, a proprietary software that builds facility with Excel and other business software.

³The experiment is registered under AEA registry RCT ID AEARCTR-0003786.

⁴A fifth course in computer science, which has two Excel assignments and is managed by a different administrator, is also included in the warning experiment. With only two Excel assignments, we can not test the effect of sanctions on cheating in the third assignment, and thus, we exclude this fifth course from the analysis. The results for the effects of warning in this course, insignificant and small, are not qualitatively different from the results in the other 4 courses.

Each student has a password-protected account with the software. For each assignment, students download the shell of the spreadsheet they have to build along with the requisite data and step-by-step instructions. They also download a picture of what the finished spreadsheet should look like. The software assigns a name to the spreadsheet shell such as "npp.John.Doe.xlsx".

There are 14 different assignments distributed across the four courses. In no two classes are the assignments the same. Each assignment emphasizes various skills with Excel such as formatting, plotting, pivot tables and basic macros. Each assignment has a specific due date that varies by course. Students can upload their completed spreadsheets to their account any time before the due date. The software grades the submission and provides detailed annotation for each error. Students can correct the errors and re-submit the assignment. Students can submit the spreadsheet for grading three times. Once their assignment is at least 80 percent correct, they are awarded full credit. Based on exit surveys each semester, students spend approximately two hours on each assignment.

Assignments submitted after the due date are penalized one percentage point for each day past the due date. Nevertheless, students can still receive full credit if their final score is at least 80 percent after deducting points for lateness. Thus, students still have incentives to cheat after the due date.

A group of graduate students, under the supervision of a faculty member administer the Excel module. The module is best described as a carve-out from the course. Faculty who teach sections of the course assign between 5 to 8 percent of their course grade to the completion of the Excel module. Faculty have no other responsibility for the module. The graduate students handle all correspondence with students. At the end of the semester, the graduate students send each faculty the students' scores on the assignments. If a student completes none of the Excel assignments, he/she loses at least half a grade for the course. Half a grade is consequential as these courses help determine a student's admission to the business school and eligibility to specialize in two most sought after majors, finance and accountancy. Given grade competition, this provides a strong incentive to submit the assignments and receive full credit. Of the 3,515 students enrolled in the four pre-business classes, 3,045 enroll in the Excel module and 2,566 or 73.3% completed all

assignments.

3.2 Inclusion criteria

We received a waiver from the Institutional Review Board (IRB) obviating the need for explicit student consent to be part of the study. The waiver required that all students be treated equally so that no subset of students would be disadvantaged by the study design. We would have preferred to randomly assign both the warning and the sanctions, but had we randomly assigned the sanctions, unsanctioned students might have continued to cheat creating a disadvantage for those who were sanctioned. By not requiring that we obtain explicit consent from each student, we were able to include every student taking one of the four courses in the initial randomization (n= 3,515). Some students enrolled in the courses did not enroll in SAM and were dropped from the analysis (n=470, 13.4%). The difference in the percent of students in group A and group B that did complete assignments was only 1.6 percentage points. ⁵

3.3 Identification of Plagiarism

The software embeds a unique code in the worksheet shell as soon as a student downloads the file. The code is hidden from students and linked to their account. Students can rename the spreadsheet, but the identifying code remains. The code enables the software to identify any part of a student's spreadsheet that comes from another source.

If, for example, a student uploads another student's spreadsheet or copies any part of another student's spreadsheet, the software flags the spreadsheet as suspicious. We classify an assignment as plagiarized only if we can identify the student who originally downloaded the assignment along with the student that uses it. We can only identify the "originator" if they are also registered with the software in any semester. If, however, the student opens another spreadsheet in order to practice,

⁵There were also a few unique users that completed assignments in SAMTM that we could not match to their information from the university data system from which we randomized. This was true of 0.4% (n=12) unique users who used SAMTM and 1.2% (n=5) unique users who used SAMTM and who cheated. This could result from late enrollment in the course or because their information in SAMTM did not match their information in the university system. These unidentified users were excluded from analysis.

or build parts of the assignment and copies that portion into the spreadsheet with the embedded code, the software flags the graded spreadsheet as suspicious. Similarly, if a student seeks help from someone not in the system, and if they copy work from this external person's spreadsheet into their own, or if they pay an external person to build the assigned spreadsheet for them but attach his/her name to this externally constructed spreadsheet, then we can not characterize the work as plagiarized. We do not consider this a major source of mis-classification because the proportion of suspicious spreadsheets in which the originator could not be identified is only 9% of flagged submissions.

3.4 Overview of the study design

We randomize all students registered for the four business courses evenly into two groups, A and B within each course. Prior to the first project, Group A receives an email warning that the software can detect plagiarism; Group B serves as the control group. This design estimates experimentally the intention-to-treat effect of warnings on cheating. Prior to the second project Group B receives the warning. However, Group A's exposure to the warning in project 1 means the treatment is no longer randomly assigned exclusively to Group B. We use a difference-in-differences (DD) approach to estimate the effect of the warning on cheating in Group B using group A as a treated control (see below). All students flagged for cheating on the first two projects are informed they are on a watch list and told any further evidence of plagiarism will be reported to their professor for disciplinary action. We refer to this as the sanction. We estimate the effect of sanctions on the probability of cheating on the third project among the subset of cheaters.

Figure 2 provides a schematic of the research design. Each row represents the schedule for each of the four courses. The horizontal axis shows the week of the semester. Consider Finance. The warning to Group A is sent out at the end of the third week of the semester, one week prior to the project's due date. Group B receives its first warning at the end of the seventh week, a week prior to the due date for the second project. The email we term a sanction is sent in week eight to all Finance students flagged for cheating on the first two projects. We send additional warnings

throughout the semester as follows: to Group A before the third project in finance and Accounting 1; to group B before the 4th project in Finance and Accounting 1; to both before the 3rd project in Accounting 2; to no one before the third project in management. We continue to email students who are caught cheating on the third and fourth projects that they have been put on a watch list.

The staggered due dates for projects is not intentional. Faculty teaching sections of each course determine the due dates for each project based on how the assignments fit with the course syllabus. The differential due dates complicates the research design. First, 764 students are in more than one of the four classes. Three hundred and twenty-three of those are in Group A in one class and Group B in another. This is relevant because a student in Finance, for example, which has the earliest due date for the first assignment, may receive a warning because she is in Group A, but then does not receive a warning for her first Management project because she is in Group B. Such mis-classification can attenuate the effect of warnings in courses with due dates after those in Finance. The experiment in Finance, however, is uncontaminated by this source of mis-classification, especially for the first assignment. For this reason we present results separately for Finance, Management, Accounting 1, Accounting 2 and then all four courses combined. As we show in Table 1, however, enrollment in multiple courses is balanced across treatment assignment. We also show that pooling all students regardless of multiple-course enrollment does not affect our results meaningfully. Another aspect of the design is that students can submit assignment anytime prior to the due date. Thus, some students submit projects prior to receiving the warning and sanction. In addition, we do not know if students read the warning email. For both reasons, the experimental results are intention-to-treat estimates. Lastly, cheating involves interactions among students, which as noted above, violates SUTVA. We explore spillover effects in Appendix B.

3.5 Baseline information

We create a standard insert to each Professor's syllabus that describes the Excel module. The insert explains how to register for the Excel module, the grading system, the availability of tutoring and walk-in workshops for Excel as well as an explicit warning regarding the software's ability to detect cheating under the heading "Academic Integrity." The text of the insert is as follows:

Academic Integrity. SAM detects files shared with other students and generates a report for the instructors with the names of plagiarizing students and all parties involved. Students caught cheating will be put on a watch list pending further action with their Professor.

In addition to the course syllabus, we also create a course on the learning management system, Blackboard, titled "Spring Excel Module for Finance," for example. We post the syllabus module on the Blackboard course as well. We use the Excel Blackboard course to send all emails to students regarding the Excel module. Thus, from the beginning of the semester all students have access to information regarding the software's ability to detect plagiarism and the likely consequences if caught.

3.6 First treatment: reminder that plagiarism can be detected

The script is designed to remind/inform students that the software can detect cheating with explicit sanctions left vague.

Your next Excel assignment is due next week. As noted on the syllabus, students are expected to do their own work. The SAM system will detect any portion of your project that has been copied from another SAM user. It will also identity the person from whom you copied. Students found using others' work or sharing their own will be put on a watch list pending further action with their course instructor.

The goal is to have you become more proficient in Excel, which is an essential skill in today's workplace. Using another student's work is plagiarism and detrimental to your professional development.

The Excelhelp Team

For the second Excel assignment we send the email above to Group B. Because Group A has already received the warning, the treatment is no longer assigned exclusively to Group B. Although the two groups remain balanced on observables, we use a difference-in-differences (DD) estimator to identify the effect of the warning on Group B. We describe this procedure in more detail below.

3.7 Treatment 2: the threat of sanctions

After both groups A and B complete the first two assignments, we send an email to all students who have used other students' work as well as to students who share their work. We inform these students that according to the software they have been flagged for using another students's work or having shared their work with other students. We inform them that they are now on a watch list. Any further incidence of plagiarism in the third or fourth assignments will be reported to their professor for disciplinary actions. The script is as follows:

Dear SAM Excel User,

As noted in our previous email to you, the SAM system can detect any portion of your project that has been copied from or shared with another SAM user. The system has flagged one of your assignments as having been copied from or shared with another student. This is a serious violation of academic integrity. Your name has been put on a watch list. Any further evidence of copying or sharing your SAM Excel projects in subsequent assignments will result in notification to your Professor for further disciplinary actions.

The goal is to have you become more proficient in Excel, which is an essential skill in today's workplace. Using another student's work or sharing your work with other students is plagiarism and can have serious consequences for your academic standing and is detrimental to your professional development.

We refer to this notification as the sanction. First, we consider this notification as more than a warning because we have demonstrated the software's ability to detect cheating. Second, reporting students to their Professor can carry serious consequences given the Faculty's and the Administration's heightened concern about cheating as more classes are delivered online. We view this as a credible sanction given the likely disciplinary action by faculty and possibly Deans. ⁶

4 Experimental models

4.1 The effect of a warning

We first estimate the effect of warning students that we can detect plagiarized work on the probability of cheating. The warning is randomized between groups A and B within each course and thus, orthogonal to student characteristics. Group A (GA) receives the warning on the first project and Group B is warned prior to the second project. We estimate the following regression for each class and the first project:

$$c_{ijt}^{1} = \alpha_0 + \alpha_1 G A_i + X_i \alpha + sect_j + \psi_t + \epsilon_{it}.$$
(2)

Let c be the probability that individual i, in section j at time t cheats on project 1 as indicated by the superscript. Let GA_i be one if the student is in Group A and receives a warning a week before it is due and zero if in Group B. We include controls for the course section, the week ($class_j, \psi_t$) a student submits the project, and student characteristics (X_i) as listed in Table 1. The coefficient, α_1 estimates the intention-to-treat.

In the analysis of the second project, Group B is warned for the first time. However, Group A's exposure to the warning in project 1 means the treatment is no longer randomly assigned to

⁶Faculty differed in their response to continued cheating on Excel after the sanction. Some wanted students to be given an automatic F in the course; others insisted the student redo the assignments in order to receive credit. Still others wanted the students reported to the Dean of Students. We counseled leniency in the relatively few cases of multiple incidents of cheating and used it as a teachable moment. We argued that 20 percent of students who cheated on the first two projects submitted the third assignment prior to receiving the sanction. In these cases we made the students redo the projects or receive no credit. In other cases, a student's project was shared with multiple students unbeknownst to the original sender. In such cases we personally contacted the sender by phone and told him of the seriousness of his actions. Students were often upset and we believed genuinely contrite. We feel this was a constructive response. As we show in the Epilogue the incidence of cheating has fallen precipitously in subsequent semesters suggesting that we achieved our goal of educating and deterring students from cheating on their Excel projects.

Group B. We use a difference-in-differences (DD) approach to estimate the effect of the warning on cheating in Group B. We pool the data from the first two projects and estimate equation (2).

$$c_{ijp} = \alpha_0 + \alpha_1 GB_i + \alpha_2 P2_p + \alpha_3 (GB_i * P2_p) + X_i \alpha + sect_j + \psi_t + \epsilon_{it}.$$
(3)

The dependent variable is 1 if student i in section j cheats on project p (p=1,2). *GB* is a dummy that equals one if a student is in Group B; *P*2 is a dummy variable that is one for project 2. Group A is considered treated for both project 1 and project 2. The coefficient on the interaction term, α_3 , estimates the effect of Group B moving from being unwarned on project 1 to being warned on project 2 relative to Group A, which is always warned. The identifying assumption is that the change in the rate of cheating in Group A between projects 1 and 2 is the same that would have been observed in Group B, had the latter not been warned prior to the second project. This will fail to hold if students in Group A interpret the lack of sanctions after the first warning to mean the system cannot detect cheating, or even if it can, that sanctions are unlikely to be enforced. The assumption will also fail to hold if Group A's effect of warning fades out by the time they go to submit their second project.

4.2 The effect of a sanction

The second effect we estimate is the probability that students cheat on a third assignment given they are caught cheating on the first or second assignment. We describe this as a sanction because we inform students they have been caught, have been placed on a watch list, and are at risk for more serious penalties if they cheat again. By informing students that we have caught them cheating, we underscore our ability to identify subsequent cheating. Importantly, sanctions are not randomly assigned. Only students flagged for cheating on either the first or second project are notified. We use a difference-in-difference strategy to identify the effect of sanctions on cheating. As noted, students can submit their assignments at any point prior to the due date. Twenty percent of students who cheat on the first or second assignment submit their third assignment before receiving a sanction.

Consequently, the sanction should have no effect on their probability of cheating on the third project. We use the timing of the sanctions relative to the submission of the third assignment to identify the effect of the sanction. Specifically, we estimate the difference in the probability of cheating on the third project between cheaters who receive the sanction before as opposed to after submitting the third assignment. We pool data on cheaters from the first three projects and estimate the following regression:

$$c_{ijp} = \beta_0 + \beta_1 S b_i + \beta_2 P 3_p + \beta_3 (S b_i * P 3_p) + X i \beta + sect_j + \epsilon_i.$$

$$\tag{4}$$

The dependent variable, c_{ijp} is one if student i in section j cheats on project p (p=1,2,3). Let Sb be one if the student is sanctioned before submission of the third project and zero if the student is sanctioned after submitting the third project (Sa). Students in Sb are the ostensible treatment group. The coefficient, β_1 , estimates the average propensity to cheat on any project among students in Sb relative to Sa. Let P3 be one for the third project and zero if projects 1 or 2. Beta 2 (β_2) estimates the average rate of cheating on project 3 relative to projects 1 and 2 among all cheaters. The coefficient on the interaction of Sb and P3, β_3 , is the DD estimate or the probability of cheating on the third project among those who were sanctioned prior its submission relative to those who were sanctioned after submission of the third project. Identification relies on the standard parallel trend assumption. Specifically, we assume that the difference in propensity to cheat on the third project 2 had Sb not been sanctioned before submission of the third project. As support, we present event-study estimates comparing the difference in the rate of cheating on the first two projects between the students in Sb compared to those in Sa.⁷

⁷Some students may cheat for the first time on the third project but are not included in this regression. In the Appendix we show results that include all students regardless of whether they cheated on the first or second project. The estimated effect of sanctions is not substantially different.

5 Results

5.1 Balance

All students who register for one of the four classes and who enroll in SAM are randomized between two groups within each of four courses. The first two columns in Table 1 show the mean characteristics by assignment to group A or group B. The third column presents standardized differences, a more reliable measure of balance than t-statistics, which are sensitive to sample size (Imbens and Rubin, 2015). A joint test of whether student characteristics predict group assignment yields a χ^2 with p=0.86. The last three columns show the means and standardized differences for the subset of students that did at least one Excel project. There is no loss of balance between groups A and B in this subset of students. Subsequent analyses only include the subset of students who completed an Excel assignment.

5.2 Warnings

The results of the experiment are well described by Figures 3-6. In each we show the cumulative proportion of assignments flagged for cheating (the cheat rate) for each project in each course by week of the semester. The vertical axis shows the cumulative proportion of submitted assignments flagged for cheating. The broken vertical line marks the week the warnings are sent out and weeks with asterisks mark the assignment due date. The results for the Finance course are shown in Figure 3. The cumulative cheat rate for Group A is the solid line and Group B is the broken line. Almost two percent of assignments are flagged for cheating prior to the warning sent to Group A in Finance. Between the warning and the due date the proportion of assignment flagged for cheating increases to about eight percent. Cheating continues after the first assignment due date and peaks at approximately ten percent.⁸ The important takeaway from the first panel of Figure 3 is the lack of any substantive difference in the proportion of flagged assignments between Groups A and B. The

⁸Recall students can submit assignments after the due date and still receive full credit if their score after the penalty for lateness exceeds 80 percent.

warning about our ability to detect cheating appears to have no deterrent effect.

The same pattern persists for the second Finance project in which Group B is warned and Group A is not (Figure 3, panel 2). The proportion of flagged assignments increases from two to almost ten percent by the due date and again, there is no difference in the proportion of flagged assignments between Groups A and B.

The pattern of cheating in the other courses is similar to that observed in Finance although the rate of cheating varies substantially. Over ten percent of students in Management cheat on the first project (Figure 4). The rate of cheating grows to 19 percent on the second project (Figure 4). Again there is no difference in cheating between Groups A and B for either project. The rate of cheating in the two Accounting courses is much lower than in Finance or Management (Figures 5 and 6). This decreases our power to detect statistically significant effects. The lower cheating rate in the Accounting courses may be due to the two large online formats in these classes. Vazquez et al. (2021), for example, find that students in online classes have less contact with classmates which diminishes cheating. This is not true in our experiment. Cheating rates on the first two projects in Accounting 1 are higher in the online sections, 2.0%, than in the in-person sections, 0.9%. In Accounting 2, cheating rates are 3.9% in the online and in-person sections, so the differences in cheating rates between formats may be related to differences in the proclivity to cheat rather than format effects.

The low cheat rates in the Accounting courses may be related to the timing of sanctions in the other courses. As shown in Figure 2, the first or second projects in the Accounting courses are due after sanctions occur in Finance and Management. Since students are sometimes in multiple experimental and non-experimental courses, students who experience sanctions might be directly discouraged from cheating in accounting courses or warn others of the sanctions in the Accounting classes. Contamination being the cause of these lower cheat rates are supported by the observation that cheating rates on projects in accounting that are due after sanctions occur in Finance or Management are much lower in comparison to the same projects from the two most recent semesters

as indicated in table A.1 in Appendix A.

We formalize the lack of an effect of the warnings on cheating in Table 2. In Panel A we limit the sample to students who are enrolled in only one of the four classes to limit the potential effects of contamination from other classes. Columns (1)-(5) of Panel A show the effect of warnings on the probability of cheating on the first project in Finance, Management and Accounting 1, Accounting 2 and all courses. The coefficient on "Warned" ranges form -2.1 to 2.9 percentage points depending on the courses. In 3 out of 4 courses and overall, the change is in the opposite direction of a deterrent effect and never significant at conventional levels. Columns (6)-(10) of Panel A show the effect of warnings prior to project 2 on Group B. These figures represent the difference-in-difference estimates of equation (3). Again, there is little evidence of a deterrent effect regardless of the course. Panel B expands the sample to include all students in one or more of the four courses. Although seven of the ten estimates are negative, each is small relative to its standard errors and are consistent with a lack of an effect of the warnings as shown in Figures 3-6.

5.3 Sanctions

In the week after the second assignment is due, we send an an email to students flagged for cheating that their assignment contains material from another student or their work has been used by another student. All are informed that they have been put on a watch list and that any subsequent violations of academic integrity will involve their instructor pending further action. ⁹ Returning to Figures 3-6, we show that the sanctions have immediate effects on the cheat rate in the third assignment in Finance (Figure 3, panel 3). The proportion of third assignments flagged for cheating falls by roughly half and declines to approximately two percent by the fourth assignment (Figure 3, panel 4). The decline in cheating is also large in Management after sanctions (Figure 4). Declines in cheating

⁹Some students protested that they had not used work from someone else. In such cases, we sent them the name of the person they copied from. Often this provided sufficient evidence. Some students did not recognize the name of the sender. In many such cases one student's spreadsheet had been shared with numerous students. Similarly, some students whose spreadsheet had been submitted by multiple users argued that they had only shared their work with a friend who was struggling and could not be blamed for its use by multiple students. In all cases, students remained on the watch list but we treated these exchanges as teachable moments.

are also observed in two Accountancy courses (Figures 5 and 6).¹⁰ ¹¹

The impact of sanctions on cheating is actually larger than indicated by Figures 3-6. In Finance, for instance, 22.2 percent of students who cheat on either the first or second project submit their third project before being sanctioned. Thus, only those who are sanctioned prior to the submission of the third project are actually exposed to the treatment. We exploit the timing of sanctions relative to submissions to identify its effect. In Table 3 we show results from the estimation of equation (4). The sample is limited to the subset of students who cheated on either of the first two projects. Accounting 1 did not have any students that were sanctioned before submission of their third project, therefore they are excluded from the table. The dependent variable is one if students cheat on a project and the sample includes project 1, project 2 and project 3 separately. In Finance, the difference-in-difference estimate indicates that cheating on the third project falls 35.7 percentage points (p<.05) among those sanctioned before as compared to after submission of the third project relative to the cheat rate in the first two projects. Scaled over the mean cheat rate in the first two projects, this represented a relative change of 65 percent (Panel A, column 1). The change in cheat rates falls 40.3 percentage points (p<.05) in Management, 56.3 percentage points in Accounting 2 (p<.05) and 39.5 percentage points (p<.05) across all four classes (Panel A, columns 2 and 3). In Panel B we show results from the same regressions but we include all students enrolled in one or multiple SAMTM courses. In Finance, Management, and overall, the absolute declines in the cheat rate are similar to those in Panel A, but relative changes are larger given the prior cheat rate is lower. The relative decline in cheating is 94.5 percent (-44.3/46.9) for Finance, 83.0 percent (-39.5/47.6) for Management and 89.2 percent (-38.7/43.4) for all courses combined. In Accounting 2, the effect size is somewhat lower although still significant. The lesser decline in Accounting 2 compared to the other courses may be due to multi-course students having heard of sanctions in

¹⁰One concern is that students were able to better disguise cheating after the sanctions. If true, then we would have expected an increase in flagged assignments in which we could not identify a sender. Recall that in these cases we assumed no cheating. These rates are shown for each course and project in Figures A.1-A.4 of Appendix A. The rates of these unidentified flagged submissions are never above 3 percent for any course or project; falling off after sanctions, although not as dramatically as the cases of confirmed cheating.

¹¹Another concern is that the declines in cheating are expected given the nature of the third project or timing in the semester? In Table A.1 of Appendix A, we show cheating rates in previous semesters by course and project. In all cases cheating rates either rise throughout the semester or remain flat through the third and fourth project.

other classes prior to submission of their first project. Therefore students that had submitted their third project prior to any sanction may still have heard about sanctions in their other classes through word-of-mouth and cut back on their cheating on the first or second project. This contamination may lessen the total effect of sanctions by the third project compared to the first and second. ¹² We also used randomization inference (Athey and Imbens, 2017). We randomly assigned students to Sb and Sa and re-estimated the regressions 10,000 times. We reject the sharp null at the 0.05 level or less for each estimate in Table 3 except for single and multi course students in Accounting 2.

As noted, we were unable to randomly assign sanctions. Instead we use a difference-indifferences estimator to assess the impact of sanctions. The parallel trends assumption inherent in difference-in-difference designs is shown in figure 7. Here we show that in finance, where no sanctions occur prior to submission of the second project, the parallel trends assumption holds with almost no difference in cheating rates between the first and second project. In management, despite sanctions occurring in finance just prior to submission of the second project, the difference in cheating propensities between projects 1 and 2 is statistically insignificant. In Accounting 2, where the second project was due several weeks after sanctions in both finance and management, signs of contamination from sanctions in other courses is clearly evident. By the second project those sanctioned before submitting their third project have already started to diverge from those sanctioned after, compared to project 1. In all cases, however, there is a dramatic change in cheating rates in project 3 compared to project 1.¹³

The consistent finding across all four courses is that email warnings have relatively little effect on cheating whereas identifying cheaters and putting them at risk for disciplinary action has dramatic effects. The consistency of the results also suggests that even students in more than one of the four courses who may have received two or more warnings were unaffected. What we don't know from these findings is the extent of interactions among students.

¹²As further evidence of contamination in Accounting 2 consider Appendix table A.1. Here the cheat rates for this class are between 11-17 percent in the two prior semesters. This contrasts with a cheat rate of only 3.9 in Accounting 2 on the first two projects in the experimental semester.

¹³We show the characteristics of those in Sb compared to Sa in Appendix table A.2.

6 Peer Effects

Our experiment assumes that warnings regarding cheating and sanctions of those who were caught only affect students who receive them. In other words, we assume no peer or spillover effects. This is an unrealistic assumption as cheating in this context involves interactions among students. Manski (1993) describes three types of peer effects. Endogenous peer effects occur when student's (i) cheating on project j is affected by the rate of cheating also on project j among some grouping of students with whom student (i) interacts. Unbiased estimates of endogenous peer effects are extremely challenging to uncover statistically as every individual student is both an outcome and part of the treatment (Angrist, 2014).

By contrast, contextual or exogenous peer effects are obtained by regressing the probability that student (i) cheats on the predetermined characteristics of the putative peer group. Carrell et al. (2008) use self-reported cheating in high school as an exogenous determinant of cheating in college. Sacerdote (2001) uses the SAT score of a randomly assigned college roommate on the other roommate's freshman GPA; Glaeser et al. (2003) use the proportion of students in one's dormitory at Dartmouth who drank in high school to explain the likelihood that a student will join a fraternity. Manski (1993) describes a third effect which he terms as correlated peer effects or common characteristics shared by a group. Cheating, for example, may be correlated with students that speak the same language or are of the same race or ethnicity. Correlated peer effects are not causal because they may reflect shared characteristics that are likely related to an omitted variable. Random assignment of roommates, as in the Dartmouth studies, insures that estimates of exogenous peer effects are not contaminated by deliberate grouping among students that may be confounded with correlated peer effects (Sacerdote, 2001; Carrell et al., 2008, 2013).

We have no way of credibly identifying the relevant peer groups. The Finance and Management courses have 12 and 16 sections, respectively, of approximately 80 students per section. However, students are not randomly assigned to sections within a course and may choose them to be with friends (Sacerdote, 2001; Carrell et al., 2008, 2013). Nevertheless, cheating involves interactions among students that may be facilitated by attending the same section of a course. Based on a

sub-sample of data, we know that 55.6 percent of plagiarized files are obtained from someone in the same course section. ¹⁴ A randomized pairing of plagiarizing students with originators would result in only 11.5 percent of plagiarized files coming from someone in the same section. Thus, the course section offers a plausible, but speculative, grouping of peers. In Appendix B we show estimates of possible peer effects using students in the same course section as the relevant peer group. We agree with Angrist (2014), however, that even with random assignment to putative peer groups, the estimated association of group cheating on own cheating is unlikely to be causal.

7 Epilogue

The experiment was conducted during the spring semester of 2019. This was the first time students who plagiarized were identified and placed on a watch list. In subsequent semesters, we continued to warn students about the software's ability to identify work that was not their own. We persisted in identifying students who cheated and informing them that subsequent transgressions would involve their Professor. Figure 8 shows the cheat rate on first project by semester and course beginning in the fall of 2016. The cheat rates were prior to any identification and sanctioning of cheaters in the semesters following the experiment. In Finance and Management the rate of cheating after the spring of 2019 was 80 to 90 percent lower than levels reported in the experiment. Given the complete lack of an effect of warnings in the experiment, we suspect that subsequent warnings were viewed as more credible based on the experience of students in the spring of 2019. Many students from the experimental semester continued to learn Excel through the online module when taking any of the other four required business courses in subsequent semesters.

¹⁴Recall that we know the originator (i.e. the person who downloaded the plagiarized file originally) and receiver of plagiarized assignments. However, not all senders took the class in the same semester as the receivers and thus we lack detail data on all originators.

8 Conclusion

We test whether randomly delivered email warnings to college students about our ability to detect plagiarized work deters cheating. We contrast the effect of warnings with the threat of more serious disciplinary actions for students caught submitting work of other students. We find that randomly assigned warnings do not deter cheating. Only when we demonstrate our ability to detect cheating and notify students that they are on a watch list for subsequent assignments does cheating fall dramatically. The takeaways appear clear: boilerplate messaging in syllabi regarding academic integrity appear to be largely ignored. Even email messages delivered a week before an assignment is due are ineffective. Not until students are caught and at risk for serious disciplinary action does cheating decline on subsequent work.

Our study is unique. The software students use to submit and grade assignments is highly sensitive: there are few if any false positives. The software's specificity is less robust: if students copy material from another student or person who is not registered in the system at this college, then we do not accuse them of cheating. Approximately nine percent of flagged submissions may be false negatives.

Another limitation is the likely presence of peer effects. Our definition of cheating requires some form of sharing between students. We are able to show with a sub-sample of data that students who share their work with others are more likely to be in the same section of a course than if sharing had been random. Despite these apparent networks effects, we cannot separately identify peer effects from own propensities to cheat. Peer effects, however, do not negate the fact that our intervention has large effects on overall cheating. The sanctions likely change students risk assessments when deciding whether to cheat. The decrease in average cheat rates in later semesters suggests there may be longer-term effects of demonstrating the ability to detect cheating.

9 Declarations

9.1 Funding

Not Applicable

9.2 Conflicts of interest/competing interests

The authors have no conflicts or competing interests.

9.3 Availability of data and material

The data cannot be released publicly since the authors received IRB exemption for consent and release of the data may put subjects at risk for identification. We can provide data to editors for verification of results.

9.4 Code Availability

We can provide the Stata code upon request.

9.5 Ethics Approval

This study was approved under full IRB review.

9.6 Consent to participate

This study received a waiver of consent from IRB.

9.7 Consent to publish

This study received a waiver of consent from IRB.

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Figures and tables



Figure 1: Percent of students that plagiarized on the first project by course and semester prior to the experiment



Figure 2: Experimental Design



Figure 3: Cumulative cheating rate in the Finance course by project and week of semester







Figure 5: Cumulative cheating rate in the Accounting 1 course by project and week of semester











Figure 8: Percent of students that plagiarized on the first project by course and semester before and after the experiment

		Randomize	pq	Complet	ed SAM A	ssignment
	Group A	Group B	Diff/S.D.I	Group A	Group B	IDiff/S.D.I
Prior School Performance						
GPA	3.1	3.1	0.00	3.2	3.2	0.00
SAT Verbal	526.0	526.4	0.00	526.9	527.3	0.00
SAT Math	586.7	586.4	0.00	590.6	588.5	0.00
School Experience						
Cumulative Credits	30.9	30.7	0.01	30.2	30.4	0.01
Underclass $(\%)$	68.8	69.1	0.01	70.2	69.7	0.01
Attends Part Time (%)	10.2	10.9	0.02	9.6	9.2	0.01
Multiple Exp. Courses($\%$) ²	46.4	46.9	0.01	46.4	46.9	0.01
Completed SAM Assignment	87.4	85.6	0.05	100.0	100.0	0.00
Demographics						
Age	21.9	21.9	0.00	21.8	21.8	0.02
Female (%)	44.0	44.5	0.02	45.6	46.4	0.02
Native English Speaker (%)	50.8	52.4	0.03	50.0	51.3	0.03
Race/Ethnicity						
Asian (%)	46.7	44.5	0.04	47.9	46.3	0.03
Black (%)	9.6	11.3	0.05	9.4	11.3	0.06
Hispanic (%)	20.9	21.0	0.00	19.9	20.4	0.01
White (%)	19.9	20.1	0.00	20.6	19.5	0.03
Other $(\%)$	2.4	2.8	0.03	2.2	2.6	0.03
# of Observations ³	2,166	2,168		1,893	1,855	
# of Students	1,947	1,941		1,703	1,671	
Joint χ^2 p-value ⁴		0.857			0.963	

 Table 1: Characteristics by randomization group¹

¹ Columns under the subheading "Randomized" include all students that were randomized at the beginning of the semester. Columns under the subheading "Completed SAM Assignment" include only those students that completed at least one assignment in SAM. ² Multiple Exp. Courses (%) refers to the percentage of students that are in more than one experimental course.

³ Students are included multiple times for each course in which they were enrolled.

⁴ Joint χ^2 p-value is obtained from a joint test of the coefficients in a logistic regression that includes an indicator for group assignment as the dependent variable and the characteristics with group means imputed for missing characteristics and indicators for missing characteristics as the independent variables.

		I	nt. to tre	at ²				Ι)iff-in-di	ff ³	
	(1)	(5)	(3)	(4)	(2)		(9)	(1)	(8)	(6)	(10)
	Fin.	Mgt.	ACC1	ACC2	NII		Fin.	Mgt.	ACC1	ACC2	All
Panel A: Single cour	rse stud	lents ⁴									
Warned	1.1	2.9	-2.1	1.7	0.7		0.8	1.0	-2.1	-0.7	0.3
	(2.8)	(3.1)	(1.3)	(2.5)	(1.2)		(3.1)	(5.6)	(2.1)	(2.0)	(2.5)
Mean Cheat Rate	9.8	12.8	2.2	5.6	7.6		8.7	17.3	1.8	4.5	8.1
# of Obs	528	492	542	393	1,955		1,101	606	1,059	775	3,757
# of Students	528	492	542	393	1,955		568	498	560	406	1,987
Panel B: Single and	multi c	ourse st	tudents ⁵								
Warned	-1.4	0.2	-1.9	0.8	-0.5		-0.6	2.7	-0.7	-1.0	-0.5
	(1.8)	(2.2)	(1.2)	(1.4)	(0.0)		(2.2)	(3.9)	(1.8)	(1.6)	(1.1)
Mean Cheat Rate	10.4	10.8	2.7	4.4	7.6		10.3	14.6	1.9	3.9	8.2
# of Obs	1,131	986	806	747	3,670		2,232	1,782	1,545	1,451	7,010
# of Students	1,131	986	806	747		2,988	1,151	991	824	761	3,029

¹ Intention-to-treat estimates of warning that the software can detect cheating for project 1 and difference-in-difference estimates for project 2 (see the text). Significance levels of differences are indicated by * p < .05.

² Column (1)-(3) shows the effect for the first project where only group A students had received the warning one week prior to the due date. These are the intention-to-treat estimates as some students submitted the assignment prior to the warning and others may not have read the email. Section fixed effects and characteristics are controlled in these models.

³ Column (4)-(6) shows the effect of group B moving from being a control to a treated group while group A stays a treated group (see equation (3) in the text). Group A does not receive an explicit warning prior to project 2 but we consider them "treated" because they ⁴ Panel A includes only students that are enrolled in a single course section during the experimental semester and thus uncontaminated by were warned prior to project 1. Student and project fixed effects are included and results are clustered at the student level.

⁵ Panel B includes all students even though multi course students may be randomized into group A in one course and group B in another course. Estimates are still technically valid since randomization in each course is orthogonal to randomization in all other courses. warnings from other classes.

	Fin.	Mgt.	ACC2	All
Panel A: Single	course stud	lents		
Sanctioned	-35.7*	-40.3*	-56.3*	-39.5*
	(14.2)	(8.0)	(16.1)	(6.1)
	[0.004]	[0.000]	[0.0132]	[0.000]
Prior cheat rate	54.9	54.9	36.4	53.3
# of Obs	213	343	66	688
# of Students	71	117	22	232

Table 3: Effect of sanction on cheating.^{1,2,3}

Panel B: Single and multi course students

Sanctioned	-44.3*	-39.5*	-23.0*	-38.7*
	(6.5)	(6.4)	(10.0)	(4.1)
	[0.000]	[0.000]	[0.1881]	[0.000]
Prior cheat rate	46.9	47.6	26.8	43.4
# of Obs	579	650	253	1,590
# of Students	193	221	85	392

¹ Difference-in-differences estimates in percentage points based on equation (3) in the text and obtained from a linear probability model. The regressions include only those that were sanctioned due to cheating on the first or second project. The treated group are those sanctioned before submitting their third project. The comparison group are students sanctioned after they submit their third project and thus unaffected by the sanctions. Estimates are adjusted for covariates from table 2 and standard errors are clustered by student. Significance levels of differences are indicated by * p < 0.05. ² We also used randomization inference (Athey and Imbens, 2017). We randomly assigned students repeatedly to *Sb* and re-estimate equation (3). In all but one case, ACC2 the reported estimates in Table 3 have less than a 0.01 chance of occurrence based on 10,000 simulations. P-values of randomization inference in brackets. ³ ACC1 is not included in the table separately because zero students were sanctioned after submitting their third project. Thus there is no comparison group.

Appendix A Additional tables and figures

	Spr	ing 2018			Fa	all 2018	
Fin.	Mgt.	ACC 1	ACC 2	Fin.	Mgt.	ACC 1	ACC 2
11.7	9.8	3.0	11.8	13.6	7.7	3.1	16.8
13.2	20.5	5.1	11.5	16.4	26.7	8.1	16.4
15.4	21.5	6.0	11.0	24.8	21.8	8.8	15.8
17.3		6.8		28.2		11.5	
	Fin. 11.7 13.2 15.4 17.3	Spr Fin. Mgt. 11.7 9.8 13.2 20.5 15.4 21.5 17.3	Spring 2018 Fin. Mgt. ACC 1 11.7 9.8 3.0 13.2 20.5 5.1 15.4 21.5 6.0 17.3 6.8	Spring 2018Fin.Mgt.ACC 1ACC 211.79.83.011.813.220.55.111.515.421.56.011.017.36.86.8	Spring 2018Fin.Mgt.ACC 1ACC 2Fin.11.79.83.011.813.613.220.55.111.516.415.421.56.011.024.817.36.828.2	Spring 2018 Fa Fin. Mgt. ACC 1 ACC 2 Fin. Mgt. 11.7 9.8 3.0 11.8 13.6 7.7 13.2 20.5 5.1 11.5 16.4 26.7 15.4 21.5 6.0 11.0 24.8 21.8 17.3 6.8 28.2 28.2	Spring 2018 Fail 2018 Fin. Mgt. ACC 1 ACC 2 Fin. Mgt. ACC 1 11.7 9.8 3.0 11.8 13.6 7.7 3.1 13.2 20.5 5.1 11.5 16.4 26.7 8.1 15.4 21.5 6.0 11.0 24.8 21.8 8.8 17.3 6.8 28.2 11.5

Table A.1: Average cheat rate by project, course and semester in two immediately previous semesters.¹

¹ These percentages come from a system that records number of confirmed cheating attempts in each project/course and semester.

	Sa	Sb	Diff/S.D.
Prior School Performance			
GPA	3.2	3.2	0.15
SAT Verbal	497.6	509.4	0.13
SAT Math	595.6	585.7	0.12
School Experience			
Cumulative Credits	35.3	33.0	0.07
Underclass (%)	62.6	66.0	0.07
Attends Part Time (%)	2.6	6.2	0.16
Multiple Exp. $Courses(\%)^2$	46.1	58.7	0.25
Demographics			
Age	22.5	21.5	0.29
Female (%)	38.3	45.4	0.14
Native English Speaker (%)	16.5	24.7	0.19
Race/Ethnicity			
Asian (%)	73.0	55.8	0.35
Black (%)	9.6	7.8	0.07
Hispanic (%)	14.3	7.0	0.22
White (%)	7.0	19.5	0.33
Other (%)	3.5	2.6	0.05
Cheat rate on first two projects (%)	51.8	41.0	0.44
# of Students	115	421	
Joint χ^2 p-value ⁴		0.00	0

 Table A.2: Characteristics by sanction group¹

¹ Sa refers to the group that was sanctioned after submitting their third project and therefore sanctions could have no effect on their cheating outcome for their third project.

 2 Sb refers to the group that was sanctioned before submitting their third project and therefore sanctions may have affected their cheating outcome on their third project.

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	F	п.	Μ	gt.	All Co	ourses
	(1)	(2)	(3)	(4)	(2)	(9)
Sanctioned before submission $(\beta_1)^3$	7.0*	5.7*	5.0*	3.6*	5.4*	4.6*
	(1.9)	(1.5)	(2.0)	(2.3)	(1.5)	(1.5)
Sanctioned after submission $(\beta_2)^4$	40.3^{*}	41.9^{*}	60.4^{*}	65.2*	51.9^{*}	53.4*
	(3.3)	(7.3)	(3.8)	(6.8)	(4.5)	(4.8)
Group A unsanctioned (β_3)	0.7	0.6	-1.5	-1.2	-0.3	-0.5
1	(1.9)	(1.0)	(1.8)	(1.3)	(0.4)	(0.5)
Difference in third project cheat rate $(\beta_1, \beta_2)^5$	-33.3*	-36.2*	-55.4*	-61.5*	-46.5*	-48.8*
	(3.6)	(7.8)	(4.1)	(7.1)	(4.9)	(4.9)
Difference of prior cheat rate $(\theta_1 - \theta_2)$		6.7^{*}		-22.4*		-10.8*
		(2.7)		(2.9)		(4.7)
Difference-in-differences $[(\beta_1 - \beta_2) - (\theta_1 - \theta_2)]^6$		-42.9*		-39.1*		-38.1*
		(4.8)		(5.1)		(4.1)
Cheat rate of unsanctioned group B students	1.5	1.5	3.3	3.3	1.7	1.7
Prior cheat rate among sanctioned	46.9	46.9	47.6	47.6	43.4	43.4
# of Obs	1,003	2,992	889	2,541	3,257	9,564
# of Students	1,003	1,003	889	889	2,669	2,669
	-		-			-

Estimates for odd columns are of equation (3) in the text and obtained from a linear probability model. Effects are reported as percentage points. The reference category includes students in group B who were not sanctioned on project 1 & project 2. Estimates are adjusted for covariates from table 2 and standard errors adjust for general Huber-White type heteroskedasticity or are clustered by student for column (5). Significance levels of differences are indicated by * p < .05.

² Estimates for even columns are difference-in-differences estimates for the effect of sanctions that include the first and second projects in the regression and account for the possibility that those that submit projects before and after sanction may have different average cheat rates.

³ Sanctioned before submission is 1 if a student plagiarized a prior project and was sanctioned one week prior to their submission of the third project, due one week after sanctions went out. ⁴ Sanctioned after submission is one if a student plagiarized a prior project but submitted the third project before being sanctioned. $^{5}\beta_{1}$ - β_{2} is the effect of sanctions on the probability of plagiarism on the third project among those who were sanctioned on prior 6 [(β_{1} - β_{2})-(θ_{1} - θ_{2})] is the effect of sanctions on the probability of plagiarism on the third project among those who plagiarized prior projects if the difference between those that were sanctioned before the third project and those that were sanctioned after would projects if there are no differences between those sanctioned before and after submission of third project in average cheat rates. nave remained constant if not for the sanction.



Figure A.1: Cumulative unidentified flagged project percentage in Finance course









Accounting 1





Appendix B: Peer effects

In this Appendix we describe our attempt to estimate peer effects.

We focus on the second project in each class and asked whether the probability that student (i) in section (j) cheats on the second project (t) (c_{ijt}^2) is affected by the proportion of the student's classmates (j(-i)) in section (j) who cheat on the first project $(\bar{c}_{(-i)j1}^1)$.¹⁵

$$c_{ijt}^{2} = \gamma_{0} + \gamma_{1}c_{ij}^{1} + \gamma_{2}\bar{c}_{(-i)j}^{1} + X_{i}\Gamma + \psi_{t} + \mu_{ijt}.$$
(B.1)

Equation (B.1) captures the linear-in-means spillover effects adjusted for individual characteristics (X_i) and for a student's own cheating on the first project (c_{ij}^1). As Angrist (2014) notes, $\bar{c}_{(-i)j}^1$ is best interpreted as a jackknife instrumental variable estimate in which the first stage is individual cheating outcomes on the first project regressed on dummies for the section of each class. However, OLS and two-stage least squares estimates will tend to be equal given a weak first-stage based on many groups.¹⁶

The results are shown in table B.4. A student who cheats on the first project is 60 to 65 percentage points more likely to cheat on the second project relative to students who do not cheat on the first project (Table B.4 Panel A, columns (1), (4) and (7). Similarly, in columns (2), (5), and (8) we present the effect of the leave-one-out mean cheat rate of the first project on an individual's likelihood of cheating on the second project. The group effect differs little from the effect of own cheating, a result consistent with Angrist's (2014) suggestion of a weak first stage. Indeed, the partial R^2 on the set of course section dummies in the first stage is never greater than 0.04 (Table B.4, Panel A). Controlling for both own cheating and group cheating decreases the estimated peer effect by one third to a half (Table B.4 Panel A, columns (3), (6) and (9).

As further exploration of peer effects, we estimate equation (3) for the third project and used own and the section cheating rate on the second project as regressors (Table B.4, Panel B). The

¹⁵We suppress the class (k) subscript as j is a subset of k. ¹⁶In our context, the OLS estimate is $c_{ijt}^2 = \gamma_0 + \gamma_1 c_{ij}^1 + X_i \Gamma + \psi_t + \nu_{ijt}$ or cheating on project 2 regressed on an individual's cheating on project 1 while the approximate 2SLS would substitute the group cheating on project 1 for the individual cheating, $c_{ij}^1 = \eta_0 + \eta_1 \bar{c}_{(-i)j}^1 + X_i \Gamma + \nu_{ijt}$.

results in table B.4 Panel B shows the diminished effect of own cheating in comparison to previous projects ranging from 27-35 points. This reflects a combination of students who submit their project before sanctions and thus are untreated and students who submit projects after being sanctioned and thus drastically curtail their cheating as a result of sanctions. The estimated group effects, however, are much smaller than the effect of own cheating and flip sign and become statistically insignificant when included with own cheating (Table B.4 Panel B, columns (3), (6) and (9)).

In the end, we can not rule out significant peer effects. Sharing work with other students involves interactions. However, we remain skeptical that the coefficient on the mean cheat rate captures a causal peer effect distinct from or in addition to a student's own propensity to cheat. From an administrative perspective, the separate identification of individual from peer effects may be irrelevant to the goal of reducing cheating. The more unambiguous result is that appeals to academic integrity unsupported by credible identification of plagiarism are ineffective at curbing cheating in this context.

		Finance		Μ	anagem	ent	A	ll Cours	es
	(1)	(2)	(3)	(4)	(2)	(9)	(ک)	(8)	(6)
Panel A: Peer effects on p	project 2 ²								
Own cheat proj. 1	64.6^{*}		63.5*	61.2*		60.5*	60.4^{*}		59.5*
	(5.4)		(5.1)	(5.5)		(5.5)	(3.8)		(3.7)
Sect. mean cheat proj. 1		73.0^{*}	38.9*		87.4*	70.2*		78.0*	52.7*
		(13.0)	(10.0)		(37.0)	(33.2)		(12.4)	(11.7)
Sect. Partial R ²		0.037			0.033			0.030	
# of Obs		1,081			791			3,282	
Panel B: Peer effects on p	roject 3°								
Own cheat proj. 2	27.6^{*}		27.9*	34.8*		35.2*	34.2*		34.6^{*}
	(5.0)		(5.0)	(3.7)		(3.5)	(3.3)		(3.3)
Sect. mean cheat proj. 2		11.6	-6.7		7.9	-11.3		12.0	-9.1
		(7.1)	(1.6)		(8.6)	(9.8)		(5.2)	(6.1)
Sect. Partial R ²		0.056			0.065			0.055	
# of Obs		<i>L</i> 66			770			3,100	
¹ Estimates are of equation (4) in Estimates are adjusted for covari	ithe text an iates in Tab	Id obtained	from from a p cheating ra	linear prob tes are the th	ability moon about the number	del. Effects a of students v	re reported as who cheated o	s percentag on the imn	ge points. nediately
Sect. Partial \mathbb{R}^2 refers to the addi	itional varia	s suucint (ation expla	ined by section	on fixed effe	cts out of	us in section variation lefte	U) UI COUISC	trolling fo	r student
fixed effects. Standard errors are $m < 0.5$	adjusted fo	or a genera	ll form of het	eroskedastic	ity. Signifi	cance levels o	of differences	s are indic	ated by *
Po									

Table B.4: Probability of cheating on project given prior project own cheating and mean peer cheating by 3

² Panel A looks at the effect that own and peer cheating from project 1 has on whether students cheat on project 2, which was due before

any sanctions were sent out. ³ Panel B looks at the effect that own and peer cheating on project 2 has on students on project 3, which was due after sanctions were sent out.