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Perceived Anonymity and Cheating in an Online Experiment

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ABSTRACT

This paper presents the outcomes of an online coin-tossing experiment evaluating cheating behavior among Ukrainian students. Over 1,500 participants were asked to make ten coin tosses and were randomly assigned to one of the three treatment groups tossing coins (1) online, (2) manually, or (3) having the choice between tossing manually or online. The study outcomes suggest that students are more inclined to cheat when they perceive the coin toss to be more "private." Moreover, the students' attitudes toward corruption appear to matter for the extent of their cheating, while socio-demographic characteristics were less important.

KEYWORDS
Cheating; university; Ukraine; experiments; lottery

JEL CLASSIFICATION
C93; D73; I23

Introduction

Digitalization brings many positive effects for students, especially in terms of accessibility and educational costs. At the same time, however, digitalization may increase the risks of student cheating due to the lack of control in an online environment.

Why cheating among students? Dishonest behavior has reached an unprecedented level worldwide (Denisova-Schmidt 2020; Bretag 2020; Sabic-El-Rayess and Heyneman 2021). Moreover, as students grow into adults, what they learn about cheating – its frequency and especially its acceptance – will be taken into their professional lives. As a result, interpersonal and societal well-functioning is eroded (for more about linking cheating behavior to real-world behavior, see Schild, Lilleholt, and Zettler 2021). To investigate dishonest behavior, studies rely on lab and/or online cheating paradigms where the occurrence of cheating can be statistically determined on the aggregate by comparing the self-reporting outcomes to the known probability distribution (Cohn & Maréchal, 2018; Lilleholt, Schild, and Zettler 2020).

Indeed, in an online experiment conducted among Ukrainian students, we found non-negligible cheating in order to win a cash lottery. We asked about 1,500 participants to make ten coin tosses and count the number of tossed "heads" while randomly assigned to one of three different treatment groups. These groups tossed their coins (1) online, using a random number generator, which we indicate as *T1_gen*, (2) manually, with a physical coin, which we indicate as *T2_manual*, or (3) according to their preference, either manually or online,

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which we indicate as *T3_choice*. The participants were informed that overall 10 winners would be randomly chosen among those who indicate 8, 9 or 10 heads.

The results suggest that, in particular, students in the second and third groups cheated by inflating their reported outcomes by an average of 33% and 9–10%, respectively, compared to group (1), who already reported an average outcome slightly higher than the theoretical mean of the coin tosses.

Our study contributes to a growing literature of behavioral experiments investigating dishonesty and cheating, frequently based on the die roll paradigm by Fischbacher and Föllmi-Heusi (2013) for documenting cheating on the aggregate (rather than individual) level. Additionally, our study contributes to the literature by adding a choice treatment, in order to assess the cheating behavior when providing individuals with the private choice of either a manual or computerized coin toss. The motivation for including this treatment is that the computerized option might be perceived to have a lower level of anonymity (even though both the manual and computerized coin toss were actually anonymous). For this reason, the choice treatment allows us to study whether individuals cheat more when they are given the opportunity to select into an environment with a likely higher level of perceived anonymity, namely the manual coin toss. This appears relevant in light of the increasing importance of online or hybrid courses and exams, where cheating opportunities might be different than in (well monitored) in-class courses and tests. However, we point out that providing students with such a choice might also induce some behavioral effects by making students think more about the purpose of the study or whether to cheat or not than under the other two treatment regimes without choice, which needs to be borne in mind when interpreting the effects. Finally, our study is innovative in the way we deal with attrition by implementing balance tests across treatments based on machine learning.

From a theoretical perspective, cheating can be modeled economically by weighting the expected benefits and losses in order to make a utility-maximizing choice. The model proposed by Kerkvliet, (1994) yields three main predictions. First, an increase of the benefits that come from undetected cheating will increase cheating behavior. Second, an increase in the severity of penalties of detected cheating decrease cheating. Lastly, factors that increase the probability of cheating detection will decrease such behavior. In this sense, privacy concerns during cheating play an important role in the utility maximizing choice to be taken. This is due to the fact that a lower sense of privacy will decrease the expected benefits from cheating and lead to a decrease of cheating behavior (Kerkvliet, 1994). The predictions of the model described are closely related to our study given that we evaluate the extent to which perceived anonymity affects cheating behavior in our sample.

Particularly relevant to our study design are the experiments by Gneezy, Kajackaite, and Sobel (2018) and Abeler, Nosenzo, and Raymond (2019), where the treatments consisted of rolling dice either privately (not observed by the researcher) or on a computer, with the result of the latter being observed by the researcher. Crede and von Bieberstein (2019) refine this approach by making it common knowledge that the computerized die roll was observed. All three papers found more cheating under private than other computerized die rolling. However, the results in Crede and von Bieberstein (2019) suggest that cheating can fully disappear when monitoring is made explicit. Our results corroborate the finding that the ambiguity of the observability of actions may matter, as we also find the highest and lowest levels of cheating among physical and computerized coin tossing, respectively. This is in spite of the fact that, in contrast to Gneezy, Kajackaite, and Sobel (2018), Abeler, Nosenzo, and Raymond (2019), and Crede and von Bieberstein (2019), computerized coin tossing is anonymous in our online experiment, but obviously not fully perceived as such. Relatedly, Abeler, Nosenzo, and Raymond (2019) combined data from 90 experimental studies in economics, psychology, and sociology, and concluded that, overall, people lied relatively little, driven by preferences for being seen as honest or being honest. The authors showed robust evidence that individuals forgo, on average, approximately 75% of the potential gains that they would have obtained from lying. This contradicts the standard economic prediction that subjects would adopt payoff-maximizing reporting.

As suggested by Mazar, Amir, and Ariely (2008), individuals likely trade-off the moral costs of dishonest behavior (related to norms and values) and material considerations related to monetary benefits and costs (e.g., the probability and magnitude of punishment in the case of dishonesty). Perceived anonymity plays an important role in these considerations – this is in line with results from Lilleholt, Schild, and Zettler (2020) and Dickinson and McEvoy (2021), which claim that dishonest behavior may be more pronounced in remote environments. Lilleholt, Schild, and Zettler (2020) investigated whether non-computerized and computerized population inferred cheating tasks (C-PICT) and other implementations of the C-PICT have similar effects. The authors assigned four types of treatments; a non-computerized coin toss task (CTT), an external computerized CTT (participants tossed a coin on their own), and an internal computerized CTT (participants were provided with a computerized coin within the survey) and a monitored internal CTT. The estimated probabilities of dishonesty were 0.18, 0.14, 0.06 and 0.05, for the non-computerized, external computerized, internal computerized and monitored internal CTT respectively. The results point out significant differences in the probability of dishonesty between the non-computerized and the computerized tasks. Moreover, the participants who were explicitly monitored while playing an internal computerized CTT cheated less than those who played an external computerized CTT without being monitored.

Similarly, Gerlach et al. (2019) evaluated 558 experiments that implemented one or more of the four most widely used experimental paradigms to assess dishonest behavior; sender-receiver games, die roll tasks, coin-flip tasks and matrix tasks. Their results show that the degree and direction of dishonest behavior depend on the experimental paradigm implemented as well as on the situational and personal factors such as the investigative setting, reward size, gender and age. In the coin-flip task, wins were reported 31% more often than expected from honest reporting. However, the rate of liars was lower compared to other experimental paradigms. Concerning the situational and personal factors, the results showed that in online coin-flip experiments, subjects reported the winning side less often than the expected report if participants were honest (50%) when compared to reporting in laboratory experiments (Gerlach et al., 2019).

In another study, Schild et. al., (2019) evaluated the REVISE framework for measuring dishonest behavior that consists of three manipulations; Reminding, Visibility and Self engagement. Reminding provides cues that emphasize the importance of morality making it harder to justify dishonesty. Visibility increases individuals feeling that they are being monitored. Lastly, self-engagement generates commitment to act morally. The authors compared the effects of the three manipulations and their interactions on dishonest behavior while performing a mind-game paradigm. The mind-game paradigm consisted of receiving a bonus incentive if the number that the individual wrote down on paper coincided with the number displayed. They found a medium to large effect of visibility and a small effect of self-engagement, in reducing dishonest behavior. In contrast, the authors did not find any effect for reminding or interaction effects. The results suggest that the additive effects of visibility and self-engagement were effective at reducing dishonest behavior ('Schild et al., 2019).

Why Ukraine? Ukraine represents a very interesting case in terms of corruption. According to the Transparency International Index and the World Value Survey, Ukraine is a country that frequently practices corruption. On the other hand, Ukraine is one of the strongest examples in Eastern Europe in fighting against corruption. All of the three recent revolutions in Ukraine were instigated under anti-corruption slogans. Moreover, Ukraine's problem with domestic corruption can have an unexpectedly international impact, as in case of then-US President Donald Trump and his first impeachment, as well as the story involving then-US Vice President Joe Biden. In addition, average honesty is apparently positively correlated with per capita GDP before 1950 and with religion (Protestantism) – but neither is the case with Ukraine (Hugh-Jones 2016). The remainder of this study is organized as follows: Section 2 describes the research design. Section 3 introduces the data. Section 4 presents the empirical results. Section 5 concludes.

Research Design

Inspired by previous experiments on dishonesty, we conducted an online experiment investigating the effects of perceived digital anonymity on dishonest behavior. The experiment was conducted from May to October 2017 among Ukrainian students on our behalf by the sociological company FAMA (Ukraine). Using a Facebook account, @atlantynespisuyut (Engl.: atlantes do not cheat), which is devoted to various issues on higher education in Ukraine, including corruption in the broader meaning of the word, respondents were recruited to take part in the survey. Additionally, the sociological company that carried out the experiment invited as many student organizations as they could reach by e-mail. Potential respondents had to answer a few initial questions in an online survey, then watch a video that was assigned at random, and then continue and finish the questionnaire.[1]

It is worth noting that the survey included two experimental settings, giving it double randomization: (i) random treatment by the anti-corruption video (see our companion paper) applied to all survey participants,[2] and (ii) random treatment in the tossing experiment applied to only those who agreed to take part in the lottery.

After completing the survey, participants were asked to like and share the @atlantynespisuyut Facebook account and, optionally, to enter a lottery to win a prize of 500 Hryvnas (20 USD at the time of the experiment). The procedure was as follows: After a respondent agreed to take part in the lottery, she or he was asked to make ten coin tosses and to count the number of tossed "heads." We then randomly assigned ten entrants who got eight or more "heads" as winners of the lottery. Three tossing procedures were randomly assigned as treatments to the respondents:

1) *T1_gen*: generated, meaning that the number of "heads" was computerized online using the website random.org, but not recorded by the system of the online survey (and thus unknown to us). The random number generator was integrated into the survey form. When participants wanted to use a computerized procedure, a separate window appeared showing the result from random.org.

2) *T2_manual*: manual tossing, in which a participant tossed a coin manually and just entered the number of "heads" results; and

3) *T3_choice*: choice – i.e., having the choice between manual and online – with the number of "heads" being generated online, but asking the participants to use either the manual or computerized procedure.

From a technical perspective, reporting the result of the coin tosses was identical for all treatments. Students had to type in the number of tossed "heads" in the survey, no matter whether they obtained it from tossing a coin, from the online number generator, or from making it up. Importantly, there were no announcements concerning monitoring or cheating. In the case of the generated treatment or computerized procedure in *T3_choice*, we informed the participants that the numbers were generated via the website random.org, the results were random, and we did not record them.

Using the die roll paradigm from Fischbacher and Föllmi-Heusi (2013), the experimental design allows us to analyze cheating behavior across treatment groups, as the probabilities of getting "heads" or "tails" are 50% each. With ten tosses, the distribution of tossing results follows binomial distribution with the number of experiments/tosses equaling 10 and the probability of "success"/heads equaling 0.5, which implies the probability of getting eight or more "successes" equals approximately 0.055 (corresponding to 28 respondents getting eight or more "successes" in a sample of about 500). If no cheating occurs, one should approximately obtain this probability of claiming eight or more "successes" for each treatment. If cheating occurs, the observed probabilities should be higher than the theoretical one.

Sample Definition and Descriptives

Our initial sample of survey participants is prone to substantial attrition: out of the 9,152 participants who started the survey, 7,444 dropped out prior to the lottery treatment assignment. Such a high attrition rate is supposedly due to several reasons, including (i) curiosity about the survey, but no willingness to participate in it; (ii) sensitive questions: many respondents dropped out at the questions on personal experiences with corruption; (iii) the considerable length of the questionnaire, which required up to 30 minutes to complete; (iv) a technical problem with the videos that were part of the survey, which was solved as soon as we received a note about it; (v) unwillingness to participate in the lottery (a detailed discussion of attrition is provided in our companion paper). Therefore, the treatment was randomly assigned to only 1,708 individuals. A further complication arises due to attrition or non-response in the outcome measured after the treatment: for 194 observations with treatment assignment, the outcome (number entered) was not observed, such that effects can only be estimated for 1,514 observations.

Our final sample consists of students from all regions (*oblasts*) of Ukraine, with the majority residing in the Lviv (24.8%), Kyiv (18.1%), Dnipro (7%), Ivano-Frankivsk (5.6%), and Kharkiv (5.4) regions. Most of the respondents lived in urban areas (73.9%). Major fields of studies were humanities (29.3%), social sciences (25.7%), and technical fields, (26.5%), followed by medical (9.3%) and natural (7.8%) sciences. 75% of respondents studied in government-sponsored programs, while 25% paid private tuition fees. 18.2% were in their first year, 17.9% in the second year, 23% in the third year and 21.7% in the fourth year of a bachelor's degree. 17.7% were master-level students. The majority was born between 1995 and 1999, being 18 – 23 years old at the time of the survey. Interestingly and somewhat surprisingly, 80.8% of the respondents were female. 40.7% lived in a dormitory, 17.4% rented accommodation, and rest either lived with their parents or in their own accommodation. 38.4% indicated working part-time an average of 6.3 hours/day. Only 3.6% had gone abroad for study. To compare to what extent our sample is similar to others in related studies, we find that overall, the studies that have implemented coin toss methods to assess the degree of dishonest behavior have relied on samples of around 70 to 2200 individuals. Samples made up of students are similar in the characteristics of the sample in our study and also exhibit a greater share of women (Abeler, Nosenzo, and Raymond 2019).

21% of the respondents claimed that they most frequently obtained "excellent" grades, 44.7% grades between "good" and "excellent" and 14.1% "good." Concerning dishonest behavior, 15.7% claimed to have never used cheat sheets during exams, 48.6% to have never used ready-made course papers from the Internet, 79.4% to have never purchased course papers from others, 14% to have never copied and pasted some parts of course papers from the Internet, and only 13.6% to have never cheated during tests or exams. These relatively high levels of academic dishonesty correspond well with the results obtained in a face-to-face survey of 600 students from Lviv in 2015 (Denisova-Schmidt, Huber, and Prytula 2015; Denisova-Schmidt, Huber, and Leontyeva 2016; Denisova-Schmidt, Prytula, and Rumyantseva 2019). For this reason, to get an indication of how similar studies could rely on samples from Ukraine it should be noted that Ukrainian students tend to have a lower perception of the wrongness of cheating and a higher likelihood of engaging in cheating behaviors (Chudzicka-Czupała et al., 2016).

Results

Our results suggest that if the respondents are given the opportunity to enter the number of tossed "heads" manually, they more frequently indicate having eight, nine, or ten "heads" than the theoretical binomial distribution predicts. Figure 1 shows the frequencies of the

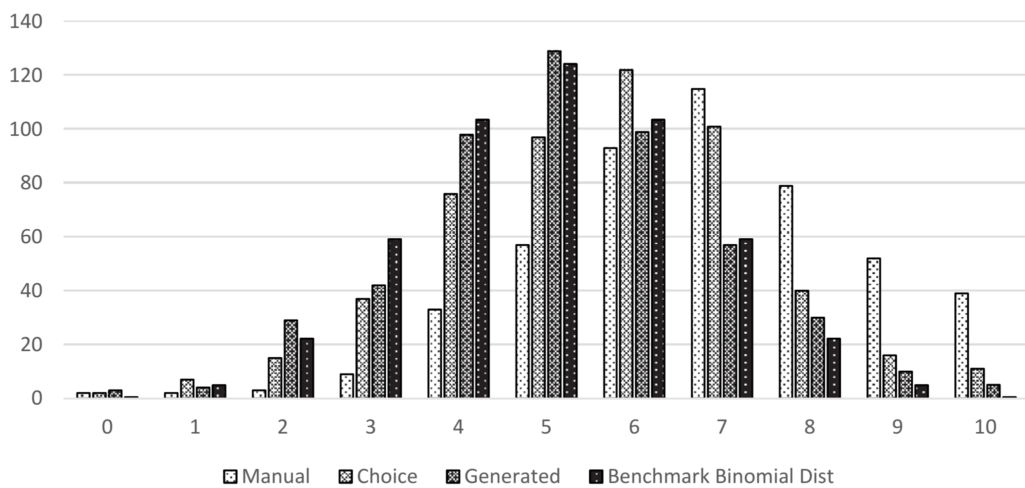

Figure 1. Distribution of the coin tossing outcomes as reported by the respondents and compared to the binomial distribution.

Table 1. The coin tossing outcomes by treatment.

	T1_gen	T2_manual	T3_choice
Total number of cases	506	484	524
Predicted by binomial dist. between 0 and 7	478	458	495
Actual number reported between 0 and 7	461	314	457
Difference predicted-actual, % of total, 0–7	3%	30%	7%
Predicted by binomial dist. between 8 and 10	28	26	29
Actual number reported, between 8 and 10	45	170	67
Difference predicted-actual, % of total, 8–10	−3%	−30%	−7%

Note: Frequencies of actually reported coin tossing outcomes as well as theoretically expected frequencies (as predicted by the binomial distribution) separately for each treatment group.

reported coin tossing outcomes for the three treatment groups and the expected results generated by the binomial distribution.

Table 1 reports the distribution of the treatments in the experiment and indicates the actual numbers of "heads" reported by respondents for a given treatment in comparison to the number of "heads" predicted by the binomial distribution, with the number of trials equal to the number of cases for each treatment. As we can see, in the case of manual treatment, the number of "heads" reported between 0 and 7 is 30% below the predicted number. Correspondingly, the number of "heads" reported between 8 and 10 is 30% above the predicted number. The difference between actual and predicted numbers of "heads" reported is much less in the case of two other treatments.

However, the result might be partly driven by selectivity bias due to attrition and the nonresponse of survey participants as discussed in Section 3, because the outcome (number entered) is not observed for 194 observations out of the 1,708 individuals for whom the treatment was randomized. Such outcome attrition after treatment assignment might jeopardize causal inference if it is at the same time associated with the treatment and background characteristics that affect the outcomes.

We ran several tests to check if the assignment of treatments was random. First, we investigated whether the distribution of the treatments among students with observed (rather than missing) outcomes is consistent with a discrete uniform distribution. The latter distribution is implied by our treatment randomization, where each treatment value is assigned with an equal probability of 1/3. For this reason, we ran a Pearson's chi-squared test for differences in the frequencies of students with observed outcomes across the three treatment states. The p-value was 0.4515, such that the uniform distribution cannot be rejected at any conventional level of significance. Second, Table 2 investigates the selectivity of outcome attrition with regard to the treatment. Outcome attrition is statistically significant across lottery treatments (highest for *T2_manual*, lowest for *T1_gen*), indicating that the treatments influence nonresponse significantly.

Table 2. Treatment effect on attrition and nonresponse.

	estimate	standard error	p-value
constant (mean of group with T1_gen number)	0.05	0.01	0.00
T3_choice number	0.07	0.02	0.00
T2_manual number	0.12	0.02	0.00

Note: The estimate in the first line corresponds to the attrition rate in the group with a generated number; estimates in the second and third line correspond to the mean differences relative to the first group.

Table 3. Covariate balance across treatments.

	T1_gen	T3_choice			T2_manual		
	mcontr	mtreat	diff	pval	mtreat	diff	pval
male	0.18	0.18	0.00	0.95	0.20	0.02	0.52
siblings	1.01	0.95	−0.06	0.32	1.03	0.02	0.79
dad college	0.02	0.02	0.00	0.94	0.01	−0.01	0.06
dad high school	0.32	0.36	0.04	0.19	0.37	0.05	0.08
dad secondary school	0.45	0.42	−0.03	0.31	0.41	−0.04	0.23
dad education missing	0.03	0.03	0.00	0.96	0.02	−0.00	0.89
mum college	0.02	0.02	−0.00	0.90	0.01	−0.01	0.47
mum high school	0.50	0.50	0.00	0.92	0.51	0.02	0.59
mum secondary education	0.44	0.43	−0.01	0.82	0.41	−0.03	0.38
mum education missing	0.02	0.02	−0.01	0.41	0.03	0.01	0.38
Ukrainian language	0.92	0.89	−0.02	0.21	0.89	−0.03	0.11
humanities	0.27	0.33	0.06	0.06	0.29	0.02	0.47
social sciences	0.27	0.26	−0.01	0.73	0.25	−0.02	0.43
technical studies	0.28	0.25	−0.03	0.28	0.27	−0.01	0.82
natural sciences	0.08	0.08	−0.00	0.88	0.08	0.00	0.89
medical studies	0.09	0.08	−0.01	0.60	0.10	0.00	0.88
sports science	0.01	0.01	−0.00	0.65	0.01	0.00	0.70
state financed	0.74	0.74	0.00	0.91	0.77	0.03	0.23
study year	3.25	3.05	−0.20	0.05	3.12	−0.14	0.17
grades	3.67	3.65	−0.01	0.83	3.65	−0.01	0.84
time to prepare	3.75	3.73	−0.02	0.78	3.71	−0.05	0.53
had entrance test	0.85	0.85	0.00	0.91	0.85	0.00	0.93
bribes: personal experience	0.06	0.06	0.00	0.75	0.07	0.01	0.50
bribes: friends	0.13	0.16	0.03	0.13	0.12	−0.00	0.83
bribes: no experience	0.67	0.64	−0.03	0.27	0.67	−0.01	0.80
presents to teacher	2.42	2.27	−0.15	0.04	2.40	−0.02	0.77
violations in uni: pers.	0.07	0.08	0.01	0.73	0.07	−0.00	0.89
viol. in uni: friends	0.23	0.22	−0.01	0.73	0.26	0.03	0.30
viol. in uni: no one	0.70	0.70	0.00	0.91	0.67	−0.03	0.38
heard: bribes in uni	3.46	3.34	−0.12	0.13	3.51	0.05	0.50
heard: pulling strings	2.91	2.81	−0.10	0.18	2.92	0.01	0.89
uses cheat sheets	2.73	2.58	−0.16	0.03	2.66	−0.07	0.32
downloads papers etc.	1.83	1.82	−0.01	0.85	1.80	−0.03	0.66
buys papers etc.	1.27	1.31	0.04	0.44	1.28	0.00	0.97
copies parts	2.89	2.88	−0.01	0.92	2.97	0.08	0.34
cheats during exams	2.76	2.60	−0.16	0.02	2.72	−0.04	0.59
lying to teacher	1.76	1.78	0.02	0.75	1.73	−0.03	0.66
asks for special treatment	1.32	1.30	−0.02	0.71	1.36	0.04	0.35
encountered bribery at uni	1.94	1.74	−0.20	0.01	1.97	0.03	0.73
infopaid	0.25	0.26	0.00	0.91	0.23	−0.02	0.46
infocorruption	0.25	0.25	0.01	0.83	0.27	0.02	0.47
infoecology	0.24	0.23	−0.01	0.83	0.25	0.01	0.73
corruption is necessity	1.48	1.49	0.02	0.71	1.59	0.11	0.04
… is means to earn money	2.49	2.64	0.15	0.11	2.61	0.12	0.21
… is crime	4.57	4.52	−0.06	0.26	4.53	−0.05	0.36
… is part of life	2.08	2.03	−0.04	0.58	2.08	0.00	0.98
… way of solving problems	2.71	2.76	0.06	0.48	2.85	0.14	0.08
… compensation for low salaries	2.79	2.77	−0.02	0.78	2.86	0.07	0.44
… temporary phenomenon	2.27	2.31	0.04	0.54	2.32	0.06	0.43
… tradition	2.91	3.00	0.09	0.31	3.04	0.12	0.18
… national peculiarity	2.61	2.58	−0.03	0.75	2.65	0.04	0.63
… is evil	4.54	4.46	−0.08	0.10	4.44	−0.10	0.06
… influences my career	1.85	1.86	0.01	0.82	1.91	0.06	0.22
… my quality of life	1.74	1.78	0.04	0.37	1.86	0.12	0.01
… my education	1.57	1.63	0.06	0.17	1.63	0.06	0.22
… my health	1.85	1.89	0.04	0.47	1.98	0.13	0.02
… my security	1.70	1.75	0.05	0.29	1.80	0.10	0.04

Note: “mcontr”: mean of control group T1_gen; “mtreat”: mean of respective treatment; “diff”: mean difference; “pval”: p-value of mean difference. Regional dummies are omitted.

In order to investigate the degree of possible selectivity bias further, we proceed with balance tests to compare covariates in different treatments. Table 3 investigates selectivity with regard to observed covariates by checking whether the treatments are still balanced when considering only cases with non-missing values in the outcomes and covariates. If this was the case, and the observed covariates comprised all the factors also affecting the outcome, then the effects of the treatments are consistently estimated (i.e., internally valid) for the sample without attrition (see for instance the discussion in Huber 2012). Balance tests comparing the covariate means of each treatment to the control (*T1_gen* number) support our hypothesis of balanced treatment, since only few variables are significantly different at 5% level. Given the large number of covariates tested and the fact that only a few exceptions were found, we are not concerned by these few cases.

Furthermore, we ran machine learning-based tests for assessing balance jointly for all covariates across treatments using the approach of Ludwig, Mullainathan, and Spiess (2017). The authors point out that problems of obtaining too many significant differences by testing several hypotheses are tantamount to overfitting – or including too many regressors while predicting a variable – in machine learning. For this reason, we investigated whether the treatment can be predicted by the covariates, which would point to imbalances. To this end, we split our data into training and testing data in order to apply the machine learning logic to the context of multiple testing. In the training data, we ran a lasso[3] logit regression of the respective treatment (vs. control) on the covariates. Lasso regression as discussed in Tibshirani (1996) allows for a data-driven selection of only those covariates that importantly predict the treatment, based on a penalization for including (too) many (and possibly non-predictive) covariates. We then used the coefficients obtained from the training data for predicting the treatment in the test data and compared the prediction to the actual treatment to compute the mean squared error (MSE). We applied 5-fold cross-validation for this purpose and took the average of the five mean squared errors obtained to reduce its variance. Furthermore, we randomly relabeled the treatment variables and re-estimated the MSE based on the same procedure (cf. Ludwig, Mullainathan, and Spiess 2017). We repeated the permutation 999 times to compute the p-value for the joint significance of the covariates as the share of permutation-based MSEs that are lower than the MSE with the correct coding of the treatment. The permutation test's intuition is that, if the covariates are balanced across treatments, relabeling the latter will not seriously affect (i.e., increase) the MSE. If, on the other hand, the covariates are predictive for the treatment, then the correct coding of the treatment should entail a smaller MSE than the permuted versions.

Table 4 reports the results, namely the p-values when running the test with the group with generated numbers (T1_gen) and either the treatment group with the choice of options

Table 4. P-values of machine learning-based tests.

	T3_choice	T2_manual	T3_choice/T2_manual
p-value	0.59	0.79	1.00

Note: P-values of machine learning-based tests of joint covariate balance across the respective treatment and the reference group with computerized coin tossing. The tests are based on lasso regressions of the treatment on the covariates and randomly permuting the treatment labels to compute p-values by comparing the predictive performance of the regressions in permuted and non-permuted data.

(T3_choice), with manual tossing (T2_manual), or the joint groups with choice (T3_choice)/manual tossing (T2_manual), respectively. None of the tests reject covariate balance at any conventional levels of statistical significance. In line with this finding, an inspection of the results of the lasso logit regressions of the treatment states on the covariates reveals that the coefficients on the covariates are equal to zero in almost all cases, suggesting that the covariates do not importantly differ across treatments. It should be noted that although we can reject selection on observables, we cannot completely rule out the selection on unobservables.

Next, we consider the average treatment effects (ATE) of the three coin tossing procedures on the numbers of successes reported thereafter. To this end, we either considered the mean difference-based estimation between the group with generated numbers and each of the groups with manual or both types of tossing, respectively, or doubly robust[4] (DR) estimation. The latter approach permits controlling for (even minor) imbalances in the covariates across treatment states in a data-driven way, namely by running lasso regressions to control for important predictors of the treatment and the outcome. To accomplish this, the method first estimates models for the conditional mean outcome under a specific treatment as well as for the conditional treatment probability as a function of the covariates based on lasso regression. In a second step, the estimated conditional mean outcomes and treatment probabilities are plugged into an expression for ATE estimation, which consists of so-called doubly robust score functions (because they "robustify" ATE estimation against errors in the estimation of the conditional mean outcomes and treatment probabilities). We refer to Belloni et al. (2017) for a more detailed discussion on DR estimation of the ATE based on machine learning.

Table 5 provides the effects on the outcome "number of 'heads' reported" based on mean differences in the first line and on the DR lasso procedure in the second. Both methods yield statistically and economically significant effects, suggesting that the treatment "T2_manual" increases the reported number on average by 1.69 relative to the control group "T1_gen" with the generated number, while the treatment effect for "T3_choice" is roughly one half. We also note that the mean of the control group "T1_gen" is 5.13 and has a standard error of 0.08. When testing this against the theoretical expectation of 5, one obtains a t-statistic of 1.73. Thus, the average is different from 5 at the 10% level of statistical significance, while it is larger than 5 at the 5% level, pointing to some (albeit limited) cheating also in the group with a generated number, "T1_gen."

In line with the findings in Gneezy, Kajackaite, and Sobel (2018), Abeler, Nosenzo, and Raymond (2019), and Crede and von Bieberstein (2019), our results suggest that students cheat to increase their chances to win a cash lottery and that cheating becomes more severe when coin tossing is perceived to be more anonymous. For the treatment "T2_manual," which arguably has the highest level of perceived anonymity, the reported number is on

Table 5. Treatment effects on "number entered."

		both T1_gen and T2_manual			T2_manual		
	mean T1_gen	estimate	standard error	p-value	estimate	standard error	p-value
mean difference	5.13	0.50	0.11	0.00	1.69	0.11	0.00
DR lasso		0.46	0.11	0.00	1.69	0.12	0.00

Note: "mean generated": mean of control group (T1_gen); "estimate": estimated effect; "standard error": heteroscedasticity robust standard error. "p-value": p-value of the effect.

average one third higher than in the control group. For the treatment “T3_choice,” the effect still amounts to a non-negligible 9 – 10% increase (relative to “T1_gen”) but clearly points to less cheating than in the case of “T2_manual.” This difference may appear surprising, given that the supposedly more private manual option was also provided, and one could have in principle picked the maximum number of both tossing approaches. However, the result could either be explained by inertia or anchoring, implying that respondents preferred entering the provided generated number over thinking about tossing choices and related cheating strategies or, again, by a lack of perceived anonymity (i.e., the suspicion that the randomly generated number is observed in the system). Additionally, the choice treatment allowed subjects to choose between two treatments that varied in the degree of perceived anonymity and consequently, in the cheating behavior of participants. For this reason, another possible explanation for this finding is that individuals that exhibited payoff more honest behavior chose the treatment that they perceived as more honest. This can be explained by individual differences in student psychological profiles, morality, and social norms (Rundle et al., 2019). This finding can be informative to educators and policymakers due to the fact the results point out that when students are given the option to cheat, some prefer not to.

It is worth noting that the survey data underlying our experiment are not representative of the whole of Ukraine, since, for example, women are substantially oversampled. Indeed, somewhat surprisingly, 82% of the respondents were female. We see two reasons for this: (i) it is common to have a higher percentage of female response rates in surveys (see Moore and Tarnai 2002; Singer, Van Hoewyk, and Maher 2000; Smith 2008); and (ii) there is a higher share of women in Ukrainian universities overall (52.3% in 2013–14) combined with a higher participation rate of students from the specialties where women are overrepresented (about 54% of participants represent the humanities and social sciences where women represent 79% and 77% of students, correspondingly). In our opinion, such a disbalance does not influence the result significantly, since the internal validity of the causal effects in our experiment follows from the random assignment of treatments. Moreover, the question of academic dishonesty from the survey as well as the perceived probability of winning the cash lottery might have led to different motivations for cheating behavior. This possibility might make the interpretation of our results less clear as we are not able to disentangle the individual motivations that lead to the observed cheating rates. As a result, we are left with a less straightforward interpretation of the results.

Conclusion

Cheating among students is threatening to become “business as usual” around the world. Educators, universities, and other decision makers should pay more attention to this aberration. The experience of cheating during university studies is highly likely to influence the future professional lives of young people. Our study suggests a very affordable remedy that can be easily implemented and maintained at many universities, even those that have a tiny budget: increasing the observation – or even the illusion of observation – of students. This tool might mitigate cheating among students significantly.

Our randomized online experiment in Ukraine demonstrated that a substantial share of students was inclined to cheat in order to win a cash lottery. When asking about 1,500 survey participants to make ten coin tosses and report the number of tossed “heads,” we found that participants who were supposed to toss the coin manually reported a number on average 33% higher than those who were supposed to report a generated number based on an online tool (and 34% higher than the theoretical average without cheating). For those provided with the choice between manual and online coin tossing, the increase still amounted to 9 – 10%. This suggests that the ambiguity about anonymity in the online context made many students abstain from cheating, despite the fact that online tosses were anonymous as well.

These findings may be of interest to educators, university administrators, and other decision makers in the education industry with respect to procedures for student evaluation. The ongoing COVID-19 pandemic has forced many universities to move to online teaching, which in turn raises questions about transparency and honesty, e.g., in online exams. Decision makers should therefore pay attention to students' inclination to cheat in unproctored settings and take this into consideration when developing guidelines for student assessment.

Notes

1. The outcomes of the first part of this experiment are reported in Denisova-Schmidt, Huber and Prytula, 2019. Subjects were randomly assigned to watch one of four short videos: three on corruption and its consequences (treatment groups) and one on modern higher education (control group). The data suggests that a video presenting an engaging story about an identifiable victim of corruption (in this case, bribery) in an accessible way was most effective in raising awareness of the negative effects of corruption. In contrast, the two treatment videos that adhered to the typical style of TV documentaries on corruption showed no important effect on the respondents' attitudes toward corruption.
2. We looked at the data to find if the video treatments create any priming effect on further cheating behavior. We regressed the treatments and their interactions on the outcomes. The results suggest that the video treatments do not affect the outcome significantly. Similarly, the interaction effects with the lottery treatments are insignificant. The only significant effect comes from the lottery treatments themselves. This result shows the absence of possible priming effects and might have differing explanations: we may speculate that time matters, and the effect of the videos does not last long enough to be captured in the approximately 10 minutes after the video. On the other hand, we can speculate that the lottery itself became a significant attractor and eliminated the video's effect completely.
3. We used the 'rlogit' command with its default values in the 'hdm' package designed by Chernozhukov et al. (2015) for the statistical software 'R'.
4. Estimation based on the 'rlassoATE' command of the 'hdm' package of Chernozhukov et al. (2015) for the statistical software 'R'.

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Availability Of Data And Materials

All data generated or analyzed during this study are included in information files.

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Appendix

Appendix 1: Survey questions used for covariate balance tests across treatments

Q1	Choose the language for filling in the questionnaire:	1 – Ukrainian 2 – Russian	Closed-Ended Question
Q4	What is your specialization?	1 – Humanities 2 – Social sciences 3 – Technical and exact sciences 4 – Natural sciences 5 – Medical sciences 6 – Sport 99 – No answer 999 – Interrupted interview	Closed-Ended Question
Q5	Please indicate your form of tuition.	1 – State-financed 2 – Fee-based 99 – No answer	Closed-Ended Question
Q7	What year are you in?	1 – First year 2 – Second year 3 – Third year 4 – Fourth course 5 – Fifth year (first year of master's degree program) 6 – Sixth year (second year of master's degree program) 7 – Internship 8 – Postgraduate studies 99 – No answer	Closed-Ended Question
Q9	Which grades do you most often get at the university?	1 – "Satisfactory" 2 – Between "satisfactory" and "good" 3 – "Good" 4 – Between "good" and "excellent" 5 – "Excellent" 99 – No answer	Closed-Ended Question
Q10	How much time each day do you spend on self-study /homework/preparation for classes?	1 – No time at all 2 – Less than 1 hour 3 – 1–2 hours 4 – 2–3 hours 5 – More than 3 hours 99 – No answer	Closed-Ended Question
Q11	Did you pass the External Independent Examination to enter the university?	1 – Yes 2 – No 99 – No answer	Polar Question
Q12	Have you, your friends or relatives ever encountered violations (bribes, gifts, help in answering) when passing the External Independent Examination?	1 – Yes, I personally 2 – Yes, my friends (relatives) 3 – No, nobody 99 – No answer 999 – Interrupted interview	Closed-Ended Question
Q13	If you or your parents ever gave presents to teachers at school (candies, books, computer equipment, etc.), or, for example, paid for voluntary-compulsory tutoring classes with school teachers, how often did it happen?	1 – Never 2 – Rarely 3 – Sometimes 4 – Often 5 – Constantly 99 – No answer 999 – Interrupted interview	Closed-Ended Question
Q14	Have you, your friends or relatives ever encountered violations (situations in admissions commissions, in granting privileges and allocation of quotas, etc.) when entering the university?	1 – Yes, I personally 2 – Yes, my friends (relatives) 3 – No, nobody 99 – No answer	Closed-Ended Question

(*Continued*)

(Continued).

Q15	Have you ever heard that bribes are taken or given in higher education institutions?	1 – Never 2 – Rarely 3 – Sometimes 4 – Often 5 – Constantly 99 – No answer	Closed-Ended Question
Q16	Have you ever heard of situations in which your friends or relatives solved their problems by pulling strings?	1 – Never 2 – Rarely 3 – Sometimes 4 – Often 5 – Constantly 99 – No answer	Closed-Ended Question
Q18.1	How often do you personally resort to the following practices in your studies: Using "cheat sheets" during exams?	1 – Never 2 – Rarely 3 – Sometimes 4 – Often 5 – Constantly 99 – No answer 999 – Interrupted interview	Multiple Response Question
Q18.2	How often do you personally resort to the following practices in your studies: Using ready-made course papers or other papers from the Internet?	1 – Never 2 – Rarely 3 – Sometimes 4 – Often 5 – Constantly 99 – No answer	
Q18.3	How often do you personally resort to the following practices in your studies: Purchasing course papers or other papers from special companies or from fellow students?	1 – Never 2 – Rarely 3 – Sometimes 4 – Often 5 – Constantly 99 – No answer	
Q18.4	How often do you personally resort to the following practices in your studies: Writing course papers or other papers by copying some parts from the Internet?	1 – Never 2 – Rarely 3 – Sometimes 4 – Often 5 – Constantly 99 – No answer	
Q18.5	How often do you personally resort to the following practices in your studies: Cheating during exams or tests?	1 – Never 2 – Rarely 3 – Sometimes 4 – Often 5 – Constantly 99 – No answer	
Q18.6	How often do you personally resort to the following practices in your studies: Lying to a faculty member when explaining learning-related issues (for example, absence from classes, failure to meet deadlines, failure to appear at an exam)?	1 – Never 2 – Rarely 3 – Sometimes 4 – Often 5 – Constantly 99 – No answer	
Q18.7	How often do you personally resort to the following practices in your studies: Asking a faculty member for an individual approach (for example, less strict requirements, loyalty, exemption from an exam)?	1 – Never 2 – Rarely 3 – Sometimes 4 – Often 5 – Constantly 99 – No answer	
Q19	Have you ever personally encountered bribery at a university (institute)?	1 – Never 2 – Rarely 3 – Sometimes 4 – Often 5 – Constantly 99 – No answer	Closed-Ended Question

(*Continued*)

(Continued).

Experiment: Interruption of the survey to watch a video. There were four videos randomly assigned to respondents. All videos are in Ukrainian. The video player was designed in such a way that it was impossible to skip watching video without stopping the survey.			
video	1 – He Paid 2 – About Corruption 3 – Essays on Ecology 4 – Modern Education		
Q20.1	What is corruption for you personally: A necessity?	1 – Definitely no 2 – Rather no 3 – Yes and no 4 – Rather so 5 – Definitely so 99 – No answer 999 – Interrupted interview	Multiple Response Question
Q20.2	What is corruption for you personally: A means of earning money?	1 – Definitely no 2 – Rather no 3 – Yes and no 4 – Rather so 5 – Definitely so 99 – No answer	
Q20.3	What is corruption for you personally: A crime?	1 – Definitely no 2 – Rather no 3 – Yes and no 4 – Rather so 5 – Definitely so 99 – No answer	
Q20.4	What is corruption for you personally: A part of life?	1 – Definitely no 2 – Rather no 3 – Yes and no 4 – Rather so 5 – Definitely so 99 – No answer	
Q20.5	What is corruption for you personally: A way of solving problems?	1 – Definitely no 2 – Rather no 3 – Yes and no 4 – Rather so 5 – Definitely so 99 – No answer	
Q20.6	What is corruption for you personally: A compensation for low salaries?	1 – Definitely no 2 – Rather no 3 – Yes and no 4 – Rather so 5 – Definitely so 99 – No answer	
Q20.7	What is corruption for you personally: A temporary situation?	1 – Definitely no 2 – Rather no 3 – Yes and no 4 – Rather so 5 – Definitely so 99 – No answer	
Q20.8	What is corruption for you personally: A tradition?	1 – Definitely no 2 – Rather no 3 – Yes and no 4 – Rather so 5 – Definitely so 99 – No answer	
Q20.9	What is corruption for you personally: A national peculiarity?	1 – Definitely no 2 – Rather no 3 – Yes and no 4 – Rather so 5 – Definitely so 99 – No answer	

(*Continued*)

(Continued).

Q20.10	What is corruption for you personally: An evil?	1 – Definitely no 2 – Rather no 3 – Yes and no 4 – Rather so 5 – Definitely so 99 – No answer	
Q22.1	How do you think corruption influences different spheres of your life: Your career prospects	1 – Absolutely negatively 2 – Rather negatively 3 – Rather positively 4 – Quite positively 99 – No answer 999 – Interrupted interview	Multiple Response Question
Q22.2	How do you think corruption influences different spheres of your life: Your quality of life	1 – Absolutely negatively 2 – Rather negatively 3 – Rather positively 4 – Quite positively 99 – No answer	
Q22.3	How do you think corruption influences different spheres of your life: Your education	1 – Absolutely negatively 2 – Rather negatively 3 – Rather positively 4 – Quite positively 99 – No answer	
Q22.4	How do you think corruption influences different spheres of your life: Your health	1 – Absolutely negatively 2 – Rather negatively 3 – Rather positively 4 – Quite positively 99 – No answer	
Q22.5	How do you think corruption influences different spheres of your life: Your security	1 – Absolutely negatively 2 – Rather negatively 3 – Rather positively 4 – Quite positively 99 – No answer	
Q24	Specify your gender please.	1 – Male 2 – Female 99 – No answer 999 – Interrupted interview	Closed-Ended Question
Q28	What is your father's educational level?	1 – Secondary, secondary special 2 – Higher 3 – Academic degree 4 – Hard to say 99 – No answer	Closed-Ended Question
Q30	What is your mother's educational level?	1 – Secondary, secondary special 2 – Higher 3 – Academic degree 4 – Hard to say 99 – No answer	Closed-Ended Question
Q32	Do you have any siblings?	1 – Yes 2 – No 99 – No answer	Polar Question