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Selection effects on dishonest behavior

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Abstract

In many situations people behave ethically, while elsewhere dishonesty reigns. Studies of the determinants of unethical behavior often use random assignment of participants in various conditions to identify contextual or psychological factors influencing dishonesty. However, in many real-world contexts, people deliberately choose or avoid specific environments. In three experiments (total N = 2,124) enabling self-selection of participants in two similar tasks, one of which allowed for cheating, we found that participants who chose the task where they could lie for financial gain reported a higher number of correct predictions than those who were assigned it at random. Introduction of financial costs for entering the cheating-allowing task led to a decrease in interest in the task; however, it also led to more intense cheating. An intervention aimed to discourage participants from choosing the cheating-enabling environment based on social norm information did not have the expected effect; on the contrary, it backfired. In summary, the results suggest that people low in moral character are likely to eventually dominate cheating-enabling environments, where they then cheat extensively. Interventions trying to limit the preference of this environment may not have the expected effect as they could lead to the selection of the worst fraudsters.

Keywords: cheating, self-selection, behavioral ethics, honesty-humility

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1 Introduction

Given the prevalence and high societal costs of dishonesty, a large number of experimental studies have attempted to identify the factors associated with fraudulent and dishonest behavior, such as self-serving justifications (Gino & Ariely, 2012); self-control (Mead et al., 2009; Shalvi, Eldar & Bereby-Meyer, 2012); collaboration with a dishonest partner (Ścigała et al., 2019); feelings of entitlement (Poon et al., 2013) or anonymity (Zhong et al., 2010). Typically, these studies randomly assign participants into experimental and control groups and measure the causal effect of the manipulated factor. However, in various real-world contexts, people are not randomly assigned to their circumstances; they often actively create, choose, and influence them. Dishonest individuals choose opportunities, organizations, and industries where cheating is easier or even tolerated (Cohn et al., 2014; Hanna & Wang, 2017). For example, dishonest people are more likely to stay in groups led by unethical leaders, while more honest people tend to leave them (Cialdini et al., 2019); those who are willing to cheat tend to prefer performance-based compensation schemes, where cheating can lead to additional reward (Cadsby, Song & Tapon, 2010); advisers with a record of misconduct are more likely to choose deceptive financial advisory firms for employment (Egan, Matvos & Seru, 2019); psychopathic and Machiavellian individuals aim for and eventually occupy high-ranking positions at companies (Babiak, Neumann & Hare, 2010); and business and economic students' lack of generosity is mostly due to self-selection of selfish people to study in these fields, not antisocial indoctrination by their discipline (Bauman & Rose, 2011; Frey & Meier, 2003).

Our paper has several aims. First, we examine the relationship between the selection of a cheating-enabling environment and cheating. Second, we examine the characteristics of individuals who selected-themselves into a cheating-enabling environment. Third, we study the effects of interventions intended to influence the rate of selection of a cheating-enabling environment.

Although the importance of the selection and sorting effects in various contexts is recognized and addressed in the large body of non-experimental literature (e.g., Heckman, 1990; Bayer & Ross, 2006; Bless & Burger, 2016), they are seldom explored in experimental studies (Gaines & Kuklinski, 2011, is a rare example). The first of a few experimental studies of selection effects concerning dishonesty found that cheating increased when participants could choose whether they would be able to cheat on a subsequent task compared to a condition in which everyone was enabled to cheat (Gino, Krupka & Weber, 2013). In another study, competition and winner-takes-all compensation schemes seemed to attract more dishonest participants (Faravelli, Friesen & Gangadharan, 2015). Yet another study found that those more willing to cheat were also more likely to opt for information that facilitated justification of dishonest behavior (Akin, 2019). Most recently, Brassiolo, Estrada, Fajardo & Vargas (2020) demonstrated that in comparison with conditions using artificial tokens, conditions enabling embezzlement of real money drove away honest individuals while attracting those more willing to cheat. Given the relative lack of attention to selection

effects in the research on dishonesty, the primary aim of this paper is to analyze these effects and their relationship to cheating.

Our second aim was to examine how people who prefer the cheating-enabling version of the game differ from others. Personality traits may shape how people understand the situations they experience, and people may select and manipulate their circumstances to fit their personality traits (de Vries, Tybur, Pollet & Van Vugt, 2016). Therefore, people with personality traits associated with cheating and dishonesty may more readily realize that an environment offers an opportunity to cheat. For example, Sherman and colleagues (Sherman, Rauthmann, Brown, Serfass & Jones, 2015) reported that low honesty-humility traits were associated with a higher tendency to experience situations in which someone is deceived. People with dishonesty-related traits may also be able to quickly evaluate whether cheating would be profitable and if so, take advantage of this opportunity (Hilbig, Zettler & Heydasch, 2012; Zettler & Hilbig, 2010). On the other hand, it may not occur to those with opposite characteristics that they could look for an opportunity to cheat.

There is a growing literature documenting associations between individual and personality characteristics and dishonest behavior. For example, some basic human values (Schwartz et al., 2012) predict unethical behavior among athletes (Ring et al., 2020) and accounting students (Mubako et al., 2020), moral disengagement was found to be positively related to unethical decision making (Detert, Treviño & Sweitzer, 2008), and HEXACO's honesty-humility personality trait was shown to be related to cheating (Heck et al., 2018; Hilbig & Zettler, 2015; Pfattheicher, Schindler & Nockur, 2019; Vranka & Bahník, 2018). However, comparatively little is known about predictors of preferences for cheating-enabling environments. Only one previous study found that honesty-humility was strongly associated with the selection of an experimental condition that enabled participants to cheat (Hilbig & Zettler, 2015).

While predictors of selection may be similar to predictors of actual cheating, it is possible that people who are willing to cheat when randomly allowed to do so differ from those who seek such opportunities themselves. In the first study, we included a wide range of personality scales that have been found to be associated with moral behavior in earlier literature. Although the relation between the honesty-humility scale and cheating can be expected based on the previous findings, we also explore various other conceptualizations of moral traits in an exploratory fashion. In addition to moral traits, we were interested whether selection of the cheating-enabling environment will be associated with risk aversion, impression management tendencies or a tendency to act prosocially and to contribute to a charity from one's reward from the experiment. It can be argued that the cheating-enabling condition is less risky, as one can misreport some outcomes in case of bad luck (Hilbig & Zettler, 2015); however, risk aversion may also lead to the avoidance of ethically controversial situations (Hennig-Schmidt, Jürges & Wiesen, 2019). Alternatively, people who are more likely to manage impressions may be less willing to self-select into the cheating-enabling environment, because such selection could be perceived as a signal of

one's preparedness to cheat (Sassenrath, 2020). Finally, we wanted to explore the relations between the selection of the cheating-enabling environment and prosocial traits and charitable donations, as it was possible that behaving charitably could compensate for the previous dishonesty (Wiltermuth, Newman & Raj, 2015); or contrary, social preferences may be contradictory to dishonesty (Hanna & Wang, 2017). In two subsequent studies, we focused mainly on the HEXACO questionnaire and the association between honesty-humility and the preference of a cheating-enabling environment.

The third aim of the paper is related to the fact that the choice of a cheating-enabling environment is often associated with costs. For example, in some countries civil servants, politicians, and police officers could be willing to work for lower salaries because they know they can get additional money by corruption, and, similarly, lobbyists, and representatives of corrupt companies could be willing to bear financial and reputation costs to maintain a corrupt environment. In addition, we tested the effect of information about the proportion of participants choosing the cheating-enabling and cheating-prohibiting environments on the selection. The design of this intervention was motivated by the literature showing that information about social norms has been effective in changing behavior in multiple domains (Benartzi et al., 2017; Hallsworth et al., 2017; Halpern, 2015; Thaler & Sunstein, 2008; Walton & Wilson, 2018).

To address the aims of the paper, we conducted three experiments, which allowed participants to choose either an environment where they were able to cheat or where cheating was impossible. All experiments used a modified mind game in which participants were rewarded for correct predictions of die rolls or coin flips (Jiang, 2013; Moshagen & Hilbig, 2017). There were two versions of the game. In one version, the reward was determined by chance, and participants could not influence it. In the other version of the game, the reward was determined by self-reported results, and therefore, participants had an opportunity to cheat. In Studies 1 and 3, the choice of the cheating-enabling environment was costless, and the participants were not informed about the choices of others. These two studies differed in particular in the number of rounds, and therefore, in the potential detectability of cheating. Study 2 included two interventions: financial costs for entering the cheating-allowing task and an intervention based on social norm information.

In all three studies, we found that individuals with a higher propensity to cheat selected themselves into the environment, which allowed them to cheat. We also demonstrated higher generalizability of the effect of selection by using samples from different countries as China, Czechia, and English speaking countries (USA, UK, Canada, etc.) (Cheek, 2017; Henrich, Heine & Norenzayan, 2010; Novakova et al. 2018). Moreover, participants who chose the version where they could cheat to obtain a financial gain in Study 1 and 2 cheated more than those who were assigned to it at random. In Study 2, we found that the fee discouraged some individuals from choosing the cheating-enabling environment. However, the fee also resulted in more cheating by those who selected this environment. Surprisingly, participants were more likely to choose the cheating-enabling environment after receiving

the information that only a low fraction of participants chose this environment in the past. Finally, Study 3 confirmed the existence of the selection effect in an environment where cheating was more anonymous than in Studies 1 and 2.

2 Study 1

2.1 Methods

The materials used in the study, data, analysis scripts, as well as preregistration of all the studies are available at https://osf.io/cj28d/.

2.1.1 Participants

Six hundred and twenty-two subjects (315 Czech and 307 Chinese, 67% female, Mdn_{age} = 21) participated in the study. Both the Czech and the Chinese samples were recruited from laboratories' subject pools. According to a pre-registered exclusion criterion, 27 participants who did not answer all attention checks correctly were excluded from analyses using individual differences measures.[1] The final sample size had a sufficient power 1-β = .80 for an effect size d = 0.23 or r = .16.

2.1.2 Design and procedure

The study was conducted in a laboratory setting in groups of up to 17 or 38 participants in Czechia and China, respectively, who worked individually on workstations separated by dividers. The experiment was administered in English (a non-native language for both groups) using a custom-written Python program. All rewards were paid in the local currency; CZK in Czechia (100 CZK = approx. 4.40 USD) and CNY in China (100 CNY = approx. 14.40 USD).

The experiment used a modified mind game (Jiang, 2013; Moshagen & Hilbig, 2017). Participants earned 35 CZK or 10 CNY for each correct prediction of whether the outcome of a fair die roll would be odd or even. Participants played three rounds of the game, with ten rolls in each round. They were informed beforehand that they would receive a financial reward according to their result in one randomly selected round. Participants' understanding of the rules was checked with a short quiz.

There were two different versions of the game: in the BEFORE version, participants had stated their predictions before the die was rolled, and then they saw the outcome.

[1]Nevertheless, these participants were included in all the other analyses. There are two reasons why the inattention during the questionnaires need not imply the inattention in the main part of the experiment. First, higher monetary rewards incentivized participants to be more attentive during the experimental task. Second, the questionnaires were included at the end of the experiment and were relatively long. Therefore, participants might have been inattentive due to fatigue. Consistently, empirical evidence suggests that the failure to pass attention checks may not be predictive of behavior in preceding unrelated experiments (Klein, 2015).

Therefore, no cheating was possible. In the AFTER version, participants were asked to make a prediction in their mind and remember it. Then a roll was made, and participants were shown the outcome and asked to state whether they had predicted it correctly or not. As the actual prediction was only in participants' minds, they could cheat and misreport even their incorrect predictions as correct.

In the first two rounds, participants played one round of the game in the AFTER and one in the BEFORE version, in random order. The first two rounds introduced participants to both versions of the task, and the number of reported predictions in the AFTER version of the task served as a *baseline* measure of cheating. For the third round, participants were randomly assigned to a control or experimental group, and they read short descriptions of both versions of the game. The participants in the control group were randomly assigned either the AFTER or BEFORE version. The participants in the experimental group were offered a choice whether they wanted to play the third round in the BEFORE or AFTER version, or whether they wanted to be assigned to one of the versions at random. See Figure 1 for a schema of the study.

After finishing the main part of the experiment, we measured participants' risk preferences using a simplified Holt and Laury (HL) task (Teubner, Adam & Niemeyer, 2015) and their social preferences by giving them an option to give a part of their reward to a charity of their choice.[2] Finally, participants answered socio-demographic questions and filled several questionnaires; namely, 60-items HEXACO scale (Ashton & Lee, 2009), work deception scale (Gunia & Levine, 2016), prosocialness scale (Caprara, Steca, Zelli & Capanna, 2005), desirable responding scale (BIDR-16; Hart, Ritchie, Hepper & Gebauer, 2015), moral agency scale (Black, 2016), measure of moral disengagement (Shu, Gino & Bazerman, 2011), and values questionnaire (PVQ-RR; Schwartz et al., 2012).[3] Participants were informed before the questionnaires that there would be attention check items in the questionnaires and that they could earn an additional reward of 50 CZK or 15 CNY if they would manage to answer all of them correctly.

At the end of the session, participants answered questions about their perception of the two versions of the dice-rolling task and completed a short debriefing in which they answered open-ended questions about the aims of the different parts of the experiment.

[2]Because participants could earn up to 100 CZK or 30 CNY in the HL task and they were informed that experimenters would know only the total amount of reward during its payment at the end of the session, participants should not have been worried that a high reward would suggest dishonest conduct in the dice rolling task.

[3]Descriptive statistics for the measures can be found at https://osf.io/r6bne/.

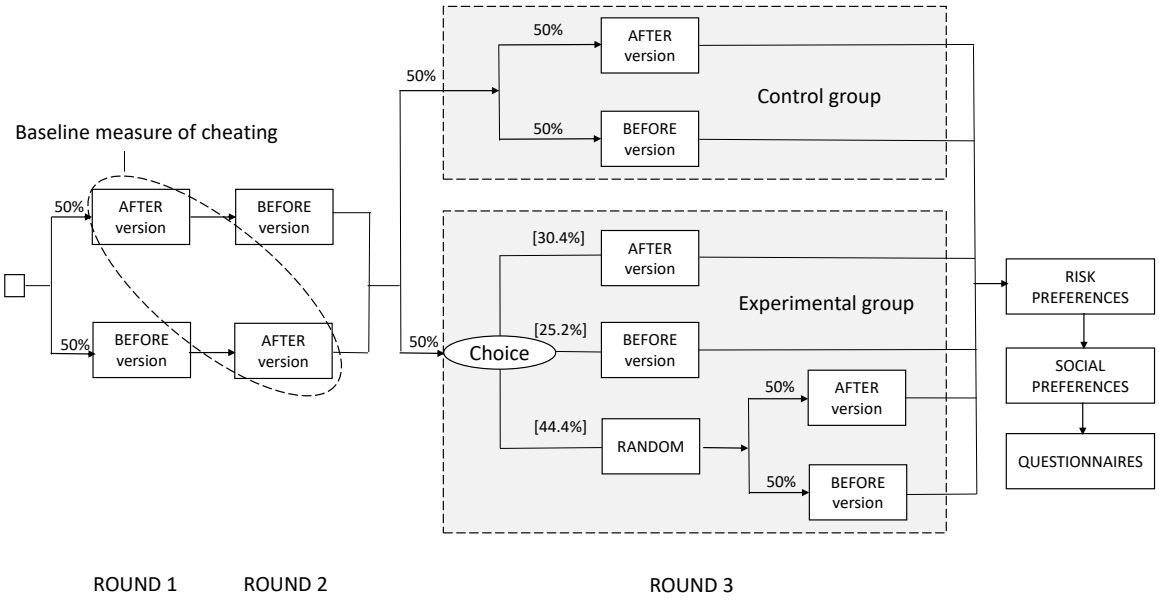

FIGURE 1: Schema of the design of Study 1. In rounds 1 and 2, participants played AFTER and BEFORE versions of the game in a randomized order. The number of reported predictions in the AFTER version of the task served as a baseline measure of cheating. In round 3, participants were randomly selected to a control and experimental group. Participants in the control group were randomly assigned BEFORE or AFTER version of the game. Participants in the experimental group chose whether they wanted to play BEFORE or AFTER version, or whether they wanted to be assigned to one of the versions at random. Then we measured, for all participants, risk and social preferences. Finally, participants answered socio-demographic questions and filled several questionnaires.

2.2 Results

The average number of reported correct predictions in the baseline measure of the AFTER version of the task exceeded five expected by chance ($t(621) = 11.25$, $p < .001$, $d = 0.45$, 95% CI [0.37, 0.53], $M = 5.84$), showing that participants cheated in the task. Czech participants on average cheated less than Chinese ($t(611) = -7.73$, $p < .001$, $d = -0.62$, CI [−0.79, −0.46], $M_{\text{Czech}} = 5.30$, $M_{\text{Chinese}} = 6.42$). The distribution of correct predictions per condition is shown in Figure 2.[4]

[4]Other comparisons of the two samples can be found at https://osf.io/xpbdq/.

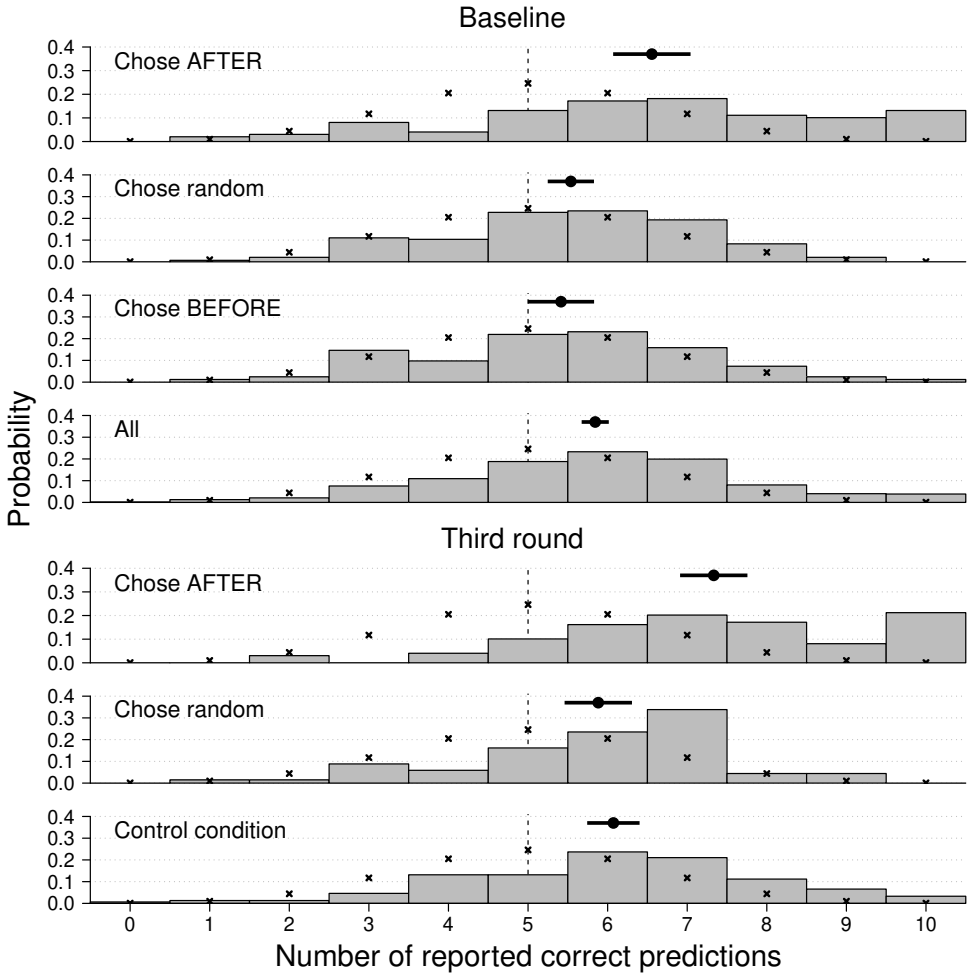

FIGURE 2: The distribution of correct predictions per condition. The figure shows the distribution of correct predictions in the baseline measure of cheating and in the 3rd round in comparison to the expected distribution. Observed means and their 95% confidence intervals are also displayed. Observations are grouped according to participants' decisions in the 3rd round. For example, "Chose AFTER" shows the distribution of correct predictions of the participants who chose the AFTER version of the task in the 3rd round.

The AFTER version of the task was chosen by 30.4% of the participants who were given the opportunity to choose a version of the task for the third round and the BEFORE version was chosen by 25.2% of participants. The remaining participants decided to be assigned the task at random. Participants who chose the AFTER version cheated more than participants in the control group who were assigned the AFTER version randomly ($t(249) = 5.01$, $p < .001$, $d = 0.65$, CI [0.39, 0.91], $M_{\text{after}} = 7.33$, $M_{\text{control}} = 6.07$), as well as more than participants from the experimental group who were assigned the AFTER version randomly ($t(165) = 4.93$, $p < .001$, $d = 0.78$, CI [0.45, 1.09], $M_{\text{random}} = 5.88$). Participants in the experimental group who chose the AFTER version also cheated more in the baseline measure of the AFTER version than those who chose to be assigned randomly ($t(242) = 4.02$, $p < .001$, $d = 0.52$, CI [0.26, 0.78], $M_{\text{after}} = 6.56$, $M_{\text{random}} = 5.54$), as well as those who

chose the BEFORE version of the task ($t(179) = 3.65$, $p < .001$, $d = 0.54$, CI [0.25, 0.84], $M_{\text{before}} = 5.41$). The latter two groups did not differ significantly from each other ($t(225) = 0.53$, $p = .597$, $d = 0.07$, CI [−0.20, 0.34]. The results therefore showed that cheaters also tended to choose the opportunity to cheat when given a choice.

Participants who chose the AFTER version of the task also cheated more in the third round than in the baseline ($t(98) = 4.18$, $p < .001$, $d = 0.42$, CI [0.21, 0.63], $M_{\text{after third}} = 7.33$, $M_{\text{after baseline}} = 6.56$). This shows that participants in the experimental condition who chose the AFTER version did not do so only because they had a high number of correct predictions in the baseline by chance. On the other hand, there was no significant difference in cheating between the baseline and the third round for those in the experimental group who were assigned to the AFTER version randomly ($t(67) = 0.35$, $p = .724$, $d = 0.04$, CI [-0.20, 0.28], $M_{\text{random third}} = 5.88$, $M_{\text{random baseline}} = 5.81$), as well as for those in the control group who were assigned to the AFTER version ($t(151) = 1.06$, $p = .291$, $d = 0.09$, CI [−0.07, 0.25], $M_{\text{control third}} = 6.07$, $M_{\text{control baseline}} = 5.89$). The possibility of choosing a version of the task, therefore, increased the rate of cheating for those who wanted to cheat. The experimental group also had somewhat higher earnings in the third round overall than the control group ($t(620) = 2.37$, $p = .018$, $d = 0.19$, CI [0.03, 0.35], $M_{\text{experimental}} = 5.89$, $M_{\text{control}} = 5.53$). This result suggests that the possibility of choosing an environment might increase the rate of cheating, at least under certain circumstances.

Out of the included individual differences measures, only honesty-humility predicted both cheating and selection of a version of the task (see Figure 3). Participants higher in honesty-humility cheated more in the baseline measure (r_S (Spearman's ρ) = −.09, CI [−.16, −.01], $p = .033$), and were also less likely to choose to have an option to cheat in the third round of the task ($r_S = -.13$, CI [−.24, −.02], $p = .019$, for choices ordered using levels BEFORE, random, and AFTER).

2.3 Discussion

Study 1 enabled self-selection of participants in two versions of the task, one of which allowed them to behave dishonestly. The results show that the participants who chose the version where they could cheat to obtain a financial gain reported a higher number of correct predictions than those who were assigned to it at random. Furthermore, those who selected the cheating enabling version reported a higher number of correct predictions than other participants even in the initial rounds before the selection, and they further increased the rate of reporting correct predictions after the selection. In line with similar previous studies, we interpret the higher number of correct predictions as "more cheating" on the aggregate (however, see Moshagen & Hilbig, 2017 for the discussion of potential problems with this interpretation). Therefore, our findings support the view that dishonest individuals seek cheating enabling environments. While some theories suggest that dishonest people may choose cheating-prohibiting environments either to overcome the temptation to cheat (Elster, 2000; Schelling, 2006) or to maintain a moral self-image (Barkan, Ayal & Ariely,

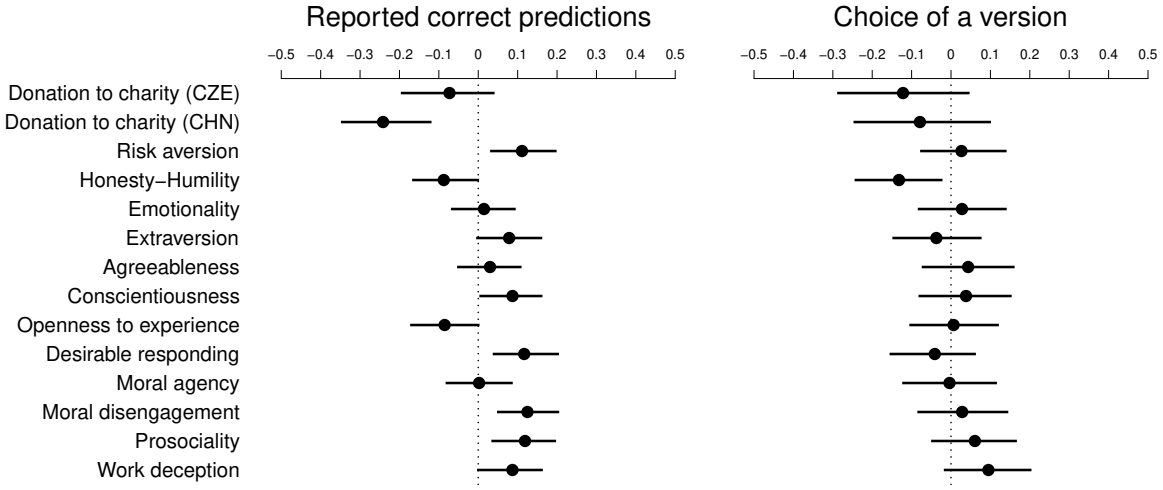

FIGURE 3: Correlations between individual differences measures with baseline cheating and selection of a version of the task in Study 1. The figure shows Spearman correlation coefficients and 95% confidence intervals for their estimates. The choice of a version had three ordered levels — BEFORE, random, and AFTER — and only participants in the experimental group were included. Donations to charity are analyzed separately for the two samples because the charities, as well as exact monetary rewards differed between them. The results from the PVQ are at https://osf.io/xpbdq/.

2015), our results show that even if this were true for some dishonest people, the selection of the cheating-enabling environment to gain an additional reward outweighs such an effect.

Because we used the number of correct guesses as a proxy for cheating, which was not possible to observe directly, there are alternative explanations of some of our findings. In particular, the positive correlation between the baseline cheating and selection of the AFTER version might have been caused by participants who chose the AFTER version of the game simply because they were luckier when they first played the AFTER version than when they played the BEFORE version. Therefore, they might have believed that the AFTER version is associated with a higher chance of winning than the BEFORE version (Rabin & Vayanos, 2010). However, this explanation is contradicted by two other results. First, participants who chose the AFTER version for the third round then cheated even more than in the baseline. Second, low honesty-humility, previously shown to predict cheating (Hilbig & Zettler, 2015; Kleinlogel, Dietz & Antonakis, 2018), was associated both with the high number of correct guesses and with the selection of the cheating-enabling environment. Both of these results suggest that cheaters, rather than individuals who were lucky when they first played the AFTER version, selected the cheating-enabling environment.

Despite the large number of measured personality characteristics, no other was associated with the selection of the cheating-enabling environment. Although some measures such as moral disengagement, desirable responding, risk-aversion, and prosociality were positively related to the number of the reported correct predictions, these correlations were quite low. The association between desirable responding and overreporting of correct pre-

dictions was in the opposite direction than in previous research (Zettler et al., 2015). A thorough analysis of these conflicting findings may be an interesting area for future research.

Finally, while the Chinese cheated more than the Czechs in the experiment, we did not find differences in other effects between the two groups. Even though the two samples differed in various individual differences measures, none of them explained the difference in cheating. It is possible that the difference in cheating was caused by cultural differences (Cohn et al., 2019; but see Pascual-Ezama et al., 2015).

In Study 1, we assumed zero costs associated with the choice of an environment. However, entering a cheating-allowing environment often requires effort and is associated with time and financial or reputational costs. The existence of entry costs could have contradictory effects on the prevalence of dishonesty. On the one hand, entry costs may deter people who are not highly motivated to cheat. On the other hand, entry costs could lead to more cheating by fraudsters who are prepared to cheat despite the costs, because they might want to recoup these costs or even view them as a kind of a fee that justifies their subsequent cheating (Shalvi et al., 2015). In Study 2, we tested how entry costs affect the rate of selection and the magnitude of cheating.

We also tested a non-financial measure that should limit the preference of the cheating-enabling environment. A social norm, that is, information about the behavior of others in a given situation, is a powerful signal of how one should behave. A reminder of a social norm usually leads to more congruent behavior. Nevertheless, the evidence about its effectiveness is not universal (Bicchieri & Dimant, 2019; McDonald & Crandall, 2015; Schultz et al., 2007). Because a minority of participants chose both the cheating-enabling (30%) and the cheating-prohibiting environment (25%) in Study 1, we were able to manipulate perceived norms by informing participants about either of the proportions in Study 2, where there was no possibility to choose the random assignment.

The large number of personality measures used in Study 1 also means that there was a higher probability of a false-positive result. Therefore, in Study 2, we used only the HEXACO personality questionnaire containing honesty-humility, the only significant predictor of cheating and environment selection. We also added a measure of the dark triad, which has been shown to be closely associated with honesty-humility (Hodson et al., 2018).

3 Study 2

3.1 Methods

3.1.1 Participants

The experiment was conducted with 501 participants (61% female, Mdn_{age} = 22) from our Czech laboratory subject pool consisting mostly of university students (n = 352). The most represented fields were humanities and social sciences (n = 125) and economics and management (n = 101). According to a pre-registered exclusion criterion, nine participants

who did not answer all the attention checks correctly were excluded for analyses using questionnaire data. The final sample size had a sufficient power $1 - \beta = .80$ for an effect size $d = 0.25$ or $r = .18$.

3.1.2 Design and procedure

The study was conducted in a laboratory setting in groups of up to 17 participants, who worked individually on workstations separated by dividers. The present study was the first in a batch of three unrelated studies. The whole experiment was administered in Czech using a custom-written Python program.

Participants played five rounds of the same modified mind game as in Study 1 with twelve rolls in each round. The reward for correctly predicting whether the outcome will be odd or even increased by 5 CZK with each correct prediction from 5 CZK for the first correct prediction to 60 CZK for the twelfth. Participants were informed that they would receive the money earned in one randomly selected round. In order to prevent participants' concerns that a higher reward would be an indicator of dishonest conduct, they were also informed that after the main dice rolling task, there would be a lottery task in which they could earn an additional reward of up to 1280 CZK and that experimenters would know only the total amount of reward at the end of the session.

As in Study 1, in the first two rounds, every participant played one round of the game in the AFTER and one in the BEFORE version, in random order. Before the third round began, participants had read short descriptions of both versions of the game, and then they chose whether they wanted to play the next round in the BEFORE or AFTER version. One half of the participants had to pay a fee of 25 CZK for the selection of the AFTER version. The other half of the participants could select the AFTER version without any fee. The BEFORE version was not associated with any fee for any of the participants. The fourth round was the same as the third, but participants were given the condition (fee or no-fee) that they had not received in the third round. In the fifth round, participants again chose between the BEFORE and AFTER version, this time without any fees. However, before making their decision, one half of participants learned that "only 30%" of participants wanted to participate in the AFTER version in a similar previous experiment and the other half learned that "only 25%" of participants wanted to participate in the BEFORE version of the task. Although these proportions reflect that in Study 1 participants had an additional option, and therefore, their choice problem differed from Study 2, we did not lie to the participants about the proportions, even though we did not disclose full information.[5] See Figure 3 for a schema of the study.

[5]Before manipulating the participants' perception, we asked them about their estimate of how many other participants would choose each of the versions of the task in the no-fee condition. On average, participants estimated that 28.2% of participants (SD = 19.3%) would choose the BEFORE version of the task (and 71.8% would choose the AFTER version). These figures suggest that participants have indeed considered the proportion of participants choosing the AFTER version in the information we provided (i.e., 30%) low, as was intended.

At the end of the session, participants provided socio-demographic information, filled several questionnaires — namely, 60-items HEXACO scale (Ashton & Lee, 2009) and Short Dark Triad scale (Jones & Paulhus, 2014)[6] — answered questions about their perception of the two versions of the dice-rolling task, and completed a short debriefing in which they answered open-ended questions about the aims of the different parts of the experiment. Attention check items were again added to the questionnaires, and participants could earn an additional reward of 50 CZK if they answered all of them correctly.

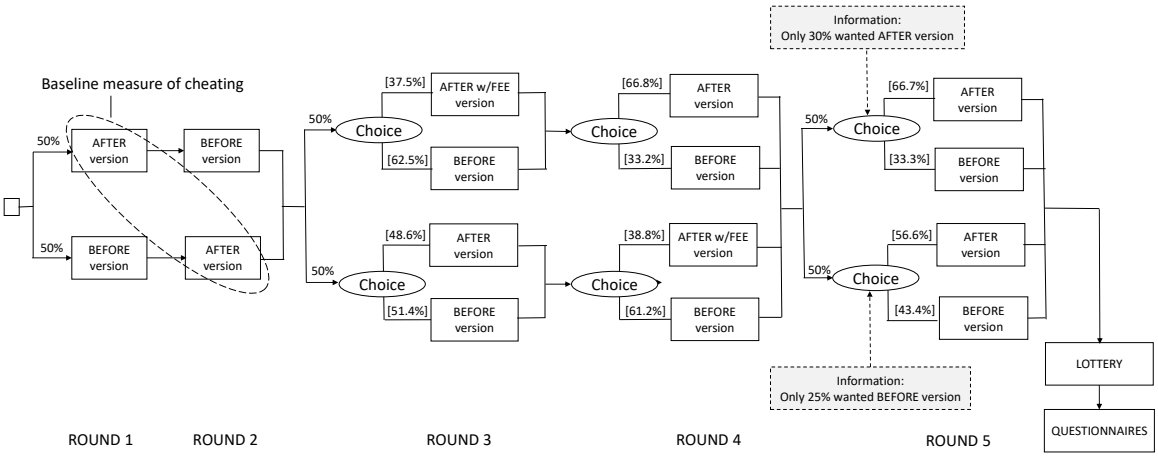

FIGURE 4: Schema of the design of Study 2. In rounds 1 and 2, participants played AFTER and BEFORE versions of the game in a randomized order. The number of reported predictions in the AFTER version of the task served as a baseline measure of cheating. In rounds 3 and 4, participants chose in random order between the BEFORE and AFTER version, and between the BEFORE and AFTER version with a fee. In round 5, participants chose between the BEFORE and AFTER version. Before making their decision, a half of participants learned that "only 30%" of participants participated in the AFTER version in a previous experiment while the other half learned that "only 25%" of participants participated in the BEFORE version of the task. Finally, all participants played a lottery, answered socio-demographic questions, and filled several questionnaires.

3.2 Results

The average number of reported correct predictions in the baseline measure of cheating exceeded the six expected by chance ($t(500) = 17.15$, $p < .001$, $d = 0.77$, CI [0.67, 0.87], $M = 7.68$), showing that participants cheated in the task.

[6]Descriptive statistics for the measures can be found at https://osf.io/r6bne/.

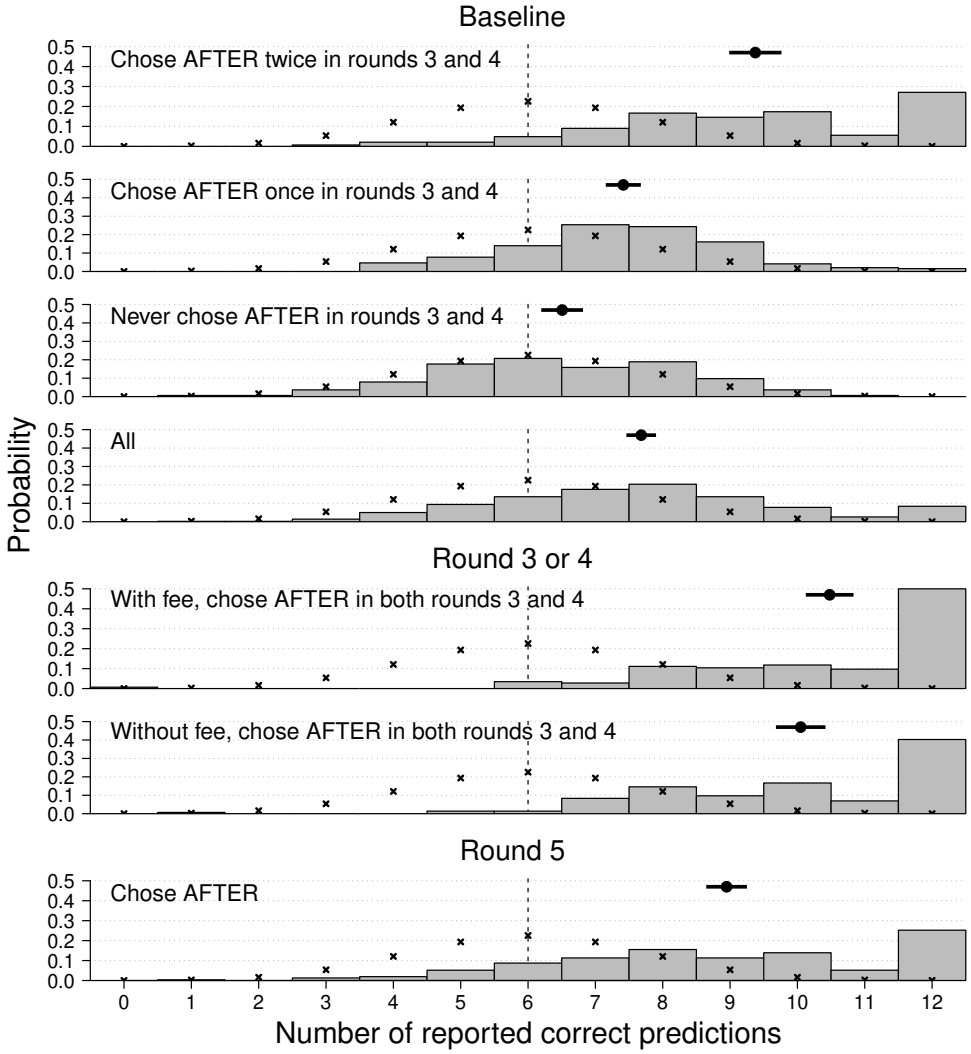

FIGURE 5: The distribution of correct predictions per condition. The figure shows the distributions of correct predictions in comparison to the expected distribution. Observed means and their 95% confidence intervals are also displayed. Observations are grouped according to participants' choices of the two versions of the task.

In the third round of the task, the AFTER version was chosen by 37.5% of participants in the fee condition and 48.6% of participants in the no-fee condition. In the fourth round, the AFTER version was chosen by 38.8% of participants in the fee condition and 66.8% of participants in the no-fee condition. A mixed-effect logistic regression with the choice of the version as the dependent variable, the presence of a fee, round number (both coded using simple contrast coding), their interaction, and the reported number of correct predictions in the baseline measurement[7] as predictors, and random intercepts for participants, showed

[7]The pre-registered analysis separated the analysis of the relationship between baseline measurement of cheating and the choice of the version from the analysis of the effect of the fee. The reported analysis is simpler and yields qualitatively the same results. The results of the pre-registered analysis can be found in supplementary results: https://osf.io/xpbdq/wiki/home/.

that participants reporting a higher number of correct predictions in the baseline measure were more likely to choose the AFTER version of the task (z = 9.86, p < .001, OR = 1.69, CI [1.52, 1.88]). Participants were less likely to select the AFTER version in the presence of a fee (z = −6.68, p < .001, OR = 0.33, 95% CI [0.24, 0.46]), and more likely in the fourth round of the task (z = 3.60, p < .001, OR = 1.76, CI [1.29, 2.39]). The difference between the fee and no-fee condition was larger in the fourth round (z = −2.91, p = .004, OR = 0.37, CI [0.19, 0.72]). When the difference between rounds was analyzed separately for the two conditions, there was no difference between the two rounds for the fee condition (t(498) = 0.21, p = .833, OR = 1.05, CI [0.69, 1.58]), but the AFTER version was more likely to be selected in the fourth round than in the third round in the no-fee condition (t(498) = 4.45, p < .001, OR = 2.43, CI [1.65, 3.60]).

Next, to test the effect of the presence of a fee on cheating, we performed a linear mixed-effect regression with the number of reported correct predictions as the dependent variable and the presence of a fee, round number, and their interaction as predictors for the 144 participants who chose the AFTER version of the task in both third and fourth rounds. The number of reported correct predictions did not differ significantly between the two rounds (t(142.0) = 1.53, p = .129, b = 0.15, CI [−0.04, 0.35]). Participants cheated more in the presence of a fee (t(142.0) = 4.54, p < .001, b = 0.45, CI [0.26, 0.65]), and this effect did not seem to differ based on the round of the task (t(142.0) = 0.56, p = .579, b = 0.36, CI [−0.92, 1.65]). Participants reported, on average, 10.48 correct predictions in the presence of a fee and 10.05 correct predictions in its absence. The average reward in the presence of the fee was 310.5 CZK and 288.2 CZK in its absence. The increased cheating therefore almost fully compensated the 25 CZK fee. Participants who chose the AFTER version only in the round when there was no fee also reported significantly more correct predictions than the expected six. Such participants who had the no-fee condition in the third round cheated less than the participants who had no fee in the fourth round (t(144) = −2.92, p = .004, d = −0.49, CI [−0.83, −0.16], $M_{\text{third no-fee}}$ = 6.83, $M_{\text{fourth no-fee}}$ = 7.80). To test the possibility of escalation of cheating observed in Study 1, we compared the number of reported correct predictions in the third round and in the baseline measure of participants who selected the AFTER version of the task in the third in the no-fee condition, where they could not be influenced by the knowledge of the fee. Unlike in Study 1, they did not cheat more in the third round than in the baseline measure (t(118) = 0.44, p = .662, d = 0.04, CI [−0.14, 0.22], $M_{\text{after third}}$ = 8.49, $M_{\text{after baseline}}$ = 8.41).

To test the effect of the information about the share of participants who chose the AFTER or BEFORE version of the task, we conducted a logistic regression with the choice of the version in the fifth round as the dependent variable and the information condition as a predictor. The model included the number of choices of the AFTER version in third and fourth rounds as a covariate. Contrary to our prediction, participants who received the information that a low proportion of participants had chosen the AFTER version were *more* likely to choose the AFTER version of the task (t(498) = 2.41, p = .016, OR = 1.09, CI

[1.02, 1.17]). The AFTER version of the task was chosen by 66.7% of participants who were informed that a low proportion of participants had chosen the AFTER version, and 56.6% of participants who were informed that a low proportion had chosen the BEFORE version. The number of reported correct predictions did not significantly differ between the participants who chose the AFTER version in the two conditions ($t(307) = -1.55$, $p = .121$, $d = -0.18$, CI [−0.40, 0.05], $M_{\text{low after}} = 8.75$, $M_{\text{low before}} = 9.18$).

Out of the included individual differences measures, honesty-humility, Machiavellianism, and psychopathy predicted both cheating and selection of a version of the task (see Figure 4). Participants higher in honesty-humility cheated less in the baseline measure ($r_S = -.24$, CI [−.33, −.16], $p < .001$), and also chose to have an option to cheat in the third and fourth rounds of the task less frequently ($r_S = -.30$, CI [−.38, −.22], $p < .001$). On the other hand, participants higher in Machiavellianism were more likely to cheat in the baseline ($r_S = .18$, CI [.09, .27], $p < .001$), and chose to have an option to cheat in the third and fourth rounds of the task more frequently ($r_S = .17$, CI [.06, .26], $p < .001$). Similarly, participants higher in Machiavellianism were more likely to cheat in the baseline ($r_S = .12$, CI [.04, .21], $p = .006$), and chose to have an option to cheat in the third and fourth rounds of the task more frequently ($r_S = .11$, CI [.02, .20], $p = .018$). However, neither Machiavellianism nor psychopathy improved the models predicting the number of reported correct predictions or choice of a version using honesty-humility (all ps > .17). On the other hand, all of the models including either Machiavellianism or psychopathy were improved by including honesty-humility (all ps < .001).

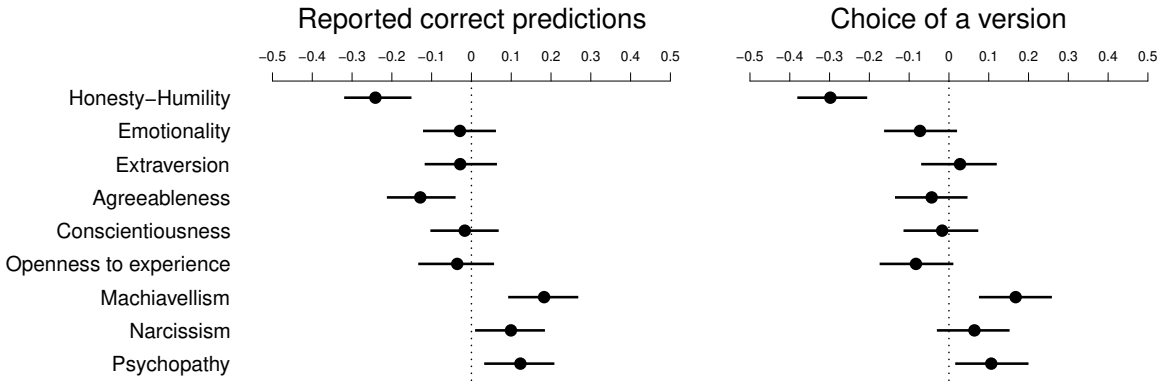

FIGURE 6: Correlations between individual differences measures with baseline cheating and selection of a version of the task in Study 2. The figure shows Spearman correlation coefficients and 95% confidence intervals for their estimates. The choice of a version corresponds to the number of choices of the AFTER version in the third and fourth rounds of the task.

3.3 Discussion

The second study replicated the results of Study 1, showing that participants who selected the cheating-enabling environment also tended to cheat more. The negative association of honesty-humility with the selection of the cheating-enabling environment was also repli-

cated. Moreover, we observed that participants high in Machiavellianism and psychopathy were more likely to choose the cheating-enabling environment. However, the association of these two dark traits with honesty-humility can largely account for the results (Hodson et al., 2018). Our findings thus again confirm that people who select cheating-enabling environments differ from those who do not. Nevertheless, unlike in Study 1, we did not find that participants who chose the cheating-enabling environment cheated more afterward.

A possible explanation for the lack of escalation of cheating is that, in Study 2, participants were not given the opportunity to choose to be assigned the version of the game randomly. Consequently, some of the more honest participants that would choose the random assignment opted for the cheating-enabling environment, but their cheating did not escalate. This explanation is in line with the view that the aggregate level of cheating is driven by a minority of dishonest individuals who cheat to the maximum, rather than by many individuals who cheat a little.

Introduction of a fee for entering the cheating-enabling environment decreased the number of participants who selected this environment; however, participants who chose it despite the fee then cheated more. The implication that increasing costs of entering or staying in a cheating-enabling environment — whether monetary, reputational, or opportunistic ones (such as lost income) — may lead to more intense cheating, has important policy implications. Payments in many occupations with agency problems, such as finance consultant (Mullainathan, Noeth & Schoar, 2012), real estate agent (Levitt & Syverson, 2008), taxi-driver (Balafoutas et al., 2013) or automobile technician (Schneider, 2012), at least partly depend on self-reported performance, which enables dishonest behavior. In order to perform these occupations, individuals have to purchase a license or incur other monetary or nonmonetary costs. Our findings suggest not only that these types of occupations may attract more dishonest individuals but also that the licensing could result in increased cheating. Similarly, comparatively low wages of civil servants, which can be seen as a form of costs, can lead to higher corruption, because only the dishonest willing to take bribes will be attracted to such positions (Van Rijckeghem & Weder, 2001).

The intervention based on social norms that we employed with the goal of reducing the number of participants choosing the cheating-enabling environment did not work as intended. The participants who were informed that only a low proportion of participants had selected the cheating-enabling environment were actually more likely to choose this environment. On the other hand, participants who were informed about the low proportions of those selecting the BEFORE version were more likely to choose it. A possible explanation is that participants thought that with a small number of cheaters, it is justifiable to cheat because not so many resources are going to be misused. This explanation is also supported by the fact that participants themselves estimated a much higher proportion of cheaters (72%). Nevertheless, our data do not allow us to test these conjectures.

We found that participants chose the AFTER version of the game with no fee more often in the fourth round than in the third round. Participants may have viewed the option

of selecting the AFTER version without paying a fee more positively in comparison to the selection associated with a fee in the previous round. In other words, participants who were aware that selection of the AFTER version can be costly perceived the selection in the no-fee treatment as "free" and, therefore, more tempting (Shampanier, Mazar & Ariely, 2007). These results are also in line with findings by Khadjavi (2014), who observed an increase in dishonest behavior when the possibility of punishment was removed in a laboratory stealing game. This explanation suggests that if a cheating-enabling environment suddenly becomes easier to access, people may be more willing to choose it not only because of the lack of the additional costs but also because of the favourable comparison to the previous costly state. This effect could have contributed, for example, to greater demand for illegal downloading of movies after the price reduction of a broadband connection (Danaher & Smith, 2014), and to stimulation of prostitution business after a high efficiency of pairing of sex-workers and their clients on websites like Craigslist became available (Chan, Mojumder & Ghose, 2019).

Because Studies 2 and 3 involved several rounds with multiple trials, a possible concern is that potential cheaters might have been reluctant to report a high number of successes to protect their positive self-image or avoid being perceived as cheaters based on the size of their final reward. To address this potential problem, in Study 3, we conducted an experiment with a single trial. Regarding personality measures, we have included only the HEXACO questionnaire because the association between dark triad and preference for the cheating-enabling environment was largely explained by their association with honesty-humility.

4 Study 3

4.1 Methods

4.1.1 Participants

We ran the experiment with a sample of 1001 English speaking participants (55% female, 44% male, $Mdn_{\text{age}} = 31$, $IQR_{\text{age}} = 17$) recruited through Prolific. The whole experiment was administered in English. The participants were paid £0.42 ($0.55) for their participation. The sample size had a sufficient power $1 - \beta = .80$ for an effect size $d = 0.13$ or $r = .09$. Twenty nine participants failed an attention check and were excluded from analyses including the honesty-humility measure which contained the attention check following a preregistered exclusion criterion.

4.1.2 Design and procedure

The procedure was similar to Study 1 with a few changes. Unlike in Study 1, each round had only one trial and participants predicted outcomes of two coin tosses in the trial rather than a roll of a die. Participants played three rounds of the game and they had been

informed beforehand that one round would be selected at random and they would get an additional monetary reward of £1 ($1.3) if they predicted both coin tosses correctly in the selected round. In the first two rounds, every participant played one round of the game in the AFTER and one in the BEFORE version, in a random order. Then they read short descriptions of both versions of the game. Half of the participants in the control group were randomly assigned one of the versions for the third round. The remaining participants in the experimental group chose whether they wanted to play the last round in the BEFORE or AFTER version.

In the last part of the experiment, participants answered socio-demographic questions and filled questions related to honesty-humility from the HEXACO scale (Ashton & Lee, 2009). One attention check item asking participants to select a specific response was added to the honesty-humility items.

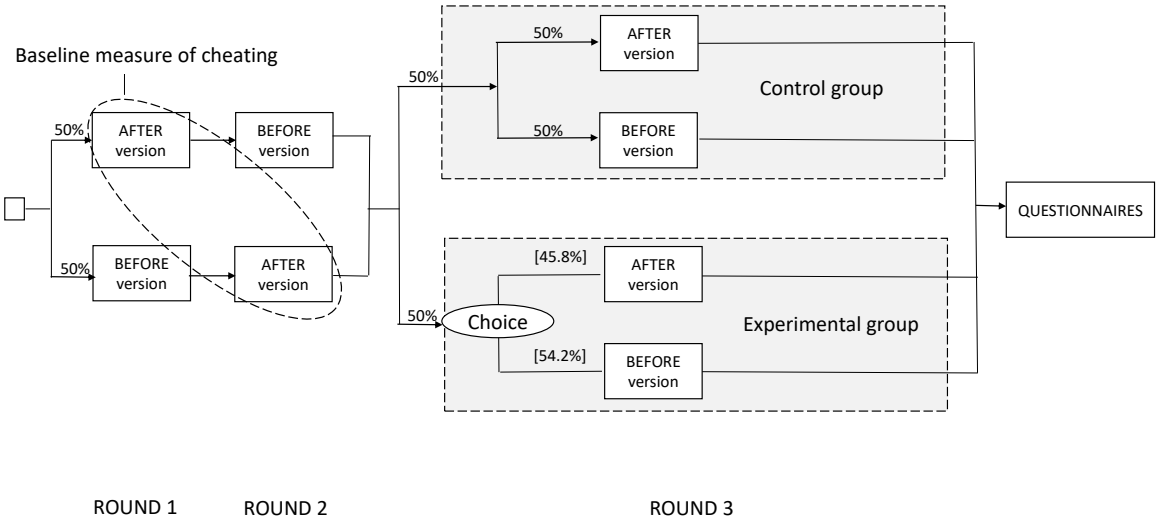

FIGURE 7: In rounds 1 and 2, participants played AFTER and BEFORE versions of the game in a randomized order. The number of reported predictions in the AFTER version of the task served as a baseline measure of cheating. In round 3, participants were randomly selected to a control and experimental group. Participants in the control group were randomly assigned BEFORE or AFTER version of the game. Participants in the experimental group chose whether they wanted to play BEFORE or AFTER version. Finally, participants answered socio-demographic questions and filled questions related to honesty-humility.

4.2 Results

In the baseline measure of cheating, 44.5% of participants reported a correct prediction, which corresponds to the estimate of d = 25.9% of dishonest participants (Moshagen & Hilbig, 2017).

In the third round, 45.8% of participants from the experimental group chose the AFTER version of the task. Participants in the experimental group who chose the AFTER version of the task were more likely to report that they correctly predicted the coin tosses in the

baseline measure of cheating than those who chose the BEFORE version of the task for the third round ($t(540) = 3.07$, $p = .002$, $OR = 1.14$, 95% CI [1.05, 1.24], $OR_d = 2.56$, $d_{\text{chose AFTER}} = 34.4\%$, $d_{\text{chose BEFORE}} = 17.0\%$; see Figure 8).[8]

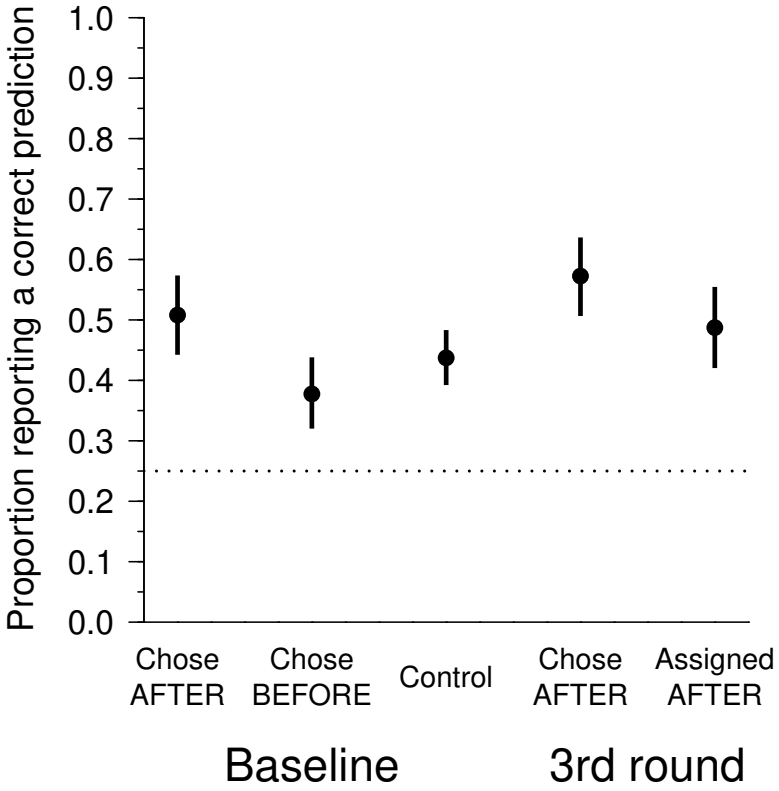

FIGURE 8: The proportion of reported correct predictions per condition, choice, and round in Study 3. The error bars show 95% confidence intervals around the proportions. The dotted line shows the expected proportion of correct predictions under chance performance.

Even though the participants from the experimental group who chose the AFTER version of the task were more likely to report that they predicted the coin tosses correctly in the third round than participants from the control group who were assigned the AFTER version of the task, the effect was not significant ($t(482) = 1.88$, $p = .060$, $OR = 1.09$, CI [1.00, 1.19], ORd = 1.63, $d_{\text{experimental}} = 43.0\%$, $d_{\text{control}} = 31.6\%$). The participants who chose the AFTER version of the task were also not significantly more likely to report a correct prediction in the third round than in the baseline measure of cheating ($z = 1.54$, $p = .124$, $OR = 1.35$, CI [0.92, 1.98], $d_{\text{third round}} = 43.0\%$, $d_{\text{baseline}} = 34.4\%$).

Honesty-humility did not predict selection of the AFTER version of the task in the experimental group ($t(524) = 0.40$, $p = .686$, $OR = 1.06$, CI [0.81, 1.37]). However, participants higher in honesty-humility were less likely to report that they had predicted the

[8]Using RRreg package (Heck & Moshagen, 2018) for the analysis proposed by Moshagen and Hilbig (2017), the procedure with one trial allowed us to estimate the proportion of dishonest participants (d) as well as effects on participants' dishonesty (OR_d) rather than just the probability of reporting a correct prediction and effects on reported correct predictions.

coin tosses correctly in the baseline measure of cheating ($t(970) = -2.84$, $p = .005$, $OR = 0.75$, CI [0.62, 0.91], $OR_d = 0.14$), as well as in the AFTER version of the task in the third round ($t(463) = -4.40$, $p < .001$, $OR = 0.53$, CI [0.40, 0.70], $OR_d = 0.16$).

4.3 Discussion

The results of Study 3 confirmed some of the findings of Studies 1 and 2. In particular, individuals who cheated in the first and second round were more likely to choose the cheating-enabling environment in the third round. This finding once again supports our hypothesis that cheaters self-select themselves into environments that allow cheating. As in the first two studies, honesty-humility was identified as a predictor of cheating. However, unlike in Studies 1 and 2, honesty-humility did not predict the selection of the cheating-enabling environment. It is possible that dishonest participants had a lower motivation to select the AFTER version of the task given that the potential additional reward was smaller. The greater anonymity in Study 3 could have also decreased the association between honesty-humility and selection of the cheating-enabling version of the task.

Although individuals who chose the cheating-enabling environment in the third round cheated more than participants who were assigned this environment randomly, this result was not significant on the conventional level. The one-shot prediction used in Study 3 was likely less sensitive to cheating than the repeated measures used in Studies 1 and 2, which could have led to the difference in results.

The individuals who chose the cheating-enabling environment also did not report significantly more predictions than in the first and second rounds. That is, as in Study 2 and in contrast to Study 1, our results in Study 3 did not show the escalation of cheating. It is again possible that the effect in Study 3 could have been smaller due to the one-shot nature of the game. Another possible explanation is that the escalation of cheating may be driven by the worst fraudsters who substantially increase their cheating rather than by incremental increases in cheating by those who were previously honest. Since in Study 3, unlike in Study 1 and 2, participants had only one trial, there was no room for escalation by increasing cheating. Future research may explore this issue further.

5 General discussion

People choose different situations based on their personality and preferences. In the case of cheating, the moral character of individuals affects the situation selection (Cohen & Morse, 2014). Moral or guilt-prone people are ready to stop behavior that could harm others and even sacrifice financial reward to do so. On the other hand, unscrupulous people seek such situations (Wiltermuth & Cohen, 2014). Accordingly, we found that participants low in honesty-humility tend to prefer cheating-enabling environments, where their rate of cheating can further escalate.

Based on our results, we recommend enriching the experimental methodology by including the possibility of selection of conditions by participants. Experimental designs typically involve measurement of behavior in assigned conditions, and even participants who would not prefer or encounter these conditions in real life are forced to deal with them in an experiment. While the ability to choose one's circumstances may be sometimes limited, inclusion of the possibility of self-selection of participants in different conditions would allow for generalization of experimental findings even in situations where people can select their environment and it would thus improve external validity of experiments.

From a practical perspective, our results show the importance of influencing self-selection of people into companies, departments, and other groups. If individuals motivated only by self-interest perceive public office as an opportunity to enrich themselves, the people with low moral character will seek to become civil servants and politicians. Indeed, studies conducted in India show that people who cheat in a laboratory task are more likely to prefer public sector jobs (Banerjee, Baul & Rosenblat, 2015; Hanna & Wang, 2017). Likewise, Ukrainian law students who cheat and bribe in experimental games are more likely to aspire to careers such as judges, prosecutors, and government lawyers (Gans-Morse, 2019). On the other hand, the self-selection of honest people exists in the Danish public sector (Barfort et al., 2019; for cross-country analysis, see Olsen et al., 2018). Selection of honest people in occupations in which dishonesty may have high societal costs could often be more effective than efforts trying to reduce dishonesty of people who have already chosen them.

The reported studies tested many effects, especially related to moderation of the studied effects by personality characteristics. While some of the tested effects were supported by strong evidence or replicated in a subsequent study, other effects were supported by weaker evidence, or the pattern of results between studies was more ambiguous. We did not control the experiment-wise error rate because we were not primarily interested in whether there is any significant association. However, the number of tested effects means that there is a higher chance that some of them are falsely positive or negative, and the positive results supported by weaker evidence should be interpreted with caution and subject to future replications.

The design used in this article can be further extended in various ways. In particular, it is possible that any determinants of cheating that have been observed in experiments without taking self-selection into account may not influence people who would be actually present in real-world cheating-enabling environments (Houdek, 2017, 2019). Such environments may include only individuals with low levels of honesty-humility personal traits who are prepared to cheat regardless of any intervention. Moreover, if an intervention makes cheating more reprehensible or costly to these individuals, they may simply move to a similar environment without the intervention. The self-selection may eventually negate any positive effects of the intervention on the overall level of cheating (e.g., Nettle, Nott & Bateson, 2012). Future studies may directly test this potential implication of our findings.

Another topic for future research are reasons for self-selection into groups. These

reasons might vary and result in a specific composition of a group, which can further influence behavior of its present or future members. For example, in certain professions (investment banker, salesperson, advertiser), dishonesty or deception could be perceived as a signal of a person's skills, and honest people may therefore avoid these professions. Such adverse selection could eventually lead to persistent dishonesty in these professions (Gunia & Levine, 2016). Yet another possibility for extension is to examine whether selection affects enforcement and punishment. With more cheaters, enforcement and punishment may be more diffused, which may attract additional cheaters in the group (Conley & Wang, 2006). While we have considered a monetary fee associated with the choice of the cheating-enabling environment, another possibility is to include non-monetary costs — such as reputational — of choosing the cheating-enabling environment. Finally, all the cheating behavior in our experiments might have been perceived as basically victimless. Future research may examine self-selection in cases where cheating has identifiable victims.

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