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Lying in Competitive Environments: A Clean Identification of Behavioral Impacts

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Lying in Competitive Environments: A Clean Identification of Behavioral Impacts

Abstract

In the last decade, forced ranking systems where employees' bonuses depend on their rank assigned by superiors have become less popular. Whereas the inherently competitive structure of ranking systems provides high effort incentives, it might also increase incentives for misconduct. Previous literature supports this view by demonstrating that, as compared to individual incentive schemes, highly competitive environments are associated with higher degrees of lying and cheating. However, it is not clear if this is driven by stronger financial incentives arising from the high marginal benefit from winning a competition, and/or the behavioral impacts of competition. Psychologically, a competitive environment alters incentives for misconduct via (i) the negative payoff externality that winning imposes on competitors, and (ii) a desire to win, i.e., succeeding in a competition is valuable per se. We design an experiment that allows us to disentangle financial and psychological incentives for misconduct and decompose the behavioral impacts. Our results provide clean evidence of a significant lying-enhancing desire-to-win-effect and an insignificant lying-reducing negative externality effect.

JEL-Codes: C900, D820, D910.

Keywords: private information, lying, contest, competition, cheating.

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1 INTRODUCTION

Incentive schemes that entail intense competition among employees are among the most controversial topics in managerial practice and academia (e.g., Lazear, 1989; Berger *et al.*, 2013; Croson *et al.*, 2015; Kampkötter and Sliwka, 2018). Many companies such as Microsoft and Yahoo implemented so-called forced ranking schemes advocated by General Electric's Chief Executive Jack Welch in the 1980s. In these schemes, managers are obliged to sort specific percentages of employees into the top and the worst category. While the top 20% are strongly rewarded, the bottom 10% might be dismissed to constantly improve the personnel's average performance (Berger *et al.*, 2013). Similarly, rank-order tournaments, in which rewards are based on relative performance evaluation, create intense competition and are commonplace (Eriksson, 1999; Bognanno, 2001; DeVaro, 2006). The literature has pointed to several desirable properties of relative performance evaluation, in particular, that they set strong incentives for effort due to the discontinuous upwards jump in payoffs associated with a higher rank (Grote, 2005). Complementing the strong material incentives, also behavioral impacts like a positive motivational effect from performance pressure (Eisenberger and Aselage, 2009; Lin *et al.*, 2018) have been proposed.

As a downside, however, critics argue that competitive remuneration leads to higher misconduct via means of cheating and sabotage (Schweitzer *et al.*, 2004; Welsh and Ordóñez, 2014). Anecdotal evidence documents that, under competitive incentives, employees cut corners to meet their targets and get promotions (Gellene, 1992; Greenwald, 2001; Zoltners *et al.*, 2016), allocate their time to window-dressing instead of productive activities (Mitchell *et al.*, 2018; Corgnet *et al.*, 2019), or even commit outright fraud (Association of Certified Fraud Examiners, 2020). Likewise, experimental findings document that competition is associated with a higher degree of misconduct compared to individual payment schemes (Carpenter *et al.*, 2010; Faravelli *et al.*, 2015; Benistant *et al.*, 2021).

While the phenomenon itself is well documented, it is unclear to which degree it can be attributed to the behavioral effects of competition rather than to the pure impact of higher-powered financial incentives. Surprisingly, to the best of our knowledge, no contribution decouples the effects of monetary incentives from behavioral motives when comparing the frequency (and the extent) of misconduct between individual and competitive incentive schemes. To see why decomposing the behavioral impacts of competition from financial incentives is essential from an applied perspective, suppose a company substitutes a competitive with a non-competitive remuneration scheme in cases where misconduct was simply motivated by high financial incentives. Then, the company might sacrifice the merits of competitive schemes (strong incentives to exert effort, filtering out noise, etc.) without reducing the degree of misconduct. This does not seem to be far-fetched, given that misconduct is also observed in companies employing indi-

vidual incentive schemes. Already in the nineties, companies, including Sears, Roebuck & Co, and GTE, reduced output-dependent bonuses for their salespeople (Ordóñez *et al.*, 2009), and high-powered bonus schemes in the financial industry have not only contributed to excessive risk-taking but also fraudulent behavior (Erickson *et al.*, 2006; Johnson *et al.*, 2009; Haß *et al.*, 2015).

We aim to fill this gap by designing an experiment based on the view that competitive reward schemes differ from those based on individual performance mainly in three dimensions. First, they set different monetary incentives due to the large payoff discontinuity arising from a high winner and a low loser prize and the interdependency with other employees' behavior. Second, lying or cheating in a contest or a forced ranking system inevitably reduces the payoff of other participants due to the inherent zero-sum character of these schemes. Assuming that most (not necessarily all) people have pro-social rather than anti-social preferences and hence put positive weight on the payoff of co-workers, this zero-sum character reduces, ceteris paribus, the willingness to lie in competitive settings. Third, the contest structure might trigger a "desire-to-win" or an aversion to being outperformed. Either way, the resulting additional value from winning fosters the susceptibility to misconduct.

Conducting an online experiment in the spirit of the die-under-the-cup paradigm introduced by Fischbacher and Föllmi-Heusi (2013), we compare the frequencies of misconduct in three treatments. Subjects in all three treatments take part in a binary lottery with the outcomes LOW and HIGH, observe the lottery outcome privately, and then report the outcome. In all treatments, reporting HIGH leads to a higher expected payoff such that subjects have an incentive to misreport a privately observed LOW outcome as HIGH. In our contest treatment *C*, two subjects compete in a simultaneous winner-take-it-all contest. The one who reports HIGH (LOW) receives the winner price (the loser price). Both subjects receive the winner price with a fifty percent probability if they submit the same report. We then use data from treatment *C* to design an individual treatment *I*, in which the expected monetary benefit from lying is the same as in treatment *C*, but the competitive nature is removed. This is achieved by implementing the same prize structure and resembling the opponent's behavior by a random variable. This procedure ensures that the expected increase in the own payoff from lying in *I* is the same as in *C*. Strikingly, we do not find evidence for higher misconduct under the competitive remuneration scheme: The frequency of high reports is only slightly higher in *C* (57.4%) than in *I* (53.9%), and the difference is not statistically significant. Accordingly, our results cast doubt on the seemingly common wisdom that competition per se causes a more substantial susceptibility to misconduct. Instead, our results suggest that ranking systems and bonuses may have similar impacts on misconduct when the financial benefits from cheating are kept constant.

Treatment *I* is designed to resemble a reward scheme based on individual performance with identical financial incentives as in treatment *C*. However, while the impact of cheating on the

own payoff is the same in treatments *I* and *C*, cheating in treatment *I* influences only the own payoff, whereas it additionally reduces the competitor's (expected) payoff in treatment *C*. To decompose this negative externality effect from the desire-to-win effect inherent to treatment *C*, we conduct the negative externality treatment *N*. This treatment is identical to treatment *I*, except that each active subject is matched with a passive subject ("a bystander"). The passive subject receives the high payment if and only if the active subject receives the low payment. All financial effects in treatment *N* are hence identical to treatment *C*, as inflating the outcome of the lottery yields the same own expected financial benefit and the same expected financial loss for someone else. The only difference remaining is that there is no competition, so that comparing treatments *C* and *N* allows us to identify the desire-to-win effect. In treatment *N*, only 49.6% of all subjects report HIGH, which is economically meaningful and significantly lower than the frequency of 57.4% in treatment *C*. Our results provide evidence that, ceteris paribus, the desire to win inherent to competitive environments fosters misconduct.

Finally, the comparison of treatments *I* and *N* allows for carving out the negative externality effect: Both treatments have identical monetary incentives and differ only in that lying yields a negative externality on the bystander in *N*. Comparing the frequency of high reports in treatments *I* and *N* shows that the negative externality causes a moderate yet statistically insignificant reduction in cheating. It is, however, strong enough to offset the positive desire-to-win, thereby rendering the overall difference between *C* and *I* statistically insignificant. This provides further support for our view that identifying the desire-to-win effect requires that *all* financial impacts of lying are the same in individual and competitive payment schemes.

Obviously, lying and cheating deviate from socially appropriate behavior. To shed light if our results are driven by norm compliance, we conducted an additional *Norms* treatment. This treatment allows us to test whether (i) a general social norm of behaving honestly exists and (ii) the social inappropriateness of lying is affected by the desire to win and the negative externality. We adopted the measure introduced by Krupka and Weber (2013) to elicit the social norm of lying. The data corroborates that lying, compared to reporting a low outcome truthfully, is considered much less appropriate. The difference in appropriateness ratings between misreporting a low outcome as high and reporting truthfully is very similar and not significantly different across treatments. This indicates that the increase in lying caused by the desire-to-win effect cannot be explained by a shift in the social appropriateness of lying.

The remainder of the paper is organized as follows: Section 2 relates to the literature. We present a simple model in section 3. Section 4 describes the experimental design, procedures, and our hypotheses. Results are shown in section 5. We discuss and conclude in section 6.

2 Related Literature

Our paper is most closely related to experiments comparing misconduct in competitive and non-competitive treatments. The earlier literature considers real-effort tasks (see the overview by Chowdhury and Gürtler, 2015). Schwieren and Weichselbaumer (2010) use the maze game introduced by Gneezy *et al.* (2003), Belot and Schröder (2013) let subjects identify euro coins, and Faravelli *et al.* (2015) use the matrix task developed by Mazar *et al.* (2008). Schwieren and Weichselbaumer (2010) compare the individual piece rate treatment to a contest of six subjects, in which only the one who reports the highest number of solved mazes is paid. Overall, they do not find a significant difference between the cheating behavior in the two treatments, but low-performing subjects lie significantly more in the contest than with piece rates. Belot and Schröder (2013) compare piece rates to a four-player contest. The contest winner receives a price of 50 euro, whereas the other three contestants get nothing. They find that both the productive effort and the lying frequency are significantly higher in the contest. Faravelli *et al.* (2015) compare piece rates to a two-player contest. Cheating is more frequent in the contest, but this effect disappears when subjects can self-select to the piece rate or the contest treatment.

Most of the experiments just discussed suggest that competitive remuneration systems lead to more misconduct than simple bonus schemes. In contrast to our experiment, however, the financial benefits from misconduct differ between the contest and the piece rate settings. With piece rates, the marginal financial benefit of misconduct is constant and independent of the behavior of all other subjects in the experiment. Conversely, the marginal benefit from misconduct in a contest depends on the number and the behavior of other contestants. These differences in the incentive structures are likely to contribute to the different findings in the literature: In Schwieren and Weichselbaumer (2010), the marginal benefit from cheating in the contest might be perceived as rather low because just one out of six contestants are paid. The fact that Belot and Schröder (2013) find more cheating in the competitive environment might hence be due to the lower number of contestants and the large winner prize of 50 euros. Faravelli *et al.* (2015) consider only two contestants. The contest also entails a piece rate component, as the winner gets \$2 per correctly solved matrix, compared to \$1 in the individual piece rate setting. The main difference to our comparison of treatments C and I is hence that the marginal expected financial benefit from cheating differs between treatments.[1]

Most of the recent literature builds on the die-under-the-cup paradigm introduced by Fischbacher and Föllmi-Heusi (2013), which we adopt as well. Subjects roll a die in private, and the payoff structure is designed to induce a strong financial incentive to misreport the outcome.

[1] In Faravelli *et al.* (2015), the payment per correctly solved maze is, on average, \$1 both in the contest and with piece rates. Marginal financial incentives to cheat, however, are quite different, as those depend in the contest on (i) the own performance, (ii) the own willingness to cheat, and (iii) the expectation on the other contestant's report.

As lying is unobservable, it needs to be studied at an aggregated level.[2] Several recent papers utilize lotteries in the spirit of Fischbacher and Föllmi-Heusi (2013) to compare two-player contests. The advantage of the lottery setting compared to real effort tasks is that the degree of misconduct cannot be influenced by the subjects' abilities and effort costs. Dato *et al.* (2019) consider a sequential contest with and without lying possibility for the first subject. They find no significant treatment effect on the second subject's lying behavior. The same holds in Dannenberg and Khachatryan (2020), who compare simultaneous contests, in which either both or just one subject can lie. Benistant *et al.* (2021) find that the lying frequency in a contest is significantly larger than with piece rates if and only if both contestants can lie. The latter two papers derive a rich set of results,[3] but the marginal benefit from lying again differs between contests and piece rates. In Dannenberg and Khachatryan (2020), the results entered by passive subjects are systematically below those of subjects who can lie[4], which changes the incentive structure of the contestant who can lie. In addition, a subject who rolls a die without the possibility to lie may be seen as a competitor. The latter argument also refers to Dato *et al.* (2019), who keep the marginal financial benefits from lying identically across all contest treatments.

Charness *et al.* (2014) consider a dynamic real-effort rank-order tournament with flat wages so that all treatments are identical with regards to the financial incentives to cheat. They find that informing subjects about their ranks increases their effort, which reinforces the view that ranking systems may be beneficial in this respect.[5] Furthermore, subjects who are informed about their rank engage in cheating and sabotage. Our identification strategy of the behavioral impacts of competition on misconduct differs in many important respects: First, Charness *et al.* (2014) do not compare the cheating behavior with information on ranks to a treatment without information, so that it cannot be excluded that subjects would have cheated even without information on ranks due to, e.g., self-image concerns or to reduce their anger about a task they disliked. Interpreting ranks as competition, there is hence no comparison of our treatment *C* to another treatment.[6] Second, while flat wages ensure that differences in treatments are not driven by different financial incentives, we are interested in comparing bonus contracts to competitive

[2]Dai *et al.* (2018) document that the behavior in the die-under-the cup paradigm provides a good predictor of cheating in the field. For a meta-study on this paradigm with non-strategic set-ups, see Abeler *et al.* (2019).

[3]Dannenberg and Khachatryan (2020) compare individual to group contests, and Benistant *et al.* (2021) focus on the impact of feedback and incentives on the lying behavior in dynamic settings. Also, considering a dynamic framework, Necker and Paetzel (2020) find that the lying frequency of strong performers in a real-effort task increases when they learn that they are matched with other strong performers.

[4]The reported outcomes could only be identical if no one lies.

[5]Gill *et al.* (2019) extend the analysis to a multi-period setting. They find that providing information about the rank has the highest positive effect on effort for subjects at the top and the bottom of the ranking.

[6]However, in individual settings without competition, Charness *et al.* (2019) find no evidence of cheating in a die-roll task if reports have no impact on payoffs.

remuneration schemes, which would be impossible with flat wages. Third, we compare three treatments to tease out the impact of the negative externality implied by competition.

Benistant and Villeval (2019) analyze a two-player simultaneous real-effort tournament. The lying behavior is neither affected by group identity nor by whether lying increases the own or decreases the opponent's final score. Several papers find that lying is likely to be reinforcing, as subjects who underestimate (overestimate) the lying frequency lie more (less) when they are informed about the actual numbers (Le Maux *et al.*, 2021; Bäker and Mechtel, 2019; Casal *et al.*, 2017; Diekmann *et al.*, 2015).[7] In addition to lying about the own outcome, the literature also considers the possibility of sabotaging the competitors' outcomes. In the seminal paper by Carpenter *et al.* (2010), sabotage occurs more frequently in contests.[8] Harbring and Irlenbusch (2011) and Conrads *et al.* (2014) find that sabotage and lying, respectively, increase in the prize spread.[9] These findings reinforce our view that the monetary incentives need to be kept constant to identify the behavioral impacts of competition.

While we introduce a second player to identify the impact of competition, other papers introduce a second player to determine the effects of groups. Conrads *et al.* (2014) compare an individual piece-rate treatment to a treatment where the two members of a group decide independently on their report and share their payoff equally. Lying is more frequent in the group treatment. A comparable result is found in Danilov *et al.* (2013) in an experiment with professionals from the financial services sector, provided that group identity is prominent. Kocher *et al.* (2018) find more lying in groups, and Dannenberg and Khachatryan (2020) show that the group effect is more pronounced in competitive settings.

Summing up, while there is a large body of literature that compares cheating and lying in treatments with and without competition, we are not aware of any other paper that keeps both the expected marginal financial benefit from misconduct and the impact on others constant across treatments.

3 The Model

To derive the utility-maximizing lying frequencies under competitive and individual incentive schemes, and to disentangle the impact of a *desire-to-win* and the *negative externality* in competition, we analyze the following simple model.

Player i takes part in a lottery, which yields a high outcome $x_i = h$ with probability p_i and

[7] Feltovich (2019) frames the decision situation as markets and compares lying in monopolies and different kinds of duopolies. While the marginal financial benefit is highest in the monopoly treatment, the lying frequencies in the duopoly tend to be rather higher than lower. This also suggests a behavioral impact of competition.

[8] As in the papers just discussed, the expected marginal financial benefit from the misconduct differs among treatments.

[9] Dato and Nieken (2014) find that sabotage frequencies of men exceed those of women.

a low outcome $x_i = l$ with $1 - p_i$. Player i privately observes x_i and then reports $r_i \in \{l, h\}$. Misreporting the actual outcome by reporting $r_i \neq x_i$ yields (internal) lying costs of c. The report influences player i's monetary payoff, which is either high, w_H, or low, w_L. Player i derives material utility from money according to an increasing function $u(w)$ with $u(w_L) = u_L < u_H = u(w_H)$. We consider three settings. In all settings, player i's probability of receiving w_H instead of w_L increases by 50 percentage points when reporting h instead of l.

Two players $i = 1, 2$ compete with each other in the *Contest* setting C. Both players privately observe the realization of their (independent) lotteries and report the outcome. If only one player reports the high outcome, she receives w_H, while the other player receives w_L. If both reports are identical, a random draw determines who of the two players obtains w_H and w_L, respectively.

Next to the additional material utility from winning denoted by $\Delta u = u_H - u_L$ and lying costs c, player i's objective function is affected by the following two motives. First, winning the contest provides an additional non-monetary utility $\hat{u} > 0$ that can be interpreted as a "desire-to-win" or "competitiveness". Results from experimental economics (Brookins and Ryvkin, 2014; Sheremeta, 2010; Cooper and Fang, 2008) as well as neuroeconomics (Dohmen *et al.*, 2011; Delgado *et al.*, 2008) provide evidence that non-monetary motives shape the evaluation of a competition's outcome.

Second, recall that the competitor receives w_L in case player i wins the contest and receives w_H. Therefore, by reporting h instead of l, player i reduces the utility of the competing player j in two respects: First, she imposes a negative externality on the other player's expected monetary payoff. Second, she reduces the probability that player j may enjoy her non-monetary utility $\hat{u}$ from winning the contest. Following a meanwhile established literature, we assume that player i has social preferences and puts relative weight $\phi \in (0, 1)$ on the other player's utility (e.g. Fehr and Fischbacher 2002).

Note that, after observing the high outcome, a player will always report h. This directly follows from positive lying costs and the weaker regard for the opponent than for herself. We thus restrict attention to the situation where player i has drawn a low outcome, $x_i = l$. Suppose the other player j submits $r_j = l$ with probability π, then player i's utility of truthfully reporting the low outcome is given by

$$\begin{aligned} U_i^C(l) &= \pi \left\{ \frac{1}{2} \left[u_L + \phi \left(u_H + \hat{u} \right) \right] + \frac{1}{2} \left(u_H + \hat{u} + \phi u_L \right) \right\} + (1 - \pi) \left[u_L + \phi \left(u_H + \hat{u} \right) \right] \\ &= u_L + \frac{\pi}{2} \left(\Delta u + \hat{u} \right) + \phi \left[u_L + \frac{1}{2} \left(1 + \pi \right) \left(\Delta u + \hat{u} \right) \right], \end{aligned}$$

whereas misreporting the low outcome as high yields

$$U_i^C(h) = \pi\left(u_H + \hat{u} + \phi u_L\right) + (1-\pi)\left\{\frac{1}{2}\left[u_L + \phi\left(u_H + \hat{u}\right)\right] + \frac{1}{2}\left(u_H + \hat{u} + \phi u_L\right)\right\}$$
$$= u_L + \frac{1}{2}(1+\pi)\left(\Delta u + \hat{u}\right) + \phi\left[u_L + \frac{\pi}{2}\left(\Delta u + \hat{u}\right)\right] - c.$$

Comparing the expected utilities shows that player i lies if and only if

$$c < (1-\phi)\frac{\Delta u + \hat{u}}{2} \equiv \tilde{c}_C.$$

In the *Negative Externality* setting *N*, player i takes part in the same lottery as in *C*, privately observes the realization, and reports the outcome. Conversely to setting *C*, however, her payoff does not depend on the action of another player: instead, the probability to obtain w_H is determined by player i's report and two random draws. With probability q, a low report $r_i = l$ leads to a $50/50$-lottery between w_L and w_H, whereas the high report $r_i = h$ yields w_H with certainty. With probability $1-q$, $r_i = l$ yields w_L with certainty, while $r_i = h$ results in the $50/50$-lottery between w_L and w_H. Setting *N* eliminates the competitive nature of setting *C* so that "a desire to win" does not affect player i's report. The negative externality inherent to competition, however, is maintained: there is a passive individual who receives the low (high) monetary payoff if player i receives the high (low) monetary payoff. Therefore, social preferences still influence player i's report. The utility of truthfully reporting the low outcome is then

$$U_i^N(l) = u_L + \frac{q}{2}\Delta u + \phi\left(u_L + \frac{1}{2}(1+q)\Delta u\right),$$

while misreporting the low outcome as high yields

$$U_i^N(h) = u_L + \frac{1}{2}(1+q)\Delta u + \phi\left(u_L + \frac{q}{2}\Delta u\right) - c.$$

Comparing the expected utilities shows that player i lies in setting *N* if and only if

$$c < (1-\phi)\frac{\Delta u}{2} \equiv \tilde{c}_N.$$

The *Individual* setting *I* is identical to *N* except that there is no passive individual whose payoff depends on the player's decision. As social preferences are muted, truthfully reporting the low outcome yields expected utility

$$U_i^I(l) = u_L + \frac{q}{2}\Delta u,$$

while misreporting the low outcome as high yields

$$U_i^I(h) = u_L + \frac{1}{2}(1+q)\Delta u - c.$$

Player i hence lies if and only if

$$c < \frac{\Delta u}{2} \equiv \tilde{c}_I.$$

Comparing the thresholds for player i's lying decision in the three settings yields the following proposition.

Proposition 1. (i) *The thresholds $\widetilde{c}$ for lying costs c such that player i lies if and only if $c < \widetilde{c}$ are larger in settings I and C compared to setting N, $\tilde{c}_C, \tilde{c}_I > \tilde{c}_N$.* (ii) *The threshold is higher in setting C than in setting I if and only if $\phi < \frac{\hat{u}}{\hat{u}+u\Delta}$.*

Proof. *Part (i).* $\tilde{c}_I - \tilde{c}_N = \frac{1}{2}u\Delta\phi > 0$. $\tilde{c}_C - \tilde{c}_N = \frac{1}{2}\hat{u}\,(1-\phi) > 0$. *Part (ii).* $\tilde{c}_C - \tilde{c}_I = \frac{1}{2}\left(\hat{u}\,(1-\phi) - u\Delta\phi\right)$ decreases in ϕ, $\frac{\partial(\tilde{c}_C - \tilde{c}_I)}{\partial\phi} = -\frac{(\hat{u}+u\Delta)}{2}$. For the minimum $\phi = 0$, $\tilde{c}_C^0 - \tilde{c}_I = \frac{\hat{u}}{2} > 0$. For the maximum $\phi = 1$, $\tilde{c}_C^1 - \tilde{c}_I = -\frac{u\Delta}{2} < 0$. Solving $\tilde{c}_C - \tilde{c}_I = \frac{1}{2}\left(\hat{u}\,(1-\phi) - u\Delta\phi\right) = 0$ for ϕ yields $\phi = \frac{\hat{u}}{\hat{u}+u\Delta}$. ■

The intuition for Proposition 1 is as follows: In all settings, misreporting the low outcome as high increases the probability to obtain the high monetary payoff by 50 percentage points. The expected financial (or material) benefits from lying are thus identical across settings. The only difference of setting *N* to setting *I* is that player i's decision is also affected by her social preferences towards the passive player j. This reduces her incentive to lie; thus $\tilde{c}_I > \tilde{c}_N$. Next, the only difference between setting *C* to setting *N* is that there is also a utility from winning the contest. As player i puts higher weight on her own than on player *N*'s utility, this increases the threshold; hence $\tilde{c}_C > \tilde{c}_N$ (*Part (i)* of the Proposition). *Part (ii)* of the Proposition shows that it depends on the importance of social preferences whether the incentives to lie are larger in settings *C* or *I*. If social preferences ϕ are sufficiently large, $\phi > \frac{\hat{u}}{\hat{u}+u\Delta}$, then the threshold $\tilde{c}$ (and hence the lying frequency) is lower in the contest. But if ϕ is low, then the "desire-to-win" dominates the negative externality effect.

4 Experimental Design and Hypotheses

4.1 Overview and task

We designed three main treatments to compare misconduct under competitive and individual incentive schemes and to disentangle the negative externality effect and the desire-to-win effect under competition. In all treatments, we employed the die-under-the-cup paradigm introduced by Fischbacher and Föllmi-Heusi (2013). This task has several desirable properties. First, it is simple and easy for the subjects to understand and execute. Second, potentially confounding impacts of ability and effort costs are ruled out as the outcome solely depends on luck. Third, misconduct is not observable, neither by the experimenter nor by other subjects. As it is not possible to detect individual lying, we need to analyze behavior on an aggregated level.

We asked subjects to roll a die in private and to report their outcome. If the result of the die roll was between 1 and 4 (5 or 6), subjects should report LOW (HIGH) as an outcome. Subjects knew that their die roll could neither be observed by other subjects nor by the experimenters. They had discretion about their report and could deviate from the true result. In our experiment, misconduct, therefore, took the form of lying about the privately observed outcome of a

lottery. In all main treatments, subjects received a fixed compensation of 1.40 GBP for their participation and an additional bonus of either 0.20 GBP or 1.20 GBP.

4.2 Treatments

Our treatments were as follows:[10] Subjects in treatment *C* participated in a simultaneous two-player contest and were randomly matched with another subject. If both subjects reported the same outcome, each of them received the high bonus (winner price) of 1.20 GBP with a probability of 50%. Otherwise, the one who reported HIGH (LOW) got the winner prize of 1.20 GBP (the loser prize of 0.20 GBP) with certainty. The impact of the paired subjects' reports on the contest prizes are summarized in Table 1. Notably, the expected financial benefit of lying in case of a low outcome was, regardless of the other contestant's report, always $0.5\,(1.20 - 0.20) \;=\; 0.5$ GBP. All subjects knew that, after the experiment, they would be informed about the report of their competitor and the resulting payment.

Report of the other participant	Your report	Bonus
Low	Low	Each of you has a 50% chance of getting the 1.20 GBP. This is decided by a random draw.
	High	You: 1.20 GBP Other: 0.20 GBP
High	Low	You: 0.20 GBP Other: 1.20 GBP
	High	Each of you has a 50% chance of getting the 1.20 GBP. This is decided by a random draw.

Table 1: Overview of bonus payment in treatment *C*

In treatment *I*, there was no competition, and the report did not affect the payment of other subjects. Similar to the original set-up of Fischbacher and Föllmi-Heusi (2013), a subject's report solely determined the own expected bonus. Holding the marginal financial benefits from lying constant across treatments was achieved as follows: We executed one session of treatment *C* with 100 subjects. 45 subjects reported LOW.[11] We used this information in treatment *I* by informing subjects that their report determined their expected bonus and that there were two possible cases. With 45% probability, subjects were in case 1 where they received 0.20 GBP or 1.20 GBP with 50% probability each when reporting LOW, and 1.20 GBP for sure when

[10] The instructions for all treatments can be found in the appendix.

[11] The remaining observations for treatment *C* have been collected together with the two other treatments in a randomized within-session format. With 56.6% high reports over all sessions in treatment *C*, the first session predicted average behavior very well.

reporting HIGH. Note that case 1 mirrored the structure in treatment *C* when the other contestant reported LOW. With the counter-probability of 55%, subjects were in case 2 and receive 0.20 GBP with certainty with a LOW report, and 0.20 GBP or 1.20 GBP with 50% probability each with a HIGH report. Case 2 thus mirrored treatment *C* when the other contestant reported HIGH. The impact of the report and the random draws are summarized in Table 2. The probabilities of 45% for case 1 and 55% for case 2 reflected the actual behavior of subjects in the contest so that the financial structure in treatment *I* indeed resembled the contest structure.[12] The subjects received no information about the origin of the probabilities for each case. They were informed about the probabilities for the two cases but did not learn which case they were actually in before submitting their report.

Case	Your report	Bonus
1 45% probability	Low	You have a 50% chance of getting the 1.20 GBP. This is decided by a random draw.
	High	1.20 GBP
2 55% probability	Low	0.20 GBP
	High	You have a 50% chance of getting the 1.20 GBP. This is decided by a random draw.

Table 2: Overview of bonus payment in treatment *I*

Distinguishing between cases 1 and 2 may seem unnecessarily convoluted as the marginal benefit of reporting HIGH instead of LOW was 0.5 GBP in both situations. However, it might have made a difference for some subjects whether the probability of receiving the higher bonus increased from zero to 0.5 or from 0.5 to 1.0. The main reason is that subjects might be inequity averse concerning the expected outcome:[13] If individuals assumed that the other contestant reported HIGH, then reporting HIGH as well might have been perceived as less morally questionable as it "levels the playing field" by assigning a symmetric winning probability of 50% to both contestants. To avoid potential confounding effects from inequity aversion, we mirrored the probabilities from treatment *C* in the other treatments. After the experiment, subjects were informed about the case they had been in and the resulting payment.

[12] This does not necessarily imply that the *perceived* financial incentives of all subjects in treatments *I* and *C* were identical. Whereas all subjects in treatment *I* knew the real financial incentives, subjects in treatment *C* had to form subjective beliefs about the contestant's report.

[13] Straightforwardly, the inequity concerning the actual division of money is always the same as one subject got the winner and one the loser price.

Our third treatment *N* was identical to treatment *I* with one exception: Subjects were informed that they were matched with passive subjects ("bystanders") who received the high bonus if and only if they received the low bonus and vice versa. The impact of the report and the random draws on the subjects' bonus payments are summarized in Table 3. Subjects in treatment *N* did not act in a competitive environment, but the impact of their report on other subjects' bonus payments resembled treatment *C*. As in treatment *I*, all subjects were informed about the case and the resulting payment after the experiment. They also knew that the "bystander" would receive the same information.

Case	Your report	Bonus
1 45% probability	Low	Each of you has a 50% chance of getting the 1.20 GBP. This is decided by a random draw.
	High	You: 1.20 GBP Other: 0.20 GBP
2 55% probability	Low	You: 0.20 GBP Other: 1.20 GBP
	High	Each of you has a 50% chance of getting the 1.20 GBP. This is decided by a random draw.

Table 3: Overview of bonus payment in treatment *N*

In each treatment, subjects had to answer four control questions before rolling a die and reporting an outcome. Each control question addressed one of the possible cases. For each case, subjects were asked for the probability of receiving the high bonus. If subjects failed to give the correct answer, they could try a second time again before seeing the correct answer.

After submitting their report, subjects in treatments *C*, *I*, and *N* were asked for their belief about the behavior of other subjects in their treatment. Our question read, "What do you think about the behavior of the other participants in this study. Out of all participants (except you) whose actual results of the die roll was LOW (outcome 1 to 4), how many will report HIGH?" Beliefs were stated on a scale from zero to 100%.[14] In addition, we used the Honesty-Humility subscale from the HEXACO to measure fairness, sincerity, and greed avoidance (Ashton and Lee, 2009),[15] and measured positive and negative reciprocity following Dohmen *et al.* (2009). Finally, we asked for sex, age, country of residence, education level, and the number of studies they participated in on the online platform during the last 12 months. We also included an

[14]For various reasons, we chose against incentivizing the elicitation of beliefs. As we did not observe the actual distribution of results, we would have to use the theoretically predicted distribution to calculate an approximation of actual lying behavior. It is even more critical to keep financial incentives across treatments constant and avoid possible confounds arising from, e.g., treatment differences in the degree of lying estimation complexity.

[15]We used a seven-point scale instead of the original five point-scale.

attention check into the Honesty-Humility survey stating "it is important that you pay attention to this study. Please tick "disagree."

We ran the sessions with the passive subjects (the bystanders) in treatment *N* after having collected the reports from the active subjects. We refer to the data collected from bystanders as treatment *B*. As the bystanders did not have to make any payoff-relevant decision, we elicited their belief about the misconduct in one of the three main treatments *C*, *I*, and *N*. Each bystander received the instructions of either treatment *C*, *I*, or *N*, and was asked to state the belief about the frequency of lying.[16] After stating the belief, all bystanders learned how their bonus was calculated, i.e., they were informed about the procedures from treatment *N* and their role as passive bystanders.

To assess whether (i) a social norm of behaving honestly, i.e., not misreporting the own outcome exists and (ii) differences in normative evaluations across treatments might help to explain differences in unethical behavior, we collected data about the social appropriateness of behavior in an additional treatment *Norms* .

In *Norms*, we closely followed the approach of Krupka and Weber (2013). Subjects are given the instructions of one of our three main treatments and asked to rate the social appropriateness of a) reporting LOW and b) reporting HIGH if the actual outcome of the die roll was LOW. Each report had to be rated as *very socially inappropriate*, *socially inappropriate*, *somewhat socially inappropriate*, *somewhat socially appropriate*, *socially appropriate*, or *very socially appropriate*. Each subject was then randomly paired with another subject, who rated the reports from the same main treatment. One of the two possible reports of subjects in the main treatment was randomly drawn for each pair, and the pair's ratings were compared. If the ratings matched, both received a bonus of 2.50 GBP and zero otherwise. After submitting their ratings, all subjects filled out the same survey as in the main treatments (except for the belief question).[17]

4.3 Sample and procedures

We preregistered our study in the AEA RCT Registry, and the digital object identifier (DOI) is: "10.1259/rct.6824-1.0." We executed our experiments online on the Prolific platform for several reasons. First, we needed a large sample size, as lying is unobservable, and our dependent variable is the share of high reports. Second, subjects needed to be sure that their actual outcome was unobservable. Whereas this is straightforward online, even with clear-cut instructions it might be doubted by some subjects in a classical lab situation. Third, we preferred to collect a sample with subjects differing across age, education, and sex to increase the generalizability of our results.

[16] They were asked precisely the same question as the active subjects in the respective treatment.

[17] Please see the appendix for a brief data analysis of the *Norms* treatment.

Prolific is a large online platform where people can participate in research and business studies. We announced a scientific study and a survey on individual decision-making. To ensure high data quality, we required subjects to be fluent in English, to reside in either UK or USA, be at least 18 years old, and to have an approval of at least 95%. All subjects were allowed to participate just once. We implemented measures to prevent restarting of the survey and self-selection into treatments. We informed the subjects that the study took about fifteen minutes and involved filling out a short survey and rolling a die. Subjects also knew the two possible bonus payment levels. If subjects were interested in participating, they followed a link taking them to the first page of our study (hosted on Qualtrics). This first page was a consent form and only subjects giving their consent entered the study. To avoid that they needed to wait for each other in treatment *C*, we did not play the contest in a live interaction. Instead, subjects in all treatments were informed about the experiment's outcome within two days after participating.

1,509 subjects participated in our study in total. We aimed for 300 subjects in each treatment.[18] Due to technical issues with the randomization within sessions, we collected 292 observations for the treatments *N* and *I*, and 318 for treatment *C*. The treatment *B* has 303 and the treatment *Norms* 304 observations. The data was collected between December 9 and 22, 2020.[19]

We excluded two subjects who did not pass the attention check from the analysis. Recall that subjects had two attempts to answer the control questions in the three main treatments. Overall, 89.22% of all subjects answered all four questions correctly at least after the second attempt and 70% even after the first attempt. Given that we provided a table with the corresponding payments and that subjects had, for each control question, only three options to choose from, it seems reasonable to assume that subjects (97) answering at least one question incorrectly twice did either not understand our set-up or did not pay close attention. This suggests excluding these subjects from the analysis. This view is reinforced by the fact that the percentage of subjects answering incorrectly twice differs among treatments; it is highest with 14.83% in treatment *I* and lowest with 6.29% in treatment *C*. There is no significant difference between treatments *I* and *N* ($p = 0.271$ in a Fisher's exact test), but the percentage of subjects failing to

[18] Our main variable of interest is the share of high reports which varies between zero and 100%. From the metaanalysis in Abeler *et al.* (2019) we expected 28% of subjects that see a low outcome to lie and report high in the I treatment. This would result in a baseline effect of 61 percent high reports. A power calculation with a total sample size of 600 (we compare the outcome between two treatments), a power of 0.8 and an alpha of 0.5 leads to a minimum detectable effect size of 0.1128 (two sample Chi-Square test).

[19] We executed a small pilot with 30 subjects in treatment *C* to test our software and set-up. This data is not included in the study. Note that we ran a session with 100 subjects in treatment *C* to collect information for the probabilities of the situations in the other treatments. The rest of the sessions have been executed with a within-session randomization approach for the main treatments. We ran the sessions for the bystanders in treatment *N* separately because these subjects did not have to roll a die.

answer correctly even after the second attempt is significantly lower in treatment *C* ($p = 0.001$ compared to treatment *I* and $p = 0.022$ compared to treatment *N*). As these subjects may confound our treatment comparisons, we exclude them and focus our analysis on the sample of 803 observations for our three main treatments (298 for treatment *C*, 247 for treatment *I*, and 258 for treatment *N*). We will refer to this sample as the *main sample* from hereon. To check for the robustness of our results, we will also consider a *restricted sample*, containing only those subjects who correctly answered all questions already in the first attempt. Table A.1 in the appendix provides an overview of the number of observations per treatment, demographics, and the share of subjects that answered all four control questions correctly after the first or second try.

4.4 Hypotheses

The comparison of the critical thresholds $\tilde{c}$ between the three settings *C*, *I*, and *N* in section 3 yield the following hypotheses regarding the behavior of subjects in our experiment:

Hypothesis 1 (Desire-to-win effect): *The frequency of high reports in the negative externality treatment N is lower than in the contest treatment C.*

Both treatments *C* and *N* share identical financial incentives and comprise a negative externality. A subject's payoff, however, depends on the report of some other subject only in *C*: the desire-to-win inherent to such a competition causes a stronger inclination to lie in *C*.

Hypothesis 2 (Negative externality effect): *The frequency of high reports in the negative externality treatment N is lower than in the individual treatment I.*

The treatments *I* and *N* only differ in the negative externality on some other subject that a subject's high report gives rise to in treatment *N*. Social preferences of subjects lead to a lower inclination to lie in *N*. Recall that, due to countervailing effects, we have no hypothesis for the comparison of treatment *C* and *I*.

5 Results

5.1 High reports

Figure 1 shows the percentage of high reports in treatments *C*, *I*, and *N* for the main sample (left panel) and the restricted sample (right panel). In the main sample, the share of high report is higher in *C* (57.38%) than in *N* (49.61%). The difference is statistically significant ($p = 0.073$), supporting Hypothesis 1. This result provides evidence that the desire-to-win fosters lying.[20] Hypothesis 2 states that the share of high reports should be higher in *I* than in *N*. In line

[20] Note that the employed test statistics on the difference between the number of high reports in *C* and *N* are very conservative for two reasons. First, a considerable fraction of subjects (approx. 33%) in both treatments

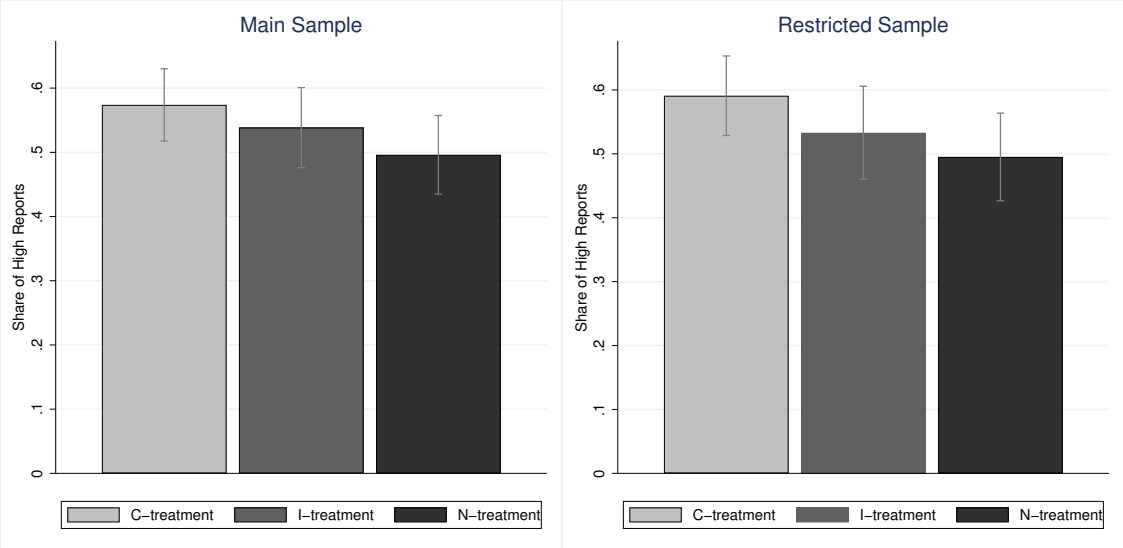


Figure 1: *Share of high reports by treatments for main and restricted sample.*

with Hypothesis 2, we observe high reports more frequently in *I* (53.85%) than in *N*, but asthe difference is not statistically significant ($p = 0.373$), our results do not support the negative externality effect.[21] Still, the effect goes in the predicted direction, and is strong enough to render the difference in the share of high reports between *C* and *I* statistically insignificant ($p = 0.436$). Accordingly, when holding all financial incentives constant, subjects in our experiment do not behave differently in the competitive and the individual treatment.

When considering only subjects who answered the comprehensive questions correctly in the first attempt, we observe a very similar pattern as in the main sample (see the right panel of Figure 1). In the restricted sample, the shares of high reports in treatments *I* (53.29%) and *N* (49.51%) are virtually identical to the main sample, while the corresponding share in *C* increases to 59.09%. Notwithstanding the lower number of observations, the level of significance of the desire-to-win effect on lying increases when comparing *C* and *N* ($p = 0.046$). The differences in the share of high reports between *I* and *N* ($p = 0.477$) and *C* and *I* ($p = 0.237$) remain statistically insignificant. Overall, all the results from the main sample also prevail with the restricted sample, which documents the robustness of our results. Furthermore, it appears that more mindful subjects care more about winning a contest, as the desire-to-win effect is stronger in the restricted sample.

observe HIGH. This reduces the ratio of observed high reports between treatments and, hence, leads to an underestimation of differences in lying across treatments. Second, although we have a specific prediction about the direction of the difference, we employ a two-sided Fisher exact test in the paper unless stated otherwise.

[21] Fischbacher and Föllmi-Heusi (2013) also conducted a treatment with a negative externality. In line with our results, they find a statistically insignificant reduction in the lying frequency.

5.2 Beliefs

We asked subjects for their belief about the percentage of other participants reporting HIGH when the actual outcome is LOW. There are two main insights: first, the ranking of beliefs among the different treatments coincides with the actual behavior. Subjects in treatment *N* expect a lower share of liars (53.36%, main sample; 52.39%, restricted sample) than subjects in treatment *I* (main sample 54.42%, $p = 0.2951$; restricted sample 54.71%, $p = 0.1780$) or treatment *C* (main sample 57.14%, $p = 0.0297$; restricted sample 57.25%, $p = 0.0175$).[22] The second insight is that subjects overestimate the actual degree of lying in all treatments. To see this, recall that the share of high reports contains lies *and* truthful high reports. In all three treatments, the average belief about the lying propensity is higher than 50%. A belief of 50% translates into a share of high report of roughly 66%, while the actual shares of high reports are below 60% in all treatments.

We elicited the same belief from the passive subjects in treatment *B*. Here, the average expected share of false high reports is not treatment-specific (56.19% in treatment *C*, 58.32% in *I*, and 56.75% in *N*; $p \geq 0.446$ for all pairwise comparisons). We did not implement comprehensive questions in treatment *B*. Given our insights from the main treatments, the missing treatment differences might be caused by subjects who did not reflect seriously enough about the situation because they could not influence their payoff.

5.3 Robustness

Next, we conduct a regression analysis to test whether (i) our main result is robust to adding control variables and (ii) personal characteristics contribute to the lying behavior. Table 4 depicts the results from probit regressions with the report as dependent variable and treatment *N* as baseline. We use the main sample in the first three specifications and the restricted sample in specifications (4) to (6). In specifications (1) and (4), we replicate the central finding: As the coefficient of the dummy for treatment *C* is positive and significant, the desire-to-win effect leads to a larger share of high reports. Furthermore, the coefficient of the dummy for treatment *I* is positive as predicted by Hypothesis 2, but – as in the non-parametric analysis â€“ not significant.

In specifications (2) and (5), we add controls for personal preferences, characteristics, and demographics. In the last step, we add a control for the subject's belief about the share of liars in their treatment in specifications (3) and (6). We observe that higher preferences for fairness correlate negatively and higher beliefs positively with high reports.[23] Other characteristics and demographics have no significant impact. In all specifications, the coefficient for treatment

[22] All p-values for the comparison of beliefs are for Wilcoxon rank-sum tests.

[23] The belief needs to be interpreted with caution, as it might (at least partially) rationalize the own lying behavior.

C remains economically and statistically significant, providing evidence for the desire-to-win effect.

	Main Sample			Restricted Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
C treatment	0.196*	0.235**	0.208*	0.242**	0.284**	0.255**
	(0.107)	(0.109)	(0.109)	(0.119)	(0.122)	(0.123)
I treatment	0.106	0.137	0.132	0.0949	0.0905	0.0812
	(0.112)	(0.114)	(0.115)	(0.128)	(0.130)	(0.131)
Belief share liars			0.00646***			0.00583***
			(0.00201)			(0.00224)
Positive reciprocity		−0.0201	−0.0313		−0.0153	−0.0236
		(0.0469)	(0.0476)		(0.0530)	(0.0537)
Negative reciprocity		0.0202	0.0143		0.00783	−0.000235
		(0.0495)	(0.0497)		(0.0554)	(0.0556)
Fairness		−0.114***	−0.102***		−0.164***	−0.153***
		(0.0357)	(0.0361)		(0.0408)	(0.0411)
Sincerity		0.0190	0.0108		0.0143	0.00903
		(0.0407)	(0.0412)		(0.0462)	(0.0467)
Greed avoidance		−0.0127	−0.0100		−0.00234	0.00241
		(0.0380)	(0.0380)		(0.0427)	(0.0428)
# prev. studies		0.00120*	0.00113		0.00128	0.00122
		(0.000705)	(0.000712)		(0.000843)	(0.000851)
Female		−0.0227	−0.0203		−0.0314	−0.0393
		(0.0946)	(0.0950)		(0.108)	(0.108)
Undergrad or higher		0.129	0.149		0.113	0.124
		(0.0917)	(0.0923)		(0.106)	(0.106)
Age		−0.00506	−0.00399		−0.00443	−0.00310
		(0.00357)	(0.00359)		(0.00415)	(0.00420)
Constant	−0.00972	0.544**	0.125	−0.0122	0.768**	0.366
	(0.0781)	(0.273)	(0.305)	(0.0874)	(0.306)	(0.344)
Observations	803	803	803	630	630	630
Pseudo R2	0.0030	0.0244	0.0337	0.0049	0.0376	0.0454
Log likelihood	−552.59771	−540.76161	−535.58437	−432.25479	−418.02149	−414.65661

Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: *Determinants of Lying Frequencies. Probit regressions with the report as the dependent variable.*

6 Conclusion

In the last decades, incentive schemes based on the relative performance of employees have been criticized, and many companies abolished or at least mitigated them. Arguably, while competitive pressure may set high incentives to perform well, it may also incentivize employees to game the system, cheat, and even commit outright fraud.

Laboratory experiments on cheating and lying in contests also support the view that competition leads to a higher degree of misconduct, but the reasons are less clear. First, as the expected marginal financial benefit from misconduct tends to be higher in competitive payment schemes, it cannot be excluded that differences in the observed behavior are (mainly) driven by differences in financial incentives. We account for this issue by designing our non-competitive payment scheme (treatment *I*) such that the expected financial benefit from lying about the outcome of a lottery is the same as in our competitive payment scheme (treatment *C*). We find no significant difference in the frequency of high reports between the individual and the competitive payment scheme.

While our design ensures that the expected financial benefit from lying is the same in treatments *C* and *I*, there are still two differences. First, treatment *C* includes competition, which may induce the lying-enhancing desire-to-win-effect. Second, lying in treatment C lowers the payoff of someone else, which reduces the willingness to lie for subjects with other-regarding preferences. To isolate the desire-to-win effect, we use the negative externality treatment *N*. In treatment *N*, lying yields identical consequences on the own payoff as well as on the payoff of someone else, so any difference between treatments *N* and *C* can safely be attributed to the desire-to win effect. This is our main contribution: By keeping all financial incentives constant with and without competition, we show that the desire-to-win effect leads to a significantly higher frequency of lying.

In this paper, we implement misconduct via lying about the outcome of a lottery, thereby following a widespread experimental approach. The main reason why we choose lotteries is that preference costs from losing a contest should be minimum when the outcome neither depends on abilities nor on effort. Complementing the die-under-the-cup paradigm introduced by Fischbacher and Föllmi-Heusi (2013) with a real-effort setting may reinforce the desire-to-win effect if subjects feel under pressure or disadvantaged by the real effort task used in the experiment. Furthermore, one might conjecture that the non-monetary utility from winning is larger when achieving to win the competition via a true high report than via means of lying. This could attenuate the size of the desire-to-win effect, but should be less of a concern in a real-effort setting where winning is always at least partly rightfully earned by productive effort. Both factors may lead to a higher desire-to-win effect in a real-effort setting. If the difference in lying is already significant in the lottery setting, it should be even more pronounced in other

settings.

However, our conservative design concerning the comparison of treatments *C* and *N* comes at a mirror-imaged cost: If the desire-to-win effect is indeed larger in real-effort tasks, then the lying frequency in treatment *C* might turn out to be significantly higher than in treatment *I*. From an applied perspective, this would imply that one needs to be more careful with competitive payment schemes at higher levels of the hierarchy (assuming that tasks become, on average, more challenging).

APPENDIX

A1: DESCRIPTIVE STATISTICS

Treatment	# subjects	Female	Age	Undergrad or higher	# prev. Studies	HIGH report
C treatment	298(242)	0.51(0.50)	34.66(34.42)	0.60(0.62)	57.32(58.70)	0.5738(0.5909)
I treatment	247(182)	0.46(0.40)	34.25(32.92)	0.49(0.54)	66.07(65.07)	0.5385(0.5330)
N treatment	258(206)	0.48(0.46)	34.29(34.17)	0.54(0.56)	63.40(61.17)	0.4961(0.4951)
B treatment	303	0.50	33.45	0.60	63.39	–
Norm treatment	303	0.50	33.68	0.61	61.57	–

Table A.1: *Descriptive statistics for all treatments. Results for the main treatments are based on the main sample, the results for the restricted sample are reported in parenthesis. We report average results for all variables except for the number of subjects.*

A2: SOCIAL NORM OF LYING

To shed light on whether a social norm of (not) lying exists and it is, given that it exits, treatment-specific and might help to explain treatment differences in actual lying behavior, we elicited the social norm of (i) truthfully reporting the low outcome and (ii) misreporting it as high for each treatment separately in the *Norms* treatment.

Figure A.1 depicts the mean appropriateness rating of the two possible reports for each treatment. Recall that subjects had to choose one of six possible ratings between *very socially inappropriate* (coded as -1) and *very socially appropriate* (coded as 1). It is evident that for all three main treatments, a strong social norm of behaving honestly emerges. The mean appropriateness rating of truthfully reporting a low outcome is positive such that it is clearly a socially appropriate action. On the contrary, the mean appropriateness rating of misreporting the low outcome as HIGH is negative, rendering it a socially inappropriate action.[24] According to a two-sided t-test, the difference between the two norm ratings is significantly different from zero for all three main treatments ($p < 0.01$), demonstrating that a lie is significantly less appropriate than a truthful low report.

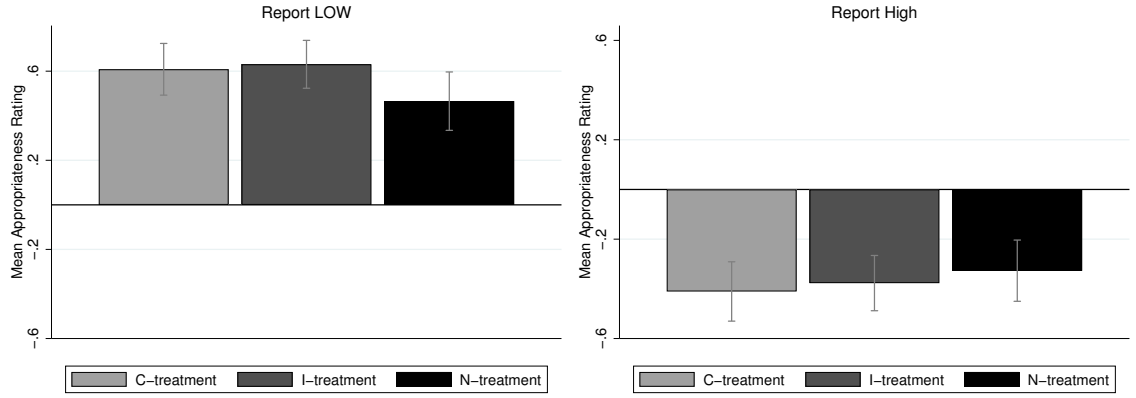


Figure A.1: *The left (right) panel shows the mean appropriateness rating of report LOW (HIGH) given the actual result was LOW by treatments.*

Recently, it has been shown that eliciting social norms with the help of the Krupka-Weber method and introducing a preference for norm compliance helps to explain behavior in lab experiments (Kimbrough and Vostroknutov, 2016; Barr *et al.*, 2018; Chang *et al.*, 2019). In particular, differences in behavior across treatments could be traced back to changes in the social norm. A first look at treatment differences in the mean appropriateness ratings, however, directly reveals that this is not the case in our experiment. Recall that the difference between the two norm ratings is a measure that describes how much less appropriate it is to lie instead of reporting truthfully. We do not find a significant treatment effect on that measure in any two

[24] For all three main treatments, it holds that the modal response is to rate a truthful low report as *very socially appropriate* and misreporting the outcome as high as *socially inappropriate*.

treatments. Accordingly, the negative externality and the desire to win do not affect the social norm of lying.[25]

A3: INSTRUCTIONS

Consent Form

Welcome to our study!
First, we give some general information about our team, the aim of the study, and data protection.

Aim and data collection:
We are interested in individual decision making and personal characteristics. *[main and bystanders in N treatment: We ask you to answer a survey about your attitudes towards others and give some predictions about the behavior of individuals.* [*only for main treatments: In addition, you will be asked to roll a die and report the outcome.] [only for Norm treatment: We askyou to answer a survey about your attitudes towards others and evaluate the choices of other participants.]* We will ask you to give us your Prolific ID to ensure that we can pay you. In our study, we will also use the demographic information such as age or education you provided on Prolific.

Important: All information we provide in this study is true. You will never get inaccurate information.

Risks and benefits:
There are no physical or emotional risks associated with this study that would go beyond the risks of daily life. Your participation in this study will help us to better understand individual decision making.

Payment:
You will receive a fixed payment of 1.40 GBP for taking part in our study. In addition, you can earn a bonus. The payment will be sent to you within two days after completion of this study.

Confidentiality:

[25] If anything, as can be seen in Fig A.1, there is a weaker norm of behaving honestly in the N-treatment, which is in contrast to the result that the frequency of lying is lowest in that treatment.

The information collected in this study may be published in a report or a journal article and presented to interested parties, including possibly, but not exclusively, members of editorial boards and scientific committees. In no circumstances will your Prolific ID be disclosed to people outside the research group. No personal data (e.g. your IP address) will be collected. Other information (e.g., survey responses, time of the study) will be kept by the researchers and may be used for further studies.

Your rights as a participant:
Participation is entirely voluntary. You may leave the survey at any time without any penalty or prejudice.

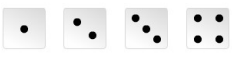
Do you wish to participate

- Yes, continue
- No, leave survey

Please enter you Prolific ID

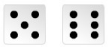
_____-

[Instructions for Treatment C]
Before the survey starts, you will play a simple game where you can earn an additional bonus that will be added to the 1.40 GBP you receive for the survey.
You will be matched with another participant who is also taking part in this study. You will not learn this participant's ID nor will they learn yours.
In this game, you will have to roll a six-sided die. You are free to choose how to obtain the outcome of a die roll, by using either a physical or a virtual die. Hence, you will roll the die in private, so that the outcome cannot be seen by anyone else. After you will have privately observed the outcome of the die roll, we will ask you to report the result of your die roll.
An outcome of



means the result is "Low".

An outcome of
means the result is "High".



The participant you are matched with will also roll a die in private and report either “High" or “Low." You will not be informed about their report before you enter your report. Also, the other participant will not be informed about your report before entering their report.
The table below shows how your and the other participant’s bonus payment depend on the reports of both of you. In all cases, one of you will receive a bonus of 1.20 GBP and the other one a bonus of 0.20 GBP.

Report of the other participant	Your report	Bonus
Low	Low	Each of you has a 50% chance of getting the 1.20 GBP. This is decided by a random draw.
	High	You: 1.20 GBP Other: 0.20 GBP
High	Low	You: 0.20 GBP Other: 1.20 GBP
	High	Each of you has a 50% chance of getting the 1.20 GBP. This is decided by a random draw.

The bonus will be sent to you within two days after completion of this study. You will also receive a message via the Prolific system informing you about the reports of both participants and the resulting payment. The other participant will also receive such a message.

[Control questions]
Before we start the game, we want to make sure you have understood the set-up.
Please answer the questions below to the best of your knowledge.

[Table from above was shown here.]

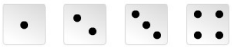
Imagine the following situation:

[Each of the following questions was shown on a separate page. Participants could try twice and were informed about the correct answer afterwards.]

Suppose you have reported “Low”. If the other one reports “Low”, how likely is it that you get the bonus of 1.20 GBP?

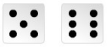
Suppose you have reported “Low”. If the other one reports “High”, how likely is it that you get the bonus of 1.20 GBP?
Suppose you have reported “High”. If the other one reports “Low”, how likely is it that you get the bonus of 1.20 GBP?
Suppose you have reported “High”. If the other one reports “High”, how likely is it that you get the bonus of 1.20 GBP?

[Instructions for Treatment N]
Before the survey starts, you will play a simple game where you can earn an additional bonus that will be added to the 1.40 GBP you receive for the survey.
You will be matched with another participant who is also taking part in this study. You will not learn this participant’s ID nor will they learn yours.
In this game, you will have to roll a six-sided die. You are free to choose how to obtain the outcome of a die roll, by using either a physical or a virtual die. Hence, you will roll the die in private, so that the outcome cannot be seen by anyone else. After you will have privately observed the outcome of the die roll, we will ask you to report the result of your die roll.
An outcome of



means the result is “Low”.

An outcome of



means the result is “High”.

The participant you are matched with will also fill out a survey and receive 1.40 GBP but will not roll a die and cannot submit a report. As explained in detail below, this participant’s bonus payment depends on your report.
With 45% you will randomly be assigned to Case 1. With the remaining probability of 55% you will be randomly assigned to Case 2. Before you submit your report, you do not know if you will be assigned to Case 1 or 2.
The table below shows how your and the other participant’s bonus payment depend on the assigned case and your report. In all cases, one of you will receive a bonus of 1.20 GBP and the other one a bonus of 0.20 GBP.

Case	Your report	Bonus
1 45% probability	Low	Each of you has a 50% chance of getting the 1.20 GBP. This is decided by a random draw.
	High	You: 1.20 GBP Other: 0.20 GBP
2 55% probability	Low	You: 0.20 GBP Other: 1.20 GBP
	High	Each of you has a 50% chance of getting the 1.20 GBP. This is decided by a random draw.

The bonus will be sent to you within two days after completion of this study. You will also receive a message via the prolific system informing you about the case and the resulting payment. The other participant will also receive a message with information about your report, the case, and the resulting payment.

[Control questions]
Before we start the game, we want to make sure you have understood the set-up.
Please answer the questions below to the best of your knowledge.

[Table from above was shown here.]

Imagine the following situation:

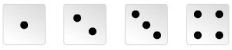
[Each of the following questions was shown on a separate page. Participants could try twice and were informed about the correct answer afterwards.]

Suppose you have reported “Low.” How likely is it that you get the bonus of 1.20 GBP if you are in Case 1?
Suppose you have reported "Low". How likely is it that you get the bonus of 1.20 GBP if you are in Case 2?
Suppose you have reported “High.” How likely is it that you get the bonus of 1.20 GBP if you are in Case 1?
Suppose you have reported “High.” How likely is it that you get the bonus of 1.20 GBP if you are in Case 2?

- 0% (Never)

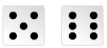
- 50%
- 100% (Always)

[Instructions for Treatment I]
Before the survey starts, you will play a simple game where you can earn an additional bonus that will be added to the 1.40 GBP you receive for the survey.
In this game, you will have to roll a six-sided die. You are free to choose how to obtain the outcome of a die roll, by using either a physical or a virtual die. Hence, you will roll the die in private, so that the outcome cannot be seen by anyone else. After you will have privately observed the outcome of the die roll, we will ask you to report the result of your die roll.
An outcome of



means the result is "Low".

An outcome of



means the result is "High".

With 45% you will randomly be assigned to Case 1. With the remaining probability of 55% you will be randomly assigned to Case 2. Before you submit your report, you do not know if you will be assigned to Case 1 or 2.
The table below shows how your bonus payment depends on the assigned case and your report. In all cases, you will receive a bonus of 1.20 GBP or 0.20 GBP.

Case	Your report	Bonus
1 45% probability	Low	You have a 50% chance of getting the 1.20 GBP. This is decided by a random draw.
	High	1.20 GBP
2 55% probability	Low	0.20 GBP
	High	You have a 50% chance of getting the 1.20 GBP. This is decided by a random draw.

The bonus will be sent to you within two days after completion of this study. You will also receive a message via the Prolific system informing you about the case and the resulting payment.

[Control questions]
Before we start the game, we want to make sure you have understood the set-up.

Please answer the questions below to the best of your knowledge.

[Table from above was shown here.]

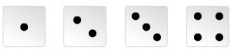
Imagine the following situation:

[Each of the following questions was shown on a separate page. Participants could try twice and were informed about the correct answer afterwards.]

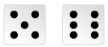
Suppose you have reported “Low.” How likely is it that you get the bonus of 1.20 GBP if you are in Case 1?
Suppose you have reported "Low". How likely is it that you get the bonus of 1.20 GBP if you are in Case 2?
Suppose you have reported “High.” How likely is it that you get the bonus of 1.20 GBP if you are in Case 1?
Suppose you have reported “High.” How likely is it that you get the bonus of 1.20 GBP if you are in Case 2?

- 0% (Never)
- 50%
- 100% (Always)

[All Main Treatments]
Getting started:
If you have not done so yet, please get a die or use a virtual one.
Now please roll the die and report either “High” or “Low.”
Remember



means the result is “Low”, and



means the result is “High”.

Please report either “High” or “Low” by checking one of the two boxes below.

- High
- Low

[Belief]
What do you think about the behavior of the other participants in this study? Out of all participants (except you) whose actual result of the die roll was “Low” (outcome 1 to 4), how many will report “High”?

[Answer was recorded via a slider ranging from zero to 100%]

[Instructions for passive subjects (bystanders) in the N Treatment]
You will receive 1.40 GBP for answering this survey. In addition, you will receive a bonus.
In the following, we will show you the set-up for a study we recently ran on the Prolific platform. We will ask you for your belief about the behavior of the participants in the study we just ran. Your answer neither influences your fixed payment nor your chance of getting the bonus. For the scientific value of our study, it is important that you state your belief truthfully.
We first show you the exact instructions these participants saw. Then, we will ask you for your belief.
On the next screen, we will show you the exact instructions. All participants also received 1.40 GBP for answering a survey:

[Subjects then saw the instructions of the C, N, or I Treatment.]

[Belief bystanders]
After reading the instructions of the study we recently ran on the Prolific platform, we now ask you to state your belief. What do you think about the behavior of the participants in this study? Out of all participants whose actual result of the die roll was “Low” (outcome 1 to 4), how many will have reported “High”?

[Answer was recorded via a slider ranging from zero to 100%]

[If subjects saw the instructions from the C Treatment]
We now explain to you how your bonus is calculated. In a study similar to the one just shown to you, participants also decided on whether to report “Low” or “High”. In contrast to the study just shown to you, there was no interaction with other participants. However, one participant was randomly matched with you. If this participant gets the high bonus of 1.20 GBP, you get the low bonus of 0.20 GBP. Also, if this participant gets the low bonus of 0.20 GBP, you get the high bonus of 1.20 GBP. This participant knew that you get 0.20 GBP if they get 1.20 GBP and the other way round.

[If subjects saw the instructions from the N Treatment]
We now explain to you how your bonus is calculated. In the study just shown to you, you played the passive role, i.e. you were randomly matched with one of the participants. If this participant gets the high bonus of 1.20 GBP, you get the low bonus of 0.20 GBP. Also, if this participant gets the low bonus of 0.20 GBP, you get the high bonus of 1.20 GBP.

[If subjects saw instructions from the I Treatment]
We now explain to you how your bonus is calculated. In a study similar to the one just shown to you, participants also decided on whether to report “Low” or “High.” One participant was randomly matched with you. If this participant gets the high bonus of 1.20 GBP, you get the low bonus of 0.20 GBP. Also, if this participant gets the low bonus of 0.20 GBP, you get the high bonus of 1.20 GBP. This participant knew that you get 0.20 GBP if they get 1.20 GBP and the other way round.

[Instructions Norm treatment]
We will describe the design of a study on decision making which we ran on the Prolific platform. Participants in this study decided between different options. We will ask you to evaluate the degree at which these possible choices are socially appropriate or not. Specifically, for each possible choice, we will ask you to rate this choice as "socially appropriate" and thus "consistent with moral or proper social behavior" or "socially inappropriate" and thus "inconsistent with moral or proper social behavior."
By socially appropriate, we mean choices that most people agree to be the "correct" or "ethical" choice. Another way to think about this is that, if an individual selects a socially inappropriate choice, then many other people might be angry at the individual for doing so. For each option, please answer as truthfully as possible, based on your own view of what constitutes socially appropriate or socially inappropriate behavior.
To give you an idea of how this task will proceed, we will go through an example and show you how you will report your responses. Note that the example only serves to familiarize yourself

with rating choices as socially appropriate or inappropriate. After the example, we will describe the actual situation for which you will rate choices.

Example:
At a local coffee shop a person observes that someone has left their wallet on a table. The person then has four possible choices: 1) take the wallet, 2) ask others nearby if they own the wallet 3) do nothing 4) or hand the wallet to the shop manager.
The person needs to pick one out of these four possible choices.
The table below presents a list of all of the person's possible choices. If this was the actual situation and not the example, we would ask you to rate each of those four choices as "very socially inappropriate", "socially inappropriate", "somewhat socially inappropriate", "somewhat socially appropriate", "socially appropriate" or "very socially appropriate" by ticking the respective box.

possible choices	very socially inappropriate	socially inappropriate	somewhat socially inappropriate	somewhat socially appropriate	socially appropriate	very socially appropriate
take the wallet						
ask others nearby if the wallet belongs to them						
do nothing						
hand the wallet to the manager						

Recall that by "socially appropriate" we mean choices that most people agree is the "correct" or "ethical" thing to do. To see how to fill the table suppose hypothetically and arbitrarily that your opinions are as follows: 1) taking the wallet is "very socially inappropriate, " 2) asking others nearby if the wallet belongs to them is "socially appropriate", 3) leaving the wallet where it is is "somewhat socially inappropriate", and 4) handing the wallet to the shop manager is "very socially appropriate". Then, you would need to indicate your responses as follows:

possible choices	very socially inappropriate	socially inappropriate	somewhat socially inappropriate	somewhat socially appropriate	socially appropriate	very socially appropriate
take the wallet	X					
ask others nearby if the wallet belongs to them					X	
do nothing			X			
hand the wallet to the manager						X

After these explanations we now proceed to our actual study which we ran on Prolific:
Person A, a participant in that study, had to make a choice by picking one of two options. We will ask you to rate each possible choice just as in the example above.
Your bonus payment will be calculated as follows: First, the software will randomly select one of Person A's possible choices. Secondly, the software will randomly match you with another participant that also evaluates Person A's possible choices. If your report for the selected choice matches the report of this participant, you will receive a bonus of 2.50 GBP. Otherwise your bonus will be zero.
For example, if the example above would be the actual task and the possible choice "Leave the wallet where it is," was selected by the software, we would compare your report with the report of the other participant for this choice. If your report had been "somewhat socially inappropriate," then your bonus would be 2.50 GBP if the participant you are matched with also evaluated the choice as "somewhat socially inappropriate", and zero otherwise.
We now present the situation for which we will ask you to rate the participants' possible choices. The participants have also been recruited on the Prolific platform. On this screen, you will read the exact instructions that participants in the original study have seen.

[Subjects then saw the instructions of the C, N, or I Treatment.]

[For subjects that saw instructions from the C Treatment]
You have now read the exact instructions that participants in the original study have seen. In short, the situation can be summarized as follows:
Person A was matched with another participant. Both participants had to roll a die in private and report either "High" or "Low."
Both participants would get a bonus, but only one could get the high bonus. After both participants submitted their report, both reports were compared. If only one participant reported "High", this participant got the high bonus whereas the other participant got the low bonus.

If both participants submitted the same report (both “High” or both “Low”), a random draw decided who got the high and who the low bonus.
For both participants reporting “High” instead of “Low” increased the probability to get the high bonus by 50%.

[For subjects that saw instructions from N Treatment]
You have now read the exact instructions that participants in the original study have seen. In short, the situation can be summarized as follows:
Person A had to roll a die in private and then report either “High” or “Low.” Person A was matched with another passive participant.
Both participants would get a bonus, but only one could get the high bonus. If Person A got the high bonus, the other passive participant got the low bonus. Likewise, if Person A got the low bonus, the other passive participant got the high bonus.
It depends on Person A’s report who got the high and who the low bonus. In any case, reporting “High” instead of “Low” increased the probability for Person A to receive the high bonus and, in turn, decreased the probability for the passive participant to receive the high bonus, by 50%.

[For subjects that saw instructions from I Treatment]
You have now read the exact instructions that participants in the original study have seen. In short, the situation can be summarized as follows:
Person A had to roll a die in private and then report either “High” or “Low.”
Person A could earn a high or a low bonus, and reporting “High” instead of “Low” increased the probability to receive the high bonus by 50% in any case.

[All subjects in Norm Treatment]
Suppose Person A has rolled the die and the actual result is “Low” (die roll of 1,2, 3, or 4 leads to “Low”).

Please rate each of the two possible choices of Person A as “very socially inappropriate”, “socially inappropriate”, ”somewhat socially inappropriate”, “somewhat socially appropriate”, “socially appropriate,” or ”very socially appropriate”. Please tick the respective box.

possible choices	very socially inappropriate	socially inappropriate	somewhat socially inappropriate	somewhat socially appropriate	socially appropriate	very socially appropriate
report "Low"						
report High						

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