University of Innsbruck

Don't hate the player, hate the game: Uncovering the foundations of cheating in contests

Glenn Dutcher, Daniela Glätzle-Rützler, Dmitry Ryvkin

Working Papers in Economics and Statistics

2016-29

Research Platform eeecon Empirical and Experimental Economics
University of Innsbruck
http://eeecon.uibk.ac.at/

University of Innsbruck
Working Papers in Economics and Statistics

The series is jointly edited and published by

- Department of Banking and Finance
- Department of Economics
- Department of Public Finance
- Department of Statistics

Contact address of the editor:
Research platform “Empirical and Experimental Economics”
University of Innsbruck
Universitaetsstrasse 15
A-6020 Innsbruck
Austria
Tel: + 43 512 507 7171
Fax: + 43 512 507 2970
E-mail: eeecon@uibk.ac.at

The most recent version of all working papers can be downloaded at
http://eeecon.uibk.ac.at/wopec/

For a list of recent papers see the backpages of this paper.

Don't hate the player, hate the game: Uncovering the foundations of cheating in contests.

Glenn Dutcher [*]
Daniela Glätzle-Rützler [†]
Dmitry Ryvkin [‡]

October 1, 2016

Abstract

Contests are meant to attract the best performers and incentivize high effort, however, they may also attract cheaters who try to win via illicit means which crowds out the best performers. We use a laboratory experiment to explore the role of self-selection in contests with a possibility of lying in a real effort task. Contrary to common wisdom, we do not find evidence that contests disproportionately attract "intrinsic" cheaters. However, we find that contests fail at selecting the best performers, as no difference is observed in the actual or perceived ability of those who selected into the contest versus those who selected into a comparable noncompetitive pay scheme.

JEL classification codes: D02, K42, M52, C90
Keywords: contest, cheating, entry, experiment

[*] Contact author, the Department of Economics, Ohio University, dutcherg@ohio.edu
[†] University of Innsbruck, daniela.ruetzler@uibk.ac.at
[‡] Florida State University, dryvkin@fsu.edu

1 Introduction

There's nothing for me to change from. It's not like I ever considered myself a bad person. I made a horrible mistake and I'm sorry.

-Bernie Madoff (2014)

Reporter: *When you raced, was it possible to perform without doping?*

LA: *That depends on which races you wanted to win. The Tour de France? No.*

-Lance Armstrong interview excerpt (2013)

Infamous acts of dishonesty highlight a key concern about *contests* – reward schemes based on relative performance – that they incentivize both productive and counterproductive behaviors. The spotlight is typically centered on the dishonest individual, and the public's ire is directed toward the "bad person." In their criticism, pundits imply that *they* would refrain from such a despicable act if placed in the same position. This leads to the idea that in order to get ahead in competitive settings, one must be morally lax. Prior studies have found a link between unethical behavior and social class (Piff et al., 2012); however, it is still unknown if immoral-leaning individuals tend to select into competitive schemes or if the competitive scheme serves as the impetus for immoral acts (or both); i.e., whether contests bring out the worst *in* us or the worst *among* us. Additionally, it is unknown if immoral acts serve as a substitute for ability and how this potential trade-off alters the original intent of the contest to identify and reward the best of the best. In the present study, we address these fundamental questions in a controlled laboratory experiment. When doing so, it is necessary to understand the interplay between ability, propensity to lie and selection into contests, which ultimately can help contest designers implement more efficient contest mechanisms and assist in accurately assessing the costs and benefits of a contest.

On the surface, the appeal of contest mechanisms is quite intuitive: They motivate contestants to exert high effort and identify the "best" by giving an incentive for the best to enter a contest where only the best of the best will win. In other words, contests are useful in solving problems associated with asymmetric information about hidden actions and hidden ability. Multiple studies have confirmed this very basic intuition (e.g., Ehrenberg and Bognanno, 1990; Eriksson, Teyssier and Villeval, 2009; Cason, Masters and Sheremeta, 2010). However, it was also noted (e.g., Berentsen 2002; Kräkel, 2007; Gilpatric, 2011) that, in the presence of the aforementioned information asymmetries, contest mechanisms provide an increased incentive to cheat.

Although many acts of dishonesty are relatively high-profile, the nature of cheating – coupled with the potential effects of self-selection – limits the use and availability of naturally occurring data. Thus, carefully controlled experiments are a promising alternative methodology. In this study, we report the results of a real effort laboratory experiment designed to identify the interplay between selection into and cheating in contests. Our results call into question two commonly held beliefs about contests. First, we show that contests may not attract the best performers when output is subject to manipulation (via lying). Second, we do not find evidence that people selecting into contests are inherently more inclined to lie or are particularly selfish because they do not exhibit higher than average levels of lying in a comparable noncompetitive setting and they do not take more than others in a standard dictator game. These two countervailing results imply contests may not be as good as hoped at identifying the best of the best, but at least they do not select out the most lying individuals.

We implemented an experiment focusing on endogenous sorting of subjects between a competitive and noncompetitive pay schemes with a possibility of lying. The experiment consisted of two main parts, with subjects performing a timed, real-effort math task. After the math task was completed, subjects self-reported how many problems they solved correctly and were paid based on this reported number. In the first part, subjects did not know until after their performance that they would be paid based on self-reporting. Thus, the first part serves as a baseline for measuring ability and general propensity to lie. In the second part, the subjects were either put into a two-person contest or could select between a contest and a piece-rate pay schemes. The results of these two treatments are compared against a baseline where, in the second part, subjects were put into a piece-rate pay scheme. The two main parts were followed by a modified dictator game and a belief elicitation stage measuring subjects' beliefs about others' abilities and lying behavior. As we show, these beliefs play the key role in explaining our results.

We find that when subjects face a decision of whether to enter the contest or not they take into account two factors: Perceived own relative ability and perceived level of lying by others. The *ceteris paribus* directions of these effects are as expected in that perceived own relative ability positively affects selection into a contest whereas the perceived lying by others hinders entry. Importantly, we find no link between subjects' lying behavior in a piece-rate setting and their choice of a contest mechanism. When examining beliefs, we also find a very interesting effect coming from the entry decisions of lower-ability subjects. Some of these subjects enter the contest knowing they are of lower ability than others and, at the same time, they believe the amount of lying in the contest to be quite low. One potential justification for this entry is that the intention of these subjects is to enter and lie by a greater amount than they believe others will. This is supported by our data; however, not all low-ability subjects enter the contest. We also observe high-ability subjects choosing not to enter the contest when they believe there will be rampant lying in it. Overall, due to the observed over-entry by low-ability subjects and insufficient entry by high-ability subjects, we find no difference between the average ability levels of those who chose the contest and those who chose the piece-rate scheme.

In general, we observe very little lying in any of the piece-rate settings; however, this is not the case in the contest settings. We find that subjects who are forced into a contest lie at a very high rate; even more than those who self-select into the contest. This result is driven by ability and beliefs – lower-ability subjects lie more and lying is positively related to a subject's beliefs about others' lying behavior. We also find that risk preferences are an important component in that less risk-averse subjects lie more and are more likely to choose the contest. Finally, we identify an association between lying in the noncompetitive piece-rate setting and lying in the subsequent contest, endogenously chosen or exogenously imposed. We also find no correlation between contest selection and subjects' choices in a dictator game, implying that selfishness was not a selection characteristic. Thus, the generalized answer to our opening question is that contests bring out the worst *in* us but not necessarily the worst *among* us.

2 Related Literature

We will first outline below the literature on lying in contests and then highlight some of the literature on contest selection. We will also briefly discuss the related literature on sabotage in contests.

The preponderance of theoretical articles on lying in contests rely on analogies from sports contests which involve doping. One of the main aims of this literature is to explore which mechanisms are most effective to eliminate cheating. The typical cost-benefit analysis is applied to these situations to show that given certain parameter values, contestants are more likely to cheat and in some instances, the less able will win the contest (Berentsen, 2002). Another interesting finding coming from this literature is that as the number of contestants increases, so does the prevalence of cheating (Gilpatric, 2011 and Ryvkin, 2013).

There is also some empirical evidence on cheating in contests. In general, sports contests are often susceptible to cheating (Preston and Szymanski, 2003). In the lab, Schwieren and Weichselbaumer (2010) show that low performers cheat more often in a contest pay scheme which they attribute to either reputational concerns (or self-image) or increasing the chance to win the contest. In a coin sorting task, Belot and Schroeder (2013) find that when given the opportunity, subjects lie about their performance to increase their payment – an effect which is exaggerated in a competitive setting. Gino and Pierce (2009) show that when the stakes to lying are higher/more salient (as is the case in contests), lying is more likely to occur.

The study related most closely to ours is Faravelli et. al (2015), which, to the best of our knowledge, has been done concurrently. They address a similar question, but use a different design and a different task, and some of their results and conclusions are also quite different from ours. Likely, many of the divergent results are due to different design choices. Specifically, Faravelli et. al (2015) implement a within-subject design where subjects are asked to complete a matrix task in a piece-rate setting and a contest setting before they make a choice on which one they prefer. Subjects were given 20 matrices to solve, but only 10 of those were solvable, and subjects did not know in advance which ones. Thus, lying in the experiment of Faravelli et. al (2015) could be partially motivated by reciprocity and fairness concerns: Subjects who spent time on unsolvable matrices were likely frustrated and felt it was fair for them to lie. Such motives for lying are not present in our experiment. Thus, the two studies apply to different settings, both of which are externally relevant but lead to different results. Faravelli et al. (2015) find very little difference between the levels of lying in the piece-rate setting and the exogenous contest setting (and no difference when selection is allowed), while we find much larger differences in all instances. They also find lower output in the exogenous contest while we do not. Additionally, from a post-experiment open-ended survey they find that low-ability subjects select out of the contest and high ability subjects select into it, while we find many low-ability subjects select into the contest and many high ability subjects select out of it. Thus, the common finding between the two studies, that average output is the same in the contest and piece-rate pay schemes, is due to different reasons. Perhaps most importantly, Faravelli et al. (2015) find that lying propensity is a factor for subjects selecting into the contest, while we do not. We also add a belief elicitation mechanism that allows us to examine the effects of perceived relative ability and others' lying behavior. Thus, the two studies use different methods and have very different implications for their respective settings; therefore, we view our study as complementary.

When examining selection, theoretically, it was shown that selection could improve or dampen the effects of tournament incentives (Fullerton and McAfee, 1999; Hvide and Kristiansen, 2003). Empirically, selection was found to be an important determinant along several dimensions (Ehrenberg and Bognanno, 1990; Camerer and Lovallo, 1999; Lazear, 2000; Niederle and Vesterlund, 2007; Eriksson, Teyssier and Villeval, 2009; Cason, Masters and Sheremeta, 2010; Datta Gupta, Poulsen and Villeval, 2013). Dohmen and Falk (2011) did not consider lying, but used a combination of experiments and field data to study which type of subject self-select into a tournament pay scheme. Importantly, they found that higher ability subjects, males and more risk-loving subjects were more likely to select into a variable pay scheme. In addition to this, they found that results from a trust game did not predict subjects' choices of a payment scheme when other factors were controlled for.

Related to our research topic is the literature on sabotage in contests. Before highlighting its main findings, we would like to point out that lying and sabotage are different, although related, phenomena, both behaviorally and theoretically, and should not merely be viewed as substitutes. One important difference between lying and sabotage is that lying affects one's own output, and hence affects the payoffs of others only indirectly, whereas sabotage affects the output and payoffs of others directly. Thus, sabotage is likely to have higher moral costs and may be utilized by different types of players. Another difference is that, unlike lying, sabotage is targeted and may even generate positive externalities for players unaffected by it. In the presence of selection, because of the targeted nature of sabotage, high-ability players have an incentive to avoid the contest, which has direct implications for the effectiveness of the contest mechanism. The same is not immediately evident in the case of lying. Thus, although our results may provide some insights into the relationship between selection and sabotage behavior in contests, they should be taken with caution, and more research on the topic is needed.

Sabotage in contests was first introduced theoretically by Lazear (1989) using a tournament model based on Lazear and Rosen (1981). The author shows that as the spread between the winner and loser prizes increases, work effort increases but so does the return from engaging in sabotage. Konrad (2000) extends this analysis by using a tournament model with homogeneous agents based on Tullock (1980) and shows that as the number of contestants increases, the amount of sabotage should decrease. Konrad (2000) relies on the fact that engaging in sabotage, albeit increasing own probability of winning, helps other contestants as well, and at the some point it is no longer worthwhile for agents to engage in sabotage. Chen (2003) expands on this work by looking at heterogeneous agents in the Lazear and Rosen (1981) framework and shows that the higher-ability workers are more likely to attract sabotage, and those more proficient at sabotaging are more likely to use it.

Empirical evidence testing these theories is still somewhat sparse. One of the field studies which attempts to do so, Garicano and Palacios-Huerta (2005), finds that as the incentives increase to win a soccer match in the Spanish league, so does the sabotage. Balafoutas, Lindner and Sutter (2012) find a similar effect in Judo matches when rules prohibiting sabotage were relaxed. Carpenter, Matthews, and Schirm (2010) use an ingenious field experimental design to show not only that tournaments with a possibility of sabotage result in instances of sabotage, but also that subjects realize this potential and contribute less effort than in a piece-rate pay scheme or a tournament without sabotage. Field studies of these issues are rare because of the difficulties involved in observing sabotage in the field. This challenge makes lab experiments an especially useful tool for testing contest corruption. Somewhat surprisingly, the literature in this realm is still limited and does not consider selection. The most prominent articles in this area are due to Harbring and Irlenbusch (2007, 2008, 2011). They mostly confirm the elements of the above theories regarding the effects of the prize spread and workers' heterogeneity on the instances of sabotage. In particular, they find that sabotage is greater when lower ability workers are in the majority, and that they direct this sabotage towards the higher ability workers.

To conclude, though the above literature provides insights into our research question, it does not explicitly examine how the propensity someone has for lying, combined with their ability and other personal characteristics, affects their selection into a contest where lying is possible, and how this selection affects the overall effectiveness of the contest as an incentive provision and talent attraction mechanism. One exception is the study of Faravelli et al. (2015), but, as discussed in detail above, there are substantial differences between their study and ours, to the extent that in many cases our results are the opposite of theirs.

3 Theoretical Framework and Predictions

3.1 Piece Rate with Lying

Consider a risk-neutral agent[1] reporting output $y = a + l$. Here, a is the true output the agent produces, and l is the level of lying the agent chooses to misreport her true output by. We assume, for simplicity, that a is not the agent's choice variable but her innate ability, measuring the output she can produce by exerting full effort without lying; thus, the only choice variable for the agent is l. As we show later, this assumption is justified empirically in our setting.

Compensation is based on reported output, and the agent is paid at the piece rate $r > 0$. The agent incurs a cost associated with lying, $cg(l)$, where $c > 0$ is the agent's lying aversion parameter, and $g(\cdot)$ is a strictly increasing convex function, with $g(0) = 0$. Costs of lying include moral costs and, if present, a possibility of punishment if cheating is detected. The agent's payoff is, thus,

$$\pi = r(a + l) - cg(l). \tag{1}$$

Payoff maximization yields the optimal level of lying $l^*_{PR}(c)$ satisfying the equation

$$r = cg'(l) \tag{2}$$

provided $r > cg'(0)$, and zero otherwise. Notably, the optimal level of lying is independent of the agent's ability a.[2]

H1: In the piece-rate scheme, the level of lying is independent of ability.

3.2 Tournament with Lying and Pay Scheme Selection

Consider now a tournament mechanism with two agents indexed by $i = 1, 2$. Each agent i only observes her own parameters (a_i, c_i) that are drawn for both agents from a commonly known joint distribution $F_{ac}(a, c)$.

[1]We assume risk-neutrality throughout this section for the ease of exposition. In the experiment, we obtain subjects' risk-aversion measure and use it as a reduced-form control in the data analysis.

[2]This feature of the model is a consequence of our assumption that effort is not a choice variable, i.e., subjects do not substitute lying for effort and only use lying as a last resort to inflate their output on top of the maximal output they can produce without lying. Our experimental results support this assumption.

As before, agent i's output is $y_i = a_i + l_i$ where l_i is the agent's chosen level of lying.

The tournament compensation scheme is a *rank-based piece-rate scheme* structured as follows: The agent with a higher reported output receives a piece rate r_1, whereas the agent with a lower output receives a piece rate $r_2 < r_1$. In case of a tie, the winner is determined randomly.[3]

Suppose agent 2 chooses the level of lying l_2 according to a bidding function $b(a_2, c_2)$. Let $F_2(\cdot)$ denote the distribution of random variable $y_2 = a_2 + b(a_2, c_2)$.[4] Then, assuming agent 1's parameters are (a, c), her expected payoff from choosing the level of lying l is

$$\pi_1 = [r_2 + \Delta r F_2(a + l)](a + l) - cg(l). \tag{3}$$

Here, $\Delta r = r_1 - r_2$ is the spread between the two piece rates. Equation (3) has an intuitive structure: Agent 1 is guaranteed piece rate r_2, and receives an increase in piece rate of Δr if she wins.

Maximizing π_1 with respect to l and setting $l = b$, obtain the following functional equation for the symmetric equilibrium bidding function $b(a, c)$:

$$r_2 + \Delta r F_2(a + b) + \Delta r F_2'(a + b)(a + b) = cg'(b). \tag{4}$$

Let $l_T^*(a, c)$ denote the solution of (4), assuming it exists.

Equations (2) and (4) allow us to compare the average levels of lying in the piece-rate and tournament schemes. For comparability, consider the piece-rate scheme with $r = (r_1 + r_2)/2$. Calculating the expectation of both sides of Eqs. (2) and (4) over the realizations of (a, c), assuming interior solutions, obtain

$$r_2 + \frac{\Delta r}{2} = \mathrm{E}(cg'(l_{PR}^*)), \tag{5}$$

$$r_2 + \frac{\Delta r}{2} + \Delta r \mathrm{E}(F_2'(a + l_T^*)(a + l_T^*)) = \mathrm{E}(cg'(l_T^*)). \tag{6}$$

Note that function F_2 is increasing, which implies that the third term in the left-hand side of (6) is positive. We conclude that the expected marginal cost of lying in the tournament scheme is higher than in the piece-rate scheme. This statement is true for all increasing marginal cost functions, therefore l_T^* first-order stochastically dominates l_{PR}^* if both are treated as random variables. In particular, the average level of lying is higher under tournament.

H2: The average level of lying is higher in the tournament scheme than in the piece-rate scheme with $r = (r_1 + r_2)/2$.

[3] In what follows, for simplicity, we assume that distribution F_{ac} is smooth and ties occur with probability zero.

[4] For a given function b, F_2 is derived from F_{ac}.

We now explore how the equilibrium level of lying $l_T^*(a,c)$ depends on the agent's ability a. Letting $y_T^* = a + l_T^*$ and differentiating both sides of (4), with $b = l_T^*$, with respect to a, obtain

$$[2\Delta r F_2'(y_T^*) + \Delta r F_2''(y_T^*) y_T^*] \left(1 + \frac{\partial l_T^*}{\partial a}\right) = c g''(l_T^*) \frac{\partial l_T^*}{\partial a},$$

which gives

$$\frac{\partial l_T^*}{\partial a} = \frac{2F_2'(y_T^*) + F_2''(y_T^*) y_T^*}{\frac{c}{\Delta r} g''(l_T^*) - 2F_2'(y_T^*) - F_2''(y_T^*) y_T^*}.$$

The numerator of this expression is negative provided the marginal benefit of output, cf. the left-hand side of (4), is decreasing in equilibrium, which is a standard property of tournament models. Then the denominator is positive (it can also be seen from (4) that the denominator will be positive due to the second-order condition holding for the maximization of profit π_1), and thus the equilibrium level of lying decreases in ability.

H3: In the tournament scheme, the level of lying decreases in ability.

In addition to directly comparing agents' behavior between the piece-rate and tournament schemes, we are also interested in the analysis of tournament entry decisions, i.e., agents choosing between the two compensation schemes. It is clear that, other things being equal, agents with higher abilities and/or lower costs of lying will be more likely to select into the tournament. Importantly, the equilibrium distribution of (a,c) among those who enter the tournament will be modified endogenously, and thus subjects' (possibly incorrect) beliefs about who will and who will not enter the tournament will play a key role in their decisions. Specifically, we anticipate that subjects who believe (perhaps mistakenly) that their *relative* ability is high and/or believe (perhaps mistakenly) that their *relative* cost of lying is low, will be more likely to choose the tournament scheme. In the experiment, we elicit subjects' beliefs about others to address these issues.

H4: (a) Subjects with a higher ability and/or lower cost of lying are more likely to choose the tournament scheme.

(b) Subjects who believe that their relative ability is higher and/or their relative cost of lying is lower are more likely to choose the tournament scheme.

4 Experimental Design

4.1 Basics

We recruited 196 subjects (47% of them female) using ORSEE (Greiner 2004) from the pool of pre-registered university students. Each subject participated in one of eight sessions lasting a little less than an hour. Subjects made an average of €9.37, not including a show-up fee.

4.2 Procedures and Treatments

We conducted three main treatments where the primary task in all three was a real-effort task of adding five two-digit numbers. This task has many benefits including the wide variance in ability and outcomes which have been shown to be gender-neutral. Because of these attractive features, such a task has been widely used in the experimental literature looking at competitive preferences (see for example Niederle and Verstlund 2007; Cason, Masters and Sheremeta, 2010; Sutter and Glätzle-Rützler, 2014;).

The structure of the experiment is summarized in Table 1. The main section of the experiment consisted of two parts and subjects were informed at the beginning that only one of these parts would be randomly chosen for payment at the end. When subjects arrived to the room, they were randomly assigned a seat where there was a green pen and instructions for Part 1.[5] Once the instructions were read out loud, a worksheet containing 40 math problems was handed to the subjects face-down.[6] Once all subjects received a worksheet, they were told to begin the 5-minute round. After 5 minutes, we told them to stop and we immediately collected their green pens and passed out black pens. Subjects were then informed that the answers were available on demand on their computer screen (z-Tree; Fischbacher, 2007) and they would need to grade their own work. Subjects were paid €0.30 for each problem they reported (in the program) to have solved correctly. In Part 1, all subjects faced the same piece-rate incentive scheme regardless of the treatment they were in.

Part 2 followed the same procedures with a few exceptions. After all subjects completed Part 1, we gave them a pink pen and new worksheets (face down) with 40 new math problems. The timing and collection of the pens was the same as in Part 1. Specifically, the subjects were told that all of the rules from Part 1 pertaining to the task and grading/reporting were the same.

The only variation across sessions was present in Part 2 where the subjects participated in one of three treatments: piece-rate (PR-PR), contest (PR-C) or contest selection (PR-CS).

In Part 2 of the PR-PR treatment, we repeated the piece-rate pay scheme used in Part 1. This was

[5] Sample instructions are given in the Appendix.
[6] A sample worksheet is given in the Appendix.

done in order to determine if subjects changed their lying behavior and/or their performance after having experienced Part 1 and gaining full information on how the problems are graded. This serves as a control to the other treatments.

In Part 2 of the PR-C treatment, subjects were randomly and anonymously paired with another subject in the session.[7] The number of solved problems each subject reported was compared against the number of reported problems of the subject they were paired with. If their reported number was higher, they received €0.5 per reported problem whereas if they reported a lower number, they receive €0.1 per reported problem. If both reported the same number, they each received €0.3 per reported problem.

In Part 2 of the PR-CS treatment, subjects could choose to participate in a piece-rate pay scheme or a contest pay scheme where the payment in the contest pay scheme was as described in the PR-C treatment and the payment in the piece-rate was equivalent to the payment used in Part 1.[8]

Several features of this design deserve further explanation. At no point in the experiment did we collect the worksheets from the subjects. Once all parts of the experiment were finished, someone not affiliated with the experiment paid the subjects. Subjects were told of this procedure.[9] After all subjects had left the lab, we wrote the subject number and session number on each of the worksheets. The worksheets were later graded and the number of problems subjects solved correctly was recorded alongside the number of problems they reported. This procedure has a couple of nice features. First, when subjects performed the task in Part 1, they did not know they would be self-reporting for their payment. Thus, the results in Part 1 serve as a valid measure of task-specific ability, and the level of misreporting at the end of Part 1 measures the subject's initial propensity to lie. This is further assured because we gathered the green pens immediately following the task in Part 1 and gave them pink pens in Part 2. The latter method was employed to ensure that the answers from Part 1 could not be manipulated. Furthermore, because the worksheets were not collected until after the subjects left and their payment was given by someone not affiliated with the experiment, their fear, either socially or monetarily, of being punished for lying was minimized.[10] Thus, they were left to "discover" on their own the possibility to cheat further eliminating experimenter demand effects.

Following Part 2, subjects played a modified dictator game where they and a subject in a future session started with an endowment of €2. Their task was to decide if they wanted to change the allocation or keep

[7] Subjects were never given information on the identity or the past behavior of the subjects they were paired with.

[8] If there were an odd number of participants who chose the contest pay scheme, the output of the odd-man-out was compared to a random other subject who also chose the contest pay scheme.

[9] This procedure was used to ensure appropriate social distance between the subjects and the experimenters. This will reduce experimenter demand effects found in Hoffman, McCabe, Shachat and Smith (1994).

[10] Another concern was that subjects may not lie out of fear that the experimenters may pass judgment on them afterwards. So, as an additional control, we ran a different PR-CS treatment with 41 subjects where subjects were told at the beginning that they could take all experimental materials, including their worksheets, with them. Though we cannot check the amount of lying in this treatment, the average number of reported correct answers of 22.4 is not statistically different than the 24.2 reported in the other PR-CS treatment (t-test $p = 0.53$, Kolmogorov-Smirnov (KS) test for equality of distributions $p = 0.87$.

it as is where the person they were paired with had no choice and simply received the money that was sent to them. Once all subjects had made their choices in the dictator game, they played a lottery that captured a subject's risk preferences. In the lottery, they were endowed with €2 and could invest, in €0.10 increments, any of this in the lottery that paid half of the time. If the computer drew 51-100, the lottery was a winner and the subject received €2 + invested amount×2. If the computer drew a number between 1-50, the lottery was a loser and the subject received €2 – invested amount. Thus, the lottery had a linearly increasing expected value between €2 – €3. In line with Parts 1 and 2, only one of Parts 3 or 4 was randomly chosen for payment.

Finally, we elicited subjects' beliefs about what others reported and how many problems they believed others truly solved.[11]. The difference between the two gives us a measure of how much lying they believed occurred in both Parts 1 and 2. To conclude, we gave the subjects a demographic questionnaire.

(Table 1 about here)

5 Results

5.1 Overview

Because our theoretical framework relies on the distribution of ability and lying propensity being common knowledge, i.e., on the accuracy of subjects' beliefs, we begin the analysis with a general overview of subjects' perceived relative ability – the difference between own actual (known) output and the perception of others' output. This measure is important to determine if lower ability subjects select into a contest because they have incorrect beliefs that they are of a high ability or because they wish to lie more. In order to see that the two measures are very similar, in Figure 1, we graphically present perceived ability by treatment and ability and present, in Table 2, the same data by payment scheme and ability.[12] Low ability subjects are defined as those whose actual output is below the median within each treatment and the remainder are classified as high ability. Note that the categorization of low/high ability is always done according to their actual output in part 1

(Figure 1 about here)

(Table 2 about here)

[11]Because of the nature of the question and the experimental design, we were not able to incentivize the last part of the experiment. Nonetheless, we find consistencies in the data which leads us to conclude that subjects answered this question truthfully.

[12]A McNemar's test confirms there is no difference in the distribution of perceived ability at the session or treatment level.

The main finding from Table 2 and Figure 1 is that the subjects basically get their relative ranking correct.[13] The high ability subjects in both pay schemes believe they are better than the average subject. Likewise, the low-ability subjects believe they are worse than average. Beyond the simple averages, we also note that 73% of the subjects who are low ability think they are average or worse and 86% of those who are high ability think they are average or better. Even though subjects did not receive feedback about their outcome relative to others, it is likely that they have a basic idea of where they stand because of experience through their schooling. Even though subjects correctly estimate their relative ability, their beliefs are not completely accurate. Low ability subjects underestimate others' ability whereas high ability subjects overestimate others' ability. So, going forward, we will use perceived relative ability rather than actual ability because perceived ability is a better predictor of subjects' actions. Nonetheless, there is a strong correlation between perceived relative ability and actual relative ability. In a one-variable OLS regression of actual relative ability on perceived relative ability, the slope estimate is 0.977 with $p < 0.01$. Actual relative ability is defined as the difference between own output and the median output within a session.

5.2 Institutional Effects on Lying

We will now focus on lying behavior sans selection concerns; i.e., we will compare the PR-PR with the PR-C treatments. Table 3 displays the summary statistics on the amount of lying, pooled by perceived ability, in each pay scheme. For ease of exposition, we use a binary classification in the summary statistics. In the more careful regression analysis that follows, we will use a continuous variable. 51.5% of the sample was classified as high ability and the remainder as low ability.

(Table 3 about here)

From Table 3 we can see that the level of lying in Part 1 is quite low, but the experiment average of 0.58 is statistically different from zero ($p < 0.01$). We also observe that there is no difference in the amount of lying in Part 1 by perceived ability ($p = 0.426$)[14]. In Part 2, subjects increased the number of correctly solved problems when compared to Part 1, but breaking this down by ability we see that this only holds for low-ability subjects in the PR-PR setting. As in Part 1, we see once again that there is no difference in the amount of lying in the PR-PR setting by perceived ability ($p = 0.461$). This is not true in the contest setting as the amount of lying by low-ability subjects in the contest is much greater than high-ability subjects in the same setting ($p < 0.01$). Not only is there a difference in absolute terms, but the amount of lying by

[13] This does not mean that we do not observe overconfidence. On the contrary, looking at the first rows for each pay scheme in Table 1, we generally observe overconfidence because the average perception of own ability is statistically greater than others' in 3 of 4 instances.

[14] Unless otherwise mentioned, the pairwise comparisons are the result of a t-test.

low-ability subjects is about 61% of the number of problems they solved correctly which is much higher than the 4% for the high-ability subjects. These simple comparisons imply that the contest induces more lying than the PR setting and this effect is intuitively driven by perceived ability.

Though the above is useful as a first look at lying, we will now turn to a more detailed regression analysis in Table 4 to establish our first results. The dependent variable in our regressions is how much lying a subject engaged in. In column 1, which examines the level of lying in Part 1, the main explanatory variable is *perceived relative ability.* Also of chief concern for us is the variable *contest* (=1 if the subject was in the contest pay scheme). We include this variable even in part 1 to show that the randomization of subjects to treatments is valid. To control for other aspects of relevant behavior, we also include *keep* (how much was kept in the dictator game), *invest* (how much was invested in the risk game; a higher amount invested indicates less risk aversion), *impatience* (the result of a survey question where 10 is the most impatient), *competitiveness* (the result of a survey question where 10 is the most competitive), *religiosity* (the result of a survey question where 10 is the most religious), *trust* (the result of a survey question where 10 indicates they trust others the most), *age* (the subject's age in years) and *male* (=1 if male). The model in column 2, which examines lying in Part 2, includes an explanatory variable *amount of lying in part 1* (the amount the subject misreported in Part 1) which serves as an additional control, whereas the model in column 3 includes interactions of *contest* with *perceived ability* and *invest.* Including the interaction of *invest* with *contest* is important given that the amount of lying in Part 2 is not correlated with the amount invested in the investment game in the PR-PR setting ($p = 0.78$) but it is correlated in the PR-C setting ($p = 0.02$).

(Table 4 about here)

The results in the regression confirm what is shown in Table 3. Column 1 highlights that there is no difference in lying behavior in Part 1 by treatment or perceived ability. This is consistent with our theoretical predictions and justifies our use of a model in which effort is not a choice variable. Turning to Part 2, the positive and significant sign on *contest* in Column 2 shows that the contest induces more lying whereas the negative and significant sign on the variable for *perceived relative ability* shows the negative correlation between ability and the amount of lying. Column 3 explains why there is more lying in contests. The negative and significant coefficient estimate on the interaction term *perc. rel. ability*×*contest* and the now insignificant effect of *perceived relative ability* implies there is no effect of ability on lying in Part 2 PR and more lying by low-ability subjects in the contest; i.e., once subjects are in the contest, strategic lying occurs. We can also see by the positive and significant effect *invest*×*contest*, and the resulting insignificant effect of *invest*, that risk-seeking also leads to more lying in the contest. This leads to our first result.

Result 1: Contests induce more lying than a piece-rate setting. This effect is driven by lower ability and less risk-averse subjects who lie more in the contest.

Result 1 shows that low-ability subjects lie more in contests, even after controlling for individual characteristics such as risk preferences. If a subject lies more because they think others are lying more, this results in the ratcheting effect often observed. Columns 4-6 in Table 4 presents regression analysis where the dependent variable is once again the amount of lying a subject engaged in. The three additional models now include the explanatory variable *expected lying* in each part and, in Model 6, an interaction of *expected lying* with *contest*.

Looking first at columns 4 and 5, we see that the beliefs about others lying behavior affect the amount of lying in a predictable manner: those who thought others were lying more also lied more as shown by the positive coefficient on the variable *expected lying*. The model in column 6 explains this behavior more precisely. In Part 2, the positive and highly significant coefficient on *expected lying*×*contest* and the insignificant effect on *expected lying* implies that expected lying affected those in the contest much more than those in the piece-rate setting.[15] This leads to our second result which further explains why lying in a contest is higher.

Result 2: Subjects in a contest lie more if they believe others are also lying more.

In summary, we have found that there is more lying in the contest due to perceived relative ability, beliefs of others lying, and risk preferences. The presence of strategic lying, i.e., the dependence of lying on perceived relative ability, is robust to these controls as evidenced by the significant effect of *perc. ability*×*contest* in column 6 of Table 4.

We note that Results 1 and 2 are also consistent with the notion of *context-specific* costs of lying. Indeed, subjects' moral costs of lying may decrease in a contest environment if they believe that lying in contests is more socially acceptable. Although we cannot exclude this explanation with our data, we note that, first, our theoretical predictions would only be enhanced if costs of lying in contests were lower; and second, the presence of a robust dependence of the level of lying in the contest on ability (and the absence of such a dependence under the piece-rate scheme) indicates that context-specific costs of lying cannot fully explain the increase in lying in the contest environment. Result 2 is also indicative of what may happen in a dynamic environment; a thought we expand on in the final section.

[15]This variable is only marginally significant in Part 1 (column 4 of Table 4) but is not significant in Part 2 (column 6 of Table 4).

5.3 Selection

We will now turn to selection effects and restrict our analysis to the PR-CS treatment. In the prior section we found that ability and norm-related motives affect the amount of lying. In this section, we aim to understand if the same motives are considered when choosing a pay scheme. We begin with a general outline.

When given the choice, about 40% of the subjects chose the contest, but, surprisingly, we find no differences in perceived ability between those who chose the contest and those who chose the piece-rate pay scheme ($p = 0.324$). We also find no difference in actual ability ($p = 0.820$). Additionally, we find that there is no difference in perceived relative ability between Part 1 and Part 2 of those subjects who chose the PR setting ($p = 0.817$); however, the same is not true for those who chose the contest pay scheme. Those who chose the contest believed their relative ability in Part 1 was higher than in Part 2 ($p = 0.035$). To see this point more clearly, Figure 2 shows the cumulative distribution of perceived ability in both the piece-rate (line with squares) and contest (line with circles). The left panel highlights those who chose the piece-rate whereas the right panel details those who chose the contest. On the horizontal axis is the perceived relative ability where negative (positive) numbers imply the subject believed they were worse (better) than the average subject in a given pay scheme. As alluded to previously, a couple of things stand out from this figure. First, it is shown that subjects believed their relative ability was higher in the piece-rate setting than the contest setting; i.e., subjects thought those who chose the contest had a higher ability. The second noticeable finding is that about 41% of the subjects who chose the piece-rate thought they were just as good or better than the average person in the contest and about 45% of the subjects who chose the contest thought they were just as good or worse than the average person who chose the contest. In other words, quite a few subjects who should be selecting into the contest are opting out whereas quite a few who should be opting out are entering. This implies that perceived ability is not the only metric subjects used when deciding whether to enter the contest.

(Figure 2 about here)

To expand the analysis beyond perceived ability, Table 5 presents summary statistics of subjects' beliefs about others' lying behavior. These beliefs could help explain subjects' selection behavior because subjects may be less willing to enter a pay scheme the more lying they believe occurs in it. The first two columns display the subjects' beliefs about how much lying will occur in each setting and the last column is a t-test for the difference in the beliefs. As a reminder, regardless of the pay scheme chosen, we asked the subjects their beliefs for the contest pay scheme and the piece-rate pay scheme – the table reflects both sets of beliefs.

(Table 5 about here)

One important finding in this table is that high-ability subjects who chose the piece-rate thought lying would be more prevelant in the contest setting than the piece-rate setting. No other group of subjects thought there would be a difference in the amount of lying between the piece-rate or the contest. This serves as a possible explanation for the selection effects. High-ability subjects who should be entering the contest are staying out because of non-ability related reasons – possibly, their aversion to the rampant lying they believe occurs in the contest. The same cannot be said of the low-ability subjects who chose the contest: they believed they were low ability but entered the contest anyway. Once in, they lied much more than they thought others would. To put these suggestions on firmer ground, we turn once again to regression analysis.

Table 6 presents marginal effects probit estimates where the dependent variable is the probability of choosing the contest. In the first column we include perceived relative ability in each pay scheme while in column 2, we include *perceived lying (in contest)* and *perceived lying (in piece-rate)* which is a subject's perception of how much lying occurred in each pay scheme. The other controls are as described previously.

(Table 6 about here)

There are a couple of noteworthy findings in columns 1 and 2. First, a subject's perceived relative ability in the PR is more important than their perceived relative ability in the contest. The direction of the effect is positive, as expected, meaning the better the subjects thought they were, the more likely they were to enter the contest. We also see in column 2 that the amount of perceived lying in the piece-rate and the contest is strongly correlated with the subject's choice.[16] A one unit increase in the perceived amount of lying in the contest (piece-rate) results in a 4.6% decrease (5.8% increase) in the probability a subject chose the contest. Notice that this is after accounting for a subject's competitive attitudes (*competitiveness*), other-regarding behavior (*keep*), risk preferences (*invest*) and other factors. This leads to our third result.

Result 3: The less lying a subject believes occurs in a contest, and the better she thinks she is than those in the piece-rate, the more likely she is to enter the contest.

Subjects' general propensity to lie is given by the variable *amount of lying in part 1* which accounts for how much a subject was willing to lie absent the contest framework. From both models in Table 6, we find no evidence that a subject's general propensity to lie affects their selection into the contest.

[16] We observe some regularities that are consistent with subjects answering the beliefs questions truthfully. First, high and low ability subjects basically get their relative ranking correct. If they were answering untruthfully, this is unlikely. Second, the amount of lying they report others engage in is either (a) mostly correct or (b) is in line with theory. If they were using the belief elicitation as self-justification for own lying, they would surely say that others lied much more than them. This is rarely the case except for high ability subjects who selected into the PR and then they do not lie very much. Low ability subjects in the contest reported that others actually lied *less* than them ($p = 0.05$ for those in the contest with no choice and $p = 0.09$ for those in the contest who chose the contest). These regularities are in line with the strategic considerations as we have modeled.

Result 4: Subjects' propensity to lie in the piece-rate setting does not affect the probability of selection into the contest.

We also observe from Table 6 that contests attract less risk-averse subjects, as captured by the positive and highly significant coefficient on the variable *invest*.

5.4 Misreporting in Contests by Treatment

We now turn our attention to uncovering differences in lying in the two contest treatments when selection is and is not allowed. A simple pairwise comparison of the average amounts of lying in the contests of treatments PR-C and PR-CS produces no statistically significant difference (5.04 in PR-C vs. 2.37 in PR-CS; $p = 0.146$). It is of interest, however, to control for individual-level heterogeneity because subjects who selected into the contest in PR-CS may be different from the general population in PR-C.

As mentioned in the previous section, less risk-averse subjects are more likely to select in the contest. The amount invested in the risky gamble is about €0.26 more in the contest of PR-CS treatment as compared to the contest of PR-C (1.51 in PR-C vs 1.77 in PR-CS; $p = 0.024$). At the same time, averaging over all subjects in the two treatments, there is no difference in the amount invested ($p = 0.948$). This implies that the difference we find is due to selection. We find no other differences between the two contests, most notably there is no difference in the contestants' perceived ability or expectation of others lying.

Table 7 shows the results of regression analysis. In column 1, the dependent variable is the amount of lying in Part 2 and the primary explanatory variable is *contest selection* (=1 if treatment is PR-CS). The other control variables are as described previously. Given that risk-aversion was shown to correlate with lying in the PR-C and that risk-averse subjects could select out of the contest in the PR-CS, but not in the PR-C, the model in column 2 replaces the single variable for risk-aversion with an interaction of risk aversion with PR-CS and PR-C.[17] To check for robustness, in Model 3 we include expectations of others' lying behavior.

(Table 7 about here)

From Model 1, we see that in an exogenously imposed contest, perceived relative ability and risk preferences are important for explaining the amount of lying in the contest. From Model 2, we see that risk aversion is only significant for those subjects who in the exogenously imposed contest and once these interactions are included, the significant effect on the PR-CS treatment goes away. Model 3 confirms the robustness of our final result.

[17] Note that the same results can be obtained with a post estimation Wald test. We prefer the current form due to the ease of presentation.

Result 5: When subjects are allowed to self-select into a contest, they lie less, ceteris paribus, than when they are placed into it exogenously. This effect is primarily explained by risk preferences.

Result 5 is consistent with prior studies which found that allowing subjects to select a preferred mechanism led them to take more responsibility for their actions (e.g., Dal Bó, Foster and Putterman, 2010). So, even though subjects lie more in contests, allowing them to choose their preferred mechanism may have an effect of increasing the costs of lying.

6 Discussion and Conclusions

When given an opportunity, people lie more in competitive environments than otherwise. The goal of this study is to understand the foundations of this lying behavior. To do so, we rely on experiments where subjects can lie to earn more money in either a contest setting or a piece-rate setting. Our main treatment conditions revolve around how a subject arrives into the contest setting: they either can or cannot choose the pay scheme they face.

With the possibility of selection, subjects acted as if they have different costs of lying and these costs, coupled with ability, are driving their choice of a pay scheme. More specifically, the higher the relative amount of perceived lying in the contest, the more likely a subject is to select the piece rate. Likewise, we observed low-ability subjects selecting into the contest who correctly inferred their ability and did not believe much lying would occur in the contest. Once in the contest, they were willing to lie at a very high rate. This led to our somewhat surprising result that there were no statistical differences in the measures of ability, perceived or actual, for those who chose the contest and those who chose to stay out. In other words, contests where lying is possible do not select out the best performers.

We find that in our environment subjects do not needlessly lie as we observe very little lying in the piece-rate setting (Gneezy, 2005), and no "learning to lie" over time (no differences in the amount of lying between the first and second parts of the PR-PR treatment). However, when in the contest, subjects will bear the intrinsic costs of lying in exchange for the external reward, more so the lower their perceived relative ability. This is consistent with some anecdotal evidence. For example, a recent Wall Street Journal article reported a correlation between the number of times a broker failed a basic exam and the number of "black marks" on their record.[18] A secondary, and expected, finding is that a subject's level of lying is positively correlated with her beliefs about others' lying behavior.

Interestingly, we find no evidence that subjects who lie more in the piece-rate setting are more likely

[18] See the article "Brokers who failed test have checkered records" (The Wall Street Journal, April 15, 2014). The black marks include, among other things, criminal charges and/or customer complaints from unauthorized trading.

to enter the contest. We also find no correlation between the selfishness of a subject, as measured by the amount they allocated in a dictator game, and their contest entry decision. This goes against the common wisdom that contests attract intrinsically "bad" people and places more weight on the institutional effects of contests.

Though we only study a simple static framework, our results are suggestive of what may happen in a dynamic setting. Perceived lying by others and own willingness to lie, in conjunction with perceived ability, are determinants of contest entry. Once in the contest, we find that the perceived amount of lying in a contest is positively correlated with own lying behavior. These results suggest that, over time, one may observe the ratcheting effect of wide-spread cheating that clouds many professional sports, politics and other competitive settings..

Finally, our results suggest other directions for the future research on the mechanisms aiming to reduce such instances of cheating in contests. Specifically, future studies on contest design could focus on the information available to the contestants and also work on mechanisms which reduce the riskiness of the contest. The last point stems from our finding that lying and contest entry are more prevalent for the less risk-averse. Of course, the problem of lying in contests can be eliminated completely if an appropriately severe and inevitable punishment mechanism is in place; however, such mechanisms are often not feasible.[19] For example, recent scandals with automakers misreporting technological issues with their vehicles (e.g., most recently,Volkswagen, Honda, Kia, Toyota, Hyundai and GM have been hit with large fines), banks involved in shady transactions (Bank of America, J.P. Morgan Chase, Citigroup, UBS, Wells Fargo), or the British Petroleum oil spill in the Gulf of Mexico, all resulted in the companies paying fines and essentially continuing to operate as usual. For environments where regulation enforcement is weak and punishment for cheating, even if it is imposed, is tolerable, our results suggest that between two punishment mechanisms with equal expected payouts the regulator should prefer the one with less risk, i.e., with a higher probability of detection and lower fine (as opposed to a mechanism with a lower probability of detection and higher fine). More generally, the whole calculus of the efficiency of probabilistic punishment mechanisms may change when endogenous selection into contests is taken into account. There is also a dynamic effect which could come into play. Even if those in the contest believe that the punishment mechanism discourages lower-ability subjects from entering, they should update their perception of their own relative ability. If updating occurs correctly, our results indicate that the incentive to lie also increases. We believe a very promising avenue of future research is to test the implications of our study and further examine the interaction of punishment

[19]This is an unlikely mechanism for several reasons. The nature of cheating and the cost of detection often prohibit detection with certainty. In many contests, elimination from the contest may be seen as a disproportionate response to the infraction committed. It may also be the case that contestant elimination leads to other adverse effects when the contest is made up of sufficiently few contestants– leading to an industry closer to that of a monopoly – or when the contestant being eliminated comprises a large portion of the economy.

mechanisms with selection.

7 Acknowledgements

We would like to thank David Cooper, Matthias Sutter and seminar participants at the French Experimental Economics Association Meeting, Lyon (2013), International Economic Science Association Meeting, Zurich (2013), North American Economic Science Association Meeting, Santa Cruz (2013) and Texas Experimental Association Symposium (TExAS), College Station (2014) for helpful comments. We are also grateful for funding support from a grant from the Forschungsförderpreis der Hypo Tirol Bank 2012 and Tiroler Wissenschaftsfonds (Project 0404/1440)

8 References

Berentsen, Aleksander. 2002. “The Economics of Doping.” European Journal of Political Economy, 18(1): 109-127.

Balafoutas, Loukas, Florian Lindner and Matthias Sutter. 2012. "Sabotage in Tournaments: Evidence from a Natural Experiment" Kyklos, 65(4):425-441.

Belot, Michéle, and Marina Schröder. 2013. "Sloppy work, lies and theft: A novel experimental design to study counterproductive behaviour." Journal of Economic Behavior & Organization 93: 233-238.

Camerer, Colin, and Dan Lovallo. 1999. “Overconfidence and Excess Entry: An Experimental Approach.” The American Economic Review, 89(1): 306-318.

Carpenter, Jeffrey, Peter Hans Matthews, and John Schirm. 2010. “Tournaments and Office Politics: Evidence from a Real Effort Experiment.” The American Economic Review, 100(1): 1-18.

Cason, Timothy N., William A. Masters, and Roman M. Sheremeta. 2010. “Entry into winner-take-all and proportional-prize contests: An experimental study.” Journal of Public Economics, 94(9-10): 604-611.

Chen, Kong-Pin. 2003. “Sabotage in Promotion Tournaments.” Journal of Law, Economics, & Organization, 19(1): 119-140.

Datta Gupta, Nabanita, Anders Poulsen, and Marie-Claire Villeval. 2013. “Gender Matching and Competitiveness: Experimental Evidence.” Economic Inquiry, 51(1): 816-835.

Dohmen, Thomas, and Armin Falk. 2011. “Performance Pay and Multidimensional Sorting: Productivity, Preferences, and Gender.” The American Economic Review, 101(2): 5565-590.

Ehrenberg, Ronald G., and Michael L. Bognanno. 1990. "Do Tournaments Have Incentive Effects?" Journal of Political Economy, 98(6): 1307-1324.

Eriksson, Tor, Sabrina Teyssier, and Marie-Claire Villeval. 2009. "Self-Selection and the Efficiency of Tournaments." Economic Inquiry, 47(3): 530-548.

Faravelli, Marco, Lana Friesen, and Lata Gangadharan. 2015. "Selection, tournaments, and dishonesty" Journal of Economic Behavior and Organization, 110: 160-175.

Fischbacher, U. (2007). "Z-tree: Zürich toolbox for readymade economic experiments." Experimental Economics 10: 171-178.

Fullerton, Richard L., and R. Preston McAfee. 1999. "Auctioning Entry into Tournaments." Journal of Political Economy, 107(3): 573-605.

Garicano, Luis, and Ignacio Palacios-Huerta. 2005. "Sabotage in Tournaments: Making the Beautiful Game a Bit Less Beautiful." Brown University Working Paper.

Gino, Francesca, Erin L. Krupka, and Roberto A. Weber. 2013. "License to cheat: Voluntary regulation and ethical behavior." Management Science 59(10): 2187-2203.

Gilpatric, Scott M. 2011. "Cheating in Contests." Economic Inquiry, 49(4): 1042-1053.

Gino, Francesca and Lamar Pierce. 2009. "The Abundance Effect: Unethical Behavior in the Presence of Wealth." Organizational Behavior and Human Decision Processes. 109: 142-155.

Gneezy, Uri. 2005. "Deception: The Role of Consequences" The American Economic Review. 95(1): 384-394.

Harbring, Christine, Bernd Irlenbusch, Matthias Krakel and Reinhard Selten, 2007. "Sabotage in Corporate Contests - An Experimental Analysis," International Journal of the Economics of Business, 14(3): 367-392

Harbring, Christine and Bernd Irlenbusch. 2008. "How Many Winners are Good to Have? On Tournaments with Sabotage." Journal of Economic Behavior and Organization, 65(3): 682-702

Harbring, Christine, and Bernd Irlenbusch. 2011. "Sabotage in Tournaments: Evidence from a Laboratory Experiment." Management Science, 57(4): 611-627.

Hoffman, McCabe, Shachat and Smith. 1994. "Preferences, Property Rights, and Anonymity in Bargain Games." Games and Economic Behavior, 7(3): 346-380.

Hvide, Hans K., and Eirik G. Kristiansen. 2003. “Risk Taking in Selection Contests.” Games and Economic Behavior, 42(1): 172-179.

Konrad, Kai A. 2000. “Sabotage in Rent-Seeking Contests.“ The Journal of Law, Economics, & Organization, 16(1): 155-165.

Krakel, M. 2007. “Doping and Cheating in Contest-Like Situations,” European Journal of Political Economy, 23: 988-1006.

Lazear, Edward P. 2000. “Performance Pay and Productivity.” The American Economic Review, 90(5): 1346-1361.

Lazear, Edward P. 1989. “Pay Equality and Industrial Politics.“ Journal of Political Economy, 97(3): 561-580.

Lazear, Edward P., and Paul Oyer. 2012. Personnel Economics.

Lazear, Edward P. and Sherwin Rosen. 1981. "Rank-Order Tournaments as Optimum Labor Contracts." The Journal of Political Economy, 89(5): 841-864 *The handbook of organizational economics*, 479.

Niederle, Muriel, and Lise Vesterlund. 2007. “Do Women Shy Away from Competition? Do Men Compete too Much?” The Quarterly Journal of Economics, 122(3): 1067-1101.

Piff, Paul, Daniel Stancato, Stepane Cote, Rodolfo Mendoza-Denton and Dacher Keltner. 2012. “Higher Social Class Predicts Increased Unethical Behavior.” Proceedings of the National Academy of Science, 109(11): 4086-4091.

Preston, Ian and Stefan Szymanski. 2003. “Cheating in Contests.” Oxford Review of Economic Policy, 19(4): 612-624.

Ryvkin, Dmitry. 2013. “Contests with doping.” Journal of Sports Economics, 14(3): 253-275.

Schwieren, Christiane, and Doris Weichselbaumer (2010). "Does Competition Enhance Performance or Cheating? A Laboratory Experiment. Journal of Economic Psychology, 31(13): 241-253

Sutter, Matthias, and Daniela Glätzle-Rützler Forthcoming. Gender Differences in the Willingness to Compete Emerge Early in Life and Persist. Management Science.

Tullock, Gordon., 1980. “Efficient Rent Seeking,” in J.M Buchanan, R.D. Tollison and G. Tullock (eds.), Toward a Theory of the Rent-Seeking Society (College Station: Texas A&M Press).

9 Tables and Figures

9.1 Tables

	PR-PR	PR-C	PR-CS
Number of Ss	50	50	96
Part 1	Piece rate - 5 min.	Piece rate - 5 min.	Piece rate - 5 min.
Part 2	Piece rate - 5 min.	Forced contest - 5 min.	Contest selection - 5 min.
Part 3	Dictator game	Dictator game	Dictator game
Part 4	Risk elicitation	Risk elicitation	Risk elicitation
Part 5	Belief elicitation	Belief elicitation	Belief elicitation

Table 1: Experimental Design and Treatments.

	Part 1 (piece rate)		t-test	Part 2 (piece rate or contest)		t-test
	Own output	Beliefs about others' output	p-value	Own output	Beliefs about others' output	p-value
Piece-Rate All	19.111	17.753	0.004	20.176	18.676	0.003
Piece-Rate Low-ability	13.714	15.276	0.000	15.469	16.551	0.030
Piece-Rate High-ability	23.593	19.810	0.000	24.085	20.441	0.000
Contest All	18.148	16.668	0.007	19.807	19.392	0.567
Contest Low-ability	12.659	13.591	0.056	14.568	16.284	0.056
Contest High-ability	23.636	19.745	0.000	25.045	22.500	0.003

Table 2: Own true output and the belief about the others' true output by pay scheme and own true ability. The categorization of low or high ability is by actual ability. All data is reported in this table (196 subjects). The top panel is PR-PR and PR-CS (those who chose PR) and the bottom panel is PR-C and PR-CS (those who chose C). Reported p-values for "All" are the result of a two-sided test while the p-values for ability comparisons are the results of a one-sided test.

	Part 1 (piece rate)		Part 2 (piece rate or contest)		t-test for differences between Part 1 and Part 2: p-value	
	Correctly solved	Amount of lying	Correctly solved	Amount of lying	Correctly solved	Amount of lying
Piece-Rate All	20.08	0.46	21.34	0.72	0.037	0.555
Piece-Rate Low-ability	15.74	0.26	18.17	0.35	0.013	0.851
Piece-Rate High-ability	23.78	0.63	24.04	1.04	0.726	0.575
Contest All	17.82	0.80	19.18	5.04	0.032	0.003
Contest Low-ability	14.08	1.54	15.83	9.58	0.126	0.005
Contest High-ability	21.27	0.12	22.27	0.85	0.121	0.116

Table 3: Summary statistics for ability and amount of lying by pay scheme. Only observations from PR-PR and PR-C are used. Ability is defined as perceived relative ability. Reported p-values are the result of a two-sided test.

Amount of Lying	(1)	(2)	(3)	(4)	(5)	(6)
	Part 1	Part 2	Part 2	Part 1	Part 2	Part 2
perceived relative ability	-0.128	-0.450***	-0.016	- 0.155	-0.523***	-0.014
	(0.093)	(0.144)	(0.120)	(0.099)	(0.119)	(0.116)
contest	0.418	3.651***	-0.436	0.305	1.426	-1.814
	(0.725)	(1.235)	(2.097)	(0.684)	(0.935)	(1.884)
expected lying				0.265*	0.851***	0.221
				(0.138)	(0.134)	(0.199)
expected lying×contest						0.750***
						(0.227)
perc. rel. ability×contest			-0.695***			-0.766***
			(0.216)			(0.164)
amount of lying in part 1		0.427***	0.376***		0.483***	0.429***
		(0.126)	(0.132)		(0.091)	(0.078)
keep (selfishness)	0.416*	0.942	0.482	0.399	0.629	0.137
	(0.248)	(0.786)	(0.743)	(0.248)	(0.563)	(0.527)
invest (risk seeking)	0.361	2.334***	0.247	0.306	1.713**	0.110
	(0.578)	(0.868)	(0.890)	(0.560)	(0.752)	(0.805)
invest×contest			3.200**			2.377
			(1.585)			(1.466)
impatience	0.161	-0.096	-0.049	0.163	-0.032	0.049
	(0.144)	(0.296)	(0.275)	(0.142)	(0.247)	(0.222)
competitiveness	0.139	0.208	0.117	0.119	-0.142	-0.222
	(0.090)	(0.279)	(0.251)	(0.089)	(0.263)	(0.237)
religiosity	0.151	-0.132	-0.180	0.172	0.043	-0.007
	(0.156)	(0.230)	(0.234)	(0.161)	(0.193)	(0.198)
male	-0.214	-0.126	0.824	-0.317	0.348	1.458
	(0.441)	(1.847)	(1.715)	(0.453)	(1.481)	(1.447)
Constant	-0.455	-4.086	0.390	-0.263	-1.780	2.037
	(1.567)	(5.854)	(5.421)	(1.540)	(4.915)	(4.566)
Observations	100	100	100	100	100	100
R-squared	0.155	0.361	0.457	0.187	0.603	0.714

Table 4: Results of OLS regressions for the amount of lying in Parts 1 and 2. Robust standard errors in parentheses. Significance level: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The variables for age and trust are included in the regression, but not in the table so the table will fit on one page.

Beliefs of others lying				
		Piece-Rate	Contest	t-test for differences in beliefs
Selected PR	All	0.65	2.99	0.006
	Low ability	0.85	1.86	0.130
	High Ability	0.37	4.48	0.021
Selected Contest	All	1.11	1.08	0.916
	Low Ability	1.03	0.80	0.487
	High Ability	1.17	1.27	0.825

Table 5: Summary statistics for beliefs of subjects in Part 2 when subjects could choose their preferred pay scheme.

Probability to enter the contest	(1)	(2)
perceived lying		-0.046**
(in contest)		(0.020)
perceived lying		0.058**
(in piece-rate)		(0.026)
perceived relative ability	-0.030*	-0.019
(in contest)	(0.016)	(0.016)
perceived relative ability	0.049***	0.043***
(in PR)	(0.016)	(0.016)
amount of lying in part 1	-0.040	-0.046
	(0.026)	(0.053)
keep (selfishness)	-0.034	-0.049
	(0.0693)	(0.0671)
invest (risk preference)	0.327***	0.302***
	(0.0100)	(0.105)
impatience	0.029	0.033
	(0.020)	(0.020)
competitiveness	0.013	0.012
	(0.026)	(0.026)
religiosity	-0.001	-0.003
	(0.020)	(0.020)
trust	-0.017	-0.030
	(0.025)	(0.026)
age	-0.00936	-0.00576
	(0.019)	(0.019)
male	0.025	0.001
	(0.124)	(0.125)
Observations	95	95
Pseudo R-squared	0.219	0.254

Table 6: Marginal effects probit estimates for the probability a subject chooses the contest pay scheme. One observation is dropped due to unanswered survey questions. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Amount of Lying	(1)	(2)	(3)
	Part 2	Part 2	Part 2
contest selection	-3.263*	3.466	4.616
	(1.742)	(3.615)	(3.999)
perceived relative ability	-0.637***	-0.640***	-0.702***
	(0.141)	(0.142)	(0.102)
invest (risk seeking)	2.907**		
	(1.142)		
invest×forced contest		3.615***	2.677**
		(1.278)	(1.087)
invest×contest selection		-0.329	-0.817
		(2.091)	(1.971)
expected lying			0.928***
			(0.090)
amount of lying in Part 1	0.391**	0.347*	0.370***
	(0.161)	(0.175)	(0.109)
keep (selfishness)	0.523	0.579	0.425
	(0.861)	(0.855)	(0.561)
impatience	0.026	0.033	0.011
	(0.299)	(0.297)	(0.238)
competitiveness	-0.048	-0.078	-0.305
	(0.373)	(0.367)	(0.293)
religiosity	-0.322	-0.309	-0.024
	(0.257)	(0.260)	(0.197)
trust	0.183	0.077	-0.060
	(0.358)	(0.356)	(0.288)
age	-0.181	-0.187	-0.099
	(0.170)	(0.169)	(0.146)
male	1.259	1.740	1.762
	(2.125)	(2.119)	(1.697)
Constant	2.596	2.009	0.607
	(6.840)	(6.896)	(5.135)
Observations	88	88	88
R-squared	0.409	0.417	0.738

Table 7: OLS regression results for the amount of lying in the Part 2 contests of the PR-C and PR-CS treatments. Robust standard errors in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

9.2 Figures

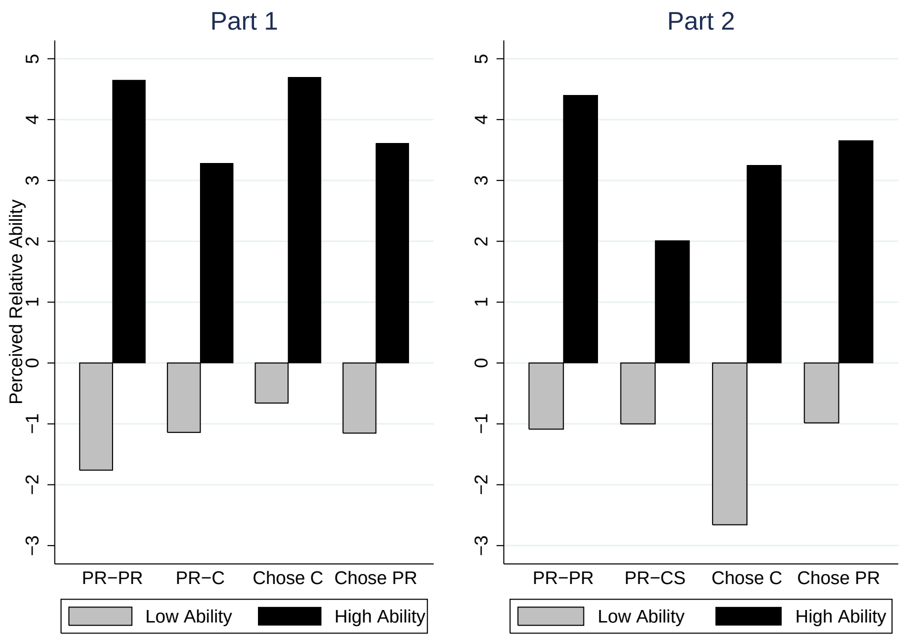


Figure 1: Perceived relative ability by actual ability and treatment.

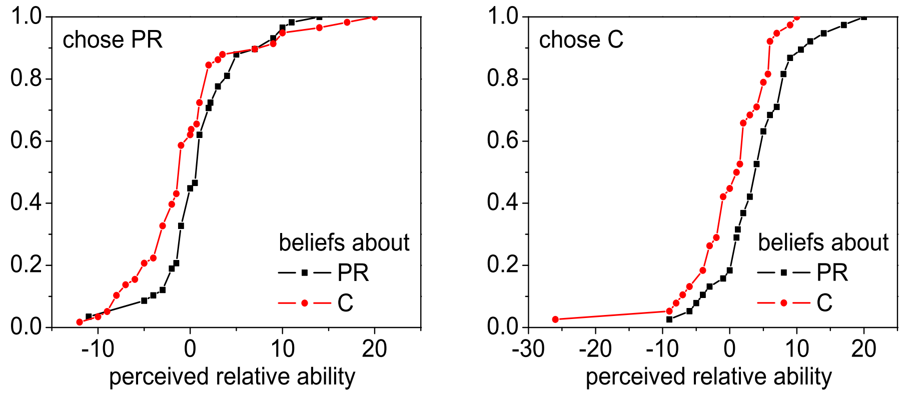


Figure 2: The cumulative distribution of subjects' beliefs about their relative ability for those who chose PR (left) and those who chose C (right).

10 Appendix

10.1 Experimental Instructions (items in italics were only on the experimenter's instructions)

Experimental Instructions Welcome to this experiment on decision making! We thank you for your participation. You will have the chance to earn money based on your decisions and the decisions of others. All decisions and answers will remain confidential and anonymous. To ensure anonymity, your decisions in the experiment are only linked to your subject number and your payoffs from the experiment will be distributed by someone who is not currently present and who will not observe your decisions. It is extremely important that you put away all materials including external reading material, pens and pencils and turn off your cell phones and any other electronic devices. If you have a question, please raise your hand and I will come by and answer it privately. Also, please do not talk to each other during the experiment.

Today's experiment consists of several parts. The instructions for the first part are given below.

Part 1: Task: In the first part you will be given five minutes in which to correctly solve as many number addition problems as you can. Every problem is finding a sum of five two-digit numbers. You must perform these calculations without the use of a calculator. To get familiarized with the task, please spend a little time trying to solve the following two examples. These examples are not paid and are merely here for you to practice. Feel free to write on this sheet using the pen on your desk.

51	95
12	44
43	55
15	84
90	41

After 30-45 seconds, ask if someone needs more time and if not, announce the answers.

Before the round begins, each of you will be provided with two work sheets, which include 40 problems. Please leave the worksheets upside down on your table and only turn them over when we tell you to do so. When I tell you to start, use the pen on your desk to work through the problems and record the answers on the work sheets. You can use the space next to each problem to perform calculations. After the five minutes are over please stop working on the problems and put down the pen. You will see the remaining time on your computer and we will also tell you, when the five minutes are over. We will then collect all pens, so that nobody will be able to work on the problems any longer. When the time is up to collect the pens we will ask you to pass all pens to the right end of your row.

Payment: At the end of the experiment, we will randomly select either part 1 or part 2 (which follows next) for payment. We have prepared two cards, one which is numbered "1" and one which is numbered "2" which we will let one of you randomly choose from. If the card with the number "1" is chosen, then part 1 will be paid, if the card with the number "2" is drawn, then part 2 will be paid If part 1 is randomly selected for payment, you will receive €0.3 (i.e. 30 Cents) per correct problem solved. If for example, you solve 10 problems correctly, you would earn €3 if Part 1 is randomly chosen for payment.

Are there any questions? (pause) If not, I will pass out the worksheets now.

Hand out work sheets and announce that they may begin; click on the button on the computer screen to start the five minute timer

After five minutes, announce the five minutes is up and they should stop working and collect pens.

In a moment, you will see a computer screen which has an empty box with a label "the number of problems solved correctly." The answers for the problems can be seen by clicking on the button labeled "show answers." At this time, please check your answers and type how many problems you solved correctly. Your payment in this part will be computed by the number you type in. Please note that each answer is labeled with a number which corresponds to the number on the work sheet. Once you are done, please hit the "continue" button. Once everyone has hit the continue button, I will hand out instructions for Part 2.

Part 2 (Piece rate): Task: The task in Part 2 is the same as in Part 1. As in Part 1, you will be handed two work sheets with 40 problems in total. You will be given five minutes to solve as many problems as you can. At the end of the five minutes, you will once again be asked to turn in your pens, check your answers, and report the number of problems you solved correctly.

Payment: The payment in Part 2 is the same as in Part 1. As a reminder, if Part 2 is chosen for payment, you will receive €0.3 (i.e. 30 Cents) per reported problem. If for example, you report 10 problems, you would earn €3 if Part 2 is randomly chosen for payment.

Nothing is changed from Part 1.

Please note that as in Part 1, your payment will be computed by the number you type in after the time is up. Are there any questions? (pause) If not, I will pass out the worksheets now.

Hand out work sheets and announce that they may begin; click on the button on the computer screen to start the five minute timer

After five minutes, announce the five minutes is up and they should stop working and collect pens.

In a moment, you will see a computer screen which has an empty box with a label "the number of problems solved correctly." The answers for the problems can be seen by clicking on the button labeled "show answers." At this time, please check your answers and type how many problems you solved correctly. Your payment in this part will be computed by the number you type in. Please note that each answer is labeled with a number which corresponds to the number on the work sheet. Once you are done, please hit the "continue" button. Once everyone has hit the continue button, I will hand out instructions for Part 3.

Part 2 (Tournament): Task The task in Part 2 is the same as in Part 1. As in Part 1, you will be handed two work sheets with 40 problems in total. You will be given five minutes to solve as many problems as you can. At the end of the five minutes, you will once again be asked to turn in your pens, check your answers, and report the number of problems you solved correctly.

Payment: You will be randomly matched with another person in this room. You will never know who you are matched with and they will never know who you are. If Part 2 is randomly chosen for payment and you report more problems correctly solved than the person you are matched with, then you will receive €0.5 (i.e. 50 Cents) per reported problem. If you report fewer problems than the person you are matched with, then you will receive €0.1 (i.e. 10 Cents) per reported problem. If you and the person you are matched with both report the same number, the two of you will split the total payoff, which means, that you and the person you are matched with will each get €0.3 (i.e. 30 Cents) per reported problem. If for example, you report 10 problems and the person you are matched with reports 9 problems then you would receive 10*€0. 5 = €5 and the person you are matched with would receive 9*€0.1= €0.9 if Part 2 is randomly chosen for payment. If on the other hand the other person reports 11, you would receive 10*€0.1=€1 and the other person would receive 11*€0.5=5.5.

Please note that as in Part 1, your payment will be computed by the number you type in after the time is up. Are there any questions? (pause) If not, I will pass out the worksheets now.

Hand out work sheets and announce that they may begin; click on the button on the computer screen to start the five minute timer

After five minutes, announce the five minutes is up and they should stop working and collect pens.

In a moment, you will see a computer screen which has an empty box with a label "the number of problems solved correctly." The answers for the problems can be seen by clicking on the button labeled "show answers." At this time, please check your answers and type how many problems you solved correctly. Your payment in this part will be computed by the number you type in. Please note that each answer is labeled with a number which corresponds to the number on the work sheet. Once you are done, please hit the "continue" button. Once everyone has hit the continue button, I will hand out instructions for Part 3.

Part 2 (Choice): Task The task in Part 2 is the same as in Part 1. As in Part 1, you will be handed two work sheets with 40 problems in total. You will be given five minutes to solve as many problems as you can. At the end of the five minutes, you will once again be asked to turn in your pens, check your answers, and report the number of problems you solved correctly.

Payment: If Part 2 is randomly chosen for payment, your payment in it will depend on your choice of Option 1 or Option 2. If you choose Option 1, you will be paid in exactly the same manner as in Part 1. That means that if Part 2 is chosen for payment and you choose Option 1, you will receive €0.3 (i.e. 30 Cents) per reported problem. If for example, you report 10 problems, you would earn €3 if Part 2 is randomly chosen for payment and you chose Option 1.

If you choose Option 2, you will be randomly matched with another person in this room who also chose Option 2. You will never know who you are matched with and they will never know who you are. If Part 2 is randomly chosen for payment and you report more problems correctly solved than the person you are matched with, then you will receive €0.5 (i.e. 50 Cents) per reported problem. If you report fewer problems than the person you are matched with, then you will receive €0.1 (i.e. 10 Cents) per reported problem. If you and the person you are matched with both report the same number, the two of you will split the total payoff, which means that you and the person you are matched with will each get €0.3 (i.e. 30 Cents) per reported problem. If for example, you report 10 problems and the person you are matched with reports 9 problems then you would receive 10*€0.5 = €5 and the person you are matched with would receive 9*€0.1= €0.9 if Part 2 is randomly chosen for payment and you chose Option 2. If on the other hand the other person reports 11, you would receive 10*€0.1=€1 and the other person would receive 11*€0.5=5.5.

Please note that as in Part 1, your payment will be computed by the number you type in after the time is up. Are there any questions? (pause) If not, please turn to your screens and select Option 1 or Option 2. Click the Continue button when you make your decision.

I will pass out the worksheets now.

Hand out work sheets and announce that they may begin; click on the button on the computer screen to start the five minute timer

After five minutes, announce the five minutes is up and they should stop working and collect pens.

In a moment, you will see a computer screen which has an empty box with a label "the number of problems solved correctly." The answers for the problems can be seen by clicking on the button labeled "show answers." At this time, please check your answers and type how many problems you solved correctly. Your payment in this part will be computed by the number you type in. Please note that each answer is labeled with a number which corresponds to the number on the work sheet. Once you are done, please hit the "continue" button. Once everyone has hit the continue button, I will hand out instructions for Part 3.

Part 3: Following are instructions for Parts 3 and 4. Similar to Parts 1 and 2 of this experiment, only one Part will be chosen for payments. If, at the end of the experiment, the card with the number "1" is drawn, then Part 3 will be paid out. If instead the number "2" is drawn, then Part 4 will be paid out. In Part 3 you are asked to make a decision that will affect you and a random person who is not currently present in the room and does not participate in this experiment. That person will be selected randomly from among participants of a future experiment in this lab. You and that person you are matched with are each endowed with €2. Your task is to decide whether you want to keep the allocation as is or if you want to change the allocation. There are two boxes on your screen. One labeled "amount for me" and the other labeled "amount for other." You must enter how you wish to allocate this money by typing numbers into these boxes, which must sum to €4, in €0.1 (i.e. 10 Cents) increments. This means that you can either take away some/all of the money from the person you are matched with or you can give some/all of your money to the person you are matched with. Please note that the other person has no choice in this decision.

Part 4: As a reminder, this part is only paid out if we randomly select the card with the number “2” at the end of this experiment. In Part 4, you are endowed with €2. Your task is to decide how much of this money (from €0 to €2 in €0.1 increments) you want to invest into the following lottery.

If you win the lottery you get €2 + your invested amount*1.5 If you lose the lottery you get €2 – your invested amount

There is a 50% chance that you will win the lottery which is determined by the computer. This means that the computer will randomly draw a number between 1 and 100 and if the number drawn is greater than 50, you will win the lottery. Otherwise, you will lose the lottery.

As an example, if you invest €1.4 in the lottery you will get €4.10 (2+1.4*1.5) if you win the lottery and €0.6 (2-1.4) if you lose the lottery.

Full examples are given in the following table:

Your Investment	Your Payment if you win	Your payment if you lose
€0.0	€2.0	€2.0
€0.1	€2.2	€1.9
€0.2	€2.3	€1.8
€0.3	€2.6	€1.7
€0.4	€2.8	€1.6
€0.5	€3.0	€1.5
€0.6	€3.2	€1.4
€0.7	€3.4	€1.3
€0.8	€3.6	€1.2
€0.9	€3.8	€1.1
€1.0	€4.0	€1.0
€1.1	€4.2	€0.9
€1.2	€4.4	€0.8
€1.3	€4.6	€0.7
€1.4	€4.8	€0.6
€1.5	€5.0	€0.5
€1.6	€5.2	€0.4
€1.7	€5.4	€0.3
€1.8	€5.6	€0.2
€1.9	€5.8	€0.1
€2.0	€6.0	€0.0

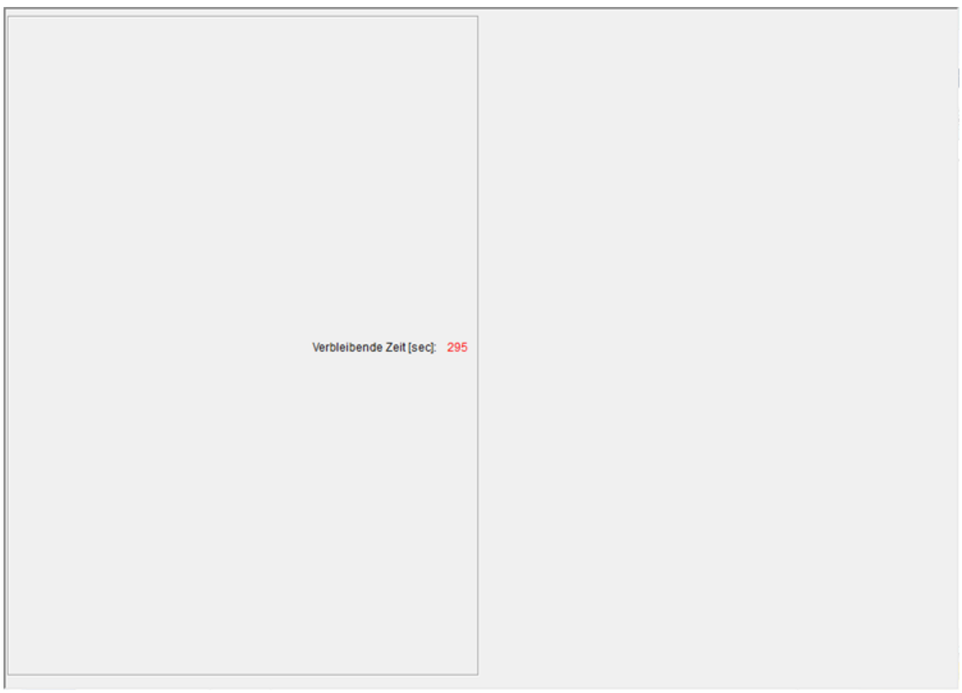
Part 5:

Task: For Part 5, your task is to correctly guess how many problems on average all participants in this room have reported in Part 1 and Part 2.

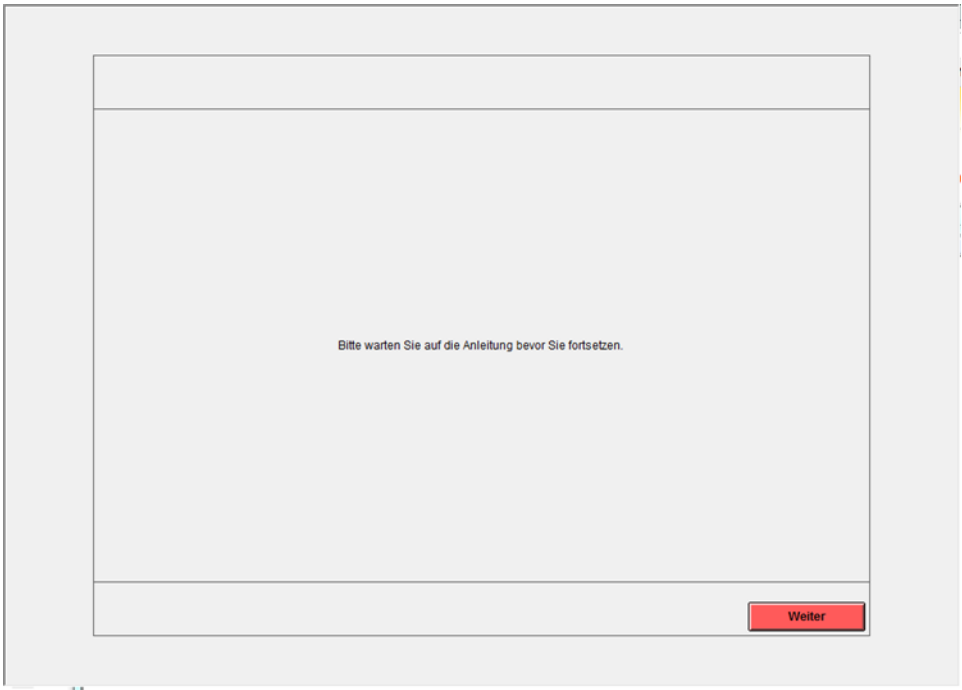
Payment: You will get paid for your guess regarding the Part which will be randomly chosen for payment. Your payment is based on how close your guess is to the average. If your guess for the randomly chosen Part is +/- 1 the true average, you will receive an extra €2 while if your guess is +/- 2, you will receive €1, otherwise you will get 0.

(*Note, in treatment choice we need to ask in part 2 separately for piece rate and tournament and incentivize them twice.) Next Screen Now tell participants that they should guess how many problems on average all participants in this room have truly solved correctly in part 1 and part 2. (No incentives)*

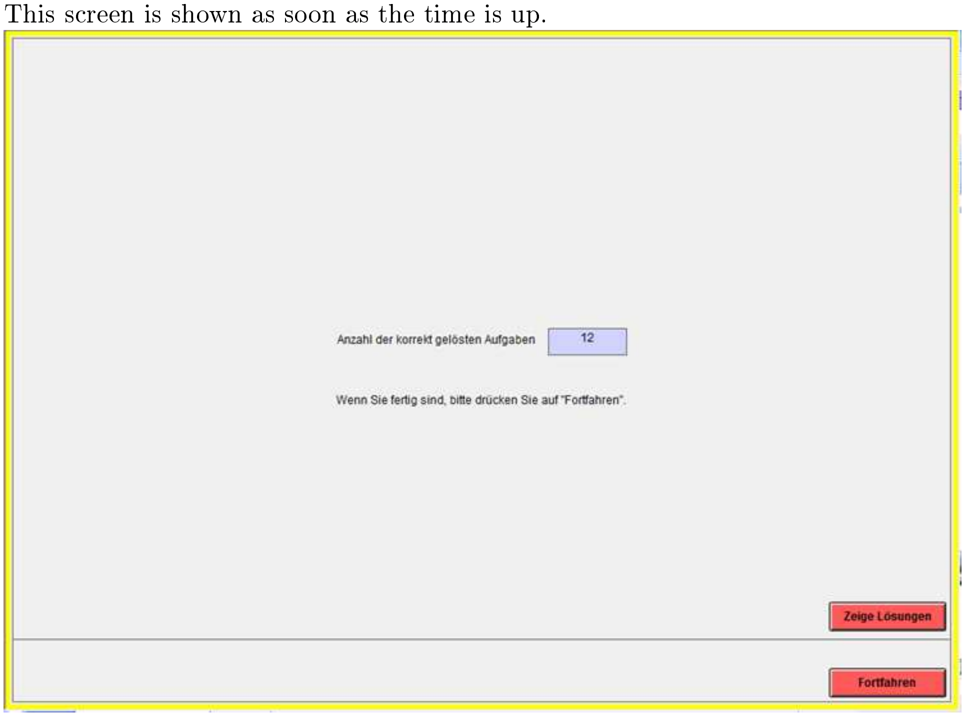
10.2 Screen-shots



This is the time screen they see once you hit the “ok” button on the server.

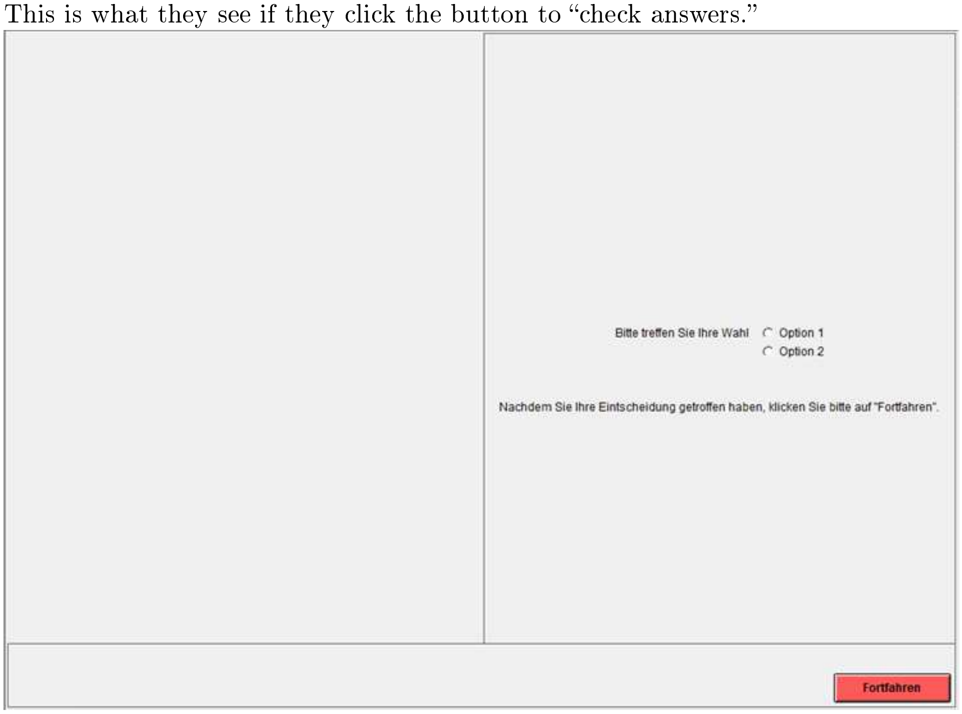


This screen is shown as soon as the time is up.



This is the screen they see after everyone hits continue on the wait screen.

This is what they see if they click the button to "check answers."



In the choice treatment, this is the screen they see to make their selection

University of Innsbruck - Working Papers in Economics and Statistics
Recent Papers can be accessed on the following webpage:

http://eeecon.uibk.ac.at/wopec/

2016-29 **Glenn Dutcher, Daniela Glätzle-Rützler, Dmitry Ryvkin:** Don't hate the player, hate the game: Uncovering the foundations of cheating in contests

2016-28 **Manuel Gebetsberger, Jakob W. Messner, Georg J. Mayr, Achim Zeileis:** Tricks for improving non-homogeneous regression for probabilistic precipitation forecasts: Perfect predictions, heavy tails, and link functions

2016-27 **Michael Razen, Matthias Stefan:** Greed: Taking a deadly sin to the lab

2016-26 **Florian Wickelmaier, Achim Zeileis:** Using recursive partitioning to account for parameter heterogeneity in multinomial processing tree models

2016-25 **Michel Philipp, Carolin Strobl, Jimmy de la Torre, Achim Zeileis:** On the estimation of standard errors in cognitive diagnosis models

2016-24 **Florian Lindner, Julia Rose:** No need for more time: Intertemporal allocation decisions under time pressure

2016-23 **Christoph Eder, Martin Halla:** The long-lasting shadow of the allied occupation of Austria on its spatial equilibrium

2016-22 **Christoph Eder:** Missing men: World War II casualties and structural change

2016-21 **Reto Stauffer, Jakob Messner, Georg J. Mayr, Nikolaus Umlauf, Achim Zeileis:** Ensemble post-processing of daily precipitation sums over complex terrain using censored high-resolution standardized anomalies

2016-20 **Christina Bannier, Eberhard Feess, Natalie Packham, Markus Walzl:** Incentive schemes, private information and the double-edged role of competition for agents

2016-19 **Martin Geiger, Richard Hule:** Correlation and coordination risk

2016-18 **Yola Engler, Rudolf Kerschbamer, Lionel Page:** Why did he do that? Using counterfactuals to study the effect of intentions in extensive form games

2016-17 **Yola Engler, Rudolf Kerschbamer, Lionel Page:** Guilt-averse or reciprocal? Looking at behavioural motivations in the trust game

2016-16 **Esther Blanco, Tobias Haller, James M. Walker:** Provision of public goods: Unconditional and conditional donations from outsiders

2016-15 **Achim Zeileis, Christoph Leitner, Kurt Hornik:** Predictive bookmaker consensus model for the UEFA Euro 2016

2016-14 **Martin Halla, Harald Mayr, Gerald J. Pruckner, Pilar García-Gómez:** Cutting fertility? The effect of Cesarean deliveries on subsequent fertility and maternal labor supply

2016-13 **Wolfgang Frimmel, Martin Halla, Rudolf Winter-Ebmer:** How does parental divorce affect children's long-term outcomes?

2016-12 **Michael Kirchler, Stefan Palan:** Immaterial and monetary gifts in economic transactions. Evidence from the field

2016-11 **Michel Philipp, Achim Zeileis, Carolin Strobl:** A toolkit for stability assessment of tree-based learners

2016-10 **Loukas Balafoutas, Brent J. Davis, Matthias Sutter:** Affirmative action or just discrimination? A study on the endogenous emergence of quotas *forthcoming in Journal of Economic Behavior and Organization*

2016-09 **Loukas Balafoutas, Helena Fornwagner:** The limits of guilt

2016-08 **Markus Dabernig, Georg J. Mayr, Jakob W. Messner, Achim Zeileis:** Spatial ensemble post-processing with standardized anomalies

2016-07 **Reto Stauffer, Jakob W. Messner, Georg J. Mayr, Nikolaus Umlauf, Achim Zeileis:** Spatio-temporal precipitation climatology over complex terrain using a censored additive regression model

2016-06 **Michael Razen, Jürgen Huber, Michael Kirchler:** Cash inflow and trading horizon in asset markets

2016-05 **Ting Wang, Carolin Strobl, Achim Zeileis, Edgar C. Merkle:** Score-based tests of differential item functioning in the two-parameter model

2016-04 **Jakob W. Messner, Georg J. Mayr, Achim Zeileis:** Non-homogeneous boosting for predictor selection in ensemble post-processing

2016-03 **Dietmar Fehr, Matthias Sutter:** Gossip and the efficiency of interactions

2016-02 **Michael Kirchler, Florian Lindner, Utz Weitzel:** Rankings and risk-taking in the finance industry

2016-01 **Sibylle Puntscher, Janette Walde, Gottfried Tappeiner:** Do methodical traps lead to wrong development strategies for welfare? A multilevel approach considering heterogeneity across industrialized and developing countries

2015-16 **Niall Flynn, Christopher Kah, Rudolf Kerschbamer:** Vickrey Auction vs BDM: Difference in bidding behaviour and the impact of other-regarding motives

2015-15 **Christopher Kah, Markus Walzl:** Stochastic stability in a learning dynamic with best response to noisy play

2015-14 **Matthias Siller, Christoph Hauser, Janette Walde, Gottfried Tappeiner:** Measuring regional innovation in one dimension: More lost than gained?

2015-13 **Christoph Hauser, Gottfried Tappeiner, Janette Walde:** The roots of regional trust

2015-12 **Christoph Hauser:** Effects of employee social capital on wage satisfaction, job satisfaction and organizational commitment

2015-11 **Thomas Stöckl:** Dishonest or professional behavior? Can we tell? A comment on: Cohn et al. 2014, Nature 516, 86-89, "Business culture and dishonesty in the banking industry"

2015-10 **Marjolein Fokkema, Niels Smits, Achim Zeileis, Torsten Hothorn, Henk Kelderman:** Detecting treatment-subgroup interactions in clustered data with generalized linear mixed-effects model trees

2015-09 **Martin Halla, Gerald Pruckner, Thomas Schober:** The cost-effectiveness of developmental screenings: Evidence from a nationwide programme *forthcoming in Journal of Health Economics*

2015-08 **Lorenz B. Fischer, Michael Pfaffermayr:** The more the merrier? Migration and convergence among European regions

2015-07 **Silvia Angerer, Daniela Glätzle-Rützler, Philipp Lergetporer, Matthias Sutter:** Cooperation and discrimination within and across language borders: Evidence from children in a bilingual city *forthcoming in European Economic Review*

2015-06 **Martin Geiger, Wolfgang Luhan, Johann Scharler:** When do Fiscal Consolidations Lead to Consumption Booms? Lessons from a Laboratory Experiment *forthcoming in Journal of Economic Dynamics and Control*

2015-05 **Alice Sanwald, Engelbert Theurl:** Out-of-pocket payments in the Austrian healthcare system - a distributional analysis

2015-04 **Rudolf Kerschbamer, Matthias Sutter, Uwe Dulleck:** How social preferences shape incentives in (experimental) markets for credence goods *forthcoming in Economic Journal*

2015-03 **Kenneth Harttgen, Stefan Lang, Judith Santer:** Multilevel modelling of child mortality in Africa

2015-02 **Helene Roth, Stefan Lang, Helga Wagner:** Random intercept selection in structured additive regression models

2015-01 **Alice Sanwald, Engelbert Theurl:** Out-of-pocket expenditures for pharmaceuticals: Lessons from the Austrian household budget survey

University of Innsbruck

Working Papers in Economics and Statistics

2016-29

Glenn Dutcher, Daniela Glätzle-Rützler, Dmitry Ryvkin

Don't hate the player, hate the game: Uncovering the foundations of cheating in contests

Abstract
Contests are meant to attract the best performers and incentivize high effort, however, they may also attract cheaters who try to win via illicit means which crowds out the best performers. We use a laboratory experiment to explore the role of self-selection in contests with a possibility of lying in a real effort task. Contrary to common wisdom, we do not find evidence that contests disproportionately attract intrinsic cheaters. However, we find that contests fail at selecting the best performers, as no difference is observed in the actual or perceived ability of those who selected into the contest versus those who selected into a comparable noncompetitive pay scheme.

ISSN 1993-4378 (Print)
ISSN 1993-6885 (Online)