No. 40 november 2008

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Research Paper Series
Thurgau Institute of Economics and Department of Economics
at the University of Konstanz

THURGAU INSTITUTE
OF ECONOMICS
at the University of Konstanz

LIES IN DISGUISE

AN EXPERIMENTAL STUDY ON CHEATING

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November 25, 2012

Abstract

We present a novel experimental design to measure honesty and lying. Participants receive a die which they roll privately. Since their payoff depends on the reported roll of the die, the subjects have an incentive to be dishonest and report higher numbers to get a higher payoff. This design has three advantages. First, cheating cannot be detected on the individual level, which reduces potential demand effects. Second, the method is very easy to implement. Third, the underlying true distribution of the outcome under full honesty is known, and hence it is possible to test different theoretical predictions. We find that about 20 percent of inexperienced subjects lie to the fullest extent possible while 39 percent of subjects are fully honest. In addition, a high share of subjects consists of partial liars; these subjects lie, but do not report the payoff-maximizing draw. We discuss different motives that explain the observed behavioral pattern.

Key words: Lie detection, honesty, deception, experimental design

JEL Classifications: C91, D63, H26.

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Acknowledgements: We thank Katharine Bendrick, Armin Falk, Ernst Fehr, Holger Herz, Michael Näf, Sabrina Teyssier, and the participants of the IAREP conference in Paris, the GfEW conference in Goslar, the TIBER workshop in Tilburg and the participants of seminar participants in Innsbruck, Jena, Zurich, Madrid and Oxford for helpful comments. We thank Katharine Bendrick for copyediting the manuscript.

1. INTRODUCTION

The question of whether, how, and why people lie has always attracted people's and researchers' attention. On the one hand, the existence of dishonesty is frequently experienced in our daily lives. DePaulo et al. (1996) asked people how often they lied and found that people lie in 20 to 31 percent of their social interactions. In business life, fraud is part of the game. When asked whether their management is likely to cut corners in tough times 40% of the employees taking part in the Ernst & Young European Fraud Survey (2009) tended to agree and another 30% even strongly agreed. Among academic economists as well, List et al. (2001) found a substantial fraction of researchers reporting certain types of unethical behavior in their discipline. In this paper we study why people do not always lie if lying is the payoff-maximizing strategy. Is it a matter of morals? Or do people like to avoid being perceived as liars? We present a new and simple experimental design that makes it possible to detect lies when subjects face no threat of being caught individually. Instead we can draw inferences on the population's overall behavior. We find a surprisingly robust pattern of lying behavior and evidence that people not only care about their income but also about maintaining a favorable self-image with respect to honesty and non-greediness. In particular, people try to disguise their lies.

Questions about humans' honesty have already attracted researchers from psychology (for a survey, see Hyman, 1989). For economists, until recently lying was not an issue as it was assumed that a person would always lie if benefits are high enough to cover the risk of punishment upon detection (Lewicki, 1984). However, this assumption is apparently too pessimistic. For example, most studies on tax compliance find higher compliance rates than predicted by models that are only based on material incentives like audit and penalty rates, and find that social and institutional factors matter as well (Andreoni et al., 1998; Torgler, 2002). Evidence for honest behavior has also been shown in studies in the labor market using field studies and field experiments (Evans III et al., 2001; Nagin et al., 2002; Schweitzer et al., 2004; Grover and Hui, 2005). In experiments, honesty has been investigated using games in which players can announce future moves or can reveal (non-verifiable) private information. For example, a substantial fraction of people reveal private information against their material self-interest (Gneezy, 2005; Hurkens and Kartik, 2009) or keep promises even when it is costly (Charness and Dufwenberg, 2006; Sánchez-Pagés and Vorsatz, 2007; Vanberg, 2008).

Standard economics assumes that people lie when it is in their material interest to do so. As we have seen, this view is overly pessimistic and in order to get empirically informed predictions, we have to take into account preferences for honesty. Dufwenberg and Gneezy (2000) assume that people are honest and keep promises because they feel guilty if others' expectations are disappointed. Kartik (2009) assumes that people have a preference for promise-keeping or truth-telling per se, which is supported by experiments by Sánchez-Pagés and Vorsatz (2007) and Vanberg (2008). A third thread of research on ethical behavior is based on people's concern about how others or they themselves will assess their behavior (Bénabou and Tirole, 2003; Ellingsen and Johannesson, 2004). This motive supports moral hypocrisy since people like to appear moral without actually being so (Batson et al., 1997; Batson et al., 1999 p. 535; Batson et al., 1999; Schweitzer and Hsee, 2002; Tenbrunsel and Messick, 2004; Dana et al., 2007).

In order to address the underlying motives and the situational factors affecting lying and honesty, we present a simple experiment where no material incentives for honesty exist. Therefore, any abstention from lying must be interpreted as a reaction to – possibly internalized – social rules or preferences. In this experiment, subjects are informed that they must roll a die, which will determine a payoff. The payoff equals 1, 2, 3, 4, or 5 CHF for the corresponding die number rolled and zero if the number rolled is 6. Since the experimenter cannot observe the number that was actually rolled, subjects can report any number without risking that cheating will be detected. Although we cannot observe lying at the individual level, the distribution of the reported numbers reveals information about patterns of lying behavior at the group level.

Our study aims at deepening our understanding of lying behavior. The design not only allows the detection of lies, but furthermore the assessment of the distribution of lying behavior in a given population. We find that only about one fifth of people lie fully and act in line with the assumption of payoff-maximization. About 39% of the subjects seem to resist the monetary incentives to lie and remain honest. Another 20% of the subjects obviously do not tell the truth but do not maximize their payoff either; we refer to this behavior as partial lying.[1] In a series of different treatment conditions, we vary the level of stakes, the payoff structure, the consequences of lying in terms of externalities and the degree of anonymity. We find that lying is reduced if it causes negative externalities to other participants and it increases if we introduce a double blind procedure. However, the effects are small and the patterns of lying remain the same. In all situations, we find lying as well as partial lying. We tested for consistency of behavior over time by investigating the behavior of people who decided in exactly the same situation more than once and find that honesty decreases significantly. However, partial lying does not vanish. In order to investigate this finding in more detail, we additionally ran experiments where we elicited beliefs. We find that it is possible to disguise a lie, in particular toward inexperienced people. Finally, we discuss different motives that can explain the observed pattern. Using our different treatment conditions we can show that simple models of lying aversion suggested in the literature are

[1] Of course, whether people are honest depends on the specific situation. Actually, this is part of what we will show in the paper. So, the absolute numbers that we report have to be taken with care.

disproven by our data and offer the maintaining of a favorable self-concept as a potential explanation.

In our experiment we focus on lying behavior with a minimum of social interaction. In this respect, our experiment is closely related to the experiments conducted by Pruckner and Sausgruber (forthcoming), Nagin and Pogarsky (2003) and Mazar et al. (2008), who interpret honesty as compliance with a given rule where it is also possible to cheat only partly. Pruckner and Sausgruber (forthcoming) collected field data on how many customers pay for a newspaper when it is sold out of a box, with payment into a cash-box. They found that more than 30% of the people paid something for the newspaper. But on average, people paid only one third of the required price. Mazar et al. (2008) also addressed the question of subjects' honesty towards the experimenter. In their experiment, subjects had to complete a test. They were paid according to the number of correct answers. In one treatment, the number of correct answers was checked by the experimenter. In other conditions, the subjects themselves corrected their sheets. The treatment conditions differed with respect to how easy it was for the experimenter to detect a potential fraud. On average, subjects reported about 10% more questions solved when they had the possibility to cheat. As in our experiment, subjects did not cheat maximally. None of the subjects reported to have solved all questions. Mazar et al. (2008) conclude that the low extent of dishonesty and the fact that subjects do not claim the maximum amount when cheating can be explained with the aim to maintain a positive self-concept.

Methodologically, our design is related to the procedure of Batson et al. (1997) where subjects were asked to assign a good and a bad task to themselves and another subject by reporting the result of a coin flip and to the random response method used in social psychology (Warner, 1965). It is also related to those designs that compare distributions of results (e.g., scores in a test) in situations where cheating is possible with situations in which it is not (Schweitzer et al., 2004; Mazar et al., 2008). All these methods share the advantage that individuals do not have to fear detection because it is never possible to tell whether a subject lied or not. Compared to the latter studies, our design has two main advantages, however. First, it is much simpler and needs much less time to be conducted. Second, we know the underlying true distribution of the outcome if people behave honestly. This makes it possible to assess honesty without the need to conduct control experiments. This feature is also crucial since it allows the comparison of the observed pattern of partial cheating with theoretical models of lying aversion models.

The remainder of this paper is organized as follows. In Section 2 we present our experimental design and the procedure. Section 3 presents our results. In Section 4 we discuss our findings in the light of several theoretical explanations. Section 5 concludes.

2. EXPERIMENTAL DESIGN

Our experiment is a one-shot individual decision-making situation. It took less than 10 minutes to conduct it. For this reason we did not recruit subjects for this experiment only. Instead we asked other experimenters whether we could add our experiment to the end of their sessions. At the end of such an experimental session we distributed six-sided dice among the participants. They were informed not to touch the die until requested to do so. The experimenter then told the participants that the following very short experiment had nothing to do with the experiment they had just participated in and that instructions would be given on the screen.

Subjects then read these instructions and were informed that they were going to receive an additional payoff for filling in a questionnaire and that this payoff would be different for each participant. To determine their individual payoff, the participants were requested to roll a die and to memorize the figure rolled. The payoff would equal 1, 2, 3, 4, or 5 CHF if the die number that came up was the corresponding payoff amount, and 0 CHF if the die number that came up was a 6. Participants were explicitly called to roll the die more than once in order to check whether the die was fair. It was highlighted on every screen that only the first throw was relevant for the payoff and therefore should be kept in mind. On the last instruction screen, participants reported the number rolled together with the resulting payoff. Appendix A contains an English translation of the screenshots of the baseline treatment.

In this experiment, lying means reporting a different number than the one actually rolled on the first roll. It was impossible to detect lying on the individual level. The consequence of lying is only that this person will receive a different – usually higher – payoff than deserved by the rules of the game. Since the experimenter cannot see what number was rolled, subjects can easily be dishonest. We made it as obvious as possible that it was impossible for the experimenter to find out what number a subject actually rolled. First, we asked subjects to throw the die more than once. This ensured that the subject did not have to care that the entered number was face up on the die. We told the subjects that the multiple rolling was a possibility for them to make sure that the dice were not loaded. They were not only allowed to roll the die twice but as many times as they wanted to. Secondly, the experimenters were not in the same room as the subjects during the experiment. It was not possible for the experimenters to walk through the lab and to note the actual figures rolled. Thirdly, we wanted to make it easier for them not to tell the truth. It might be easier to report a number that was actually rolled in one of the following throws, even if it was not the payoff relevant first number, than it is to invent a number from scratch. Still, it was explicitly mentioned on every screen that the first number was the relevant number and that they had to keep it in mind throughout the experiment. Another excuse was the number 6. This number is higher than the other numbers but was payoff minimizing in our experiment. Subjects who rolled a 6 could feel unfairly treated and tempted to correct this unfairness by reporting a higher number.

In order to make the experiment as plausible as possible, we told the subjects that the reason for rolling the die was to determine the payoff for filling in a questionnaire. It is clearly not very plausible to pay subjects differently for doing exactly the same task. Still, it is more plausible to let them roll the die in order to determine a payoff for doing something instead of just letting them roll the die and paying them without any explanation.

2.1 Procedure

The participants were students from the University of Zurich and the Swiss Federal Institute for Technology in Zurich. Sessions for this experiment were conducted at the computer laboratory of the Institute for Empirical Research in Zurich from summer 2004 until spring 2007. We had a total of 746 participations. Payments were made in cash, in Swiss francs (= CHF; 1 CHF corresponded to about $0.80 at that time) and handed out to the participants immediately after the session together with the payment of the other experiment.

The experiment was programmed using the software z-Tree (Fischbacher, 2007). Recruiting was partly done by ORSEE (Greiner, 2004). The sessions were run at the end of the sessions of other experiments. The recruiting process was organized for the preceding experiments. As an artifact of the procedure of adding the sessions of this experiment to other experimental sessions, we had to control for multiple participation by checking the identity of the participants after the experiments. This was done by manually comparing their names, surnames and fields of study and generating a personal ID for every person. In this way, we were able to restrict our analysis to the inexperienced subjects and look at the results of those taking part a second time separately.

2.2 Control Treatments

We are interested in the general pattern and stability of lying behavior. Additional to our baseline treatment we ran several control treatments with only slight variation in the procedure. These control treatments allow us to draw inferences on how robust lying behavior is. In these sessions, half of the subjects played the baseline experiment as explained above and the other half took part in the control treatment. This procedure guarantees that different experiences in the preceding experiment are balanced among the control and the treatment group in our experiment. Furthermore, it avoids a recruiting bias as a consequence of different recruiting procedures for different preceding experiments.

Stakes and Payoff Structure

The first two control treatments address the question of whether payoffs matter for lying behavior, so we altered stakes and the structure of payoffs. The first control treatment was a high stake treatment where we addressed the question of whether patterns of lying depend on stakes. Do people lie differently when stakes are higher? Do they lie more, because the monetary incentives are higher, or less because the moral concerns are more salient? For the payoff, we only applied a factor of 3 compared to the baseline treatment, which is not very high. Rolling a 1 resulted in a payoff of 3 Swiss francs, rolling a 2 in 6 Swiss francs, and 3 in 9, 4 in 12, 5 in 15, and 6 in 0, respectively. Accordingly, the incentives were changed in the following, potentially opposing ways. Lying was financially more rewarding for the participant. On the other hand, reporting a higher number increases the size of the lie. As a second control we also altered payoffs, but in a different way. We changed the marginal payoffs of lying by paying 4.90 CHF instead of 4 CHF when reporting a 4. In this treatment, the payoffs of reporting 4 and 5 are rather similar and outcome-based lying aversion models would predict that the frequency of 4 and 5 becomes more similar.

EXTERNALITY

The third control treatment refers to the results of Gneezy (2005), who found that the consequences of lying matter to the person lying. By imposing an externality on another subject his treatment allows us to draw inference on whether it matters if the lie affects other subjects' payoffs. In this control treatment, a second subject received the remaining part of 5 CHF dependent on the first subject's reported die roll. The situation is similar to a dictator game except that the dictators were instructed to use the die to determine the distribution of the 5 CHF. As in the other treatments, this treatment was conducted together with a baseline treatment. This means that there were three types of subjects. One third of the subjects were in the baseline treatment, one third was in the role of dictators in the externality treatment and one third consisted of recipients in the externality treatment.

ANONYMITY

In our baseline experiment, subjects could not be caught lying. Still, the experimenter could – based on the reported number – update his belief about the subjects' honesty. Our double anonymous treatment excludes this possibility, as it was impossible for the experimenter to find out what number a particular subject reported. This is similar to a control treatment applied in (Mazar et al., 2008) where subjects had to shred all evidence for their real behavior and just took the money they claimed. We ran a treatment where it was not only impossible to tell who *rolled* what number, but additionally absolutely impossible to tell who *reported* what number. Subjects who care about what the experimenter might think about them reporting a certain number could now be sure that it was impossible to reveal their decision on an individual level. To create such a situation, we had to alter the procedure in the following way.[2] At the end of the session, subjects received a die and could take an envelope

[2] This is the only control treatment where the baseline treatment too was changed and processed without computers. The screens were only used to explain the experiment.

from a box that the experimenter presented to the subjects one after the other. Each envelope contained five coins (real money) worth 1 CHF and a second empty envelope inside. We applied this procedure in order to avoid that people could believe that there are hidden marks on the envelopes that would enable us to identify the decisions. The instructions on the screen were similar to the former baseline treatments. Participants were requested to roll the die and to take the coins gained out of the envelope, then to put the remaining coins into the spare envelope, seal it and give it back to the experimenter. In the double anonymous treatment subjects had to deposit the sealed envelopes anonymously in a box at the door; in the baseline treatment we requested subjects to leave the sealed envelope on their desk in the laboratory. Thus, in the baseline situation the experimenter could walk through the lines, collect the envelopes and match the reported numbers with the data after the experiment. Coins and envelopes were prepared in a way to make sure that it was impossible to hear how many coins were taken out or given back. The double anonymous procedure made it as obvious as possible that we had no chance to trace back any decisions on the individual level. As we could not control for repeated participation by checking names later, any former participants at this experiment were excluded in the recruiting procedure from the very start.

NO DIE TREATMENT

People who do not report the number associated with the highest payoff are most likely motivated by a preference for honesty. However, other motives such as not appearing greedy might also be relevant. In order to assess the importance of such motives for reporting lower numbers, we conducted an additional control experiment. The experiment is very similar to the baseline treatment of the experiment above. The participants were told that they would receive an additional payoff for filling in a questionnaire and that this payoff would not be the same for everybody. Instead of letting them throw a die to elicit their payoffs they just had a choice of 6 different payoffs (0, 1,2,3,4 or 5 CHF). There was no incentive related to honesty to claim anything other than 5 CHF, as there was no rule telling anyone to take less.

2.3 Repetition

Does lying behavior change when an action is repeated? Repeated participation allows us to test whether people behave differently when they are in the very same situation a second time. People are often confronted with similar situations repeatedly. Project reports for example usually have to be made quarterly or even weekly. To be able to infer how lying behavior affects daily life it is therefore important also to know how it evolves in habits or routines as these reduce uncertainty about the true procedure. Although we did our best to make sure that there is no detection risk, some subjects might have been concerned about it. The overall procedures of our experiments allow us to observe behavior in a repeated situation. As mentioned above, we did not exclude former participants in this experiment when recruiting for a new session. By manually comparing names of the participants after the sessions we could find out how many times this very subject had participated before. This allows us to compare the behavior of inexperienced participants with experienced ones in a panel data set.

2.4 Beliefs

Next we were interested in the subjects' beliefs about others' behavior. This control is important as it allows us to tell what distribution of reported payoffs subjects expected to be a part of. To find this, we ran another session of our experiment eliciting the subjects' beliefs about the reported distribution. 60 subjects took part in this control treatment. Instead of asking them to roll the die they read the complete instructions of the baseline treatment and then were asked to guess what they thought people had reported. They were paid for this task dependent on the accuracy of their guesses. Procedures were as follows: Subjects were informed that they would have to guess the behavior of other participants in a previously run experiment. Then they read the instructions of the baseline treatment and had to guess what percentage of participants earned which payoff. They were paid 5 CHF if they guessed every percentage correctly. Their payoff was reduced by 0.04 CHF for every percentage point deviation from the correct percentages. The instructions can be found in the appendix.

3. RESULTS

In this section, we first present the results of the baseline treatment and show the main patterns of lying behavior. Next, we take a look at the results of the control treatments and show that the observed patterns in lying behavior are robust to treatment variations.

3.1 BASELINE RESULTS

A total of 389 participants took part in the baseline treatment as inexperienced subjects. Figure 1 shows the resulting distribution of reported payoffs. It is obvious that this distribution is not uniform (a Kolmogorov-Smirnov one sample test is significant at the 1 percent level ($p<1\%$)). Numbers below 4 are significantly less frequently reported than the expected true value of 16.7% (1/6). The percentages of numbers 4 and 5 are significantly above the expected 16.7% (see binomial tests in Table 1).

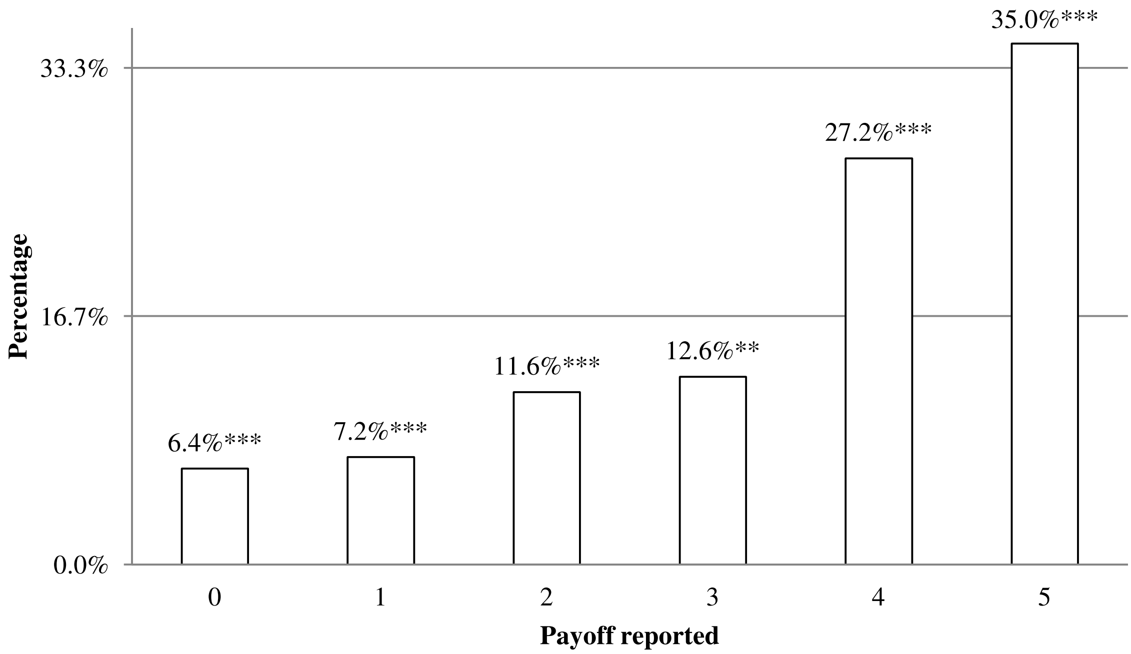

Figure 1. Percentage of reported number of subjects in baseline experiment; first participation only (stars display the significance of two-sided binomial test that the observed percentage differs from 16.7% (*=10%-level, **=5%-level, ***=1%-level)).

Higher numbers appear with higher probability. With the exception of the comparison between 0 and 1 as well as between 2 and 3 the frequency of higher numbers is even significantly higher than that of any lower offer.[3] This monotonically increasing distribution implies that some subjects tend to report a number higher than they actually rolled. If we assume that people do not lie to their disadvantage, the positive share of subjects reporting zero shows that at least some people are honest. The fraction of people who reported a payoff of 0 gives us the possibility to estimate the fraction of honest people. Assuming that no person reporting a payoff of zero is lying, we can estimate the percentage of honest people to be as

[3] One-sided binomial test whether, when restricting the data to two numbers, these two numbers occur with probability different from 0.5. For all pairs with the exception of (0,1) and (2,3), the conditional probability for the higher number is significantly above 1/2 at the 5% level.

large as 39%.[4] A homo economicus type suffers no cost when lying. Hence, he would always report a 5. Our results indicate that the percentage of people acting as income maximizers can be estimated at maximum to be 22%.[5] Another interesting observation is that not all lying subjects lie maximally. Significantly more than 1/6 of the subjects report 4. This is evidence that some subjects neither report the truth nor report 5. Instead they choose to report 4.

Summing up, we find the following three characteristics in the pattern of behavior:

(1) **Honest subjects:** The fraction of people reporting a payoff of 0 is **positive**.

(2) **Income maximizing subjects**: The fraction of people reporting a 5 is **above 1/6**.

(3) **Partial liars:** The fraction of people reporting a **4 is above 1/6.**

Next, we show how these basic patterns are affected by changes in the treatment conditions by looking at the results of the control treatments.

3.2 Control Treatments

Table 1 shows all the results of our experiment. In the first part, it shows the distributions of reported payoffs in the control treatments and the inexperienced participants' results for the corresponding baseline treatments. For each cell, a one-sided binomial test reporting whether

[4] Assuming that unconditionally honest people in fact roll a uniform distribution of numbers, it is reasonable to take the number of people reporting a payoff of 0 to estimate the percentage of honest people in each number reported. As 6.4% reported a payoff of 0, we can estimate the percentage of unconditionally honest people at 6*6.4% = 39%. If there were also people who report payoff of 0 although they rolled another number, the 39% is an upper limit for the number of honest people.

[5] In the Baseline treatment, 35% reported a 5. Assuming that nobody who has actually rolled a 5 reports anything other than 5, we can estimate that the maximal percentage of people acting as a homo economicus type is 22% ((35%-17%)*6/5). The multiplication with 6/5 is necessary to take into account those income maximizers who actually rolled a 5.

the percentage is above/below the expected true value of 16.7% (1/6) is given. Additionally we report p-values of a Fisher exact test comparing the distributions of payoffs reported in the two treatment groups.

	Fisher exact test (FE)[1] or signed rank test (WSR)[2]	**Share of subject (in percent) who reported corresponding payoff; one-sided binomial tests that it is smaller (larger) that 100%/6. *(+) =10%-level, ** (++) =5%-level, *** (+++) =1%-level**					
		0	**1**	**2**	**3**	**4**	**5**
a) Baseline							
baseline (n=389)		6.43***	7.20***	11.57***	12.60**	27.25+++	34.96+++
b) High Stake Sessions	FE 0.100						
baseline (n=79)		2.53***	10.13*	15.19	15.19	17.72	39.24+++
high stake (n=80)		11.25	5.00***	15.00	8.75**	27.50+++	32.5+++
c) 4.9 Sessions	FE 0.518						
baseline (n=128)		7.03***	4.69***	9.38**	12.50	24.22++	42.19+++
4.9 (n=125)		8.00***	5.60***	14.40	10.40**	29.60+++	32.00+++
d) Externality Sessions	FE 0.344						
baseline (n=80)		8.75**	7.50**	7.50**	8.75**	40.00+++	27.50++
externality (n=78)		8.97**	12.82	8.97**	16.67	25.64++	26.92++
e) Double Anonymous Sessions	FE 0.969						
baseline (n=140)		5.71***	8.57***	10.71**	17.14	28.57+++	29.29+++
double anonymous (n=137)		6.57***	8.76***	10.22**	17.52	24.09++	32.85+++
b) No Die Session							
no die (n=34)		0.00***	2.94**	0.00***	0.00***	11.76	85.29+++
g) Repetition	WSR 0.000						
first participation (n=111)		11.71*	9.91**	13.51	12.61	20.72	31.53+++
second participation (n=111)		4.50***	3.60***	5.41***	9.01**	25.23++	52.25+++
h) Repetition: report in 2nd participation	FE 0.171						
first report 0-3 (n=53)		3.77***	5.66**	9.43	15.09	28.30+++	37.74+++
first report 4 (n=23)		4.35*	4.35*	0.00**	4.35*	21.74	65.22+++
first report 5 (n=35)		5.71*	0.00***	2.86**	2.86**	22.86	65.71+++
i) Repetition: report in 1st participation	FE 0.075						
second report 0-3 (n=25)		12.00	20.00	28.00	12.00	12.00	16.00
second report 4 (n=28)		14.29	3.57**	14.29	21.43	17.86	28.57+++
second report 5 (n=57)		10.34	8.62*	6.90**	8.62*	25.86+++	39.66+++
		Average belief (in percent) about reporting corresponding payoff; signed rank test that belief differs from 100%/6 * =10%-level, ** =5%-level, * =1%-level**					
j) Belief Treatment							
inexperienced (n=41)		9.34***	13.88***	14.78	17.00	16.80	28.20
experienced (n=19)		3.84***	5.74***	8.21**	12.05**	22.58**	47.58***

1) Reports the p-value of a Fisher exact test comparing the distributions of payoffs reported in the two treatment groups.
2) Reports the p-value of a Wilcoxon signed rank test that in both participations the same number is reported.

Table 1. Summary of all treatments.

Due to the underlying random process of the die we expect some variation when there are only few observations in a category. Still, when we look at the results we see that the patterns of lying are very robust with respect to characteristics (1) to (3). In every control group we have a positive fraction below 16.7% reporting a payoff of 0 indicating that there are honest subjects. The fraction of subjects reporting 5 is always significantly above 16.7%. Apart from the baseline treatment in the high stake session, the fraction of subjects reporting 4 is significantly above 16.7% in all treatments.

The results of these control treatments show that lying behavior in general is very robust to treatment variation. In the high stake treatment, subjects received a payoff which was tripled with respect to the payoff in the baseline treatment. We still observe unconditionally honest behavior as well as lying and partial lying. Thus, the results remain stable even when the stakes are tripled. These results are in line with the results of Mazar et al. (2008) who also didn´t find effects of stakes in deception. Either the effect of increasing the benefits of lying is counterbalanced by the negative effect of increased costs of lying when a person earns more through deception, or the concept of lying is not directly related to stakes at all. The change in the payoff structure in the "4.9" treatment leads to almost identical results as in the high stakes treatment, with characteristics (1) to (3) still holding. This change in the relative payoffs does not seem to make 4 more attractive. We observe a lower fraction of 5s, which is consistent with the fact that it is cheaper to report 4 instead of 5. However, a similar pattern is also found in the high stakes treatment. In the externality treatment there is a shift to lower numbers, but this shift is not significant. The distribution does not change much when the remainder of the 5 CHF is given to another subject instead of being kept by the experimenter, and the results (1) to (3) still hold. Thus, also due to limited power, we cannot confirm the notion of Gneezy (2005) or Hurkens and Kartik (2009), who show that people take the consequence of a lie into account. Interestingly, our main results (1) to (3) do not change when we implement a double anonymous procedure. The fraction of subjects who choose 0 remains positive and the fractions of subjects who choose 4 or 5 both remain significantly above 1/6. We find that slightly more subjects choose 5 and fewer subjects choose 4 in the double anonymous treatment compared to the baseline treatment in these sessions. However, this difference is not significant. To conclude, those subjects who reported their payoff by anonymously throwing the remainder of the five francs packed in a sealed envelope into a box did not behave differently from those in the baseline treatment, where the experimenter could match the reported number to the individual. This result again confirms those of Mazar et al. (2008) and shows even more strongly how little lying behavior depends on reputational concerns towards others. In the no die treatment, there was no incentive related to honesty to claim anything else than 5 CHF. Still, as reported in Table 1 (f), only 85% claimed a payoff of 5 CHF. 4 out of 34 claimed 4 CHF, and one person claimed 1 CHF.

3.3 REPETITION

111 participants took part at least twice in different sessions of this experiment. This allows us to track their behavior over time individually. Since the behavior of subjects does not differ significantly between the treatments, we include in this analysis the decisions in all treatments and do not restrict the analysis to the baseline treatment. Table 1 gives an overview of the results in the first and second participation in (g). People reported higher payoffs when they participated a second time. The fraction of subjects choosing 4 or 5 rises from 52% in the first participation up to 77% in the second participation, a highly significant difference (Wilcoxon matched pair signed rank test Prob >|z|=0.000).[6] However, the distribution of reported numbers remains robust with respect to the characteristics (1) to (3) above. We still observe people reporting 0 and the fractions for reported numbers 4 and 5 are above 16.7%.

[6] This increase is most likely not the result of selection. In the first participation, we find no difference in subjects who participated more than once compared to those who participated only once.

When calculating the fraction of honest subjects using our estimation method we find that 27% of the subjects are honest but that 42% of the subjects are lying maximally.[7] As we have panel data we can investigate how people change their behavior and compare first participation behavior directly with second participation behavior in both directions. This is shown in Table 1 (h) and (i) and in Figure 2. Figure 2 shows the cumulative distributions of the reports in the second participation conditional on their behavior in the first participation and allows us to check for consistency over time. Those subjects who reported 0-3 in the first participation also report lower numbers in the second participation than those subjects who either reported a 4 or 5. Interestingly, the distribution of the second reports of those who reported a 4 or 5 in the first participation is identical. However, lying is very frequent, as all distributions are significantly to the right of the uniform distribution.

In Table 1 (i), grouping is done the opposite way round. Here we look at the behavior in the first participation conditional on the reports in the second participation. Those 25 subjects who reported 0-3 in the second participation are most interesting. The distribution of their first reports is almost identical to the uniform distribution. This indicates that the 25 subjects who report a number below 4 in the second participation are truly honest. We can take this as a base and estimate the fraction of unconditionally honest subjects to be at least 35%.[8] Additionally we see that those reporting a 5 in the second participation reported a slightly higher payoff in their first participation than those reporting a 4.

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[7] 4.5% reported a 0 when participating a second time. Thus, we can estimate the percentage of unconditionally honest people at 6*4.55 = 27%. 52 reported a 5. Assuming that nobody who has actually rolled a 5 tells anything else than 5, we can estimate that the maximal percentage of people acting as homo economicus is 42% ((52%-17%)*6/5).

[8] 25 of 110 subjects are honest and rolled 0-3 in the second participation. As some honest subjects rolled 4 and 5 in the second participation we have to multiply 25 by 6/4 to have the true fraction of 37.5 of 110 corresponding to a percentage of 35% of unconditional honest subjects.

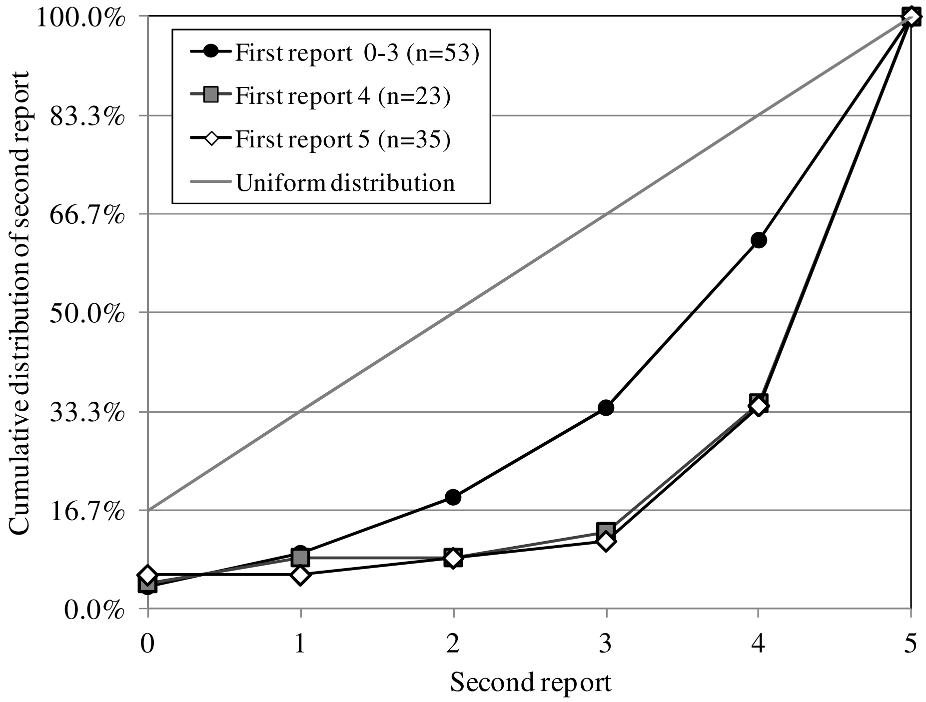


Figure 2. Cumulative distribution of reported numbers at the second participation conditional on their behavior in the first participation.

3.4 BELIEFS

60 subjects took part in the beliefs treatment. 41 subjects were inexperienced, meaning that this was the first time they took part in this experimental series. 19 subjects were already experienced when asked for their beliefs. In Figure 3, we display the average belief separately for inexperienced and experienced subjects, and in Table 1 (j), the corresponding data can be found. Figure 3 shows that average beliefs increased in the reported number and therefore higher numbers are more suspicious. As averages of beliefs can be misleading, we also categorized the reported beliefs according to the shape of the distribution they assumed in Table 2. Whereas 29% of the inexperienced participants reported a belief corresponding to a uniform distribution of reported numbers, this type of belief completely vanishes among the

experienced participants. Most frequently a monotonically increasing distribution of payoffs is expected whereas some subjects expect a centered distribution which even overestimates the effect of partial lying. Additionally we are interested in what fraction of subjects anticipates partial lying and reports a belief of more than 1/6 for the die number 4. 61% (25 out of 41) of the inexperienced participants did not anticipate others' partial lying. They reported a belief of a fraction of less than 1/6 for reporting a 4. Among the experienced subjects this percentage drops to 32% (6 out of 19). Most of them expect partial lying.

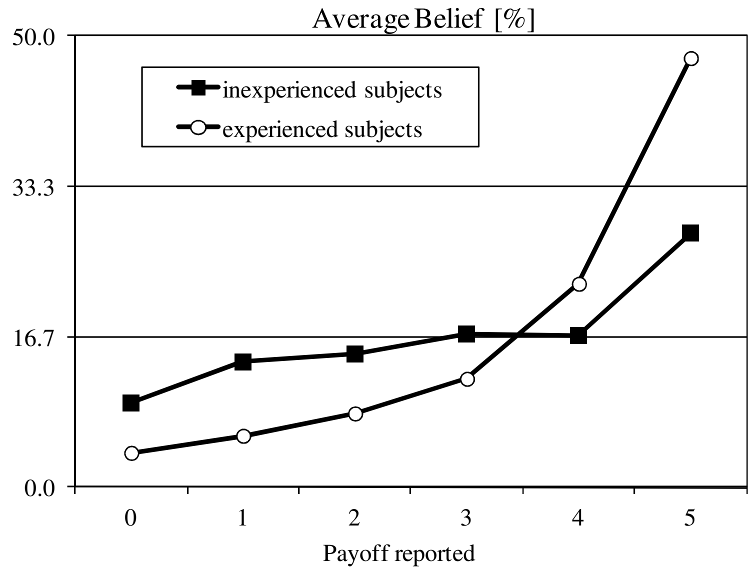


Figure 3. Average belief conditional on payoff reported. Subjects are experienced if they participated in a session of this experimental series before.

	inexperienced participants	experienced participants	all
uniform	12 (29%)	0 (0%)	12 (20%)
centered	9 (22%)	4 (21%)	13 (22%
monotonic	17 (41%)	13 (68%)	30 (50%)
other	3 (7%)	2 (11%)	5 (8%)
	41	41	(n=60)

Table 2. Frequency of type of distribution reported. Uniform: subject reported a belief of 16.7 percent for each report. Centered: subject reported a distribution of beliefs with a mode in the center. Monotonic: subject reported a monotonically increasing distribution of beliefs.

To sum up, the observed main characteristics (1) to (3) are robust to changes in stake, externality, anonymity and experience. In every control treatment we observe honesty, lying and partial lying. The results of the belief treatment show that beliefs are qualitatively in line with actual behavior. However, a large share of inexperienced participants ignores the incentive to lie. Experience obviously changes subjects' beliefs about partial lying. In the next section we discuss our results in the light of several theories trying to encompass lying behavior.

4. MOTIVES FOR THE OBSERVED LYING PATTERN

The following section discusses several possible explanations for why people sometimes abstain from lying and especially why people lie partially. We will show that a simple model of lying aversion helps to understand full liars and honest subjects, but cannot explain partial lying. Therefore, we shift our focus towards theories that include the idea of disguising lies by acting ambiguously and in a way that is difficult to interpret.

LYING AVERSION

Most experimental and theoretical studies on lying explicitly or implicitly assume that people are honest because lying causes bad feelings. For instance, Charness and Dufwenberg (2006) and Dufwenberg and Gneezy (2000) assume that people feel guilty if they disappoint others by lying to them. Vanberg (2008), on the other hand, shows in an experiment that people dislike the act of lying per se. First, we will now check whether simple lying aversion can explain our findings. A very simple model of lying aversion presumes that subjects balance their material payoff against disutility from lying. This disutility can be modeled as a function of the amount gained by lying, e.g. the difference of the payoff earned by lying and the payoff earned when being honest. If we assume payoff and lying disutility to be additively separable, and that disutility from lying monotonically increases in the monetary gain which results from lying, then we can explain the monotonically increasing distribution of reported numbers including characteristics (1) and (2). Assuming in addition that the disutility from lying is either increasing or decreasing in the marginal monetary benefit of lying, it is possible to show that this leads to a monotonically increasing distribution of reported payoffs explaining characteristics (1) and (2), but never (3). Those with decreasing disutility in the marginal benefit of lying lie as soon as the benefit is sufficiently high and therefore will report a 5 if their number is sufficiently low. Those with increasing disutility in the marginal benefit of lying lie as long as the gain does not reach a particular threshold. They will over report their number by a fixed amount. Thus, if they rolled a low number, the number they choose to report may be below 5. However, numbers below 5 cannot occur with higher frequency than 1/6 in the aggregate. For example, subjects who report 3 instead of 0 would report 5 instead of 3. Thus, the number 3 cannot occur with a frequency higher than 1/6. The behavior of an income maximizing subject and the behavior of an honest person can both be considered as special cases of these models. In the next section, we discuss how the desire to disguise the lie is compatible with our observations.

LIES IN DISGUISE

If people do not care about the objective lie but about the credibility of the lie then it makes sense to restrict the lie and not report the maximum possible amount. This increases the likelihood that the lie is not perceived as a lie by others. It is even possible that some subjects report a lower number than what they rolled, with the purpose of being 'credible' towards others. Two questions are relevant with respect to this credibility argument. What actions can be assumed to be credible, and whose judgment is relevant? Is a 4 assessed differently than a 5 with respect to honesty? Disguising a lie by only reporting 4 instead of 5 is only possible if participants have particular beliefs about the others' behavior. For example, if the frequency of 4s is higher than 1/6 it is clear that some subjects who reported a 4 lied. If more people reported a 4 than a 5, then a 4 is less credible than a 5, in particular if we assume that people are more likely to report honestly if they rolled a 5 than when they rolled a 4. The belief treatment shows that subjects believe on average that the number 5 is chosen more frequently that the number 4. In addition, about half of the subjects believe that the number 5 is more frequently chosen than 4 and one third believes the opposite. We find that about 60% of the inexperienced subjects believe that 4 is not over reported. This fraction halves for experienced subjects. Thus, for many subjects, in particular for inexperienced subjects, reporting a 4 is perceived as more honest than reporting a 5. From the point of view of the person who has to report a number, this means that choosing 4 can appear honest in front of outsiders, at least if they are inexperienced.

If it were the experimenter's judgment that the subjects worried about, and they tried to appear honest towards him, we would expect a change in behavior in the double anonymous situation. In particular, partial lying should disappear in this situation. We observe only a modest decline in deception and still observe partial lying even in the double anonymous situation, which is in line with results of Mazar et al. (2008) who could show that people do not try to lie more credibly when they are informed about the average chosen action, and therefore know which actions are credible. Of course, people could care about the experimenter's judgment with respect to the behavior of the whole group. Such a motive cannot be excluded with the double anonymous procedure. Nevertheless, appearing personally honest in front of the experimenter can account for only little of the observed partial lying.

A second line of reasoning assumes that people abstain from lying in order to maintain a favorable self-concept. So, Bénabou and Tirole (2002) develop a model where people endogenously manage their memories in order to keep up a positive self-concept. A related idea is that of self-reputation, where today's choices are weighted with how they will be assessed in the future and how they will influence preceding choices (Bénabou and Tirole, 2004). In this line of reasoning people engage in self-deception in order to maintain their positive self-concept (Bodner and Prelec, 2003). What are the consequences of such a concept for the subjects' behavior in our experiment? People would try to perceive themselves as good. Two favorable traits could be relevant in the situation we are looking at here, honesty and non-greediness. Concerning honesty, people could obtain satisfaction from being not more dishonest than other people. For this reason, the arguments on beliefs developed above apply to this situation as well. Greed is also supposed to be an unfavorable trait. Subjects could try to avoid appearing greedy by reporting a 4 instead of a 5. If this is an important motive, then it should also occur when subjects are not instructed to report the result of rolling a die but when they can claim any payoff between 0 and 5. We tested for this motive with the no die treatment. We found that only 85% claimed a payoff of 5 CHF; 4 out of 34 claimed 4 CHF, and one person claimed 1 CHF. It seems that 15% of people have a willingness to pay in order not to appear (or be) greedy. This finding is in line with the observed high frequency of 4s in our main experiments.

Technically, the desire to appear honest can also be modeled with disutility from relative lying aversion. One could define the lie based on the cumulative distribution of the true state and measure the relative lie as the increase in the cumulative distribution relative to the possible increase. For example, if 3 is reported instead of 1, then instead of a value in the top 5/6, a value in the top 3/6 percent is reported. Thus, the relative lie equals 2/5=(2/6)/(5/6). If people have convex cost in this relative lying, then a high frequency of 4s is possible. Kartik (2009) suggests a more general model of lying aversion, which includes the absolute as well as the relative lying aversion specification. Translated to our game, he assumes that marginal lying aversion with respect to the number reported increases in the number reported and decreases in the number rolled. This model is very general and can be compatible with all our observations, for instance, if we assume that the marginal disutility from lying is particularly high when we lie by reporting a 5 instead of a 4. However, why should marginal disutility from lying depend on the relative lie? If people believe that people with higher numbers are more likely to be liars then a natural interpretation of such a model is based on the idea that people dislike appearing to be liars. Thus, such a model could be interpreted as a reduced form model of models like (Bénabou and Tirole, 2004).

Our results do not allow identification of one single reason for the observed pattern of lying, in particular, for the observed pattern of partial lying. Most likely different reasons are relevant for different people and it seems that maintaining a favorable self-image is one of the relevant motives. Nevertheless, the phenomenon of partial lying is robust, and an important anomaly in and of itself.

5. CONCLUSION

Summing up our observations we find that the pattern of lying does not change when stakes, consequences or anonymity is altered. We always observe liars, honest subjects and some subjects who lie partially. A model of absolute lying aversion can explain the monotonically increasing pattern of our results. By assuming some heterogeneity in the subjects' disutility of lying it is possible to model the observed behavior of income maximizing subjects as well as that of honest subjects. Nevertheless the model fails to explain the observed pattern of partial cheating. Something prevents some people from lying fully. Using a double anonymous procedure, we were able to show that people do not care particularly about the experimenter's judgment, but that it is possible that they try to uphold a favorable self-concept.

In daily life it is important to be ready to tell compelling lies if ever somebody will doubt the truth of a story. Not lying maximally might leave more space for arguing that one has indeed been honest. If ever anybody is called upon to tell what he has done in our experiment, the person who can say he reported a 4 with a clear conscience might seem more credible. By not reporting a 5 one obviously is not completely greedy and income-maximizing. Reporting a number below 5 might be below the radar of an internal or external moral detection radar. The true type and motivation can be disguised and the signal of probably being an honest person can be upheld because one can claim to not having lied like a true liar.

We presented our method for the first time in 2007, and the working paper has been circulating since 2008. The design is a new type of experiment with the potential to be used in further innovative settings. There is now some literature that uses this design for different research question. We discuss some of these topics in Section 5.

For example, Conrads et al. (2011) have used it in order to investigate team incentives, and Suri et al. (2011) implemented the game in an online labor market. In a variant of the game, the die was covered in a cup in order to increase the credibility that the experimenter cannot observe the outcome (Shalvi et al., 2010; Lewis et al., 2012). Bucciol and Piovesan (2011) use a coin instead of a die, a method which has also been used by Houser et al. (2012), Abeler et al. (2012), and Cohn et al. (2012). The latter two studies extend our study in an important way since they address other subject pools than students. Finally, Greene and Paxton (2009) instructed people to predict the outcome of a random device and asked them to report their guess after the random draw was executed. This method has the advantage that lying is impossible to observe and it is particularly useful in fMRI studies because the subject does not need to operate the random device.

As we were unable to find a definitive explanation for the observed fraction of reported 4s, it also leaves open the interesting question of disguised lies. Several theories of self-concept and self-reputation have the potential to provide ideas for further applications of this design. Empirically, one has to address whether people only lie upward or whether they are even willing to report lower number in order to appear honest. Finally, we now know the pattern of lying but we do not know yet how credible these lies are. It would be interesting to analyze how lying is related to trust and trustworthiness and whether people not lying maximally are assessed as being more trustworthy than those lying fully. Since we hypothesize disguise to be an important motive for partial lying, experiments in which subjects asses the credibility of others' lies are a natural next step.

APPENDIX A – INSTRUCTIONS

The instructions were presented on screen. They were originally in German.

A.1. BASELINE TREATMENT

Screen 1:

For the following questionnaire you will receive a small additional payoff. However, this payoff is not the same for every participant. You determine your own payoff by throwing your die twice as soon as you are asked to.

Your first throw decides on how much you receive. You can see the exact payoff from the following chart. It will remain on the screen until you have entered your throw.

The second throw only serves to make sure that the die is working properly. You may of course throw the die more than twice. However, only the first throw counts.

If you have any question, please raise your hand. If you are ready, please press OK

[Table containing number thrown and payoff as shown on Figure below.]

Screen 2:

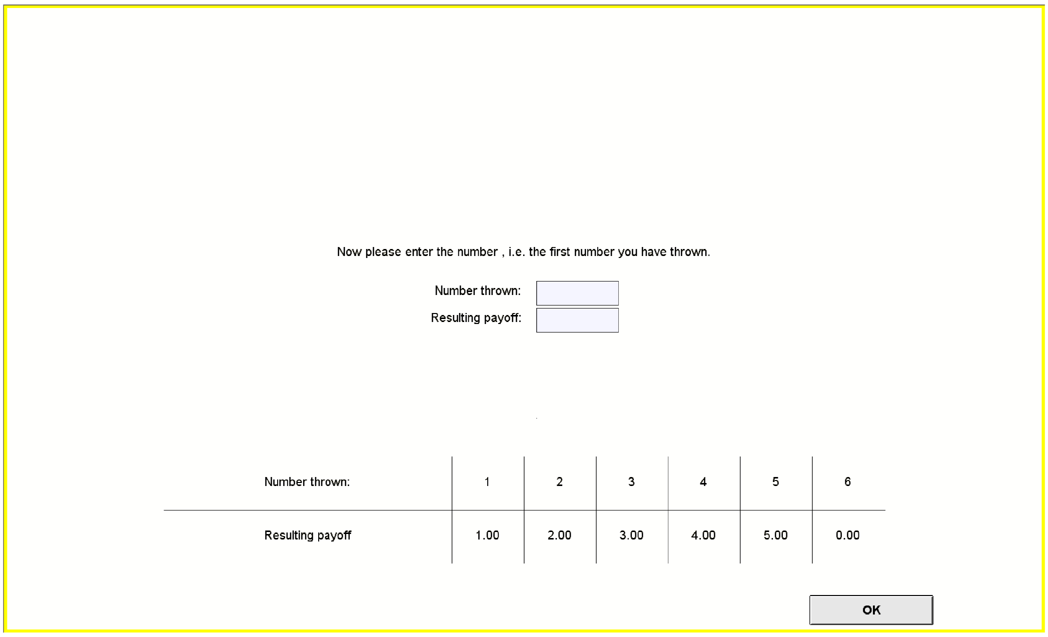
Please throw the die now.

Please keep in mind the first number you have thrown.

If you have thrown the die, please press OK.

[Table containing number thrown and payoff as shown on Figure below.]

Screen 3: Report of number and payoff as shown below.


Now please enter the number , i.e. the first number you have thrown.

Number thrown:

Resulting payoff:

Number thrown:	1	2	3	4	5	6
Resulting payoff	1.00	2.00	3.00	4.00	5.00	0.00

OK

A.2. BELIEF TREATMENT

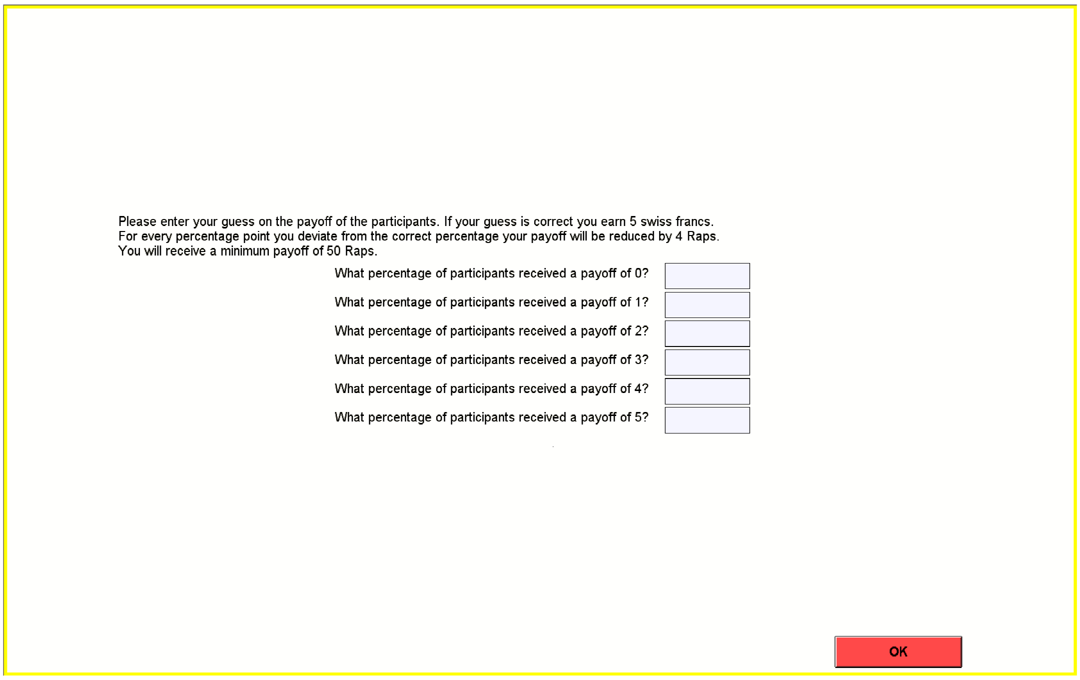
Screen 1:

On the following screen, you will see the instruction for an experiment where 153 participants had taken part. You will not take part in this experiment. Instead, we want you to guess the behavior of these 153 participants. If your guess is accurate you can earn an additional payoff of up to 5 Swiss Francs.

Screen 2-4:

Screens 1-3 of the baseline treatment headed with the text „The other participants saw the following screen“

Screen 5: Report of guess as shown below.



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