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Fairness and Cheating

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Abstract: We present evidence from a laboratory experiment showing that individuals who believe they were treated unfairly in an interaction with another person are more likely to cheat in a subsequent unrelated game. Specifically, subjects first participated in a dictator game. They then flipped a coin in private and reported the outcome. Subjects could increase their total payoff by cheating, i.e., lying about the outcome of the coin toss. We found that subjects were more likely to cheat in reporting the outcome of the coin flip when: 1) they received either nothing or a very small transfer from the dictator; and 2) they claimed to have been treated unfairly. This is consistent with the view that experiencing a norm violation is sufficient to justify the violation of another norm at the expense of a third party. This result extends the growing literature on social norms.

Keywords: cheating; social norms; experimental design

JEL classification: C91; D03; D63;

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1. Introduction

Economists are accustomed to invoking the assumption of rationality. Thus, it is intriguing that not all people who can improve their situation by lying actually do so (e.g., Sutter, 2009; Fischbacher and Heusi, 2008, Gneezy, 2005). There are many possible explanations for this behavior, but we focus on social norms. In particular, we ask whether adherence to the social norm “you shall not cheat” is modulated by fairness concerns. Are individuals who feel they have been treated fairly in an interaction with others less likely to subsequently cheat than those who believe they have been treated unfairly? In this paper, we discuss the laboratory experiments we designed to address this issue and the results we obtained.

Knowing how and whether norm violations propagate is fundamentally important in forming institutions to foster civil societies. A well-known argument, the so-called *Broken Window Theory* of Wilson and Kelling (1982), suggests that when individuals observe frequent violations of a social norm, the probability that they themselves will conform to this norm declines. Keizer et al. (2009) report data from field experiments consistent with this possibility and show that even unrelated norms may be eroded. To the best of our knowledge, however, our data are the first to rigorously assess the causal link between one’s being treated unfairly and one’s subsequent decision to behave unfairly (i.e., to cheat) in a different domain.

We investigate whether the amount subjects receive in a dictator game affects the probability that they will cheat in a subsequent task. We consider both an objective measure of fairness (the amount a subject has received) and a subjective measure (the receiver’s assessment of the proposer’s transfer elicited after the dictator game). Additionally, we implement a “no intentions” treatment wherein receiver earnings are not the result of an intentional dictator decision but instead determined by a random mechanism. This treatment allows us to distinguish between a norm-based explanation for cheating and other possible motives, such as income-targeting or reducing peer inequality. Our results strongly support the idea that experiencing unfair treatment does indeed induce subjects to cheat.

In our experiment, cheating occurs when one misreports the outcome of a coin flip in order to receive a greater payoff. Note that cheating does not affect the payoffs of any other participant except the cheater, and in particular does not impact the partner one had in the dictator game. The self-reported outcome of a random event as an indicator for cheating was used in lab experiments by Fischbacher and Heusi (2008), as well as Bucciol and Piovesan (2009). The latter study is more closely connected to ours, in that Bucciol and Piovesan study the outcome of a coin flip, while Fischbacher and Heusi use the roll of a die. Our study adds to this experimental device a first stage, the dictator game, to manipulate the objective experience of unequal treatment and perceptions of fairness. We further extend existing studies by proposing a simple, nonparametric estimator of the fraction of cheaters in the underlying population.

The remainder of this paper is organized as follows. Section 2 reviews related literature. Section 3 describes the design of our experiment, and Section 4 presents the results. Section 5 reports the "no intentions treatment." Section 6 concludes with our findings.

2. Related literature

Social norms are explicit or implicit rules that define (context specific) appropriate behavior. They may be enforced by internal or external sanctions (Coleman, 1990). While norm violations often result only in informal punishment by the relevant group (e.g., shunning behavior), they may also trigger formal (even severe) legal sanctions. Even when the risk of formal or informal punishment is near zero, people are often remarkably reluctant to violate social norms to benefit themselves. Humans have a tendency toward norm-obedience. Consequently, it is particularly interesting to examine situations where this tendency fails.

Numerous studies show that breaking a norm is more acceptable when it is a reciprocal response to another person's socially ill-perceived action (see, e.g., Fehr et al. (1993) for a labor market application). Greenberg (1990) reports a field experiment in which a company subjected its workers to a temporary pay-cut due to the loss of a large contract. The experimental variation was the way in which the company communicated this measure across plants. Workers at one production site perceived the pay-cut to be unfair, while at the other site it was carefully explained so as not to evoke this feeling. Greenberg shows that the workers in the "unfair" condition responded with a substantial increase in employee theft, but this was not observed in the "fair" condition.

Nevertheless, other people's norm violations can impact norm obedience even absent reciprocity motives. For example, Keizer et al. (2009) demonstrate that people litter significantly more when the environment is more "disorderly." In the same paper, Keizer et al. show that an envelope hanging out of a mailbox with a €5 note attached was stolen twice as often when the mailbox was covered with graffiti than when the area was clean. This result points to the existence of a "cross-norm (dis)inhibition effect." Remarkably, general disorder seems to induce a violation of the no-stealing norm, which is not only widely accepted but also legally protected.

In economics, cheating has been studied in the context of cheap-talk games (e.g., Sutter (2009); Charness and Dufwenberg (2006); Gneezy (2005)), where subjects send messages that can be used to deceive their counterparts. While these studies have taken an important first step in understanding situations in which people resort to deception, they do not address the propagation of norm violations.

Experimental evidence on unobserved cheating is only recently beginning to accumulate. In the experimental tournament of Freeman and Gelber (2010), subjects cheated by misreporting the number of correctly solved mazes. The misreporting varied according to monetary incentives.

Likewise, Pruckner and Sausgruber (2008) observed that 2/3 of newspaper readers took advantage of an opportunity to take a newspaper without paying. Moreover, 90 percent of those who did pay actually paid less than the full price. This sort of "incomplete cheating" also occurred in an experiment conducted by Fischbacher and Heusi (2008), wherein subjects reported the result of a private die roll to determine their payoff. Fischbacher and Heusi found that subjects shaded the outcome of the roll favorably, but did not take the maximum earnings advantage offered by the lying opportunity. Note that a possible explanation for incomplete cheating is an individual's desire to preserve a favorable self-concept (Mazar et al., 2008).

Finally, Bucchiol and Piovesan (2009) conducted an experiment in which children tossed a coin in private and reported the result, knowing that they would receive a reward only if they reported one of the two outcomes. Bucchiol and Piovesan found that an overall 86% of children reported the profitable outcome, suggesting a substantial proportion of them cheated. A second treatment, in which children were explicitly instructed not to cheat, significantly reduced cheating.

3. Design of the experiment

The experiment was conducted in MELESSA, the experimental laboratory at the University of Munich. Participants were recruited from the lab's subject database, which is maintained using software written by Greiner (2004). A total of 502 individuals participated in one of 21 sessions.[1] Most subjects were undergraduates from the University of Munich. Average earnings were €10.5, including a €4-show-up fee, for an average duration of around 35 minutes. The experiment was implemented in paper and pencil format and consisted of two stages: a decision situation (the dictator game) and the coin flip game. Subjects were not aware of the second stage until the

[1] 20 sessions comprised of 24 subjects, one session of only 22 subjects.

dictator game was resolved. While the dictator game took around 25 to 30 minutes, the coin flip part lasted no more than 5 minutes.

The dictator game was conducted using the strategy method. At first, subjects did not know whether they would be in the role of proposer or receiver. We asked them to decide how they would split an initial endowment of €8 between themselves and another person in the room if it turned out that they were in the role of the proposer. The intended transfer had to be marked on the decision sheet and could range from €0 to €8 in increments of €2. Subjects knew that after they made their decision, half of them would be randomly selected as proposers, and their choice would be implemented. The remaining half would be passive receivers, whose earnings would be determined solely by the decision of the matched proposer. A short quiz ensured that all subjects understood the rules of the dictator game, as well as the experimental procedure.

The randomization of roles and pairs was implemented by means of 24 identical envelopes. Each envelope contained a tag with a code consisting of a letter (from A through L) and a number (either 1 or 2). Thus, a tag might read A1 or A2, B1 or B2, C1 or C2, and so on. The number indicated the role in the game (1 for the proposer, 2 for the receiver) and the letter matched each participant with another person in the room. Those with an identical letter formed a pair, with player 1 as proposer and player 2 as receiver. After the randomization procedure, the sheets were collected and matched with the decision sheet of the respective counterpart, and the associated payoffs were recorded. Then subjects were informed of their earnings.

Not computerizing the experiment allowed us to provide receivers with their matched counterpart's decision sheet. As a result, receivers could read directly how much the proposer had transferred. They could also see the proposer's tag number, which was written on the sheet as well. This procedure guaranteed full anonymity while allowing receivers to verify that the matching procedure was performed correctly and not manipulated by the experimenter.

Receivers were also given a sheet on which they had to indicate, using a four-point scale, how fair they considered the behavior of the other person. They could rate the matched proposer as either “fair,” “rather fair,” “rather unfair,” or “unfair.” In deliberately excluding a neutral category, we sought to avoid having subjects pick this category without further reflection. Proposers received a confirmation that their decision was actually payoff relevant, and an additional blank sheet to ensure that every subject received the same number of sheets, which made it impossible to figure out the roles of other subjects in the lab by counting the number of sheets.

After the dictator game was resolved, subjects were informed that they would get a chance to increase their earnings before the session closed. While the experimenter explained the rules of the coin flip game, an assistant provided each participant with a €1 coin. Subjects were told that they would have to flip the coin and that their payoff would be determined by the upper side of the coin. If the coin landed on heads, their payoff would be €1 (i.e., they could keep the coin); if the coin landed on tails, their payoff would be €3. Then subjects were instructed to flip the coin and to report the outcome by checking the appropriate box on the sheet. Finally, the earnings from the coin flip and the dictator game were paid out and the experiment concluded.

We did not want to invite people to cheat. Indeed, we never mentioned the possibility. We also did not remind them to be honest. Nevertheless, the environment was quite simple, and it likely occurred to most subjects that cheating was a riskless way to earn two additional Euros.

It is important to emphasize that cheating was understood by the subjects to be riskless. Indeed, all seats in the laboratory were separated and view-protected; it was clear that cheating could not be detected. Furthermore, we explicitly instructed subjects to flip their coin in such a way that nobody else could observe the outcome.

4. Results

We begin with the dictator game. Table 1 shows the distribution of those transfers that were actually implemented after random assignment of roles (the allocation decisions of assigned proposers) and the hypothetical transfers of assigned receivers (recall that we used the strategy method). A chi-2 test confirms that both distributions are not significantly different (p-value= 0.942). On average, €1.90 were transferred, meaning that proposers retained about 76% for themselves, which is within the usual range of outcomes in dictator games (see Camerer, 2003, Chapter 2).

We use the amount transferred as an indicator of how fair the receiver was treated. Evidence from a large number of ultimatum games presented in Camerer (2003) demonstrates that on average, one-third of the receivers reject offers between 20% and 30% of the endowment, indicating that a substantial proportion of subjects perceives such a division as unfair. Camerer also shows that the rejection rate of offers between 0% and 10% is usually far above 50%, and in many experiments close or equal to 100%. We therefore interpret proposer decisions with a transfer below an equal split – €0 or €2 – as potentially unfair.

Table 2 shows the distribution of receivers' subjective fairness ratings. Almost 44% of receivers perceived that they had been treated "fairly." The response options "rather fair," "rather unfair," and "unfair" were all chosen by less than one fifth of our sample. In Table 3, we combine the responses "fair" and "rather fair" (62.6%), as well as "unfair" and "rather unfair" (37.5%) in a cross-tabulation with the amount received (also combined into three categories of about equal size). There is a statistically significant association (χ^2=97.01, p-value<0.001) between the two variables: Subjects who received a larger amount were more likely to say that they have been treated (rather) fairly. In no case was an equal split rated as "unfair" or "rather unfair."

Next, we turn to the outcome of the coin flip. 374 of the 502 subjects (74.5%) reported the high-payoff outcome (tails), which is significantly different from the outcome of a fair coin (p-value<0.001). This finding shows that cheating occurred on a broad basis.

We can go one step further and nonparametrically estimate the fraction of cheaters in the population from which our sample of subjects was drawn. The identification problem can be described by a simple mixture model with two types, cheaters and non-cheaters. With fair coins and a large sample, half of the subjects should have flipped tails and the other half heads. Those fifty percent of subjects who flipped tails (the high-payoff outcome) had no incentive to lie; thus, it seems reasonable to assume that they reported the true outcome. However, among the other half who flipped heads (the low-payoff outcome), some might have lied.

From the observed sample proportion of tails, we can estimate the implied proportion of cheaters in the population nonparametrically. The mixture model assumes (quite naturally) that the population from which the subjects were drawn consists of two types: cheaters and honest subjects. We can characterize the observed proportion of tails p in the population by

$$p = \gamma \cdot 1 + (1-\gamma) \cdot 0.5 = 0.5(1+\gamma).$$

A fraction γ of the members of the population cheats and reports tails with a probability of 1, while honest members of the population report tails only with probability 0.5. We will call γ the cheating rate. After solving for γ, we can use the sample analog of p (i.e., the observed fraction of tails) to obtain a consistent estimate of the population cheating rate γ as 2·(74.5% – 50%) = 49%. By the same logic, cheating rates can be computed for subsamples of our subjects. Figure 1 illustrates that the proportion of tails in a (sub)population maps directly into the implied fraction of cheaters via a linear function.

Table 4 contains information about the proportion of subjects who reported tails, and a comparison of cheating rates by experimental outcomes and demographic background variables. The proportion of tails is significantly different from 50% for all sub-groups.

The implied cheating rate is higher among receivers than among proposers (53% vs. 45%). Interestingly, this difference is almost entirely due to the high cheating rate of those receivers who earned nothing. When comparing only proposers and receivers with a positive payoff (at least €2), the fraction of cheaters is almost identical – even though proposers earned roughly twice as much as receivers with non-zero earnings (€6.12 vs. €3.08). The fact that the cheating rates are similar, despite a huge earnings gap, seems to weigh against an "income-targeting" explanation for cheating. If subjects have an individual income target in mind when they come to the experiment, and if those who have not yet reached their income target have a higher propensity to cheat, groups with different income levels should also exhibit markedly different cheating rates.

The self-reported fairness rating helps to explain cheating among receivers. The cheating rate of those who rated their counterpart as "fair" or "rather fair" is 46.5%, compared to 64% among those who perceived to have been treated "unfairly" or "rather unfairly" (p-value=0.0078).

Cheating seems to be correlated with distributional preferences in the sense that subjects who revealed stronger other-regarding preferences in the dictator game were more likely to be honest. In particular, proposers who shared half of their endowment with the receiver cheated significantly less than their more selfish peers (cheating rates are 37.5% vs. 48.5%). The same finding emerges when considering the hypothetical transfers of receivers. Those who would have implemented an equal split were significantly more honest than those who would have retained a larger share for themselves.

We also find that men cheat more often than women (p-value = 0.000), which is in line with Dreber and Johannesson (2008). The cheating rates are 60% vs. 40%, respectively. Moreover, experienced subjects (defined as having participated in more than one previous experiment in the MELESSA lab) are significantly less honest than subjects who participated in no more than one previous experiment (61% vs. 39%). Cheating rates by subjects' major field of studies are similar and we do not find significant differences, with the exception that students in mathematical fields tend to be slightly more dishonest. However, sample sizes become (relatively) small when testing for differences across study fields.

5. "No Intentions" Treatment

In this section, we present a treatment designed to disentangle motives for cheating. Unfair dictator behavior may not be the only reason why receivers with a low payoff perceive it as legitimate to seek compensation via cheating. Our design creates two types of inequality – hierarchical inequality (between a dictator-receiver pair) and peer inequality (among receivers). Hierarchical inequality is created by the intentional decision of the dictators. In contrast, peer inequality is the recognition by receivers that the decisions of different dictators are likely to induce inequality among receivers. While a receiver cannot observe the earnings distribution among his peers, he will likely believe that his earnings are higher than average if the dictator has implemented an equal split, but lower than average if the dictator has transferred nothing to him. Therefore, it might be argued that subjects not only cheated because they were treated unfairly by the dictator or fell short of their income target, but also because they observed that some (or most) of their peers were likely doing better.

To discriminate between a violation of the fairness norm and competing cheating motives, we implemented a treatment that preserves earnings inequality across receivers but where unfair dictator intentions are absent. To do this while preserving the timing and procedures in the lab, we replaced the dictator game with a neutral task of the same length. This avoided distortion due to a correlation between the cheating propensity of subjects and the time spent in the lab. The task consisted of answering a questionnaire which was unrelated to cheating and social norms.[2] As with the dictator game, the questionnaire was completed using paper-and-pencil. After completing the alternative task, subjects were informed of their earnings. However, they were also informed that others earned either more, less or the same as they did. By implementing this earning inequality via a random mechanism, we captured peer-inequality among receivers that also occurs in the dictator game yet avoided acts which might be viewed as particularly fair or unfair. An attractive feature of this design is that it also provides evidence on an income-targeting explanation for differences in cheating rates.

We implemented these sessions in the following way. When subjects entered the lab, they followed the usual procedure of drawing a seat number. They did not know it, but this procedure also determined their earnings for the experiment. On their seat they found a sealed envelope which had been prepared before the experiment and randomly placed. Subjects were instructed not to open the envelope until told to do so. Each envelope contained a note stating how much a subject would earn for participating in the experiment. After all subjects finished the questionnaire they were told that they would receive either 4, 6, or 8 Euros. We chose the distribution of payoffs to reflect the distribution of the most frequent receiver earnings in each session of the "no intentions" treatment.[3] We then instructed subjects to open their envelope to find out how much they had earned and proceeded with the coin flip game just as in the

[2] The questionnaire contained hypothetical choice questions about housing demand and mobile phone use, as well as the same demographic background questions as used in the dictator game.
[3] We did not match earnings above the equal split in the dictator game due to the low number of observations with 6 or 8 Euros in the dictator game.

"fairness" treatment. Total earnings consisted of the amount specified in the note plus the payoff associated with the coin flip.

We ran 10 sessions of this condition with a total of 238 subjects. Results are summarized in Table 5.[4] The aggregate cheating rate was 42.86%, which is slightly, and insignificantly, lower than the overall cheating rate in the "fairness" treatment (49.00%). It is comparable to the cheating rate among proposers, which was 45.02%, but significantly lower than the cheating rate across receivers (52.98%). This is consistent with our initial hypothesis that cheating is higher among subjects who feel treated unfairly.

Unlike the fairness treatment, implied cheating rates are not monotonically related to amounts earned. In particular, the implied cheating rate among those with €4 earnings (41.30%) is lower than among those who earn €6 (52.12%), while the implied cheating rate among €8 earners is 36.00%. Moreover, cheating rates are statistically identical between those who earned the minimum amount and those who earned more (see Figure 2). This result is evidence against both peer inequality and income-targeting as motives for cheating. Our data suggest, rather, that experiencing unfair treatment leaves one more likely to cheat on a subsequent decision.

The same conclusion emerges from a regression framework. Table 6 reports average marginal effects for a probit regression where the dependent variable is 1 if a subject reports tails. Thus, positive marginal effects correspond to an increased likelihood of cheating. Column 1 contains results for receivers in the "fairness treatment" and shows that the probability of reporting tails is significantly higher for those subjects who received zero in the dictator game vs. those who received a positive amount. Male subjects and those with more lab experience were also significantly more likely to report tails, while none of the dummies for major subject was significant.

[4] Due to the smaller sample size in the "no intentions" treatment, some comparisons of cheating rates, e.g., across study major, suffer from a low number of subjects per category.

Column 2 of Table 6 reports the results of the same specification for the "no intentions" treatment. If income-targeting or peer inequality are to explain the variation in cheating rates across earnings, the dummy for earning the minimum amount should continue to be a significant predictor of reporting tails. However, column 2 shows that this is not the case; the marginal effect is insignificant and very close to zero. Being male is again positively associated with reporting tails, but not significantly so, and the strong experience effect from the fairness treatment vanishes completely.

Since the proportion of inexperienced subjects was much lower in the "no intentions" treatment, we wanted to ensure that differences in sample composition were not driving our main result. Therefore, as a robustness check, we repeated the analysis using only subjects who had not previously participated in more than seven experiments (the number which corresponds to the most experienced subject in the fairness treatment (column 3)). We further estimated a weighted regression where we weighted lab experience so that it corresponded to the distribution in the fairness treatment (column 4). Neither of these robustness checks changed our result that randomly assigned earnings are uncorrelated with the decision to report tails in the "no intentions" treatment.

6. Conclusion

We reported data from an experiment designed to test how violations of the fairness norm implicit in the dictator game affect individuals' propensity to cheat. Since we could neither manipulate nor observe social norms directly, we used two approaches to measure fairness as a social norm in the dictator game: 1) the amount transferred; and 2) the receiver's subjective fairness perception of the amount received. Our analysis shows that both measures significantly predict cheating. To rule out alternative explanations, namely income-targeting or reducing peer inequality, we used a random mechanism to create the same degree of earnings inequality so that subjects were not exposed to unfairness.

Our results appear to show that the perception of being treated unfairly by someone else increases an individual's propensity to cheat. This suggests that an individual is less likely to follow social norms in an environment where norms are perceived to be weak, in line with recent theoretical models and a growing body of empirical results on the role of social norms. This emerging literature improves our understanding of situations in which a considerable fraction of people do not conform to the social norm of being honest.

The causal link suggested in this paper is important for decisions in normative environments, such as the decision to evade taxes. A tax system may be perceived as unjust if tax rates are excessively high or if it contains loopholes which allow certain segments of the population to substantially reduce their tax burden. An experimental study by Spicer and Becker (1980) showed that in a situation that was specifically framed in a taxation context, subjects indeed tried to evade taxes more often when they were exposed to higher than average tax rates. Our study suggests that this connection may be generalized to other sources of unfairness.

Methodologically, our experimental design creates an environment that allows us to study how interaction with others affects individuals' propensity to cheat. The limitation, however, is that individual cheating cannot be observed. Thus, inferences must be based on aggregate statistics that characterize differences between observed and predicted distributions of self-reported outcomes of a random event. This approach requires large sample sizes to find significant effects. Nevertheless, we did find significant effects, namely that subjects who perceive they have been treated unfairly by their respective dictators cheat more often in a subsequent coin flip game.

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Tables and Figures

Table 1: Hypothetical and actual transfers in the dictator game

	Proposers: actual transfers		Receivers: hypothetical transfers	
Amount	Frequency	Percentage	Frequency	Percentage
0	96	38.25	122	48.61
2	75	29.88	53	21.12
4	77	30.68	68	27.09
6	2	0.80	2	0.80
8	1	0.40	6	2.39
Total	251	100.00	251	100.00

Table 2: Subjective perception of the fairness of transfers made (receivers)

Perceived Fairness	Frequency	Percentage
Rather fair	110	43.82
Fair	47	18.73
Unfair	45	17.93
Rather Unfair	49	19.52
Total	251	100.00

Table 3: Association between transfer received and subjective fairness rating (receivers)

Perceived Fairness	Amount received			Total
	0	2	≥ 4	
(Rather) Unfair	69 (71.88%)	25 (33.33%)	0 (0.00%)	94 (37.45%)
(Rather) Fair	27 (28.13%)	50 (66.67%)	80 (100.00%)	157 (62.55%)
Total	96	75	80	251 (100.00)

Table 4: Comparison of cheating rates

variable	values	N	% tails	implied % of cheaters	p-value from test for equality of proportions
All subjects		502	74.50	49.00	
Role	Proposer Receiver	251 251	72.51 76.49	45.02 52.98	0.0745*
Earnings (Proposers)	≤ 4 6 8	80 75 96	68.75 74.67 73.96	37.50 49.34 47.92	≤4 vs. 6: 0.1370 6 vs. 8: 0.9400 ≤4 vs. >4: 0.1014
Earnings (Receivers)	0 2 ≥ 4	96 75 80	82.29 70.67 75.00	64.58 41.34 50.00	0 vs. 2: 0.0025*** 2 vs. ≥4: 0.2795 0 vs. >0: 0.0038***
Hypothetical Transfer (Receivers)	0 2 ≥ 4	122 53 76	78.69 79.25 71.05	57.38 58.50 42.10	0 vs. 2: 0.8904 2 vs. ≥4: 0.0668* 0 vs. >0: 0.1755
Fairness rating	(rather) fair (rather) unfair	157 94	73.25 81.91	46.50 63.82	0.0078***
Gender	Male Female	215 287	80.47 70.03	60.94 40.06	0.0000***
Lab experience	0 or 1 >1	279 223	69.53 80.72	39.06 61.44	0.0000***
Major field of study	Economics Business Law Medicine Education Maths etc. other	35 77 34 56 62 62 176	68.57 75.32 76.47 71.43 77.42 82.26 72.16	37.14 50.64 52.94 42.86 54.84 64.52 44.32	Econ vs. Maths: 0.0093* Maths vs. other: 0.0062* all other pairwise comparisons insignificant

"Maths etc." equal 1 if major subject is Mathematics, Physics, or Engineering.
*** p<0.01, ** p<0.05, * p<0.10

Table 5: Comparison of cheating rates in the "no intentions" treatment

variable	values	N	% tails	implied % of cheaters	p-value from test for equality of proportions
All subjects		238	71.43	42.86	
Earnings	4	92	70.65	41.30	4 vs. 6: 0.1694
	6	71	76.06	52.12	6 vs. 8: 0.0498 **
	8	75	68.00	36.00	4 vs. 8: 0.4847
Gender	Male	102	74.51	49.02	0.0963 *
	Female	136	69.12	38.24	
Lab experience	0 or 1	39	71.79	43.58	0.9210
	>1	199	71.36	42.72	
Major field of study	Economics	26	80.77	61.54	
	Business	24	75.00	50.00	
	Law	18	77.78	55.56	
	Medicine	10	80.00	60.00	
	Education	33	63.64	27.28	
	Maths etc.	21	70.00	40.00	
	other	97	69.07	38.14	

*** p<0.01, ** p<0.05, * p<0.10

Table 6: Probit regression for reporting the better outcome

	(1)	(2)	(3)	(4)
	"fairness treatment" only receivers	"no intentions" treatment	"no intentions" treatment; only subjects with lab experience < 7	"no intentions" treatment; weighted regression
Received zero	0.0901 *	-0.0087	0.0184	-0.0826
	(0.0535)	(0.0604)	(0.0682)	(0.0615)
Male	0.1371 ***	0.0558	0.1027	0.1060 *
	(0.0528)	(0.0632)	(0.0691)	(0.0591)
Lab experience > 1	0.1120 **	-0.0061	-0.0155	0.0049
	(0.0523)	(0.0781)	(0.0792)	(0.0580)
Major Econ	-0.1343	0.1061	0.1272	0.1939 ***
	(0.1211)	(0.0881)	(0.0907)	(0.0683)
Major Business	0.0155	0.0421	0.0340	-0.0313
	(0.0762)	(0.0985)	(0.1090)	(0.0889)
Major Law	-0.0128	0.0788	0.1517	-0.0654
	(0.1123)	(0.1059)	(0.1063)	(0.1854)
Major Medicine	-0.0691	0.1175	0.1145	0.1377
	(0.0998)	(0.1219)	(0.1262)	(0.1293)
Major Education	0.0663	-0.0527	-0.0094	-0.1386
	(0.0743)	(0.0933)	(0.0984)	(0.0936)
Major Maths	-0.0279	-0.0146	-0.1751	-0.2380 **
	(0.0975)	(0.0981)	(0.1327)	(0.1112)
Log-likelihood	-130.0347	-140.2994	-106.5333	-126.7586
# observations	251	238	184	236

Notes: Average marginal effects reported. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.10

Figure 1: Relationship between the proportion of tails and the implied fraction of cheaters

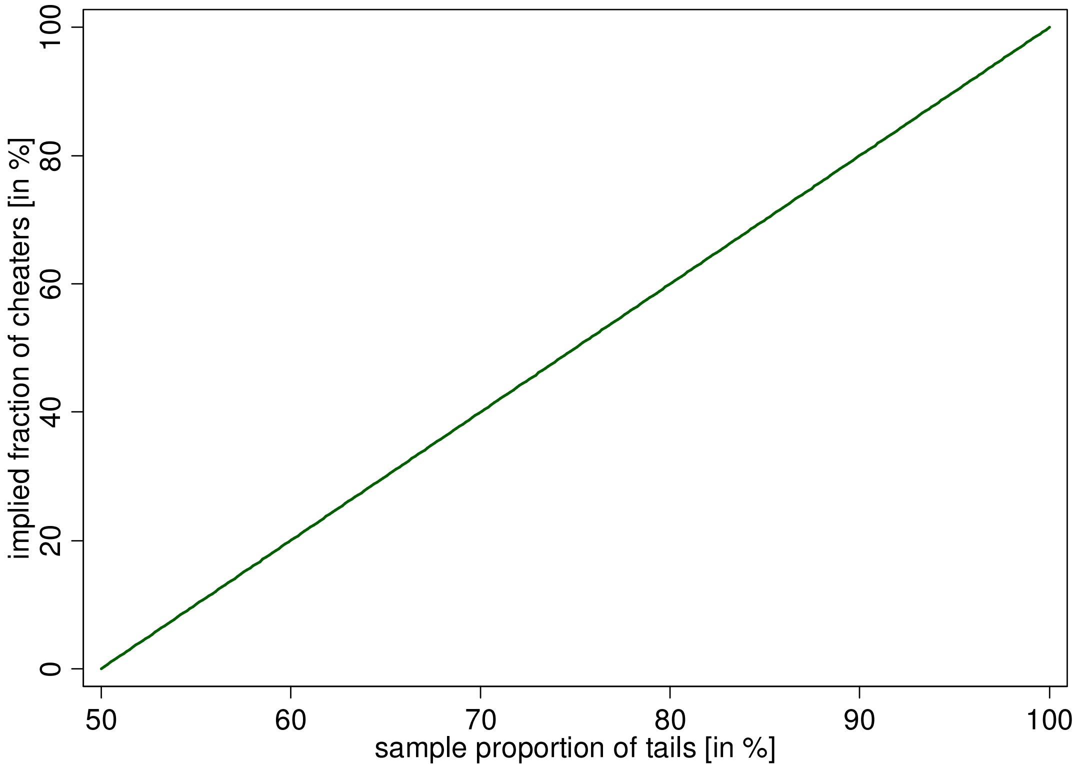
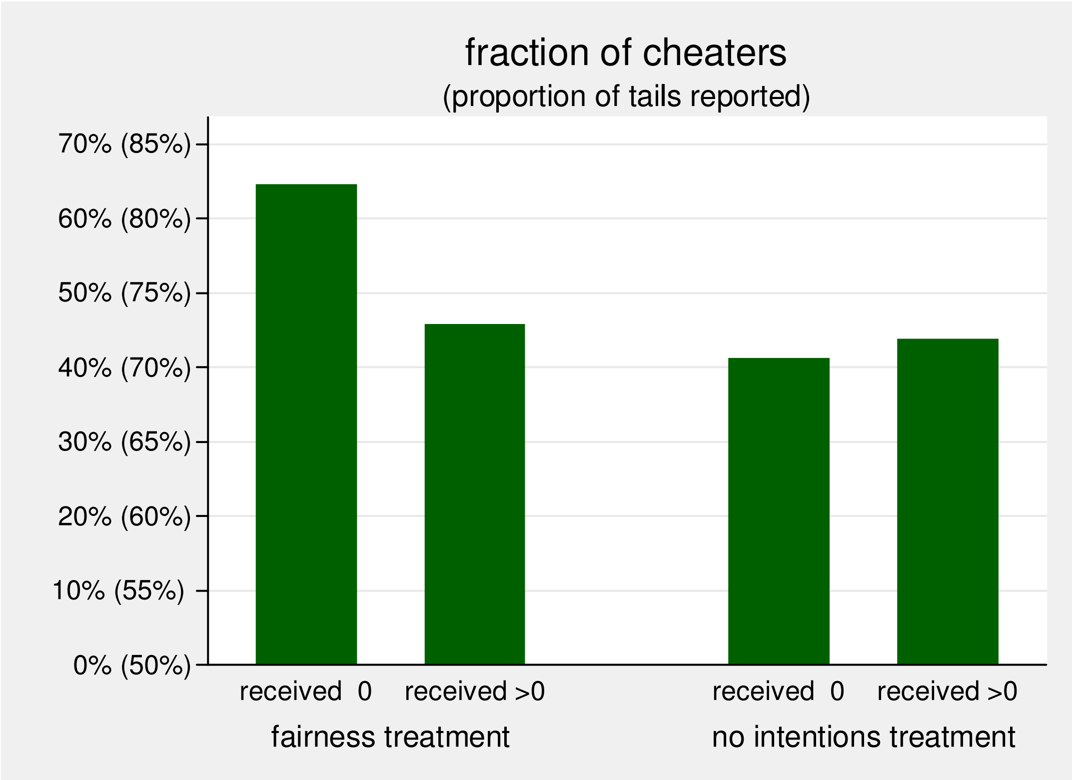


Figure 2: Cheating rates (proportion tails reported) by earnings in both treatments



Appendix: Instructions

The Experiment

In this experiment there will be two roles, which will be referred to as **Person 1** and **Person 2**, respectively. You will be randomly assigned to one of these roles. In the course of the experiment you will be randomly and anonymously matched with another participant, who will be in the opposite role. As mentioned above, you will not receive any information about the other participant's identity.

Procedure

At the beginning, Person 1 receives an initial endowment of $8. Then Person 1 has to divide the $8 between himself and Person 2.

The **payoff to Person 1** is €8 minus the amount sent to Person 2.

The **payoff to Person 2** is the amount that Person 1 has sent.

When deciding about how much to send, Person 1 can choose **one** of the following options:

- Send €0 to Person 2 and keep €8; or
- Send €2 Euros to Person 2 and keep €6; or
- Send €4 to Person 2 and keep €4; or
- Send €6 to Person 2 and keep €2; or
- Send €8 to Person 2 and keep €0.

We will ask you to indicate your preferred option **before you know if you actually are Person 1**.

Whether you will be Person 1 or Person 2 will be determined randomly. Therefore you will have to pick an envelope later. Inside there will be a tag with a combination of a letter and a number (either 1 or 2). So at tag might read A1 or A2, B1 or B2, C1 or C2, and so on. The number indicates whether you will be Person 1 or Person 2. The letter matches you with another participant in the opposite role. For example, the person who gets A1 is matched with the person who gets A2.

So, if you have a "1" you will be Person 1. Your earnings will correspond to the option which you chose. If you have a "2" you will be Person 2. Then your earnings will be the amount sent by your matched Person 1.

Quiz

This short quiz ensures that you understand the features of the experiment. You cannot earn anything in the test, but it might help you to make a good decision afterwards.

Question 1:

Imagine you have chosen to send €4 to Person 2 **in case that it turns out that you are Person 1**, and to keep €4 for yourself.

Then you pick an envelope which contains the tag **X2**. This means that your role is Person 2.

Your matched counterpart (i.e. the person with the tag **X1**) has indicated that he would keep €6 and send only €2.

a) How much do you earn in this case? ___

b) How much does your matched counterpart in the role of Player 1 earn? ___

Question 2:

Now imagine that you have chosen to keep €8 and send nothing to Person 2. Then you pick an envelope with the tag **X1**. This means that your role is Person 1.

a) How much do you earn in this case? ___

b) How much does your matched counterpart in the role of Player 2 earn? ___

Your Choice

So far you do not know if you are Person 1 or Person 2. Now you have to decide what you will do **if it turns out you will be Player 1.**

Please decide now how much you want to give to Person 2 by ticking the appropriate box.

IMPORTANT: Think carefully about your decision! Once you ticked the box, you cannot change it any more.

How much of the €8 would you send to Person 2?

€0

€2

€4

€6

€8

We will continue after all subjects have made their decisions. Please wait quietly.

Questionnaire

Please answer the following questions about yourself. Your anonymity remains guaranteed.

1) Are you Male or Female
- o Male
- o Female

2) What is your major?

____

3) What is your age?

____

4) In how many MELESSA experiments did you participate so far? If you are not sure please give an approximate number.

______

Only dictators:

You have been randomly assigned to the role of Person 1.

Your payoff is €8 minus the amount that you decided to send to Person 2.

Please wait for a moment until you receive further instructions.

Only receivers:

You have been randomly assigned to the role of Person 2.

You received a sheet with the choice of the person who has been matched with you. On this sheet you can see how much Person 1 has sent to you. This is your payoff from this experiment, which we will pay out to you later.

Now we would like to know your opinion about the fairness of Person 1. How would you rate the behavior of Person 1?

Fair
Rather Fair
Rather Unfair
Unfair

Please wait for a moment until you receive further instructions.

All subjects:

Now you have the chance to increase your earnings.
You will get a €1 coin from us. We will ask you to flip the coin and to report the result below. Depending on how lucky you are you can earn the following:

If you flip **heads** on the upper side, you have earned €1. In this case you keep the €1 coin.
If you flip **tails** on the upper side, you have earned €3. In this case you keep the €1 coin and you will receive a further €2 at the end.

Now please flip the coin.
Flip the coin at your seat and make sure that the coin doesn't fall to the ground and that none of the other participants can see the result of the coin flip!

Please mark what was on the upper side of the coin:

- **Heads**
- **Tails**

Thank you for your participation!
For the payment we will now call you person by person. Please take all sheets with you.