

Vetter, Stefan; Houser, Daniel; Winter, Joachim

Conference Paper

Fairness and Cheating

Beiträge zur Jahrestagung des Vereins für Socialpolitik 2010: Ökonomie der Familie - Session: Social Preferences, No. E13-V2

Provided in Cooperation with:

Verein für Socialpolitik / German Economic Association

Suggested Citation: Vetter, Stefan; Houser, Daniel; Winter, Joachim (2010) : Fairness and Cheating, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2010: Ökonomie der Familie - Session: Social Preferences, No. E13-V2, Verein für Socialpolitik, Frankfurt a. M.

This Version is available at:

<https://hdl.handle.net/10419/37413>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Fairness and Cheating

Daniel Houser

George Mason University
dhouser@gmu.edu

Stefan Vetter

University of Munich
stefan.vetter@lrz.uni-muenchen.de

Joachim Winter

University of Munich
winter@lmu.de

August 2010

Abstract: We present evidence from a laboratory experiment 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. Then subjects flipped a coin in private and reported the outcome; they could increase their total payoff by cheating. Those subjects who received zero or a small transfer from the dictator, and those who said that they were treated unfairly, were more likely to cheat when reporting the outcome of the coin-flip. 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;

Acknowledgements: We are grateful to Armin Falk, Urs Fischbacher, Li Hao, Dirk Hofmann, Martin Kocher, Sandra Ludwig, Klaus Schmidt, Daniel Schunk, Sebastian Strasser and Christian Traxler, as well as to participants of the University of Munich's MELESSA seminar, the 10th internal meeting of the SFB/TR15, the 4th Nordic Conference in Behavioral and Experimental Economics, and the ESA 2010 World Meeting. Financial support was provided by the Deutsche Forschungsgemeinschaft via SFB/TR 15 and MELESSA. For providing laboratory resources we kindly thank MELESSA of the University of Munich. Valuable research assistance by Benedikt Siegler and student assistants of MELESSA is gratefully acknowledged.

1. Introduction

For economists accustomed to invoking the assumption of rationality it is intriguing that not all people who can make themselves better off by lying actually do so (e.g., Sutter, 2009; Fischbacher and Heusi, 2008, Gneezy, 2005). Among the many explanations for this behavior, we focus on social norms. Specifically, we ask whether adherence to the social norm stipulating “you shall not cheat” is modulated by fairness concerns: In particular, are individuals who feel that they have been treated fairly in an interaction with others less likely subsequently to cheat than those who believe they have been treated unfairly? This paper presents the design of, and results from, laboratory experiments that allow us to address this issue.

Knowing how and whether norm violations propagate is of fundamental importance to the formation of 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 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 assess rigorously the causal link between one’s being treated unfairly and one’s subsequent decision to behave unfairly (to cheat) in a different domain.

We investigate whether the amount subjects received in a dictator game affects the probability that they 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 where 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 that it is indeed the experience of unfair treatment which induces 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 they study the outcome of a coin flip whereas Fischbacher and Heusi use the roll of a die. Our study adds to this experimental device a first stage, the dictator game, in order 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. This estimator is derived from a mixture model naturally implied by our environment.

The remainder of this paper is organized as follows. Section 2 reviews related literature. We describe the design of our experiment in section 3 and present the results in section 4. In section 5 we report the “no intentions” treatment. Section 6 discusses our findings and concludes.

2. Related literature

Social norms are explicit or implicit rules which define (context specific) appropriate behavior, and which might be enforced by internal or external sanctions (Coleman, 1990). Norm violations often result only in informal punishment by the relevant group (e.g., shunning behavior). On the other hand, norm violations might 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 in order to benefit themselves.

The human tendency towards norm-obedience leaves it particularly interesting to examine situations in which it fails.

Numerous studies show that breaking a norm is more acceptable when doing so is a reciprocal response to a socially ill-considered action by another person (see for example Fehr et al. (1993) for a labor market application). Greenberg (1990) reports a field experiment where 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 so that the workers at one production site perceived the pay-cut to be unfair, while at the other site it was carefully explained in order 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.

However, 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 often 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. These studies have taken an important first step in helping one to understand situations in which people resort to deception, but 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, and misreporting varied according to monetary incentives. Also, 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 the experiment of Fischbacher and Heusi (2008), where subjects reported the result of a private die roll to determine their payoff. They 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, Bucciol 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. Bucciol and Piovesan find evidence that a substantial proportion of children cheated since an overall 86% reported the profitable outcome. 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

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

aware of the second stage until the 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 are 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 making their decision, half of them were to be randomly selected as proposers, and their choice was to be implemented. The remaining half would be passive receivers, whose earnings would solely be determined 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 of them contained a tag with a code consisting of a letter (from A through L) and a number (either 1 or 2). So 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, matched with the decision sheet of the respective counterpart, and the associated payoffs were recorded. Then subjects were informed about their earnings.

By not computerizing the experiment it was possible to return to receivers the decision sheet of their matched counterpart. In this way they could directly read off how much the proposer had transferred. Furthermore, they could see the proposer's tag number which was written on the sheet as well. This procedure still guarantees full anonymity and has the

advantage that receivers could verify that the matching procedure was done correctly and not manipulated by the experimenter.

In addition to their counterpart's decision sheet, receivers were also given a sheet on which they had to indicate how fair they considered the behavior of the other person using a four-point scale. They could rate the matched proposer as either "fair", "rather fair", "rather unfair", or "unfair". By deliberately excluding a neutral category we were seeking to avoid that 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 would close. 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 was determined by the upper side of the coin. If it was heads their payoff would be €1 (i.e. they would keep the coin), while it would be €3 if it was tails. Then subjects were instructed to flip the coin and to report the outcome by ticking 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. However, the environment is 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 has been 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, which indicates 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 decision with a transfer below an equal split – €0 or €2 – as potentially unfair.

Table 2 shows the distribution of receivers' subjective fairness rating. Almost 44% perceived that they have 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 ($\chi^2=97.01$, p -value<0.001) between the two variables: Subjects who received a larger amount are 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 ($p\text{-value} < 0.001$). 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 were 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 and 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 $2 \cdot (74.5\% - 50\%) = 49\%$. 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 report 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). Similar cheating rates despite a huge earnings gap seem 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. The reason is that 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. 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. Without knowing it, this 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 which stated how much a subject would earn for participating in the experiment. This revealed they would receive either 4, 6, or 8 Euros. Note that, net of the €4 show-up payment, the distribution of payoffs corresponded to the distribution of the most frequent receiver earnings (0, 2 and 4 Euros) in each session of the control treatment.² Before flipping the coin, subjects were told that their total earnings would consist of the amount specified in the note in addition to the payoff associated with the coin flip.

The neutral task consisted of answering a questionnaire with survey design applications which was entirely unrelated to cheating and social norms.³ As with the dictator game, the questionnaire was paper-and-pencil based.

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 was comparable to the cheating rate among proposers, which was 45.02%, but significantly

² We did not match earnings above the equal split in the dictator game because of the low number of observations with 6 or 8 Euros in the dictator game.

³ The questionnaire contained hypothetical choice questions about housing demand and mobile phone use, and the same section on demographics which was also distributed after the dictator game. The questionnaire is available upon request.

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.

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.

The implied cheating rate of subjects who earned €6 (52.12%) is significantly higher than for those who earned €8 (36.0%), but it is lower than among those with a payoff of only €4 (41.30%). Figure 2 demonstrates that, in contrast to the fairness treatment, there is no significant difference in cheating rates between those who only earned the minimum amount and those who earned more. This result is evidence against both peer inequality and income targeting as motives for cheating. Our data rather suggest 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. Positive marginal effects therefore 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.

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 want to make sure that differences in sample composition are not driving our

main result. As a robustness check we therefore repeated the analysis using only subjects who previously had not participated in more than seven experiments, which corresponds to the most experienced subject in the fairness treatment (column 3). Furthermore, we 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 changes 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 the effect of the violation of the fairness norm implicit in the dictator game on individuals’ propensity to cheat. Since we can neither manipulate nor observe social norms directly, we used two approaches to measure fairness as a social norm in the dictator game. First, the amount transferred and second, the receiver’s subjective fairness perception of the amount received. Our analysis shows that both measures significantly predict cheating. In order to rule out alternative explanations, namely income targeting or reducing peer inequality, we implemented a treatment where subjects are not exposed to unfairness but where we created the same degree of earnings inequality via a random mechanism.

Our results are evidence that the perception 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 established in this paper is of importance to tax evasion. 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 which 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 the effect of interaction with others on individuals' propensity to cheat to be studied. The limitation, however, is that individual cheating cannot be observed so that inference 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. We did nevertheless 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.

References

- Buccioli, A., and M. Piovesan, (2009):** “Luck or Cheating? A Field Experiment on Honesty with Children.” mimeo.
- Camerer, C. (2003):** *Behavioral Game Theory: Experiments in Strategic Interaction*. Princeton University Press.
- Charness, G., and M. Dufwenberg (2006):** “Promises and Partnership.” *Econometrica*, 74(6), 1579-1601.
- Coleman, J.S. (1990):** *Foundations of Social Theory*. Harvard University Press, Cambridge (MA).
- Dreber, A. and M. Johannesson (2008):** “Gender Differences in Deception.” *Economics Letters*, 99, 197-199.
- Fehr, E., G. Kirchsteiger, and A. Riedl (1993):** “Does Fairness Prevent Market Clearing? An Experimental Investigation.” *The Quarterly Journal of Economics*, 108(2), 437-459.
- Fischbacher, U., and F. Heusi (2008):** “Lies in Disguise – An experimental study on cheating.” Thurgau Institute of Economics Working Paper.
- Freeman, R., and A. Gelber (2010):** “Prize Structure and Information in Tournaments: Experimental Evidence.” *American Economic Journal: Applied Economics*, 2(1), 149-164.
- Gneezy, U. (2005):** “Deception: The Role of Consequences.” *American Economic Review*, 95(1): 384-394.
- Greiner, B. (2004):** “An Online Recruitment System for Economic Experiments.” In K. Kremer and V. Macho, eds., *Forschung und wissenschaftliches Rechnen*. Göttingen: Gesellschaft für Wissenschaftliche Datenverarbeitung, pp. 79–93.
- Greenberg, J. (1990):** “Employee Theft as a Reaction to Underpayment Inequity: The Hidden Cost of Pay Cuts.” *Journal of Applied Psychology*, 75(5), 561-568.
- Hoffman, E., K. McCabe, K. Shachat, V. Smith (1994):** “Property Rights, and Anonymity in Bargaining Games.” *Games and Economic Behaviour*, 7, 346-380.
- Keizer, K., S. Lindenberg and L. Steg (2009):** “The Spreading of Disorder.” *Science*, 322, 1681-1685.
- Mazar, N., O. Amir, and D. Ariely (2008):** “The Dishonesty of Honest People: A Theory of Self-Concept Maintenance.” *Journal of Marketing Research*, 45(6), 633-644.
- Pruckner, G., and R. Sausgruber (2008):** “Honesty on the Streets: A Natural Field Experiment on Newspaper Purchasing.” Mimeo.
- Spicer M.W. and L.A. Becker (1980):** “Fiscal inequity and tax evasion: An experimental approach.” *National Tax Journal*, 33(2), 171-175.
- Sutter, M. (2009):** “Deception through telling the truth?! Experimental evidence from individuals and teams, *Economic Journal*, 119, 47-60.
- Wilson, J.Q., and G.L. Kelling (1982):** Broken Windows, *The Atlantic Monthly*, March, 29-38.

Tables and Figures

Table 1: Hypothetical and actual transfers in the dictator game

Amount	Proposers: actual transfers		Receivers: hypothetical transfers	
	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 6 8	92 71 75	70.65 76.06 68.00	41.30 52.12 36.00	4 vs. 6: 0.1694 6 vs. 8: 0.0498 ** 4 vs. 8: 0.4847
Gender	Male Female	102 136	74.51 69.12	49.02 38.24	0.0963 *
Lab experience	0 or 1 >1	39 199	71.79 71.36	43.58 42.72	0.9210
Major field of study	Economics Business Law Medicine Education Maths etc. other	26 24 18 10 33 21 97	80.77 75.00 77.78 80.00 63.64 70.00 69.07	61.54 50.00 55.56 60.00 27.28 40.00 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 * (0.0535)	-0.0087 (0.0604)	0.0184 (0.0682)	-0.0826 (0.0615)
Male	0.1371 *** (0.0528)	0.0558 (0.0632)	0.1027 (0.0691)	0.1060 * (0.0591)
Lab experience > 1	0.1120 ** (0.0523)	-0.0061 (0.0781)	-0.0155 (0.0792)	0.0049 (0.0580)
Major Econ	-0.1343 (0.1211)	0.1061 (0.0881)	0.1272 (0.0907)	0.1939 *** (0.0683)
Major Business	0.0155 (0.0762)	0.0421 (0.0985)	0.0340 (0.1090)	-0.0313 (0.0889)
Major Law	-0.0128 (0.1123)	0.0788 (0.1059)	0.1517 (0.1063)	-0.0654 (0.1854)
Major Medicine	-0.0691 (0.0998)	0.1175 (0.1219)	0.1145 (0.1262)	0.1377 (0.1293)
Major Education	0.0663 (0.0743)	-0.0527 (0.0933)	-0.0094 (0.0984)	-0.1386 (0.0936)
Major Maths	-0.0279 (0.0975)	-0.0146 (0.0981)	-0.1751 (0.1327)	-0.2380 ** (0.1112)
	-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

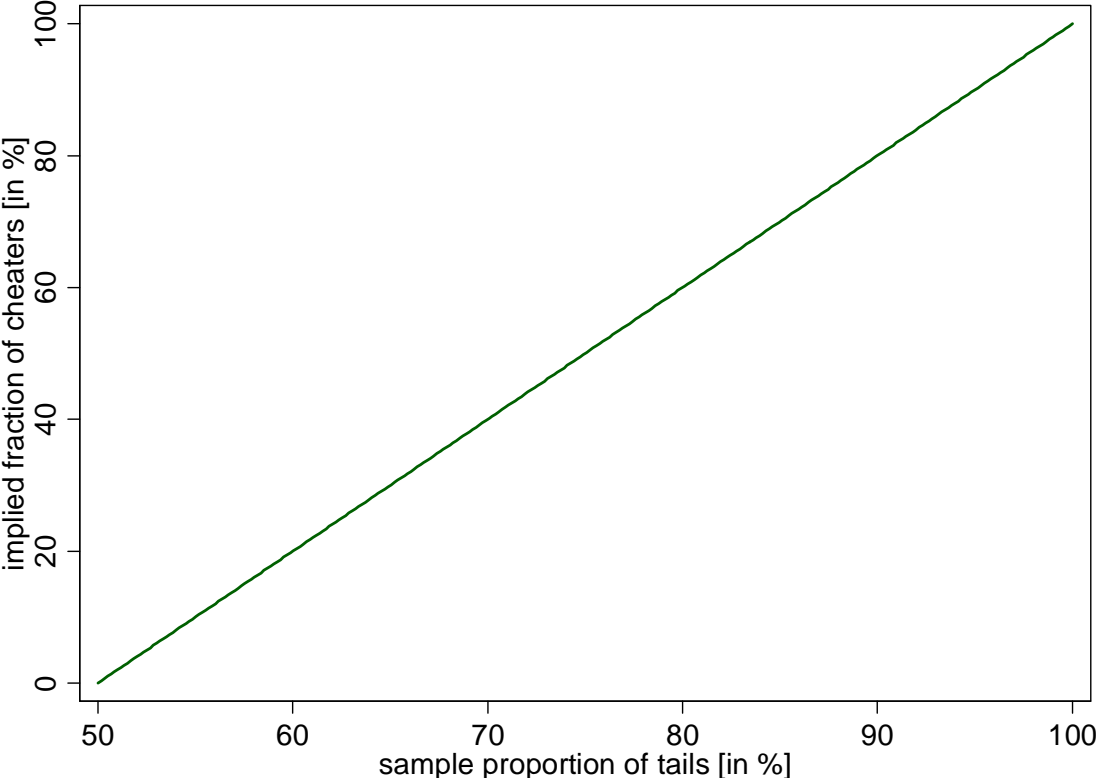
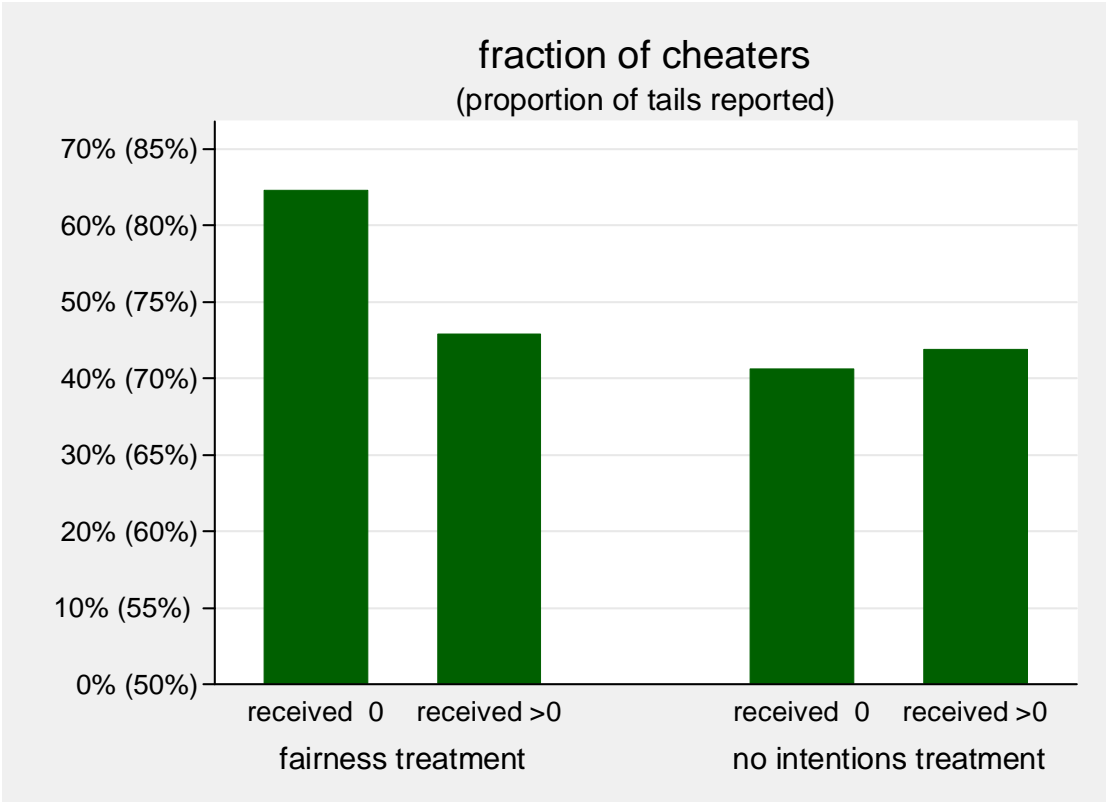


Figure 2: Cheating rates (proportion tails reported) by earnings in both treatments



Appendix A: 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 a 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.

Test

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

- Male
- 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.

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.