Experiments on Ethical Behavior in Strategic Interactions: Cooperation, Honesty, and Fairness

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1 Introduction

This thesis adds further pieces to the large mosaic of human morality. It contains four essays on the topics of cooperation, honesty, and fairness. The purpose of this dissertation is to gain a better understanding of how individual ethicality unfolds in strategic interactions, where two or more persons interact and their actions determine each others payoffs. Using economic laboratory experiments we analyze how strategic concerns and the decision making environment influence ethical behavior. One central objective of this thesis is to better understand the roots of ethicality in situations with strategic interdependencies in order to make prescriptions about what firms and organizations can do to foster good behavior.

The main motivation for this thesis stems from the prevalence of scandals and fraudulent behavior in the corporate realm. Present research has shown that persisting unethical behavior is not due to a small number of immoral persons who commit large unethical action but rather a large fraction of persons who commit small acts of unethical behavior (Bazerman and Tenbrunsel, 2011a). The persistence of such behavior can not only lead to great costs for corporations, e.g., as a result of legal proceedings, but also increase distrust in the free market economy, which ultimately might endanger firms' existence (Banerjee, 2007). These potential consequences underline why corporations should have a vital interest in understanding which organizational measures foster ethicality.

The perseverance of immoral conduct in the business world, however, has led to the question of how much of this is due to the inherent immorality of individuals and how much of this is due to the decision-making environment, i.e, to the structure and incentives resulting from exchange (e.g. Falk and Szech, 2013; Trevino, 1986). This thesis focuses on the latter part, and asks how the decision-making environment influences ethical behavior.

Empirical research on ethicality has typically focused on individual decision making without strategic interactions. In a series of path-breaking studies Milgram (1963), for example, studies how individuals obey authorities in an individual decision making context. Furthermore, the famous trolley-problem (Foot, 1967), initially used as a thought experiment by moral philosophers to study moral values, is now a well-renowned tool in the cognitive and behavioral sciences (Knobe and Nichols, 2008) to study when and why people apply utilitarian or deontological ethics.

Thus, it does not come as a surprise that empirical researchers in ethics have focused on individual attitudes and moral judgments, rather than focusing on interactions. One explanation for this development could be that the traditional discourse in ethics has viewed individual moral behavior as people reasoning about moral dilemmas and then (potentially) following these moral principles.

Contrary to the traditional approach, this thesis focuses on individual's ethicality in strategic interactions. Moreover, the thesis tries to investigate illuminate individual ethicality when variables of the decision-making environment change. To understand what drives ethical behavior in firms and inform designers of organizations this seems to be a promising approach since employees in firms frequently interact and these interaction are influenced by the organizational setup. Important variables of the decision-making environment within organizations can be incentives, such as how employees are paid, or more subtle factors like employee knowledge about the good or bad behavior of others, or how certain firm policies are represented.

Ethical behavior in strategic interactions can have many different facets. This dissertation focuses on three of them. The first is cooperative behavior in teams. In modern firms, cooperation among employees is crucial to the overall success. However, in several collaborative situations, it is individually rational - from a standard economic point of view - for employees to refrain from exerting effort, i.e., not to cooperate with her fellow employees, although it would be mutually beneficial for the complete enterprise. Particularly when employees know and see each other on a day-to-day basis the plain economic point of view seems simplistic and one might think that cooperation can be easily sustained. However, in large firms teams often work together in different time-zones and in different places and are not able to monitor and potentially control each other. This makes it much more difficult to enforce cooperative behavior. Consequently, we ask in Chapter 2: How does subjects' knowledge of others' behavior influence their willingness to cooperate?

The second aspect is honesty competitive environments. To align individuals' interests with those of the firm, companies are increasingly make use of performance-based incentive schemes. One prominent performance-based incentives scheme is a tournament incentive scheme. Here, employees are paid based on their performance relative to others. Employment in nearly every organization entails participation in tournaments, even if such incentives are not explicit, e.g., via the payment scheme. An example of an implicit tournament is the promotion of employees. From a standard economic point of view, this incentive structure should increase productive effort by employees. However, since it is always beneficial for employees to be marginally better then their fellow employees, this incentive might also be responsible for unethical conduct such as lying. In Chapter 3 we ask: How does the strength tournament incentives affect honest reporting of one's own performance?

The third aspect of ethicality is fairness which is covered from two different angles in the last two chapters. The first angle pertains to the individual view of different fairness notions and how these fairness notions influence bargaining. The second angle considers how individuals react when fairness norms are violated.

Fairness and equity considerations are a major source of conflicts in the workplace. When principals assign work among several employees, when bonus payments are distributed or when employers and employees negotiate about the distribution of future earnings, equity and fairness arguments are often present. From a normative perspective, however, it is sometimes not clear what fairness really means (Konow, 1996). In Chapter 4 we ask: How do outside options, i.e., an individual's payment in case of a negotiation breakdown, influence equity considerations in bargaining?

A further aspect of fairness is covered in the last chapter. It tackles the question when unfair behavior is punished by unaffected bystanders. There are many situations in organizational life where employees observe the wrongdoing of fellow employees but remain silent. For example, when hospital doctors take bribes from persons in need or if bureaucrats do not give public orders to companies although they deserve it by law. Often unaffected colleagues or bystanders observe this wrongdoing but do not act and report or punish the wrongdoer. From a standard economic point of view it seems rather clear that this kind of moralistic punishment will never occur: If bystanders cannot expect any material gain from punishing the wrongdoer she will never engage in a retributive act. There is, however, a growing interest by economists in the question how moral inclinations lead individuals to engage in punishment although they are not affected (e.g., Lergetporer et al., 2014). In Chapter 5 I argue that two situational variables influence moralistic punishment. First, we ask how framing of unfair behavior influences moralistic punishment differently. Second, we ask when the victim of norm violations does not observe the bystanders action, will she punish?

Although every chapter in this thesis deals with a very different aspect of human morality in strategic interactions all essays have certain aspects in common. In every chapter we employ the very same research method, i.e. the economic laboratory experiment. Because researchers have the possibility to exogenously vary factors of the decision-making environment and observe corresponding changes in behavior this method is a powerful tool to investigate moral behavior (Falk and Heckman, 2009). In addition to that, decisions of our participants in all studies have monetary consequences for them and for other participants. This is evident for employees' behavior in real firms making the applied research method more valid than questionnaire studies that are usually applied to study the questions of cooperation, honesty and fairness. Moreover, in every experimental paradigm used in this thesis there exists a clear-cut economic prediction, which is, without saying too much at this point, systematically violated.

The following pages summarize each chapter. They will sketch the motiviation, the experiments, results and implications to the academic debate.

The first essay *On the Role of Limited Feedback in Voluntary Contribution Games* is based on joint work with Bernd Irlenbusch (Irlenbusch and Rilke, 2013). Both authors contributed equally to this research. The essay in chapter 2 addresses the research question how limited knowledge about the behavior of others influences contributions in a social dilemma and how good and bad examples, i.e., good and bad behavior of others, influences cooperation. It is one of the most established empirical observations in experimental economics that people cooperate when others cooperate and do not cooperate when others do not do so (Fischbacher et al., 2001). This behavior is typically called conditional cooperation. For conditional cooperation to unfold, it is, however, crucial what individuals know about the behavior of others, i.e., which feedback they receive. In laboratory experiments on public goods, subjects normally have perfect knowledge if others cooperate or not. In most social dilemmas outside the laboratory, however, this is obviously not the case.

This chapter experimentally investigates the effects of limited feedback on contributions in a repeated public goods game. We test whether feedback about good examples (i.e., the maximum contribution in a period), in contrast to bad examples (i.e., the minimum contribution), induces higher contributions. When the selection of feedback is non-transparent to the subjects, good examples boost cooperation, while bad examples hamper cooperation. No significant differences are observed between providing good or bad examples when the feedback selection rule is transparent. The results of this experiment have several implications for organizational designers. In order to achieve efficient team production, cooperation or voluntary contributions the data highlights that the management of beliefs is important. This is potentially has particular importance in situations where sanctioning institutions to foster cooperativeness are undesirable or simply impossible to implement.

The second essay *The Effects of Incentives on Honesty in Tournaments* is based on a joint work with Julian Conrads, Bernd Irlenbusch, Anne Schielke, and Gari Walkowitz (Conrads et al., 2014). All authors contributed equally to this work. Chapter 3 focuses on how tournament incentives affect individuals' inclination to act dishonestly. Tournaments, i.e., payment systems that are based on relative performance comparisons, are ubiquitous in corporate life. Under this kind of incentive, a group of employees works for a period of time and is then paid based on the relative performance of the group members. One employee of the group might be promoted to a higher hierarchy level. Typically, the strength of tournament incentives results from the difference in the payoff for the winner and loser of the tournament, i.e., the prize spread (Lazear and Rosen, 1981; Grund and Sliwka, 2005). Harbring and Irlenbusch (2011) study experimentally that as the difference between winner and loser prize increases working efforts increase. The results of their experiment show, however, that workers invest more in sabotage activities as the prize difference increases.

Despite the fact that these types of payment schemes offer an incentive to destruct co-workers efforts, in Chapter 3 of this thesis we study their impact on being dishonest, which has been largely neglected in the existing research. We apply the die rolling experiment of Fischbacher and Heusi (2013) to a two-player tournament incentive scheme. In three treatments we vary the difference between the winner's and the loser's prize from one to five Euro. Irrespective of the price difference solely selfish-minded individuals should always lie to the full extent across our treatments. We observe, however, that lying seems to be more pronounced as the price spread increases. Moreover, we see that a considerable number of individuals remain honest, irrespective of the strength of the incentives, and a substantial fraction of individuals lie incompletely. This means that they do not report the highest possible output. These results give indicate the existence of heterogeneity in individuals' propensity to lie under tournament incentives. It is an essential feature of this experiment that we vary the prize spread but keep the sum of prizes constant. The implementation of tournament incentives is thus equally costly across treatments. Increasing the prize spread, however, comes with the potential downside of pronounced lying. In Chapter 3 we further discuss possible implications of lying for organizations.

The third essay *On Equity Rules in Ultimatum Bargaining with Asymmetric Outside Options* is a joint work with Heike Hennig-Schmidt, Bernd Irlenbusch and Gari Walkowitz (Hennig-Schmidt, Irlenbusch, Rilke, and Walkowitz, 2014). All authors contributed equally to this project. Chapter 4 tests how outside options influence notions of fairness and equity in bargaining. Outside options, i.e., the alternatives an individual has to the existing negotiations, are arguably the most important determinant for bargaining behavior. Football players receive offers from various clubs, managers have multiple proposals from different firms, professors collect bids from several universities before entering wage negotiations. Although professional bargaining manuals recommend that negotiators should strive for a multitude of outside options, their influence on fairness in bargaining is not well understood.

We experimentally investigate multiple notions of equity in ultimatum bargaining with asymmetric outside options. Building on the generalized equity principle formulated by Selten (1978), we derive three different equity rules that can explain 43% of all proposals. Our between- and within-subject design allows us to further show that proposers use different equity rules and apply them in a self-serving manner. They tend to follow the rules that suggest the highest payoff for them. The equity principle also explains 26% of responders' minimal acceptable offers. As for proposers, their choices tend to exhibit a pattern of self-serving behavior. Combined, these tendencies lead to high inefficiency resulting from frequent rejections.

The last essay, *The Influence of Framing and Publicity on Moralistic Punishment,* in Chapter 5 investigates how framing of actions effects moralistic punishment. I conduct an experiment to examine the role of framing and publicity in motivating altruistic punishment. I consider a modified version of a dictator game where a third-party observes a dictator's behavior and can punish her. I vary how the dictator's action is framed (either as giving money to a recipient or taking money from the recipient) and whether the recipient (as a potential victim of unfair behavior) is informed about the punishment or not. Our results suggest that, although the payoff consequences for dictator and recipient are constant, third-parties are more likely to punish dictators when dictators give nothing compared to when they take everything. This result emerges only when third-parties can signal their altruistic behavior to the recipients.

Taken together, the four experimental studies of this thesis examine that individuals' ethicality, measured as cooperativeness, honesty or fairness, is predictably malleable and depends on incentives, as well as more subtle factors like feedback or framing. In the following chapters the essays will be presented in detail.

2 On the Role of Limited Feedback in Voluntary Contribution Games

How should feedback provision be designed to foster cooperation in public goods settings? One of the most established observations from the experimental research on public goods games is that the behavior of a majority of individuals can be described as conditionally cooperative, i.e., people cooperate when others do likewise. However, cooperation levels decline, as the game is repeated over time. The cause of this decline has recently been studied. Fischbacher and Gächter (2010), Neugebauer et al. (2009) and Muller et al. (2008) find that subjects do not match the other players' behavior perfectly. More precisely, they tend to contribute on average a bit less than they think their peers do and therefore contributions go down. This behavior is called imperfect conditional cooperation.[1] To explain how this behavioral tendency unfolds, it is important to understand more about how feedback contributions of others influence the decision to contribute to a public good.[2] Such investigation would offer valuable insights into designing effective feedback provision.

Typically, in repeated public goods experiments scholars provide participants with feedback about the sum of contributions or feedback about the contribution of each individual group member. However, in many social dilemmas

This chapter is based on joint work with Bernd Irlenbusch (Irlenbusch and Rilke, 2013).

[1]Neugebauer et al. (2009) use the term selfishly-biased conditional cooperation. Because we think that the underlying motive of not perfectly cooperating could have several reasons we stick to the term imperfect conditional corporation throughout the paper.

[2]Previously, experimental economists put huge efforts into investigating several mechanisms that stabilize contributions behavior over time. Chaudhuri (2011) provides an excellent survey of laboratory experiments on this topic. Research focuses on punishment (Fehr and Gächter, 2000; Gürerk et al., 2006; Xiao and Houser, 2011), incentives (Bracht et al., 2008), communication (Isaac and Walker, 1988; Bochet et al., 2006), sorting and group formation (Gächter and Thöni, 2005; Page et al., 2005; Ahn et al., 2009), moral suasion (Masclet et al., 2003) and recommendations (Chaudhuri and Paichayontvijit, 2010) with effects on contribution levels.

outside the laboratory, one has only limited information about the behavior of others.[3] Think, for example, of large teams where the individual contributions are not easily observable. Alternatively, think of individual efforts to preserve the cleanliness of public parks or train stations. In these situations, it is not always clear what others contributed to sustain the common good. Organizations and governments use the limited availability of feedback and initiate campaigns that selectively highlight appropriate or inappropriate behavior to facilitate cooperation. Awards like 'Employee of the month' in organizations or image campaigns in cities are vivid examples of these attempts. So far, little systematic evidence indicates how limited feedback about contributions affects cooperation in a repeated setting: How do good and bad examples of behavior influence cooperation and beliefs? How does the awareness of how the feedback is selected shape the inclination to voluntary cooperate? We deal with these questions in this paper.

We study a voluntary contribution game in which subjects receive feedback about a single contribution in the group, and vary the way in which this particular contribution is selected as feedback. Our experiment focuses on two simple feedback selection rules: good and bad examples. Subjects in our experiment receive feedback either about the maximum contribution (*good example*) or the minimum contribution (*bad example*) of the previous period in the group. Additional to the feedback selection rule we vary the subjects' knowledge of which rule is applied, i.e., the awareness of the feedback selection rule (*transparent*) or not (*non-transparent*). This experimental design has unique features that enable us to shed light on the questions stated above. First, we investigate cooperation when subjects are (not) aware of how the feedback is selected. Given the previously observed pattern of imperfect conditional cooperation, we hypothesize that it is easier to deviate from the given feedback and free-ride a bit when knowing that good examples are set forth rather than when the feedback selection rule is unknown. On the other hand, when knowing that bad examples will be exposed feedback is likely to induce lower bounds of what is an appropriate contribution. Second, we are able to understand more about the impact of observed good and bad past behavior, because investigate

[3] Only recently there emerged a literature where limited feedback plays a role in public goods experiments (Grechenig et al., 2010; Ambrus and Greiner, 2012; Irlenbusch and Ter Meer, 2013; Bayer et al., 2013; Hartig et al., 2013).

a repeated social interaction. Most field studies on related issues (e.g. Cialdini et al., 1990) focus on the influence of a one-time intervention. Third, our experimental setup resembles social dilemma situations outside the lab in which information about peer behavior is limited but feedback is provided selectively.

We observe that when the feedback selection rule is not transparent good examples have a clearly positive effect on average contributions. The positive effect of good examples is not observed when subjects are aware of the feedback selection rule that is applied. When bad examples are provided as feedback, knowing explicitly which feedback selection rule is at work increases cooperation compared to a situation where the feedback selection rule is not transparent. Our results also show that matching the feedback is more likely when the feedback selection procedure is non-transparent. Furthermore, as beliefs influence contributions, we find that non-transparency of feedback influences belief formation that might ultimately lead to the observed differences between good and bad examples.

This essay is organized as follows. Section 2.1 explains our experimental design. The results are presented in Section 2.2. In Section 2.3 we discuss the connection to existing research and potential applications. Section 2.4 concludes the study.

2.1 Experimental design

Our experiment consists of a 10-period standard linear public goods game with partner matching. All players are newly endowed with 20 points in each of the periods. Points may be allocated to the public good or kept in the private account. Keeping the money yields a private marginal return of one, while contributing to the public good delivers a marginal per capita return of 0.4. Thus, it is a dominant strategy not to contribute. The payoff π_i for each participant in each period is determined by

$$\pi_i = 20 - c_i + 0.4 \cdot \sum_{j=1}^{4} c_j,$$

where c_i represents the amount contributed of individual i.[4]

[4]Experimental instructions and control questions are taken from Fehr and Gächter (2000) and expanded by one paragraph (see appendix). Original instructions were in German.

One period of our experiment consists of three different stages with three separate screens. First, a subject has to decide about the number of points that she is willing to contribute to the project. On a second screen, a subject is asked to state her beliefs about the average contribution of the other three group members during this period.[5] In a third stage, all subjects in a group receive the same feedback about a single contribution in the group and no other information.[6] In the instructions and on the screens it is clearly indicated that all subjects in the group of four receive the same feedback and that the shown contribution can potentially be their own. Our experiment varies the feedback selection rule and the transparency of the selection rule.

Treatments

We implement six treatments using a 3x2 factorial design. We utilize three feedback selection rules. Subjects receive feedback either about the *maximum* (MAX) or *minimum* (MIN) contribution in a group. In addition, in a reference treatment (RAND), subjects receive feedback about a randomly selected contribution. We employ one of the feedback selection rules either *non-transparent*, i.e., subjects were not informed about the specific character of feedback, or *transparent* where subjects know which type of feedback they received.[7] To isolate the effect of the specific feedback selection rule subjects receive no other information between the periods. An overview of the treatments and their feedback conditions is shown in Table 2.1.

Procedure

The experiment was conducted at the Cologne Laboratory for Economic Research (CLER) in six sessions. A session lasted for about one hour. The

[5] Belief elicitation was incentivized: If subjects either correctly predicted the average of the other three participants or their predictions lied in a $+/-$ 0.5 range of the real average they received 5 points. Since feedback about the accuracy of their estimation may have influenced subjects' decisions in the following periods, we delivered feedback about the accuracy of their prediction at the end of the experiment.

[6] Feedback on earnings in the period is provided at the end of the last period.

[7] When the feedback selection rule is non-transparent the exact wording of the feedback on the screen was *"A contribution of one person was: XX"*, whereas when transparent feedback was presented the sentence *"The person with the maximum (minimum) contribution has contributed: XX"* was displayed. For the feedback selection rule RAND it said *"A randomly drawn contribution of one person was: XX"*.

Table 2.1: Overview of treatments

Treatment	Feedback selection rule	Number of Participants	Number of Groups
Non-transparent Feedback Selection			
MIN	min(c_1,...,c_4)	28	7
RAND	rand(c_1,...,c_4)	32	8
MAX	max(c_1,...,c_4)	32	8
Transparent Feedback Selection			
MIN	min(c_1,...,c_4)	32	8
RAND	rand(c_1,...,c_4)	32	8
MAX	max(c_1,...,c_4)	32	8

Notes: In MIN under non-transparent feedback selection we are lacking one observation due to subjects not showing up to the experiment.

experiment was computerized with the software z-Tree (Fischbacher, 2007). One point was worth 0.04€. 188 subjects were recruited with the software ORSEE (Greiner, 2003). Individuals' earnings of each period are summed up and paid out at the end. Subjects earned 12.68€ on average, including 2.5€ show-up fee. After ten periods of the public goods game a questionnaire was distributed.

2.2 Results

In this section we present the results of our experiment.[8] The left panels of Table 2.2 show the average contributions beliefs and feedback. The analysis of contributions leads to our first observation:

> ***Observation 1:*** *Average contributions are highest when the maximum contribution is provided as feedback and the feedback selection rule remains non-transparent.*

First, we compare the contributions of the different feedback selection rules for non-transparent feedback. In MAX, the average contributions are by 9.2

[8] All reported significance levels are based on two-sided tests. If not mentioned otherwise non-parametric comparisons are conducted with group averages as independent observations. Standard errors in our regressions are clustered on independent subjects groups.

Table 2.2: Average contributions, beliefs, and feedback

Treatment	Contributions	Beliefs	Feedback
Non-transparent Feedback Selection			
MIN	4.02	3.25	0.91
RAND	7.39	8.48	7.66
MAX	13.21	15.51	18.85
Transparent Feedback Selection			
MIN	7.44	7.5	2.89
RAND	5.61	5.56	4.45
MAX	8.53	10.74	14.7

Notes: In this table we display average values of contributions, beliefs, feedback over all ten periods.

(5.8) points higher than in MIN (RAND). This difference is highly significant (vs. MIN: p=.0012, vs. RAND: p=.0087, Mann Whitney U test, henceforth MWU test). In the transparent feedback selection environment we find no statistically significant difference between average contributions.

A feedback selection rule seems to have different effects on contributions, depending on whether this selection rule is transparent. Comparing transparent against non-transparent feedback selection reveals that contributions in the former are 4.7 points lower than in the latter (p=.0274) when the feedback selection rule is MAX. We find indications that contributions for MIN are higher by 3.4 points when subjects know the feedback selection rule (p=.0641).[9]

[9] No such difference could be found when comparing both RAND feedback selection mechanisms. Additional regressions in the appendix in Table 2.5 controlling for the period and the actual feedback provided further confirm this observation. Table 2.6 in the appendix gives an overview of all non-parametric comparisons.

Figure 2.1: Development of contributions over time

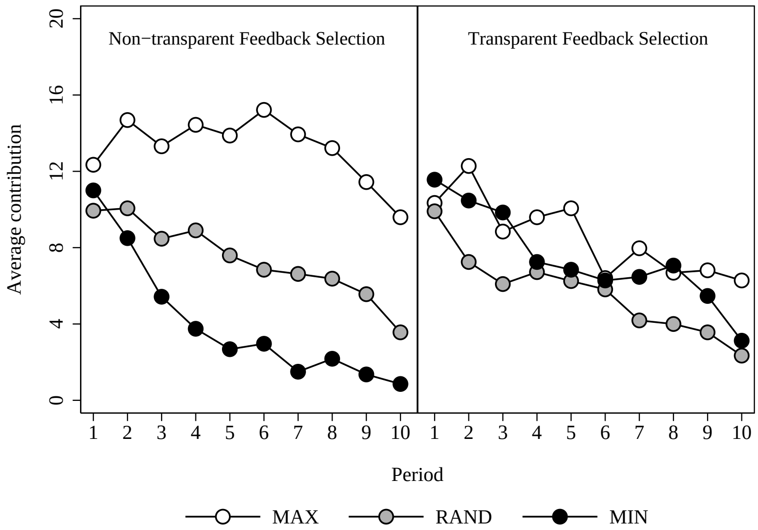
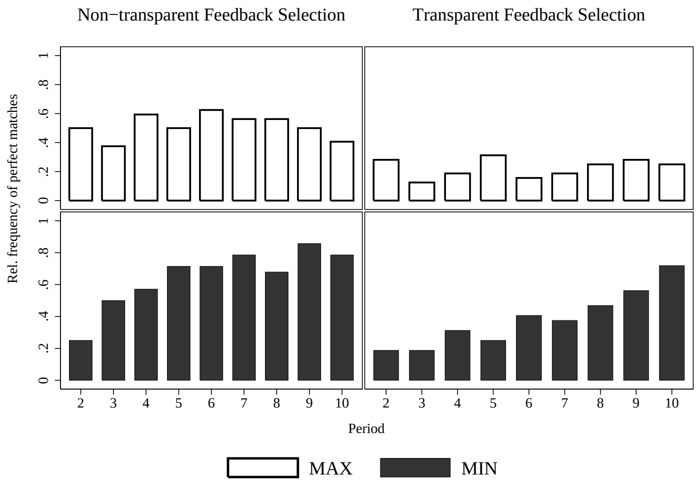


Figure 2.1 shows the development of contributions over time. Contributions initially start at around 10 points[10] and then decline as the game proceeds. There is, however, one exception. When the maximum is displayed and the feedback selection rule is non-transparent contributions are relatively stable over time and exhibit only slight endgame effects. To test this statistically, we take the average contribution from every group from the period 1 to 5 and 6 to 10 and compare the distribution of these averages with a Wilcoxon signed rank test for matched pairs for every treatment separately. All tests yield p-values smaller than .0357, except for non-transparent feedback selection MAX. Here we cannot reject the hypothesis that the average contributions between those two parts of the experiment are similar (p=.2076).

Thus, leaving the feedback mechanism non-transparent has a positive effect on contributions when the feedback selection is the maximum contribution, but a negative effect when the minimum contribution is provided as feedback. What may be potential reasons for the different effects of non-transparency over the feedback selection mechanism? The first reason we are going to explore is the likelihood of matching the previously observed feedback.

[10] According to a Kruskal Wallis test we find no difference in the distribution of subjects' first period contributions between the feedback selection rules for non-transparent (p=.3628) and transparent (p=.6255) feedback selection.

Figure 2.2: Relative frequencies of perfect matches over time



Observation 2: *Subjects match the feedback more often when the selection rule (either MAX or MIN) is non-transparent.*

For every group, we calculate the fraction of contributions that are exactly equal to the previous feedback (see lower right panel of Table 2.6 in the appendix).[11] When the maximum contribution is displayed, we find that, on average, more than half of the contributions (.51) match the previously seen feedback when the feedback selection rule is non-transparent rather than transparent (.22, p=.010, MWU test). Perfectly matching the feedback seems to be more pronounced when the minimum contribution is provided as feedback. In the case of non-transparent feedback selection .65 of the contributions match perfectly, while .38 of the contributions match the feedback if the selection is transparent. This is in line with results from previous experiments (e.g. Croson and Shang, 2008; Samak and Sheremata, 2013) which find that in social dilemmas feedback over low contributions has a influence than other feedback forms. Our results suggest that in MIN matching the feedback is more frequent when it is non-transparent than when it is transparent (p=.0419).[12] Compar-

[11]Note that this classification is rather conservative. To test the efficacy of the different feedback selection rules we stick to this classification since imperfect conditional cooperators tend to contribute slightly less than the feedback, which decreases contributions.

[12]In treatments RAND the opposite is the case: Matching is more likely when subjects know that they are aware of the feedback (p=.0155).

Table 2.3: Perfect matching of contributions and feedback

	Dependent variable 1 if Contribution$_{i;t}$ = Feedback$_{t-1}$		
Independent variables	MAX	MIN	RAND
Period	0.00432	0.0491***	0.00667
	(0.00751)	(0.00964)	(0.00786)
1 if Non-transparent	0.280***	0.217**	-0.186***
	(0.107)	(0.104)	(0.0653)
Feedback$_{t-1}$	0.00231	-0.0630***	-0.0149***
	(0.0171)	(0.0154)	(0.00462)
Observations	576	540	576
R^2	.07	.216	.071

Notes: Marginal effects of a probit regression with data from period 2 to 10. Robust standard errors (clustered on groups) in parentheses. "1 if Non-transparent" takes the value 1 if the feedback selection rule was non-transparent and 0 if not (transparent). *** Significance at $p<.01$, ** Significance at $p<.05$, * Significance at $p<.1$.

ing the fraction of contributions that perfectly match the feedback, however, we find no significant difference (p=.1715) between MAX and MIN conditions for non-transparent feedback selection. The same holds true for transparent feedback (p=.1307).

Figure 2.2 illustrates this observation for the complete course of the experiment. We display the frequency of contributions perfectly matching the feedback across our treatments and periods. We see that matching the feedback is more likely under the non-transparent feedback selection (left panel). Moreover, we observe that the fraction of subjects matching the feedback increases over time when the minimum contribution is provided, i.e., free-riding becomes more and more frequent at the end of the experiment.

Probit regressions predicting whether a contribution perfectly matches the feedback or not further elaborate on this issue and confirm the results of the non-parametric estimates. In Table 2.3, the significantly positive coefficients for the dummy variable “1 if Non-transparent” in model (MAX) and in model (MIN) confirm an increasing likelihood of imitating the previously seen feedback when subjects do not know the feedback selection rule. The positive significant coefficient for "Period" (in model MIN) captures the visual impression from Figure 2.2 that matching becomes more and more frequent in later periods when the feedback selection rule is MIN.

Another possible explanation for the treatment effect stated in observation 1 concerns the effect of beliefs on contributions.[13] As already shown, in other studies, beliefs and contributions are usually highly correlated (Fischbacher and Gächter, 2010; Neugebauer et al., 2009). As a first rough statistical measure of the influence of beliefs on contributions, we correlate both figures. According to the Spearman rank correlation test average group contributions and average group beliefs are highly significantly correlated when pooling all treatments (ρ=.92, p=.0001).[14] As beliefs are likely to impact contributions it is important to understand how beliefs are influenced by the respective feedback. When correlating average group beliefs and average previously provided feedback we find a strong positive relationship (ρ=.82, p=.0001).[15] This bi-variate analysis, however, does not uncover the underlying dynamics of the belief formation process that ultimately might lead to the observed differences between our treatments. Therefore, with the help of a regression analysis we estimate the belief formation process for every feedback selection rule separately. Our model follows Fischbacher and Gächter (2010). To explain the beliefs in period t, we use the previous belief ("$\text{Belief}_{i;t-1}$"), the feedback subjects received during the preceding period ("Feedback_{t-1}") and the period ("Period"). Additionally, to capture the effects of a non-transparent feedback selection rule, we include a dummy variable "1 if Non-transparent". The results are displayed in Table 2.4. In all specifications we find that "$\text{Belief}_{i;t-1}$" and "Feedback_{t-1}" are highly significantly positive.[16] Thus, we make our last observation:

[13]A comparison of average group beliefs (see upper right panel of Table 2.6) between non-transparent and transparent feedback selection goes into the same direction as the comparison of contributions in observation 1. For MAX (MIN) beliefs are higher (lower) by 4.77 (4.25) points when the feedback selection rule is non-transparent rather than transparent (MAX: p=.0117, MIN: p=.0109, MWU test). Interpreting beliefs is typically difficult due to problems of potential endogeneity. Note that in order to attenuate this problem, we incentivized beliefs, as described in Section 2.1.

[14]Coefficients of GLS regressions explaining contributions with beliefs (and a constant) separately for each treatment are all positive and significant at least of a .75 magnitude.

[15]Coefficients of GLS regressions explaining beliefs with previous feedback (and a constant) separately for each treatment are all positive and significant at least of a .33 magnitude.

[16]It has to be noted, that in Fischbacher and Gächter (2010) the feedback subjects received during their experiment consisted of the sum of contributions from all group members. Nevertheless, in their regressions the ratio of coefficients between previous beliefs and

Observation 3: *With MIN feedback the non-transparency of the feedback selection rule decreases beliefs compared to when the feedback selection rule is transparent. With MAX feedback the effect of non-transparency tends to go in the opposite direction, i.e., non-transparency of the feedback selection rule (slightly) increases beliefs, compared to when the feedback selection rule is transparent.*

In the model (MIN) in Table 2.4 we observe a strongly negative and significant influence of "1 if Non-transparent" when the minimum contribution is displayed as feedback. The same variable is mildly positively significant, when we compare non-transparent and transparent feedback selection for the maximum contribution in model (MAX). No such difference could be observed for random feedback selection (model RAND).

Table 2.4: Explaining beliefs

	Dependent variable Belief$_{i;t}$		
Independent variables	MAX	MIN	RAND
Belief$_{i;t-1}$	0.604***	0.599***	0.549***
	(0.05)	(0.10)	(0.09)
Feedback$_{t-1}$	0.383***	0.408***	0.347***
	(0.04)	(0.08)	(0.04)
Period	-0.204***	0.0391	-0.0324
	(0.07)	(0.07)	(0.05)
1 if Non-transparent	0.780*	-1.041***	0.312
	(0.43)	(0.33)	(0.35)
Constant	-0.390	0.645	0.390
	(0.57)	(0.88)	(0.44)
Observations	576	540	576
R^2	.73	.768	.717

Notes: GLS regressions with data from period 2 to 10. Robust standard errors (clustered on groups) in parentheses. "1 if Non-transparent" takes the value 1 if the feedback selection rule was non-transparent and 0 if not (transparent). *** Significance at $p<.01$, ** Significance at $p<.05$, * Significance at $p<.1$.

feedback appear to be very similar to ours (p. 548, Table 1).

2.3 Discussion

Feedback in simultaneous social dilemmas

So far, only a few studies have discussed the effects of feedback in social dilemmas. Weimann (1994) has undertaken a first approach. He manipulated the feedback about the average contribution, i.e., one real subject interacted with four fictitious players. In one treatment subjects were confronted with fictitious players that contributed on average 90% of the endowment in every period. In a second treatment they received the information that on average subjects contributed 16% all the time. Interestingly, despite the remarkable difference in feedback between these two conditions, Weimann observed no difference in behavior between the two conditions. As the experiment of Weimann suggests the feedback about high average contributions not necessarily promotes cooperation. One possible reason might be imperfect conditional cooperation. Alternatively, subjects might not have taken the feedback seriously after having observed that the feedback averages remained constant. In a more recent paper, Bigoni and Suetens (2012) investigated the influence of two seemingly identical formats of feedback. In all experimental treatments subjects received information about the sum of contributions of their group. However, the treatments differed in the additional information subjects obtained. Subjects saw the feedback on either the contribution of each group member or the earnings of each group member. Despite the fact that these different formats of feedback can easily be converted into each other contributions were significantly lower when earnings feedback was provided.[17] They explained that the results suggest a tendency of subjects to imitate the best performer (the subject with the highest earnings) rather than the best contributor. The work of Hoffmann et al. (2013) is more closely related to our paper. They tested the effect of centralized feedback manipulation on cooperation in a public goods setting. In their experiment, subjects received the average contribution of the entire group during the periods. In one of their treatments, this average was distorted by exaggerating it by 25% in each round. Subjects were only informed that the feedback might deviate from the actual average. Their results sug-

[17] Similar to Bigoni and Suetens (2012) Nikiforakis (2010) studied the difference between these feedback formats but included a punishment option. His results also suggest a negative influence of earnings feedback on cooperation but no difference in the frequency of punishment.

gested that this centralized feedback manipulation was indeed not effective in promoting cooperativeness. They argued that the feeling of being a sucker, i.e., being an above average contributor, is mainly responsible for this decline. Therefore, the researchers conducted a further treatment where above average contributors received feedback based on their own contribution as the average contribution of the group. All other subjects received the true average contribution as feedback. As it turned out contributions were high and stable over time. In a recent paper, Samak and Sheremata (2013) focused on the influence of visibility of participants in a standard multiperiod public goods game. In all their experimental treatments subjects received complete information about the previous contribution of every participant in their group. Additionally, the researchers provided subjects with pictures of group members that were taken before the experiment. In some treatments, only pictures of the highest contributors were shown and in other treatments only the pictures of lowest contributors were exposed. They found that revealing the identity of lowest contributors increases contributions relative to a situation in which the identity of every group member is revealed.

Leadership in sequential social dilemmas

The topic of this paper is also related to the literature on leadership in social dilemma situations. Typically, in these studies, one member of the group, the leader, contributes first. All other members observe the contribution of the leader and subsequently decide on their own contribution. Gächter and Renner (2004) showed that the leaders' contributions actually influence followers' contributions. Followers, however, fell short of the leaders' choices and exploited them. Subsequent studies showed that leadership is more effective when the position of the leader is more advantageous. Güth et al. (2007), for example, showed that leadership is particularly helpful to induce cooperation, when the leader has the possibility to exclude others.[18] Potters et al. (2007) provided evidence that leadership is more effective when the leader has exclusive information about the marginal per capita return of the public good.

[18] In a similar vein, Rivas and Sutter (2011) showed that when subjects have the possibility to voluntarily choose to be the leader the effect of leadership is even stronger compared to when the leader is chosen randomly. Gächter et al. (2012) nicely showed that cooperative leaders are more effective in convincing followers to follow. Gächter et al. (2010) also compared sequential and simultaneous contributions.

This stream of literature looks at situations in which one participant chooses her contribution first, which is subsequently shown to other group members before they decide on their own contributions. At the end of a period all group members receive feedback about all decisions from the group. In our study, we are more interested in a limited feedback and in the way in which different feedback selection rules affect cooperation. In our setting feedback selection is not tied to one participant, but to the relative amount of a contribution.

Feedback on charitable giving

Providing feedback selectively in order to enhance donations was also the subject of two field experiments of Frey and Meier (2004) and Croson and Shang (2008).[19] Both studies manipulated the information potential donors receive about previous behavior of other donors. The former study investigated how information about the number of students who previously contributed to a charity organization influences giving. In the experiment of Frey and Meier one group received feedback that more than 50% of the students gave money to the charity while the other group knew that less than 50% contributed. They found that students were more likely to give when they were informed that more than 50% of their peers had contributed.

Croson and Shang (2008) contacted regular donors of a radio station and selectively provided them with different information about the previous contribution of one other donor. Because the researchers knew how much the participants had contributed, they were able to check how the contribution of each donor changed with respect to the given feedback. As predicted, if the feedback contribution was higher (lower) than the contribution of the participant, the contribution increased (decreased). However, contributions were much strongly influenced when the contribution feedback was lower. In a somewhat different, but related approach, Huck and Rasul (2011) tested how the decision to donate is influenced by the fact that potential donors know that a very generous donor has already given a huge amount of money. It turned out that the total amount donated was significantly higher when participants were informed about the generous donor compare to when they were not given this information.

[19] Shang and Croson (2009) provided a broader review.

Potential policy implications

The variety of feedback mechanisms and leadership institutions studied does yield ambiguous evidence about how feedback influences contributions in social dilemmas. As the studies of Weimann (1994) and Hoffmann et al. (2013) highlighted the credibility of feedback might be decisive for the development of contributions. Leaders in public goods might signal what is the desired behavior but they are not necessarily successful in convincing the others to follow. Comparable field studies on charitable giving also stressed the importance of social information, i.e., information about the donation behavior of others.

Our study contributes to the discussion of feedback in social dilemmas and charitable giving. Although previous experiments have shown that punishment is a promising institution to promote cooperation (e.g. Gächter et al., 2008) in various social dilemmas, its implementation is undesirable or simply too costly. Think, for example, of large teams that work together in different locations. In these situations individual punishment might be considered as undesirable since individual efforts are difficult to observe. Feedback, for instance, can be a helpful tool to stimulate cooperation under these circumstances. The results of our experiment suggest that providing employees with examples of good behavior might have a positive effect. However, if employees are aware that this is a good example, they are likely to consider it as an upper bound of what they should provide and therefore refrain from doing more. Therefore, whether employees are aware of the feedback selection rule seems to be a decisive factor. On the contrary, a team leader who tries to encourage employees' efforts by providing bad examples is well advised to inform them about the nature of the feedback. If team members know that they are exposed to the bad example, this seems to be considered as a lower bound of appropriate behavior and prevents even lower contributions. Another domain where feedback might have relevant consequences involves donations for charities. In the light of our results, when generous donors are shown as feedback it might lead to higher donation when others do not know that these are actually good examples.

2.4 Concluding remarks

In this experimental study, we consider voluntary contribution settings in which feedback about contributions is limited. The first limitation is that only one single contribution is observable as feedback. The other limitation is that the feedback selection rule might be non-transparent, i.e., that subjects do not know whether the feedback is the minimum, the maximum, or a randomly selected contribution. These limitations play a role in numerous settings outside the lab (e.g. Croson and Shang, 2008) in which voluntary contributions are sought, i.e., think of contributions to team endeavors where the effort of only one team member can be observed or is deliberately communicated by the firm. However, the team members might not know whether the firm actually chooses a good example or a random one. Alternatively, think of a charity that seeks donations and publicizes the contribution of a previous donor.

Our setting extends the literature of repeated public goods settings in the sense that most studies either provide detailed feedback on all individual contributions or provide only aggregate feedback, like the average contributions or the sum of contributions or the average earnings or the sum of earnings. Instead we focus on the feedback about a single contribution, which in a sense is less informative but is still likely to guide subsequent contribution behavior. Admittedly, we considered only two extreme cases, assuming that the feedback selection rule is either completely transparent or completely non-transparent which is a simplification to some extent. In many cases, the feedback rule is likely to be partly transparent, i.e., one has at least some idea of whether the minimum, the maximum or a randomly selected contribution is displayed. The investigation of these partly transparent cases, however, is beyond the scope of this paper.

The experimental results show that feedback about good examples increases and stabilizes the contribution levels. To observe the positive effect of good examples, it is important that subjects are not explicitly aware that particular good examples have been selected. When participants in our experiment know that the maximum contribution is shown contributions decline. Interestingly, when subjects know that they will face the minimum contribution of the group as feedback, average contributions are higher compared to a situation when the minimum is shown but the selection rule is non-transparent.

One potential explanation for the positive effect of non-transparency of the good example could be the notion of imperfect conditional cooperation (Fischbacher and Gächter, 2010). When subjects know that good examples are shown they match the feedback less often and contributions tend to decrease.

Finally, we show that the transparency of the feedback selection rule has similar effects on individual beliefs. Thus, our results provide first insights into the effects of limited feedback provision in public goods settings and the design of how to improve contributions.

2.5 Appendix

Tables and figures

Table 2.5: Explaining contributions (by information about feedback selection)

	Dependent variable Contribution$_{i;t}$		
Independent variables	MAX	MIN	RAND
Period	-0.425***	-0.543***	-0.476***
	(0.14)	(0.07)	(0.09)
1 if Non-transparent	4.006**	-2.403**	0.920
	(1.82)	(0.96)	(0.91)
Feedback$_{t-1}$	0.256**	0.606***	0.291***
	(0.10)	(0.07)	(0.06)
Constant	7.005***	8.289***	6.592***
	(1.79)	(0.86)	(0.96)
Observations	576	540	576
R^2	.21	.372	.206

Notes: GLS regressions with robust standard errors in parentheses (clustered on groups) for the three feedback selection rules. "1 if Non-transparent" takes the value 1 if the feedback selection rule was non-transparent and 0 if not (transparent). "Feedback$_{t-1}$" controls for the contribution that subjects saw in the previous period.
*** Significance at p<.01, ** Significance at p<.05, * Significance at p<.1.

Table 2.6: Overview of contributions, beliefs, feedback, and matching

Periods	Feedback selection rule	Contributions					Beliefs				
		MAX		RAND		MIN	MAX		RAND		MIN
1st-10th	Non-transparent	13.21	>***	7.39	>**	4.02	15.51	>***	8.48	>***	3.25
		∨**		∨		∧*	∨**		∨*		∧**
	Transparent	8.53	>	5.61	<	7.44	10.74	>**	5.56	<	7.5
1st	Non-transparent	12.34	>	9.94	<	11	11.78	>	10.25	<	10.93
		∨		∧		∧	∨		∨		∧
	Transparent	10.34	>	9.91	<	11.56	9.72	<	10.09	<*	12.63
10th	Non-transparent	9.59	>**	3.56	>*	3.13	13.31	>***	5.31	>***	.32
		∨*		∨		∨*	∨		∨		∧***
	Transparent	6.28	>***	2.34	>	0.68	7.91	>*	3.31	>	3.78

Periods	Feedback selection rule	Feedback					Perfect matches				
		MAX		RAND		MIN	MAX		RAND		MIN
1st-10th	Non-transparent	18.85	>***	7.66	>*	0.91	.51	>**	.29	<**	.65
		∨**		∨		∧	∨***		∧**		∨**
	Transparent	14.7	>***	4.45	>	2.89	.22	<**	.52	>	.38
1st	Non-transparent	19.38	>***	12.25	>***	3.43	/		/		/
		∨		∨***		∨					
	Transparent	18.75	>***	6.63	>**	3.13	/		/		/
10th	Non-transparent	18.13	>***	0.75	>	0	.41	>	.25	<***	.79
		∨*		∧		=	∨		∧**		∨
	Transparent	10.75	>*	1.25	>***	0	.25	<**	.72	=	.72

Notes: In this table we display average values of contributions, beliefs, feedback for different time spans. 1st-10th represents the average values over all ten periods. 1st and 10th stand for the average in the respective period. Signs indicate significance levels of a two-sided Mann Whitney U test for which the null hypothesis that there is no difference between both treatments can be rejected.
*** Significance at $p<.01$, ** Significance at $p<.05$, * Significance at $p<.1$.

Experimental instructions (translated from German)

You are now participating in an economic experiment. If you read the following instructions carefully, you can, depending on your decisions, earn a considerable amount of money. It is therefore very important that you read these instructions with care. The instructions which we have distributed to you are solely for your private information. It is prohibited to communicate with the other participants during the experiment. Should you have any questions please ask us!

During the experiment we shall not speak of Euro but rather of points. During the experiment your entire earnings will be calculated in points. At the end of the experiment the total amount of points you have earned will be converted to Euros at the following rate: 1 Point = 0.04 €. At the end of the experiment your entire earnings from the experiment will be immediately paid to you in cash.

At the beginning of the experiment the participants will be divided into groups of four. You will therefore be in a group with 3 other participants. This group constellation will stay the same throughout the entire experiment. In all, the experiment consists of 10 periods. At the beginning of each period each participant receives 20 points. In the following we call this his or her endowment. Your task is to decide how to use your endowment. You have to decide how many of the 20 points you want to contribute to a joint project and how many of them to keep for yourself.

Your payment for each period can be calculated with this simple formula. If you have any questions about that, please ask us.

Payment = Endowment - Your Contribution + 0.4·Sum of all Contributions

This formula shows that your income consists of two parts:

1. The points which you have kept for yourself (Endowment – Your Contribution)
2. The income of the project which is 40 % of the sum of the contributions of the group.

The income of each group member from the project is calculated in the same way, this means that each group member receives the same income from the project. Suppose the sum of the contributions of all group members is 60 points. In this case each member of the group receives an income from the project of: $0.4 \cdot 60 = 24$ points. If the total contribution to the project is 9 points, then each member of the group receives an income of $0.4 \cdot 9 = 3.6$ points from the project.

In each round you have the possibility to keep points for yourself or to contribute them to the project. For each point, which you keep for yourself you earn an income of 1 Point. Assuming you contributed this point to the project instead, then the total contribution to the project would rise by one point. Your income from the project would rise by $0.4 \cdot 1 = 0.4$ points. However the income of the other group members would also rise by 0.4 points each, so that the total income of the group from the project would rise by 1.6 points. Your contribution to the project therefore also raises the income of the other group members. On the other hand you earn an income for each point contributed by the other members to the project. For each point contributed by any member you earn $0.4 \cdot 1 = 0.4$ points. After all members of your group have made their decision, you will get the information about one contribution of one person in your group for the previous period. Note: All group members will get to know the contribution of the same person. The shown contribution can also be your own contribution. All group members of your group receive after the decision the following information:
Treatment *Transparent Feedback Selection* MAX:
"The person with the highest contribution in your group has contributed: XX"
Treatment *Transparent Feedback Selection* RAND:
"A randomly determined contribution of a person was: XX"
Treatment *Transparent Feedback Selection* MIN:
"The person with the lowest contribution in your group has contributed: XX"
Treatment *Non-transparent Feedback* (MAX, RAND, MIN):
"A contribution of one person was: XX"
Additionally, we would like you to estimate the average contribution of the other three group members. Please note: If you estimate precisely the average contribution of the other three group members you will get an additional payoff of 0.2 €. In case of a deviation of + / − 0.5 points you will also get 0.2 €.

Control-questions (translated from German)

1. Each group member has an endowment of 20 points. Nobody (including yourself) contributes any point to the project in the first stage. How high is:

   a) Your income from the first stage?...........

   b) The income of the other group members from the first stage?...........

2. Each group member has an endowment of 20 points. You contribute 20 points to the project in the first stage. All other group members each contribute 20 points to the project in the first stage. What is:

   a) Your income from the first stage?...........

   b) The income of the other group members from the first stage?...........

3. Each group member has an endowment of 20 points. The other three group members contribute together a total of 30 points to the project.

   a) What is your income from the first stage if you contribute a further 0 points to the project?...........

   b) What is your income from the first stage if you contribute a further 15 points to the project?...........

4. Each group member has an endowment of 20 points. You contribute 8 points to the project.

   a) What is your income from the first stage if the other group members together contribute a further total of 7 points to the project?...........

   b) What is your income from the stage if the other group members together contribute a further total of 22 points to the project?...........

3 The Effects of Incentives on Honesty in Tournaments

Introducing competition among employees, e.g., for a bonus, is a tool used by designers of organizational incentive schemes to increase effort provision. Even if such tournament incentives are not explicitly imposed, e.g., by payment schemes, tournaments are implicitly prevalent in basically all organizations. For example, promotions in hierarchies can be interpreted as tournament competition among employees. Previous research has theoretically (Lazear and Rosen, 1981; Grund and Sliwka, 2005), and empirically (Harbring and Lünser, 2008) shown the effort enhancing effect of such tournament incentives. The downside of competitive incentives, however, is linked to employees' potential engagement in unethical behavior to win the tournament. Especially in situations when effort provision or outcomes are not fully observable and verifiable, agents might be tempted to forge results. A growing strand of literature has shown unethical conduct under tournament incentives, e.g., less helping and greater sabotaging of opponents (see Carpenter et al., 2010; Harbring and Irlenbusch, 2011). Unethical behavior can also be observed under other types of compensations schemes, e.g., goal-setting and team-incentives (see Schweitzer et al., 2004; Shalvi et al., 2011; Conrads et al., 2013). Cadsby et al. (2010) compares a tournament scheme to other incentives schemes without particularly investigating specific tournament types.

In this paper we concentrate on ethical conduct, i.e., employees' inclination to honestly report their performance, in different tournaments. We are particularly interested in how honesty is affected by increasing competition through varying the prize spread. Fischbacher and Heusi (2013) find that individuals systematically overreport the true value of a private die-roll when the reported

This chapter is based on joint work with Julian Conrads, Bernd Irlenbusch, Anne Schielke, and Gari Walowitz and has recently been published in *Economics Letters* (Conrads et al., 2014).

number determines their individual pay. We extend their experimental design to a simple two-player tournament incentive scheme with varying prize spreads between the winner and the loser. Thereby, we increase the degree of competition among the two players in order to analyze its effect on honesty.

If an individual has no costs of lying and is only interested in maximizing her own payoff she will always overreport her performance in tournaments. The growing literature on lying aversion (Gneezy, 2005; Mazar et al., 2008; Sutter, 2009; Kartik, 2009; Erat and Gneezy, 2012; Fischbacher and Heusi, 2013), however, has shed doubts on these assumptions. For example, Gneezy et al. (2013) classify subjects into liar-types with different lying costs, i.e., they find types that are totally honest or dishonest, respectively, and types that condition their lies on the given incentive structure. Gibson et al. (2013) also highlight the existence of heterogeneous preferences for truthfulness. Their studies underline the intuition that people differ in their perceived cost of lying. In particular, their results suggest that people experience either no costs of lying or high fixed costs. With respect to these findings, the aim of our study is to provide designers of incentives schemes with empirical insights into the potential adverse effects of a presumably effort enhancing compensation scheme.

3.1 Experimental design

Subjects are instructed that their payment for filling in a questionnaire will be based on a production output p_i randomly determined by rolling a fair 6-sided die.[1] We intentionally induce subjects' production output by a random procedure to abstract from concerns that lying behavior is influenced by subjects' production abilities (Charness et al., 2013). In all treatments, the production output p_i of subject i equals the number d_i shown on the die if $d_i \in \{1, \ldots, 5\}$, whereas a die roll of $d_i = 6$ results in a production output $p_i = 0$. In order to implement a tournament we extend the game by Fischbacher and Heusi (2013) in the following way: subjects are randomly and anonymously matched in groups of two, and each subject privately rolls her die such that nobody apart from her, i.e., neither the experimenter nor any

[1] The experimental instructions can be found in the appendix. The original instructions are in German.

other subject, can observe the production output p_i. Then, each group member individually submits a report r_i of her production output where r_i does not have to be equal to p_i. Production outputs reported by the two group members are compared by the experimenter. The group member who submitted the higher reported production output receives the winner prize w, while the other group member receives the loser prize l, with $w > l$. If both group members submit the same report, the player to receive w is determined by a 50/50 random draw. Within our three experimental conditions we vary the prize spread $\Delta = w - l$ from 1 to 5: in treatment T5 the winner receives 5 while the loser gets nothing, in treatment T3 the winner receives 4 while the loser gets 1, and in treatment T1 the winner gets 3 while the loser receives 2.

Our treatments are designed such that they have several characteristics in common: first, on average subjects earn 2.5 whatever they report. Second, if all subjects report their true production output, the expected payoff of each subject is 2.5. Third, if both players report the maximum production output of 5, their expected payoff also equals 2.5. Fourth, the sum of winner and loser prizes and hence the cost of implementing the respective tournament is equal to 5 across all experimental conditions.

As indicated above, the aim of our study is to examine whether a change in the prize spread has an impact on subjects' willingness to honestly report their production output. Under the assumption that lying is completely costless, it is optimal for both subjects to report the highest production output of 5 which results in expected payoffs of 2.5 for both players. Hence, in absence of lying costs, the prize spread should not influence subjects' reports and hence we should not observe any treatment differences. If we assume that subjects incur lying costs, i.e., if a subject's utility diminishes by a certain amount whenever she submits a reported production output that is different from the true production output, her willingness to be honest depends on her lying costs and potential gains from lying. Since the latter is not independent from prize spread Δ, an increase in the prize spread across our experimental conditions may well reduce honesty.

A total of 478 students (with a mean age of 24 and 54 % being female) participated in our experiment in the laboratories of the University of Bonn and the University of Cologne, and were recruited via ORSEE (Greiner, 2003). After privately rolling their die and jotting down their report on a sheet of paper, subjects were asked to fill in the questionnaire. At the end of the session participants were privately paid at a conversion rate of 1€ per prize unit. Following Fischbacher and Heusi (2013) we ran our experiment after different other experimental sessions.[2]

3.2 Results

Figure 3.1 depicts the distribution of reported production outputs across treatments. The dashed line represents the expected relative frequency of the true production output. Evidently, the observed distributions markedly differ from this benchmark.

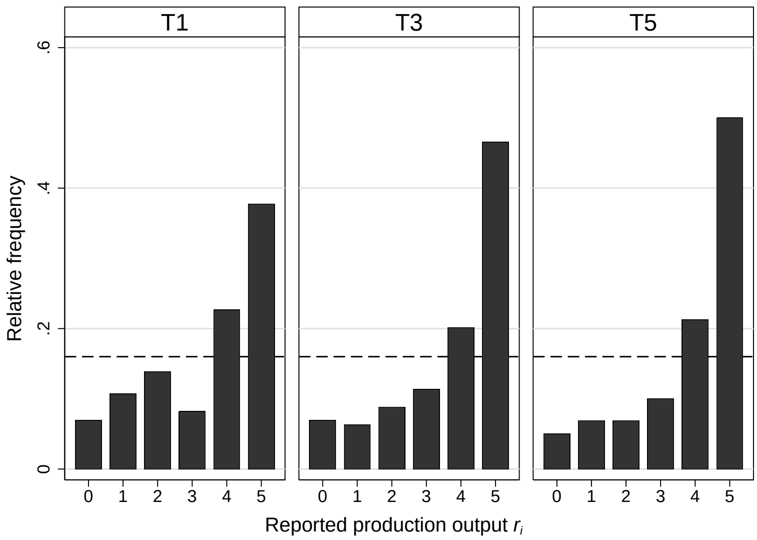


Figure 3.1: Distribution of reported production outputs. The dashed line indicates the expected relative frequency of the true reported production output.

To address our research question we need to compare the reported production outputs across treatments. In treatment T1 we observe the lowest average reported production output (r_{T1}=3.42).[3] Increasing the prize spread by 2 units

[2]The preceding experimental sessions consisted of standard experimental games like dictator, ultimatum or public goods games. To counteract potential spill-over effects we balanced our three treatments over the different types of preceding experiments.

[3]Note, that in the baseline treatment of Fischbacher and Heusi (2013) - which essentially resembles a piece-rate incentive scheme - an average of r_{FHH} =3.51 is observed. Statistically, there is no significant difference in reported production outputs between their baseline treatment and T1.

in T3 enhances the average reported production output to r_{T3}=3.71. However, the highest average reported production output of r_{T5}=3.86 can be found in T5 - the treatment with the highest prize spread of 5. Although we cannot rule out that some subjects lie to their own disadvantage (as we do not observe the true production outputs) there seems to be a tendency that subjects lie more the higher the prize spread is by exaggerating their true production output. An overview of the results can be found in Table 3.1.

Table 3.1: Overview of treatments and results

Treatments	w	l	n (% female)	$\bar{r}_{Tk}$	r_i=0	r_i=1	r_i=2	r_i=3	r_i=4	r_i=5
T1	3	2	159 (56%)	3.42	$.07^{---}$	$.11^{--}$	.14	$.08^{---}$	$.23^{++}$	$.38^{+++}$
				$\wedge^{**}$						
T3	4	1	159 (57%)	3.71	$.07^{---}$	$.06^{---}$	$.09^{---}$	$.11^{-}$	$.20^{+}$	$.47^{+++}$
				$\wedge$						
T5	5	0	160 (50%)	3.86	$.05^{---}$	$.07^{---}$	$.07^{---}$	$.10^{--}$	$.21^{++}$	$.50^{+++}$
				$\vee^{***}$						
T1	3	2	159 (56%)	3.42	$.07^{---}$	$.11^{--}$	.14	$.08^{---}$	$.23^{++}$	$.38^{+++}$

Notes: n stands for the number of observations. $w(l)$ is the winner (loser) prize. $\bar{r}$ is the average reported production output. Stars show the significance of a two-sided Mann-Whitney U test (* = 10%-level, ** = 5%-level, *** = 1%-level). Plus and minus signs display the significance of a one-sided binomial test indicating that the observed relative frequency is smaller (larger) than $\frac{1}{6}$ $---(+++)$=10%-level, $---(+++)$=5%-level, $---(+++)$=1%-level. The number of observations is uneven, because we exclude subjects that already took part in die rolling experiments at the laboratory in Cologne.

According to a Jonckheere-Terpstra Test the hypothesis that there is no difference in reported production outputs can be rejected in favor of the hypothesis that reported production is increasing in the prize spread (p=.0064, one-sided). Pairwise comparisons of the distribution of reported production outputs show higher values in T3 compared to T1 (p=.0464, Mann-Whitney U test, one-sided), and in T5 compared to T1 (p=.0064, Mann-Whitney U test, one-sided). A pairwise comparison between T3 and T5 yields no significant difference (p=.2114).[4]

According to a Jonckheere-Terpstra Test, reporting the highest possible pro-

[4]Interestingly, we find that women report significantly lower production outputs compared to men in T3 (p=.0001, Mann-Whitney U test, two-sided) and T5 (p=.0153, two-sided). When the prize spread is rather small (T1) no difference between men and women is observed. This supports observations from the literature on gender differences in lying behavior (Dreber and Johannesson, 2008).

duction output r_i=5 is more likely as the price spread increases (p=.0139, one-sided). Pairwise comparisons of the fraction of subjects reporting r_i=5 between treatments yields a significant difference between T5 and T1 (p=.027, χ^2-test). Comparing T5 against T3 and T3 against T1 yields no statistical significant differences (p=.536, p=.112). In all treatments the frequencies of the reported production output of r_i=4 exceed the benchmark threshold of .16 (p<.09 for all treatments, binomial test, one-sided). Interestingly, the number of subjects reporting 4 does not statistically differ between treatments (p=.86, χ^2-test). In addition, we observe a positive fraction of subjects (T1:.07; T3:.07; T5:.05) reporting production outputs of zeros. According to a χ^2-test there is no significant difference across treatments regarding the fractions of subjects who report zeros (p=.717).

3.3 Concluding remarks

Lying, as compared to other unethical activities has been mostly overlooked when studying the effectiveness of tournament incentives. This might be due to the fact that economists often consider lying to be costless. By focusing on the role of honesty in tournaments, this study augments the small literature on the interplay of incentive schemes and ethical behavior. Our experiment extends the simple and widely used die rolling game paradigm introduced by Fischbacher and Heusi (2013) to a two-player tournament and varies the difference between winner and loser prize. We find evidence that a larger prize spread increases subjects' propensity to be dishonest. However, we also find that not all subjects report the highest possible production output and the fraction of subjects who (truthfully) reports a production output of zero does not change with the prize spread. Thus, even in the face of competitive incentives a considerable fraction of subjects appears to be reluctant to be untruthful to the full extent. Taken together, these results suggest individual heterogeneity of lying costs (Gneezy et al., 2013; Gibson et al., 2013). When implementing tournament incentives, organizations should be aware of potential ethical misconduct, for example dishonesty of employees. In fact, employees' dishonest behavior might result in inefficient outcomes. For example, sales people who deceive their customers in order to win a bonus might harm a corporation's reputation, and employees' veiling of mistakes or overstatement of skills may hinder the organization to learn how to improve. Hence, in order to prevent the occurrence of inefficiencies, designers of incentive schemes should be especially vigilant when considering to distribute a fixed amount of money among their employees by employing relative performance measures that are not fully observable or verifiable. Unethical behavior in competitive situations can have many facets and lying over one's own performance is only one of them. Sabotage, office politics and employee theft are other possible scenarios. Taken together the study at hand and the existing literature calls for a more holistic investigation in order to gain a deeper understanding of moral transgressions in the workplace.

3.4 Appendix

Experimental instructions (translated from German)

Statistical Investigation

In order to do a statistical investigation (which has nothing to do with the previous experiment) we ask you for answering some questions. To determine your payout for answering the following questions, we firstly want you to throw a dice. If you roll a 1, 2, 3, 4 or 5 you will get the corresponding numbers of the dice as points. If you throw a 6 you will get 0 points. Furthermore, you are randomly matched to another participant of the experiment. The participant in the diad having more points will get T1{3} T3{4} T5{5} Euro as payout. The other one will get T1{2} T3{1} T5{0} Euro as payout. If both participants have equal number of points, the probability of getting paid 5 Euros is 50 percent. Please throw the dice now (you are obligated to write down the first rolled number, afterwards you are allowed to roll the dice another times to check it for fairness).

Write down here the number you initially rolled:

Please fold the sheet after finishing the exercise. The sheets are collected after all participants have finished the exercise. Subsequently the questions are distributed on additional sheets. Please, answer them carefully.

4 On Equity Rules in Ultimatum Bargaining with Outside Options

Experienced negotiators are well aware of the fact that lucrative alternatives in the case of a bargaining breakdown strengthen their own bargaining position. Authoritative manuals of successful negotiation strategies even recommend that one should strive for such outside options before entering into a bargaining situation (Fisher and Ury, 1991; Malhotra and Bazerman, 2008; Malhotra and Gino, 2011). Although experts and common wisdom suggest that outside options constitute an important determinant of bargaining outcomes, only very limited systematic research has examined bargaining behavior when outside options are available. How do bargainers in ultimatum game like situations take outside options into account? What happens if outside options are asymmetric? Do different constellations of outside options trigger different distribution rules? How does the constellation of outside options affect the outcome of a negotiation, and how does it affect the likelihood of reaching an agreement in the first place?

Think, for example, of a manager searching for a new job and an employer looking for a manager to run a new subunit. The employer needs to fill the position quickly, due to urgent customer requests. In the new job, the manager would generate a certain profit that could be divided between her and the new employer. The parties have asymmetric outside options; for example, the manager - who can always refuse the offer by the employer and leave the negotiation - holds an offer from somewhere else, and the employer could realize gains from outsourcing the planned activity. Let us suppose that both

This chapter is based on joint work with Heike Hennig-Schmidt, Bernd Irlenbusch, and Gari Walowitz (Hennig-Schmidt et al., 2014). A part of the experimental sessions have been analyzed in Rilke (2009) with a focus on how ultimatum offers correlate with personality traits.

parties have a good estimate of the value of each others' outside options (e.g., the potential employer might have an exact estimation of the market value of the manager, and the manager has insider information on gains from outsourcing). Do the outside options have an influence on how the profit is divided? One might think of various arguments that suggest different divisions. For example, one could argue that the profit should be equally divided (*equal split*) because both parties are needed to generate the profit. Alternatively, one might guarantee the outside options for each party and divide the remainder equally (*split the difference*). A third method would be to divide the profit proportionally relative to the outside options (*proportional split*). Would the negotiators follow one of these rules? If yes, which one would they apply?

In our analysis on ultimatum bargaining with outside options, we concentrate on the three previously discussed distribution rules. We refer to the three rules as *equity rules* namely, equal split, split the difference, and proportional split. One reason for this focus is that the relevance of these three rules has frequently been observed in previous studies (for a survey, see Konow, 2003). A second reason is that all three rules follow a similar logic: All three can be derived from the *generalized equity principle* proposed by Selten (1978). The generalized equity principle relies on accepted positive weights (Selten refers to them as a 'standard of comparison') assigned to each party involved in the negotiation. The weights can reflect different characteristics of the bargaining situation, such as the number of people represented by one party, the magnitude of the outside options, and a measure of power or individual contributions to a joint project in terms of money or effort. A final distribution (Selten calls it a 'standard of distribution') of an amount satisfies the generalized equity principle if the ratio between the individual payoff and the individual weight is equal for all involved parties.[1] In Section 4.1, we explain how the three

[1] The criterion of proportionality that underlies the generalized equity principle goes back at least to Aristotle, (Nicomachean Ethics, V, 5): "Let A be a builder, B a shoemaker, C a house, D a shoe. The builder, then, must get from the shoemaker the latter's work, and must himself give him in return his own. If, then, first there is a proportionate equality of goods, and the reciprocation takes place, the result will be 'equality.' If not, the bargain is not equal, and does not hold; for there is nothing to prevent the work of the one being better than that of the other; they must therefore be equated." Later, proportionality in exchange was prominently featured in many disciplines, in philosophy (Soudek, 1952), sociology (Homans, 1958; Deutsch, 1975; Cook and Hegtvedt, 1983), social psychology (Adams, 1965; Walster et al., 1973; Greenberg, 1990; Messick, 1993; Folger, 1986) and economics (Young, 1995; Konow, 2000; Balafoutas et al., 2013).

equity rules can be derived from the generalized equity principle by employing different weights and by varying the amount to which the generalized equity principle is applied. To keep the bargaining situation simple, we employ the ultimatum game (Güth et al., 1982) as our workhorse.[2] A proposer i and a responder j bargain over an amount of money a. The proposer makes an offer $a_j \leq a$ to the responder. If the responder accepts, she receives a_j and the proposer receives $a_i = a - a_j$. If the responder rejects the offer, both players receive their respective outside options, i.e., the proposer receives o_i and the responder receives o_j. In the standard ultimatum game, the outside options of both players are equal to zero (i.e., with regard to the outside options, both players have equal bargaining strength).

A large number of experimental studies have looked into the behavior within the standard ultimatum game and found a clear predominance of equal payoff offers (see, for example, Güth and Tietz, 1990; Güth, 1995; Güth and Kocher, 2013). In light of the generalized equity principle, this result does not come as a surprise because, for the standard ultimatum game, all three equity rules discussed herein suggest the same outcome: equal shares for both players.

In our experiment, we employ five different ultimatum games between and within subjects as our treatments. Proposer and responder bargain over a total amount of 240 points. Treatments vary the size of the higher outside option (either 150 or 90 points; the lower outside option is always 30 points) and the player who has the larger outside option (i.e., the proposer or the responder). In addition, we run a treatment where both outside options are 30 points, i.e., symmetric. Another feature of our experiment is that every participant runs through two ultimatum games with different outside option constellations, which enables us to trace equity notions of one individual under different circumstances.

The parametrization of our treatments guarantees that *(i)* for the asymmetric ultimatum games the three equity rules provide three different point predictions, *(ii)* in the symmetric game the proportional split is applicable,

[2] Related empirical studies use the 'claims problem' (also called the 'bankruptcy problem') to study equity norms in bargaining (for example, Gächter and Riedl, 2005, 2006; Bosmans and Schokkaert, 2009; for an extensive discussion, see also Gärtner and Schokkaert, 2012). In our ultimatum games, the sum of outside options is always smaller than the total amount available and, thus, an agreement increases efficiency. In the claims problem the situation is different because the sum of claims exceeds the available amount.

i.e., the outside options are different from zero, (*iii*) the sum of the outside options is smaller than the total amount available, i.e., agreement increases efficiency and (*iv*) in some of the treatments, one outside option is larger than the equal split.

We designed our treatments to investigate three important issues of bargaining with outside options. First, we analyze whether the generalized equity principle captures the behavior observed and, if so, which of the three equity rules are applied under the various outside option constellations. Second, in a within-subject comparison we investigate whether individuals consistently apply the equity rules if they take part in two ultimatum games that differ in their outside option constellations. Finally, we study the interplay among outside options, equity rules and rejection behavior.

One of our main findings is that the generalized equity principle proposed by Selten (1978) reflects the behavior in our experiment remarkably well. Overall, 43% of observed offers correspond to the point predictions of one of the three equity rules. In the symmetric games, most proposers offer the equal split. This behavior is predicted by all three equity rules. When comparing the behavior from the asymmetric ultimatum games across treatments, it becomes evident that not one single equity rule is prevalent. The data suggest that a proposer tends to apply the equity rule that benefits her most.[3] More precisely, the majority of proposers opt for a proportional division when they have the larger outside option of either 150 or 90. However, when the responder has the larger outside option, proposers tend to suggest splitting the endowment equally. An amount of 25% of responders' level of minimal acceptable offers is captured by the equity principle. Regarding the rejection behavior of responders, we observe high rates of rejection in games with outside options of 150 namely, in games in which the responder has an outside option that is larger than the equal split. Responders, too, tend to adopt the equity rule that favors them. This self-serving use of the equity rules by the proposers and responders often leads to rejections, i.e., inefficient bargaining outcomes. Concerning profits and efficiency gains, we find that having a large outside option, i.e., 150 does not lead to significant improvements. This finding sheds new light on the common

[3]Self-serving behavior in other bargaining and negotiation contexts underlines similar findings from related work (see, for example, Messick and Sentis, 1979; Babcock et al., 1995; Babcock and Loewenstein, 1997; Pillutla and Murnighan, 1995; Lange et al., 2010; Rodriguez-Lara and Moreno-Garrido, 2012; Rode and Menestrel, 2011).

understanding that better outside options are desirable.

Our review of related studies is guided by our main research questions. Do different equity rules explain ultimatum bargaining behavior? How do outside options shape behavior in ultimatum games?

The application of different distribution rules has also been investigated in other experimental games, e.g., in the claims problem and in the dictator game. The claims problem describes a situation where an amount of money can be distributed between players that have claims and where the amount to be distributed is smaller than the sum of these claims. Gächter and Riedl (2005; 2006), Bolton and Karagozoglu (2013) and Bosmans and Schokkaert (2009) investigated the behavior and normative judgements of individuals in a claims problem. They show that subjects' decisions and normative judgments are strongly influenced by proportionality considerations. Rodriguez-Lara and Moreno-Garrido (2012) investigated the self-serving selection of justice principles in a dictator game. Prior to the distribution choice, players could enlarge the amount to be distributed by answering trivia questions. Subjects differ in the way their (correct) answers on the quiz enlarge the amount (i.e., they differ in their productivity, which is randomly assigned). Ex ante, the authors identify three different division rules based on Cappelen et al. (2007): the egalitarian, the accountability and the libertarian principle. The egalitarian principle predicts that dictator and receiver end up with the same amount, irrespective of their productivity. The accountability principle holds subjects accountable for what they can control; here: the number of correct answers, but not the productivity. A subject's share should be proportional to the number of her correct answers. The libertarian principle does not differentiate between what a subject can influence (the number of correct answers) and what the subject cannot influence (the productivity). The results highlight a self-serving bias in justice assessments. Dictators with a lower productivity compared to the receiver tend to strive for an egalitarian distribution. Contrarily, when the dictator's productivity is higher than that of the receiver, the proposals can best be described by the libertarian or the accountability principle.

When individuals who have to distribute earnings are different with respect to certain characteristics their behavior seems to be guided by predictable distribution rules. In this respect, closely related to the present work is the study of Kagel et al. (1996). They demonstrated that subjects try to enforce different seemingly 'fair' allocation rules. In their ultimatum game experiment, they manipulated the exchange rates of the experimental currency for the two players. The players can divide the pie according to an equal dollar split or, alternatively, according to an equal chip split. Their results effectively show that subjects with a lower exchange rate try to enforce an equal dollar split which would make them better off compared to the equal chip split. However, subjects who have been assigned the larger exchange rate try to adhere to the equal chip split. The authors also observed that the disagreement over different distribution rules leads to frequent rejections.[4]

Surprisingly, although the ultimatum game is considered to be one of the main workhorses in experimental economics (see, e.g., Güth and Kocher, 2013) and outside options are considered to be one of the main influencing variable in bargaining in general only a very few studies have examined the effects of asymmetric outside options within this setting (notable exceptions are Knez and Camerer, 1995; Schmitt, 2004; Kohnz and Hennig-Schmidt, 2005; Fischer, 2005; Fischer et al., 2007). Their results can be summarized as follows: Proposers decrease their offers when they have a larger outside option than the responder but increase their offers when responders have a larger outside option. In both cases, high rates of rejection are observed, suggesting that responders think that the offers are too low. However, none of these studies investigates how different equity rules relate to players' behavior in asymmetric ultimatum bargaining. One exception is Hennig-Schmidt et al. (2010) who survey video experiments with asymmetric outside options in different games (see also Hennig-Schmidt et al., 2008). They report that subjects not only discuss equity-based division rules, they also consider them as fair and base their negotiation behavior on these rules.

Taken together, the modest literature so far shows that different outside options appear to lead negotiators to disagree about what a 'reasonable' division might be. But what are underlying distribution rules in an ultimatum bar-

[4]Bediou et al. (2012) examine equality and equity considerations in an ultimatum game with a prior production phase but equal outside options of zero. Without their knowledge, subjects in their experiment solve a quiz against an algorithm which is either programmed to win against the participant or to lose against the participant. The results indicate that winners adhere to proportional divisions and losers tend to adhere to a more equal distribution of the production outcome.

gaining context with asymmetric outside options? We extend upon the above literature and shed light on this question by systematically applying the generalized equity principle of Selten (1978) to an ultimatum bargaining context with asymmetric outside options. The principle provides three distinct equity rules that follow an equity logic but lead to different distributions. Our approach deepens the understanding of how different equity notions are at work in ultimatum bargaining situations that might ultimately lead to inefficient bargaining outcomes.

4.1 The generalized equity principle in ultimatum bargaining with outside options

In the following, we exemplify how the three equity rules can be derived from applying the *generalized equity principle* (Selten, 1978) to bargaining with (asymmetric) outside options. We focus on two players: i (the proposer) and j (the responder), who negotiate about how to divide an amount a.

The generalized equity principle proposes to balance the players' shares according to individual weights.[5] Let $r \leq a$ be the amount of money that is to be distributed. The non-negative weights w_i and w_j of players i and j reflect a certain characteristic according to which the players can be compared (e.g., their outside options, the number of people represented by a player, a measure of power, contributions to a joint project). Selten (1978) calls the vector of weights (w_i, w_j) the *standard of comparison*. A *standard of distribution* is a vector (r_i, r_j), with $r_i, r_j \geq 0$ and $r_i + r_j = r$. The generalized equity principle requires

$$\frac{r_i}{w_i} = \frac{r_j}{w_j}. \tag{4.1}$$

The standard of distribution for player i is given by $r_i = w_i/(w_i + w_j) \cdot r$.

[5] When the players' allocations are based on the assumption of common rationality and money-maximization, proposers offer at least the outside option to the responder. Thus, applying the sub-game perfect equilibrium outcome (SP) yields a share for the responder of one unit more than her outside option. The proposer would receive the rest of the complete amount. This result holds for the case that the amount to be distributed is infinitely divisible. Typically in experiments, bargaining units are integers. Thus, an offer of the size of the responder's outside option can also be an outcome of a sub-game perfect equilibrium.

Depending on the amount r and the standard of comparison w_i and w_j at least three different distribution rules can be derived for ultimatum bargaining with outside options. The candidates for the amount r are the complete amount, i.e., $r = a$, or the complete amount diminished by the respective outside options, i.e., $r = a - o_i - o_j$. Natural candidates for the weights are $w_i = w_j = 1$ (because, e.g., each bargaining party is constituted by one individual) or $w_i = o_i$ and $w_j = o_j$ (since, e.g., outside options are likely to be a major source of bargaining power).

Table 4.1: Overview of equity rules from the perspective of player i

Equity Rule		r	w_i	r_i	a_i
Equal Split	(EQ)	a	1	$\frac{r}{2}$	$\frac{a}{2}$
Split the Difference	(SD)	$a - o_i - o_j$	1	$\frac{r}{2}$	$o_i + \frac{1}{2} \cdot (a - o_i - o_j)$
Proportional Split	(PS)	a	o_i	$\frac{o_i}{o_i+o_j} \cdot r$	$\frac{o_i}{o_i+o_j} \cdot a$

Notes: r is the amount to which the equity principle is applied; $(r_i; r_j)$ is the standard of distribution, for player i. $(w_i;\ w_j)$ denotes the standard of comparison, a_i stands for the amount the player i receives in the case of agreement, o_i represents her outside option.

Equal Split

The *Equal Split* (henceforth EQ) results from the generalized equity principle when one assumes that both players have the same weight $w_i = w_j = 1$ and that r is equal to the total amount a. According to this equity rule, every player receives the same amount that is, $a_i = a_j = a/2$.

Split the Difference

The distribution rule *Split the Difference* (SD) emerges from the generalized equity principle when $r = a - o_i - o_j$ and players apply $w_i = w_j = 1$. Player i's amount is then determined by $a_i = o_i + 1/2 \cdot (a - o_i - o_j)$ and player j's amount is $a_j = o_j + 1/2 \cdot (a - o_i - o_j)$. SD yields an unequal distribution if $o_i \neq o_j$. Note, that SD does not apply to the total amount to be distributed but to the remaining pie after the outside options have been substracted.[6]

[6] Assuming that the outside options can be regarded as threat points, the distribution rule SD also follows from the Nash bargaining solution (Nash, 1953) and the Shapley value (Shapley, 1953). For a discussion, see Roth (1988); Chiu and Yang (1999); Anbarci and

Proportional Split

The *Proportional Split* (PS) can be derived from the generalized equity principle by using a standard of comparison based on the relative magnitude of outside options, i.e., $w_i = o_i$ and $w_j = o_j$ and by assuming that r is equal to the total amount a to be distributed. Each player's share represents her proportional bargaining power induced by her outside option. Player i receives $a_i = a \cdot o_i/(o_i + o_j)$ and player j receives $a_j = a \cdot o_j/(o_i + o_j)$. This equity rule also leads to an unequal distribution if $o_i \neq o_j$.

Note that EQ, SD and PS result in the same payoffs if, and only if, $o_i = o_j$.[7] In the standard ultimatum game, where $o_i = o_j$, the equal split appears to be prevalent (see Güth and Tietz, 1990; Güth, 1995).

4.2 Experiment

Experimental Design

To systematically investigate the impact of outside options on the application of the equity principle in ultimatum bargaining, we in total investigate five treatments, each employing an ultimatum game with different outside option constellations. In all treatments, the amount a to be distributed is equal to 240 points. The proposer decides on the amount she is willing to offer to the responder, while the responder simultaneously indicates the minimal offer she would be willing to accept ("mao"). If the proposer's offer exceeds or is equal to this minimum acceptable offer, the 240 points are distributed according to the offer; otherwise, subjects receive their respective outside options. Due to the large action space of the proposer in our experiment, eliciting responders' behavior with the strategy method would be inconvenient. Eliciting their behavior by asking for their mao allows us to simplify responders' decisions and enables us to analyze their behavior in more statistical depth without asking for every possible offer.[8]

In the *Baseline* treatment, both players have the same outside option of 30 points. In addition, we employ treatments with asymmetric outside options,

Feltovich (2013).

[7] Note that PS is not applicable if both outside options are equal to zero.

[8] We are aware that employing this procedure we have to assume monotonicity in responders behavior. Hennig-Schmidt et al. (2008) show that, at least in a German sample this assumption can safely be made.

where an outside option of one player is larger than the outside option of the other player. The smaller outside option is always 30 points. We systematically vary (*i*) the sizes of the larger outside option (either 150 or 90 points) and (*ii*) the player who is endowed with the larger outside option (either Proposer or Responder). Taken together, this yields five treatments: *Baseline*, P_{150}, R_{150}, P_{90} and R_{90}. An overview of all treatments and the resulting equity rules is given in Table 4.2. Our parameterization has the unique feature to clearly separate the distinct outcomes derived by the equity principle. In particular, we are able to investigate their relevance when one of the outside options is below half of the total amount of 240 (i.e., 90) or when one of the outside options exceeds half of the total amount of 240 (i.e., 150).

In order to examine the application of equity rules between and within subjects, we employ the following experimental protocol. Each subject participates in a sequence of two different treatments interacting with two different counterparts (perfect stranger-matching protocol) without feedback on the counterpart's decision between treatments. We balance the order of the treatments to be able to control for order effects. Subjects are randomly assigned to the role of the proposer or the responder and they maintain their roles across treatments.[9]

In total, we employ twelve different sequences of two treatments (each of our four asymmetric treatments is combined with *Baseline* in both orders; in addition, we combine the asymmetric treatments P_{150} with R_{150} and P_{90} with R_{90} in four sequences to control for order). An overview of all sequences can be found in Table 4.10. Technically, in each sequence, two proposers and two responders form a matching group. Each participant interacts exactly once with each of the two participants with the other role.

Procedural Details

Our experimental sessions involved 280 subjects from the University of Bonn in Germany (51 % male, average age 24 years), who were recruited via the online recruiting system ORSEE (Greiner, 2003). Each participant took part in only one of the sessions. The experiment was programmed using the software

[9] We use a neutral language in the instructions and on the computer screens: Proposers are called "Player A" and responders are called "Player B" (see the appendix for a translation of the instructions). The original instructions were provided in German.

Table 4.2: Treatments and predictions of equity rules

Treatment	o_i	o_j	EQ a_i	EQ a_j	SD a_i	SD a_j	PS a_i	PS a_j
Baseline	30	30	120	120	120	120	120	120
P_{150}	150	30	120	120	180	60	200	40
R_{150}	30	150	120	120	60	180	40	200
P_{90}	90	30	120	120	150	90	180	60
R_{90}	30	90	120	120	90	150	60	180

Notes: o_i (o_j) represents the outside option of the proposer (responder). a_i (a_j) is the share the proposer (responder) gets as a result of the respective equity rule.

z-Tree (Fischbacher, 2007). Subjects' earnings were determined by summing up earnings in both treatments in which they participated. We applied an exchange rate of 1 point equal to 0.06€; thus, 240 points are worth 14.40€. On average, subjects earned 15.78€. At the beginning of the experiment, we handed out instructions and control questions to make sure that everyone understood the general rules of the games.

4.3 Hypothesis

Our main hypothesis is based on Selten's generalized equity principle. As shown, we can derive different reasonable distribution rules from this principle for our treatments. Previous studies mentioned in our literature review suggested that subjects tend to self-servingly apply different distribution rules depending on the respective situation (e.g., having a larger or a smaller outside option). This leads us to our primary research hypothesis:

Hypothesis: *Subjects apply the equity rules in a self-serving manner.*

With respect to our experiment, we expect players with a higher outside option to prefer either PS or SD over EQ as they yield higher payoffs. Players with a lower outside option are expected to opt for an egalitarian distribution (i.e., to prefer EQ over SD or PS).

Table 4.3: Average (standard deviations) offers, maos, and rejection rates pooled over all sequences

	offer		mao		rejection rate	
Treatment	average	(s.d.)	average	(s.d.)	average	(s.d.)
Baseline	103.20	(31.72)	93.25	(38.63)	.31	(.46)
P_{150}	56.4	(27.55)	64.35	(26.31)	.57	(.50)
R_{150}	140.48	(32.37)	153.80	(23.44)	.52	(.50)
P_{90}	85.37	(31.04)	82.13	(35.38)	.37	(.49)
R_{90}	116.71	(19.41)	104.24	(23.19)	.21	(.42)

Notes: s.d. = standard deviation. When analyzing average offers and mao, we find that offers (mao) are higher the larger the outside option of the responder. Analogously, offers (mao) are lower the larger the outside option of the proposer (see Table 4.10 and the regression analyses provided in Table 4.9 in the appendix; Figure 4.2 and Figure 4.3 show distributions of decisions for each treatment).

4.4 Results

This section is structured according to our main research question: How does the presence of outside options influence the employment of different equity rules? As the generalized equity principle provides precise point predictions, we first focus on the number of proposers' offers and responders' mao that are in line with one of the three equity rules. Thereby we go beyond previous studies which mainly focused on average behaviors (we display the average offers, maos and rejection rates of our treatments in Table 4.3 which confirm the findings from the literature).

In order to investigate the relevance of equity rules, we count offers and maos that can be predicted by one of the equity rules proposed by Selten. We apply a strict point prediction classification to categorize subjects' decisions: We assume that a subject applies an equity rule if and only if she chooses the exact distribution suggested by this rule. The total number of exact hits relative to all decisions in a treatment will be denoted as the hitrate for a specific rule.

We test our hypothesis as follows: First, we separately compare the behavior of proposers and responders in treatments where the size of the larger outside option is the same but the holder in one treatment is the proposer and in one treatment is the responder. For example, we compare hitrates of a certain equity rule in P_{90} and R_{90} for proposers and responders. As previously described, each subject took part in two treatments with two different outside option constellations. We arranged the sequences of these treatments such that we can compare the usage of equity rules between and within subjects. Therefore, for each pairwise treatment comparison, we will present results from a between and a within subjects perspective.[10]

Our analysis of the generalized equity principle starts with the proposers' offers followed by an assessment of responders' mao. We then discuss efficiency.

Offers and the Generalized Equity Principle

To obtain a first estimate how equity rules influence offers, we pool all offers from all treatments and calculate the hitrate. A fraction of .43 of all offers are equitable offers in line with Selten's principle.[11] This number appears to be remarkably high given our strict point prediction rule.

In *Baseline*, where outside options are of equal size, .51 of the subjects offer EQ. This does not come as a surprise because, as stated in section 4.1, all three equity rules suggest the same offer when outside options are symmetric.

The analysis of proposers' behavior in treatments with asymmetric outside options leads us to our first observation concerning the use of equity rules:

> ***Observation 1:*** *Proposers frequently apply the generalized equity principle. They do so in a self-serving way. Proportional splits are offered more often when proposers have the larger outside option. To the contrary, they rely more often on equal splits when the responder has the larger outside option.*

Support for this observation can be found in Figure 4.1, which shows hitrates for each equity rule averaged over each of the four treatments with asymmetric

[10] We use Fisher exact tests for the between subjects comparison and McNemar change tests for the within subject analysis. If not mentioned otherwise, as we have formulated a directed hypothesis, all statistical tests are carried out one-sided. As we find no systematic evidence that offers and mao are affected by the respective sequence, we merge sequences that contain the same two treatments (e.g. P_{150}, *Baseline* and *Baseline*, P_{150}). A battery of 24 Mann Whitney U tests pairwise comparing the distribution of offers and mao of the same treatment between different sequences yields no systematic significant differences (see Table 4.10 in the appendix).

[11] This figure splits up for each treatment as follows: *Baseline* .51, P_{150}: .46, R_{150}: .24, P_{90}: .50, R_{90}: .37. As a comparison, the hitrate for the sub-game perfect equilibrium (SP) is .06 (*Baseline*: .03, P_{150}: .04, R_{150}: .26, P_{90}: .02, R_{90}: .04) of all offers.

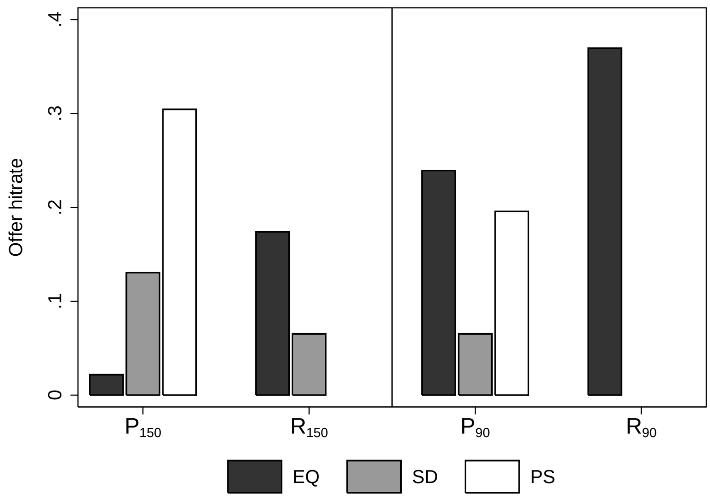


Figure 4.1: Offer hitrate for equity rules (pooled for all sequences)

outside options. It can be seen that PS explains offers particularly well, when the proposer has the larger outside option. When the responder has the larger outside option, EQ is most prevalent in offers.

In Table 4.4, we depict the relative frequencies of exact hits for each equity rule per treatment. The left panel shows the data for sequences that contain one asymmetric treatment and *Baseline.* Comparing the hitrates between subjects, we find that PS is much more prevalent when proposers have the larger outside option (p=.005 for P_{150} vs. R_{150}; p=.011, for P_{90} vs. R_{90}). This tendency gets further support by a within-subject comparison. Subjects significantly more often offer PS when they have a larger outside option of 150 (p=.001, for P_{150} vs. R_{150}).[12] For EQ and SD, we observe that fractions point into the hypothesized direction, namely EQ is more frequent when the responder has the larger outside option whereas SD is more prevalent when the proposer has the larger outside option. However, we find only mild statistical evidence comparing P_{150} and R_{150} (p=.094) for EQ.

So far, we have only focused on subjects using one of the three equity rules. How do the other subjects react to the outside option asymmetry? To capture the behavior of all subjects, we now go one step further and relax the strict

[12]Interestingly, two proposers in P_{150} make an offer that represents a *deal me out* solution (Anbarci and Feltovich, 2013), i.e., the proposer keeps her outside option (150 points) and offers the remainder to the responder (90 points). Note that this sharing rule is not captured with the generalized equity principle.

Table 4.4: Hitrate of equity rules

	Between Subjects			Within Subjects		
Treatment	EQ	SD	PS	EQ	SD	PS
Baseline	.51					
P_{150}	.04	.17	.29	0	.10	.32
	$<^{*}$		$>^{***}$			$>^{***}$
R_{150}	.21	.13	0	.14	0	0
P_{90}	.17	.08	.25	.32	.05	.13
			$>^{**}$			
R_{90}	.33	0	0	.40	0	0

Notes: The left panel shows the hitrate of equity rules from sequences that contained one asymmetric and one *Baseline* treatment. The right panel shows the hitrate for sequences with two asymmetric treatments. Stars display significance levels of comparisons of two asymmetric treatments. Significance levels: *** $p < .01$,** $p < .05$, * $p < .1$.

point prediction rule and broaden our classification strategy. For each proposer's offer, we measure the absolute distance to all three equity rules. Offers are classified according to the smallest distance to one of these rules. For example, we classify a proposer's offer as EQ when, among the three distances, the distance to EQ is the smallest. The figures support the results from our very conservative approach, making them even stronger.[13]

Using this measure as a dependent variable, we estimate simple linear probability models to further verify our results of observation 1. In Table 4.5, we predict whether a proposer offers EQ, SD or PS on a treatment dummy while controlling for the sequence of treatments. Confirming the results of our non-parametric analysis, we find that PS in P_{150} and P_{90} is significantly more prevalent compared to R_{150} and R_{90}, respectively.

In light of the hypothesized self-serving use of equity rules, the clear self-serving pattern of PS choices in P_{150} vs. R_{150} and P_{90} vs. R_{90} confirms our main research hypothesis: On the one hand, from the set of our three equity rules the PS rule yields the highest payoff for the player with the higher outside option. On the other hand, for the player with the smaller outside option, EQ yields the highest payoff, which is partially confirmed by our results.

[13]The results of this classification are shown in Table 4.11 in the appendix.

Table 4.5: Predicting hitrates of equity rules

	Dependent variable					
	EQ	SD	PS	EQ	SD	PS
	(1)	(2)	(3)	(4)	(5)	(6)
Independent variables						
1 if P_{150}	-0.136*	0.091	0.318***			
	(0.07)	(0.06)	(0.10)			
1 if P_{90}				-0.091	0.045	0.136*
				(0.09)	(0.05)	(0.07)
Constant	0.136*	-0.008	0.015	0.174	0.121	0.030
	(0.07)	(0.10)	(0.19)	(0.12)	(0.11)	(0.12)
Observations	92	92	92	92	92	92
R^2	0.087	0.068	0.189	0.062	0.103	0.149
Sequence control	yes	yes	yes	yes	yes	yes

Notes: Linear probability models (OLS) with robust standard errors (in parentheses) clustered on matching groups. The dependent variable EQ takes the value 1 if the offer is closer to the EQ prediction than to the prediction of the other two equity rules and 0 otherwise. Dependent variables SD and PS are constructed analogously. Each specification includes dummies for the specific sequence. Reference group (Constant) for (1)-(3) R_{150}, for (4)-(6) R_{90}. Significance levels: *** $p < .01$,** $p < .05$, * $p < .1$.

Thus far, we have focused on proposers' behavior in ultimatum games with asymmetric outside options. We find that 1) the equity principle is frequently used and 2) it is applied self-servingly. In the subsequent paragraphs, we shed light on responders' behavior to determine how different equity rules influence responders' minimum acceptable offers.

Rejections and the Generalized Equity Principle

Employing again a strict point prediction rule, a fraction of .26 of all maos can be classified as representing at least one of the three equity rules.[14] This fraction is somewhat lower than found in proposers' choices. An explanation for this difference might be the simultaneous nature of our ultimatum game. Responders might (strategically) anticipate proposers' equity considerations and determine their maos such that it also represents an equitable outcome (e.g., the lowest possible offer representing an equity rule). Moreover, since responders cannot be sure about the proposer's choice, they might be willing - based on their selected mao - to make small concessions in order not to 'accidentally' reject the expected offer. This especially holds if responders be-

[14]In comparison .2 of maos are decisions in line with the subgame perfect equilibrium (SP).

Table 4.6: Hypothetical rejection rates for equity rules

	Between Subjects			Within Subjects		
Treatment	EQ	SD	PS	EQ	SD	PS
Baseline	.07					
P_{150}	0	.46	.67	0	.55	.82
	$<^{***}$	$>^{***}$	$>^{***}$	$<^{***}$	$>^{***}$	$>^{***}$
R_{150}	.83	.08	0	.82	.05	0
P_{90}	.04	.42	.63	0	.50	.68
		$>^{***}$	$>^{***}$		$>^{***}$	$>^{***}$
R_{90}	.13	.04	0	.05	0	0

Notes: The left panel shows the rejection of equity rules from sequences that contained one asymmetric and one *Baseline* treatment. The right panel shows the results for sequences with two asymmetric treatments. Stars display significance levels of comparisons of two asymmetric treatments. Significance levels: *** $p < .01$,** $p < .05$, * $p < .1$.

lieve the proposers to exploit their bargaining power, i.e., to further minimally lower their equity-based offer. Because we count only exact hits of equity rules such strategies might not be captured by our above percentage representing classifications according to the equity principle.

For a more informative analysis of responders' behavior, in the following we consider hypothetical rejection rates.[15] Hypothetical rejection rates of an equity rule in a treatment are calculated by matching the point prediction of an equity rule with every mao made in this treatment and determining the frequencies of rejections. The results are displayed in Table 4.6. The left panel shows between-subject comparisons and the right panel within-subject comparisons. Statistical tests lead to our second observation:

[15]We chose this procedure because it is much harder to infer a specific preference for an equity rule from responders' maos. A simple example might highlight this: Consider a responder in P_{150} who sets her mao to 40. According to our strict point prediction rule she would be classified as PS. However, by setting this mao she does not exclude the two other equity rules as their prediction would also be accepted with this mao. Thus, one cannot easily categorize maos using a strict point prediction rule.

Observation 2: *Responders are more likely to reject PS and SD offers when proposers have the larger outside option. They reject EQ offers more often in R_{150}.*

Responders tend to reject PS and SD more often, when the proposer has the larger outside option whereas EQ is rejected more often when the responder has an outside option of 150.[16]

Statistical evidence is shown in Table 4.6. Responders are significantly more likely to reject PS and SD offers when proposers have the larger outside option. This holds for all comparisons of SD and PS for within and between-subject comparisons (all p<.004). A rejection of EQ is more likely when the outside option of the responder equals 150 (p<.0001). We find no statistical evidence that responders are more likely to reject EQ when they have an outside option of 90.[17]

Pooling all data from the two treatments with the same larger outside option in a regression analysis and controlling for the sequence of treatments confirms the results (see Table 4.7). SD and, in particular (as the coefficients and the R^2 of the respective models reveal), PS offers are more likely to be rejected by responders when proposers have the larger outside option.

Efficiency and Profits

We conclude our results section by investigating the impact of different outside option schemes and equity rules on efficiency and players' profits. More specifically, two questions are considered: How do outside options affect efficiency? Is it profitable for an individual player to have a (specific) outside option?

In our setup, efficiency can only differ across treatments due to cases of rejection as the amount distributed in the case of agreement is 240. Because the agreement amount always exceeds the sum of outside options, reaching an agreement is always efficient.

In our analysis, we calculate average relative efficiency gains and average relative additional profits. Average relative efficiency gains are calculated as the absolute efficiency gains (i.e., the amount generated in addition to the

[16]Recall responder's payoff would be 120 in case of accepting EQ.

[17]A corresponding analysis of the subgame perfect division yields that responders are more willing to reject this distribution when they have the lower outside option.

Table 4.7: Predicting rejections of equity rules

	Dependent variable					
	EQ	SD	PS	EQ	SD	PS
	(1)	(2)	(3)	(4)	(5)	(6)
Independent variables						
1 if P_{150}	-0.818***	0.500***	0.818***			
	(0.10)	(0.10)	(0.08)			
1 if P_{90}				-0.045	0.500***	0.682***
				(0.05)	(0.10)	(0.08)
Constant	0.818***	0.083	-0.068	0.129	0.000	0.068
	(0.10)	(0.18)	(0.13)	(0.09)	(0.20)	(0.18)
Observations	92	92	92	92	92	92
R^2	0.707	0.300	0.627	0.132	0.284	0.507
Sequence control	yes	yes	yes	yes	yes	yes

Notes: Linear probability models (OLS) with robust standard errors (in parentheses) clustered on matching groups. The dependent variable EQ is 1 if the mao rejects the offer predicted by EQ and 0 otherwise. Dependent variables SD and PS are constructed analogously. Each specification includes dummies for the specific sequence. Reference group (Constant) for (1)-(3) R_{150}, for (4)-(6) R_{90}. Significance levels: *** $p < .01$,** $p < .05$, * $p < .1$.

sum of outside options) in relation to the maximally possible efficiency gain (i.e., 240 minus the sum of outside options). Analogously, average relative additional profits are calculated as achieved profits additional to the outside options in relation to the maximally possible additional profits (i.e., 240 minus the players' outside option). Using these measures we account for the fact that the possible absolute efficiency gains and absolute additional profits differ across treatments.[18]

Observation 3: *Average relative efficiency gains are lower in* P_{150} *and* R_{150} *compared to Baseline.*

The left panel of Table 4.8 shows average relative efficiency gains for matching groups. In *Baseline,* the average relative efficiency gain is .69; however, this measure is .42 in both P_{150} and R_{150} (p=.0088, p=.0143, Wilcoxon signed rank test for matched pairs, henceforth WSR test, two-sided). In P_{90} and R_{90} we observe no significant difference in average relative efficiency gains compared to *Baseline.*[19]

[18] For absolute values on these measures see Table 4.12.

[19] For this comparison, we focus on the data from sequences where matching groups went through a sequence of *Baseline* and the respective asymmetric treatment.

Table 4.8: Average relative efficiency gains & profits

Treatment	Avg. relative efficiency gain	Avg. rel. add. profits Proposer	Responder
Baseline	.69	.31	.17
P_{150}	.42***	.05***	.03***
R_{150}	.42**	.09***	-.07***
P_{90}	.58	.24	.08
R_{90}	.71	.29	.06**

Notes: The left panel displays average values of relative efficiency gains. The right panel depicts the relative additional profits realized by both player types. Average values are based on matching group-level data and from sequences that involve *Baseline* and one asymmetric treatment in order to make appropriate non-parametric comparisons. Including data from all sequences does not change average values substantially.
Stars display the significance levels of a Wilcoxon signed rank test for matched pairs comparing results from the *Baseline* against the respective asymmetric treatment. Significance levels: *** $p < .01$,** $p < .05$, * $p < .1$.

Observation 4: *Average relative additional profits are lower in P_{150} and R_{150} compared to Baseline.*

The right panel of Table 4.8 displays average relative increases in profits for both players. In *Baseline,* proposers (responders) realize additional profits of .31 (.17). Having an outside option of 150 induces an increase in additional profits of only .07 (-.06) for proposers (responders). Compared to *Baseline,* these amounts are significantly lower (both $p<.01$, WSR test). In R_{90} we find a similar tendency (.04, $p=.0223$, WSR test). In P_{90}, the relative additional profits are not statistically different from *Baseline* ($p=.4222$, WSR test).

4.5 Concluding remarks

Although outside options are a key element in many bargaining situations investigations focusing on outside options have been relatively scarce in the economics literature. Especially, situations in which parties have different outside options lack systematic evidence which is addressed in this paper. We provide systematic and controlled evidence that different equity notions in ultimatum bargaining situations with asymmetric outside options are deeply rooted in behavior. We find that asymmetric outside option constellations make it harder for bargainers to reach an agreement and extend the existing literature by tracing different notions of what participants consider to be suitable allocations based on their outside options. By employing the generalized equity principle (Selten, 1978), we identify three different equity rules that we make clearly distinguishable by our experimental design. We find clear evidence that proposers' offers are in line with these simple equity rules - taken all games together, we find that 43% of all offer decisions precisely follow the generalized equity principle. The high number of proposers who try to solve the asymmetric outside option bargaining by implementing an equitable outcome is remarkable.

However, using our experimental design, we are also able to show that equity rules are not applied in a consistent manner, but rather self-servingly. More specifically, proposers are inclined to offer proportional splits when these serve their own interests. Yet, inconsistently, proposers, tend to offer equal distributions when responders would benefit from a proportional split. At the same time, we observe that responders are reluctant to accept proportional divisions when they are to their disadvantage in comparison to an also feasible equal split. Responders tend to accept proportional distributions only when they benefit them. In sum, equity rules seem to be attractive for guiding behavior by adhering (or maybe pretending to adhere) to some equity considerations. However, equity rules are rather chosen in a self-serving manner. This inconsistent application of equity rules and its conflict-enhancing effect might well be the reason for the low efficiency gains when bargainers have asymmetric outside options.

Our results underscore and extend the general validity of models of inequity aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). These models rely on an understanding of what constitutes an equitable outcome. Based on this reference point, individuals evaluate inequity which influences their utility. However, as our study demonstrated, there is not a unique reference point of equity but several potential candidates might be relevant. In this spirit, models of inequity aversion can be applied to each of the three previously discussed equity rules. We find, that their adoption as equity rules might indeed depend on the individual perspective and appears to be quite self-serving. Thus, our results highlight that there might be a need to consider different reference points for different parties involved when applying models of inequity aversion.[20]

Our findings also contribute to the literature on self-servingly biased behavior. Up to now studies on self-serving behavior were mainly able to show that individuals demand more when they feel entitled to do so. The results of our studies enlightens what exactly subjects think of what they consider to be fair from different perspectives when they are self-servingly biased, namely proportionality or equality.

Moreover, our results confirm existing doubts about a clear predominance of one specific fairness rule which is often suggested by normative models of distributive justice (Gärtner and Schokkaert, 2012). In our experiment, we employ outside options as a rather self-evident and exogenously provided standard of comparison. In bargaining situations outside the laboratory, however, it is quite often the case that a plethora of standards of comparison are available, such as in the negotiation about the manager's compensation discussed in the introduction. When the manager and the potential employer bargain over the split of the profit, outside options might not be the only reasonable standard of comparison; the efforts and investments of both parties might also contribute in the future. Likewise, in a merger between two companies, the standard of comparison for the distribution of future gains could be based on other factors than outside options, such as the pre-merger market share or the invested amounts.

In light of our results on the self-serving usage of equity rules, one might think that bargaining parties will strive not only for the equity rule most beneficial for them but rather for a standard of comparison that leads to a justifiable (self-serving) distribution. Therefore, we consider our results to be at the lower bound for self-serving behavior. The room for disagreement in bargaining potentially available outside the laboratory might be larger because the standards of comparison are likely to be less self-evident in the field. Moreover, outside options might not always be randomly assigned as in our experiment but could

[20]In this respect, our findings are in line with the literature on the 'moral wiggle room' (Dana et al., 2007), in the sense that bargaining parties have some freedom to strategically select the 'right' equity rule. It is also related to the concept of 'bounded ethicality' (Chugh et al., 2005; Bazerman and Tenbrunsel, 2011b) as subjects might actually be convinced that their adopted equity rule is actually the 'right' one while ignoring that other parties might adopt a different one.

be costly to acquire. It might well be that the relevance of our three equity rules is more pronounced if outside options are earned.

Future research needs to explore how the observed imbalances in the application of equity rules might be mitigated by, for example, explicitly taking the perspective of the other negotiator or investigating other procedures to harmonize the notions of equity (Bhatt and Camerer, 2005; Costa-Gomez and Crawford, 2006). One step into this direction might be research on why equity rules are adopted: Are they primarily employed because of self-image concerns (i.e., being a fair person) or because of the (maybe unwarranted) hope that the opponent in the negotiation might be more ready to agree if an equity norm is applied?

4.6 Appendix

Table and Figures

Table 4.9: Estimating players' decisions

	Dependent variable		
	offer	mao	reject
Independent variables	(1)	(2)	(3)
P_{150}	-55.81***	-30.45***	0.394***
	(6.28)	(5.07)	(0.12)
R_{150}	34.02***	58.92***	0.283**
	(6.53)	(4.96)	(0.12)
P_{90}	-15.78**	-8.618*	0.128
	(7.16)	(5.03)	(0.12)
R_{90}	14.37***	12.39***	-0.0694
	(4.95)	(3.89)	(0.12)
Constant	108.3***	93.13***	
	(6.22)	(3.77)	
Observations	280	280	280
R^2	.465	.43	.109
Sequence control	yes	yes	yes
p-Values (Wald-test)			
$H_0 : \beta_{P_{150}} = \beta_{P_{90}}$	.0001	.0001	.3334
$H_0 : \beta_{R_{150}} = \beta_{R_{90}}$	.0001	.0001	.1210

Notes: Models (1) and (2) display the results of a GLS regression with random effects. Robust standard errors are shown in parentheses. Model (1) contains proposers' offer, while model (2) contains the data from responders. Model (3) is a probit estimation. The reference category is *Baseline* (Constant). In the lower panel we display *p*-Values of a Wald-test comparing the size of coefficents for different treatments with asymmetric outside options.

Table 4.10: Overview of sequences and results

Sequence	Order of treatments 1st	Order of treatments 2nd	Average offer Asym		Average offer *Baseline*	Average mao Asym		Average mao *Baseline*
1	P_{150}	*Baseline*	50.83	$<^{***}$	110.00	68.00	$<^{***}$	108.25
2	*Baseline*	P_{150}	62.92	$<^{***}$	115.41	54.00	$<^{**}$	87.58
3	R_{150}	*Baseline*	125.00	$>^{***}$	95.00	152.08	$>^{***}$	92.58
4	*Baseline*	R_{150}	146.24	$>^{**}$	108.33	153.00	$>^{***}$	100.00
5	P_{90}	*Baseline*	78.00	$<^{***}$	106.67	96.25^{+}	$<$	102.75
6	*Baseline*	P_{90}	82.58	$<$	93.42	70.08	$<$	73.92
7	R_{90}	*Baseline*	117.08	$>^{***}$	92.50	111.67	$>$	99.92
8	*Baseline*	R_{90}	116.33	$>$	104.25	106.75	$>^{***}$	81.00
			P_{o_i}		R_{o_j}	P_{o_i}		R_{o_j}
9	P_{150}	R_{150}	57.00	$<^{***}$	140.92	77.50^{+}	$<^{***}$	154.58
10	R_{150}	P_{150}	54.00	$<^{***}$	151.40	56.60	$<^{***}$	155.90
11	P_{90}	R_{90}	93.33	$<^{***}$	120.83	84.33	$<^{*}$	99.42
12	R_{90}	P_{90}	88.00	$<^{***}$	111.80	77.00	$<^{*}$	98.10
(Pooled)	*Baseline*			103.20			93.25	
(Pooled)	P_{150}			56.40			64.35	
(Pooled)	R_{150}			140.48			153.80	
(Pooled)	P_{90}			85.37			82.13	
(Pooled)	R_{90}			116.71			104.24	

Notes: The number of subjects for every sequence except 10 and 12 is 24. In these sequences we have 20 subjects. Thus for *Baseline* we have 192 subjects and 92 for each treatment with asymmetric outside options in total. Stars display significance levels of a Wilcoxon signed-rank test for matched pairs comparing the distribution of decisions in the same sequence between different treatments. Significance levels: *** $p < .01$,** $p < .05$, * $p < .1$. Plus signs display the significance levels of a Mann-Whitney U test comparing the distribution of decisions in the same treatment between different sequences. Significance levels: $^{+}$ $p < .1$.

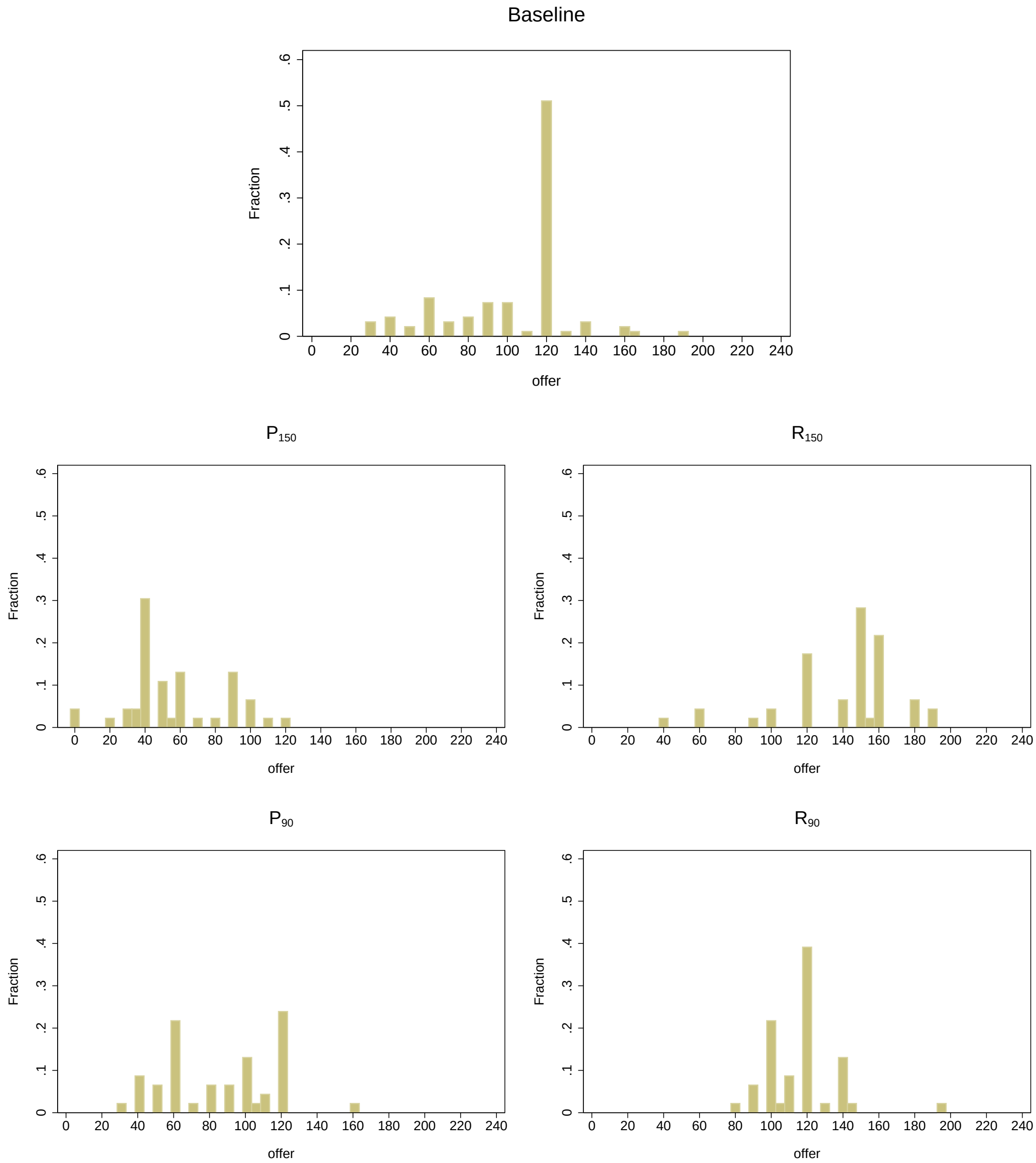


Figure 4.2: Distribution of offers in treatments (pooled over all sequences)

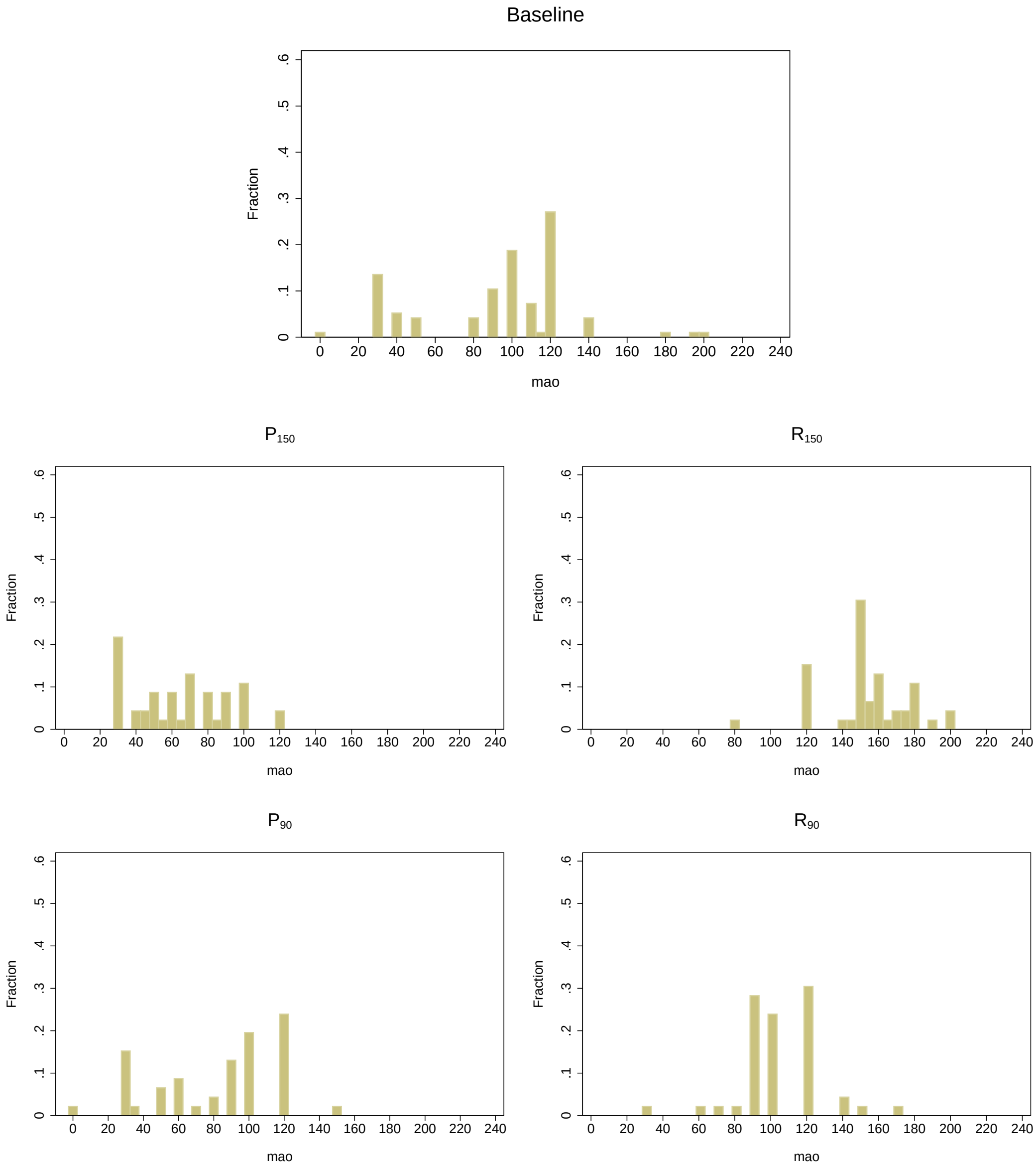


Figure 4.3: Distribution of mao in treatments (pooled over all sequences)

Table 4.11: Relaxed hitrate of equity rules (proposer)

	Between Subjects			Within Subjects		
Treatment	EQ	SD	PS	EQ	SD	PS
Baseline	1	/	/	/	/	/
P_{150}	.21	.38	.42	.27*	.23	.5
	$<^{***}$		$>^{***}$	$<^{*}$	$<^{**}$	$>^{***}$
R_{150}	.58	.42	0	.45	.55	0
P_{90}	.29	.21	.50	.36	.32	.32***
	$<^{***}$		$>^{***}$	$<^{***}$	$>^{*}$	$>^{***}$
R_{90}	.79	.17	0	.86	.14	0

Notes: The left panel shows the hitrate of equity rules from sequences that contained one asymmetric and one *Baseline* treatment. On the right we show the hitrate for sequences with two asymmetric treatments. For between (within) subjects comparisons stars display the results of one-sided Fisher (McN) test. p-Values (one-sided) in brackets. Significance levels: *** $p < .01$,** $p < .05$, * $p < .1$.

Table 4.12: Average absolute efficiency gains and profits

Treatment	Avg. abs. efficiency gain	Avg. abs. add. profits Proposer	Responder
Baseline	123.75	95.57	66.30
P_{150}	25.00***	154.58***	35.33***
R_{150}	25.00***	49.71***	143.54***
P_{90}	70.00**	126.00*	48.58
R_{90}	85.00*	90.79	98.42***

Notes: The left panel displays average values of absolute efficiency gains. The right panel depicts the absolute additional profits realized by both player types. Average values are based on matching group-level data and from sequences that involve *Baseline* and one asymmetric treatment in order to make appropriate non-parametric comparisons. Including data from all sequences does not change average values substantially.
Stars display the significance levels of a Wilcoxon signed rank test for matched pairs comparing results from the *Baseline* against the respective asymmetric treatment. Significance levels: *** $p < .01$,** $p < .05$, * $p < .1$.

Experimental instructions (translated from German)

Welcome to the experiment!

You are participating in an economic experiment and you have the possibility to earn a certain amount of money, which varies according to your decisions. Please read thoroughly the following descriptions.
During the experiment we will talk about “Taler” and not €. Hence, your payout will be initially calculated in “Taler”. The achieved total amount of money of “Taler” will be converted into € at the end of the experiment and then we will give you a cash payout, whereas

10 Taler = 0,6 €

holds true.
The decisions in the experiment
At the beginning of the experiment all participants have been randomly divided into two groups – **players in the role of A and players in the role of B** – which will interact with each other during the experiment. You will get to know neither before nor after the experiment **with whom you are interacting**. At the beginning of the experiment you will be informed of whether you are player A or B which was determined **randomly** by drawing the cabin number.
The experiment is about **splitting 240 “Taler” among player A and B**. **Player A** makes a proposal of **how to split the 240 “Taler”** among player A and player B. Player B decides **from which amount** of money he is willing to accept the proposal of player A. After both players have made their decisions, the decisions will be compared.
If the proposal of allocation of player A is in the **area of acceptance** of player B, then

- the 240 “Taler” will be split in accordance to the decisions.

If the proposal of allocation of player A is **not in the area of acceptance** of player B, then

- player A and player B will each get a guaranteed amount of money, which can be identical or different for player A and player B. Both player A and player B know the two guaranteed money amounts before the decisions are made.

Every player A interacts in **two different, sequent games** with two **different** players B. Every player B interacts in two **different, sequent games** with two **different** players A.
If you are player A you will see this screen:

Game 1/Player A
Please note: In game 1 and game 2 you are interacting with **different** players B.
Please make a proposal of how to split the **240 "Taler"** among you and player B.
Guaranteed amount of money for **yourself**, in the case of a rejection of player B: □
Guaranteed amount of money for **player B**, in the case of an acceptance of player B: □
The proposed amount of money for **yourself**: □
This implies: The proposed amount of money for **player B**: □

The decisions of player A and player B are made **simultaneously**. This implies for player B that he makes his decision **before knowing** which proposal player A will actually make.

Game 1/Player B
Please note: In game 1 and game 2 you are interacting with **different** players A.
Player A will make a proposal of how to split the **240 "Taler"** among you and player A.
Please decide from which amount of money you are willing to accept the proposal of player A.
Guaranteed amount of money for **player A**, in the case of your rejection of the proposal: □
Guaranteed amount of money for **yourself**, in the case of your acceptance of the proposal: □
The **lowest** amount of money **you** are willing to accept: □
This implies: The **highest** amount of money for **player A** you are willing to accept: □

If the proposed amount of money of player A for player B is **greater than or equal** to the lowest amount of money player B is willing to accept, then the proposal **will**

be accepted. Vice versa the **proposal** of player A will be **rejected**, if the proposed amount of money of player A is **smaller** than the lowest amount of money player B is willing to accept.

Before the experiment starts we would like you to answer a couple of control questions. These questions will help you familiarize with the decision situation. **At the end** of the experiment we would like you to answer some further questions.

In the course of the experiment any form of communication with the other participants is forbidden. Please read now once again the instructions thoroughly to make sure that you understood everything. If there are any **uncertainties** left, **please put your hand up**. We will then come to you and answer your questions.

5 The Influence of Framing and Publicity on Moralistic Punishment

The question of how punishment lead to adherence to social norms is central to the social sciences. Economists usually view sanctions through the lens of deterrence, i.e., the threat of punishment makes socially undesirable behavior less attractive and thus fosters norm compliance (Becker, 1968). Yet, costly punishment is only beneficial to individuals who can expect future monetary gains from it. Punishment, however, may have different manifestations. On the one hand, in second party punishment (or peer-to-peer punishment) where victims of norm deviance can discipline norm violators in order to extract future gains from them. For example, when cooperators in repeated prisoners dilemmas punish defectors in order to convince them to cooperate in the subsequent periods (Fehr and Gächter, 2000). On the other hand, in third-party altruistic punishment or moralistic punishment where observers of norm violations, who are not monetarily affected, can punish violators without expecting future rewards from them.[1]

A stream of studies has investigated the borderline conditions for peer-to-peer punishment to enhance cooperation (e.g., Gächter et al., 2008; Gürerk et al., 2006), trust (e.g., Fehr and Gächter, 2002; Fehr and Rockenbach, 2003), and fairness (e.g., Hoffman et al., 1994) in strategic interactions.[2] In contrast to this literature on peer-to-peer punishment it pertains to be a yet unresolved question when and why also not affected individuals punish (e.g., Lergetporer

[1] Other researchers call it *third-party punishment* (Lergetporer et al., 2014), *altruistic punishment* (Fehr and Gächter, 2002) or *moralistic punishment* (Kurzban et al., 2007). Since there seems to be no consensus on the use of one of these terms, we will stick to the term moralistic punishment throughout the paper.

[2] See also Chaudhuri (2011) for an excellent review of how second-party punishment influences cooperation.

et al., 2014; Carpenter and Matthews, 2012) the violation of norms.[3]

In this paper, we use a simple experiment to address two questions regarding moralistic punishment of unfair behavior. The first question pertains to the framing of norm violations. Will the framing of unfair behavior, as either not giving money to or taking money from - although the monetary consequences are the same - a helpless victim affect moralistic punishment? There is ample evidence that framing influences behavior in simple lottery choices (e.g., Kahneman and Tversky, 1979). How framing determines punishment behavior in strategic interactions is still not well understood.[4]

Our second research question seeks to illuminate whether publicity impacts moralistic punishment. We try to shed light on whether third parties punish differently depending on whether the victim of unfair behavior is informed about the punishment of the third-party or not. Will their behavior be affected when her action is made public to the victim, i.e., in a situation where the recipient is informed about the action compared to a situation where the action remains private and the recipient is not informed. A growing research stream in economics explores the effects of social and self-image concerns for decision making (Andreoni and Bernheim, 2009; Lacetera and Macis, 2010; Friederichsen and Engelmann, 2014). In our case, an observer who punishes when the victim will be informed about his action has at least two motives for punishing the dictator. For example, to restore justice, i.e., and equalize the final payoffs, but also to appear in good light towards the victim, i.e., receive publicity. We argue that publicity is a main driver of moralistic punishment and hypothesize that punishment will diminish when the victim is not informed about others punishment action.

We study these questions using a controlled laboratory experiment that employs a modified version of the dictator game with moralistic punishment (Fehr and Fischbacher, 2004). The dictator can decide upon a payoff distribution between herself and an inactive recipient. A third-party - the observer - sees the dictator's actions and can reduce the dictator's payoff at her own expense.

[3]Interestingly, while third-party punishment in humans seems to be a robust phenomenon, the evidence of third-party sanctioning behavior among other species, i.e. chimpanzees, seems to be mixed (Von Rohr et al., 2012; Riedl et al., 2012).

[4]In fact, there is now a growing number of papers on framing in public goods games (e.g. McCusker and Carnevale, 1995; Andreoni, 1995; Cubitt et al., 2011; Dufwenberg et al., 2011; Tan and Xiao, 2014). Evidence on how contributions are affected by framing is inconclusive (Goerg and Walkowitz, 2010; Gächter et al., 2014).

To investigate the research questions regarding moralistic punishment we employ a 2x2 design. A dictator could either give a certain amount from his endowment of 100 talers to the recipient or take a certain amount from the recipients' endowment of 100 talers (*Give* vs. *Take*). The information about the observer's punishment decision is either shared with the recipient or not (*Public* vs. *Private*). The parametrization of our experiment allows us to compare moralistic punishment behavior in strategically equivalent situations. Moreover, the experimental approach has the advantage that we have strict control over who will be informed about the punishment choice, which is very hard to ensure in situations outside the laboratory. Our main variable of interest is the observers punishment decision. In addition, we are able to investigate whether dictators anticipate their punishment and behave differently depending on framing and publicity.

Although standard economic theory and theories of social preferences would predict no differences in punishment and dictator behavior across treatments, we find remarkable differences in our experiment. The main finding of this paper is that observers punish more when dicator's action is framed as giving compared to when her action is framed as taking. When recipients will not be informed about the punishment choice, i.e., observers cannot take credit for their moral act, no such framing effect can be observed. Moreover, we find that dictators seem to anticipate these effects and behave more generously and by this deflect punishment when the punishment decision is made public.

Our study contributes to the understanding of moralistic punishment, framing and publicity in two ways. First, to the best of our knowledge no study of framing effects on moralistic punishment in dictator games has been conducted. Although, framing seem to be relevant for individual decisions under risk, the relationship of punishment and framing is not well understood. The existing studies on framing social dilemmas has led to rather inconclusive results. Compared to a social dilemma our experimental paradigm is simple and might thus lead to a better understanding when and why framing affects behavior.

Second, our study contributes to the understanding of social image concerns. We show that when bystanders will not receive credit by victims of norm violations, their willingness to engage in ethical behavior diminishes. In contrast to the existing literature on social image concerns our paper points into the direction that these concerns might stem from whether individual can receive credit from those they helped.

We will start by discussing the existing results on moralistic punishment and framing and then describe the papers on publicity. After this literature review in Section 5.1. we describe our experimental design and hypotheses in Section 5.2. In Section 5.3 we report the results. In Section 5.5 we discuss potential applications and conclude.

5.1 Related literature

Our review of papers is structured around our research questions. We will first present papers that demonstrate how framing might influences behavior in the modified dictator game employed in the present study. Then we will discuss experiments dealing with publicity concerns.

Moralistic punishment as such seems to be an increasingly recognized topic in the economics domain. First, a stream of previous studies have shown the existence and investigated its motives. Bernhard et al. (2006). Fehr and Fischbacher (2004) use an experimental design that is similar to our set-up. Interestingly, they find that roughly 60% of observers are willing to punish dictators for very low amounts of dictator giving. The same paradigm has been used by Almenberg et al. (2011). In addition, the authors give observers the possibility to reward dictators for good behavior. They report altruistic punishment for low giving rates but also see a considerable fraction of observers rewarding generous dictators.[5] All of their treatments contain a dictator game in which dictators actions are framed as "giving".[6] Moralistic punishment of decisions on "taking" is studied in the baseline treatment of Balafoutas et al. (2013). In their experiment, the dictator and the recipient are given unequal endowments, whereby the recipient has a higher endowment than the dictator. The dictator can then decide to "take" something from the recipient's

[5] In a comparable attempt, Chavez and Bicchieri (2013) analyze third-party sanctioning and rewarding behavior in an ultimatum game and come up with similar conclusions: Third-parties, after having observed unfair results of an ultimatum game, punish unfair proposers and compensate unfairly treated responders.

[6] To study the effects of willful ignorance on third party punishment, Bartling et al. (2014) also use a modified version of the dictator game. In their experiment, the dictator is instructed to "decide how many points will be credited to himself and" (p. 34) the recipient. Punishment levels are comparable to the studies by Fehr and colleagues.

endowment. An observer is able to punish the dictator. Although parameters, framing and endowments are very different from the studies discussed above, it is noteworthy that in Balafoutas et al. (2013) also roughly 50% of observers punish dictators for taking money from the recipient. This means that although the vast majority of papers on moralistic punishment in dictator games framed as "giving" proof the existence of moralistic punishment, punishment for "taking" also occurs. It lacks, however, a systematic analysis which the present study aims to provide.[7]

How the presence of others and their observation of behavior influences individuals has been the subject of studies in the social-psychology literature. It has been shown that individuals have a general tendency to act more prosocial if others observe their behavior (e.g., Latane, 1970). In an economic laboratory experiment, Hoffman et al. (1994) show that in dictator games almost no money is allocated to the recipient if the experimenter never learns how much they allocate to the recipient. By varying the observability of dictators actions, Schram and Charness (2011) try to distinguish moral from social norms. In their experiment, they vary what other participants in the experiment know about a dictator's action. After the experiment is finished, the dictator's payoff was displayed on every participants' computer screen. Additionally, the dictator had to walk through the laboratory to pick up the money. This was contrasted with a treatment in which dictators' payoffs were private and anonymous. Schram and Charness find that, when payoffs are made public, dictators are much more generous compared to a situation where payoffs are private.

How the observability of one's punishment choice influences punishment is not well established. One notable exception is the paper by Kurzban et al.

[7]Although no study investigates the effect of framing on moralistic punishment, there have been attempts to investigate the effects of framing on dictator behavior. For example, across three studies, Dreber et al. (2013) find no effect of framing on dictator behavior. The authors vary not only the different labeling of dictators strategies ("give" vs. "take") but also labeling of games ("Giving game" vs. "Taking game"). Even with a remarkably high number of observations, they do not find any statistically significant framing effect on dictators' behavior.

In seminal papers, List (2007); Bardsley (2008) expand the action set of dictators in that dictators could also take money from the recipient. He shows that generosity decreases if dictators are given the opportunity to take money from the recipient. Although these studies seem related to the present approach is different, because we keep the dictators action set constant and solely vary framing of actions.

(2007), in which the authors let third-parties observe free-riding behavior in a two player prisoners dilemma game (Study 2), i.e., one subject chooses to cooperate while the other subject defects. Third-parties then could decide to reduce the payoff of a selfish individual at their own cost. The researchers vary the degree of observability of punishment in three treatments with the following method. In one treatment, they assured the subjects that the researchers were not able to follow their individual choices. Third-parties were endowed with money that they could spend on punishing the defector. Subjects were given two envelopes. For one envelope subjects were instructed to put in the money they wanted to keep, while for the other envelope they were supposed to put in the money they wanted to spend on punishment. These envelopes were then sealed. When subjects left the room, they came across a bin where they were instructed to deposit their envelope with all other participants' envelopes. This procedure ensured that the experimenter was not able to link the punishment choice to a subject's identity. In a second treatment, researchers increased the observability by letting subjects tell the experimenter their punishment choice. In the third treatment, subjects had to state their punishment choice in front of all other participants in the experiment. The main result is that punishment levels increase as the audience is widened.[8]

5.2 Experimental design

Treatments

The impact of framing and publicity on moralistic punishment is investigated by means of a modified version of the dictator game. Three players are involved: a dictator, an observer and a recipient.[9] The game has three stages. In the first stage of the game, the dictator chooses a certain amount of taler the recipient will earn in the end. In the second stage the observer can reduce the dictators payoff. A reduction of the dictators payoff is costly to the observer and deducted from the endowment. A reduction of three talers of the dictators payoff lowers the initial endowment of the observer by one taler etc.

[8] Interestingly, Xiao and Houser (2011) show that in a multi-period public goods game, punishment has no positive impact on cooperation when others cannot see the punishment choices.

[9] To avoid any effects from these role descriptions in the experiment the different roles are called player A (dictator), player B (observer) and player C (recipient).

In the third stage players receive their payoffs and information regarding the behavior of others.

We employ the following 2 (*Give* vs. *Take*) x 2 (*Public* vs. *Private*) factorial design. In the first dimension we vary how the decision of the dictator is framed. In the treatments with *Give* frame the game is presented in the following way: The dictator is endowed with 100 taler, and the recipient has an endowment of 0 taler. The dictator in the *Give* frame can then decide to "give" 0, 20 or 50 talers to the recipient. In the *Take* frame, the dictator has 0 taler as an endowment, while the recipient has 100 taler as an endowment. The dictator in the *Take* frame can then decide to take 100, 80 or 50 taler from the recipients endowment.[10] In all four treatments the observer has an endowment of 50 taler. The treatments vary the representation of the dictators' action but are strategically equivalent. For a dictator's strategy, the recipient's payoff is either 0, 20 or 50 taler, respectively.

In the second dimension we vary whether the recipient is informed about the punishment choice of the observer. In the *Public* treatment it is known to all players that the recipient will be informed about whether the observer has punished the dictator or not and also about the amount of assigned punishment. In the *Private* treatments this information is not given to the recipient. The information conditions are explicitly stated in the instructions and on the decisions screen before subjects make their choices in the experiment. When subjects made their choices a questionnaire on their beliefs about the other participants behavior followed. Belief elicitation was not incentivized.

Procedural details

All treatments have important features in common. In every treatment, all players receive a show-up fee of 2.50€, which is not part of the subject's endowment. In the instructions and on the decision screens, we avoid terms like "punishment" or "sanction" to describe the observers' action and instead refer to a "deduction" that changes the dictators payoffs. To make sure that the

[10] The original instructions were in German. For these we used either the term "geben" for the *Give* frame or "nehmen" for the *Take* frame. We restrict the choice set of the dictator for two reasons. Previous studies on dictator games observed that dictators can be classified principally in three different types: Completely selfish dictators, slightly selfish dictators and dictators that share the endowment equally. We rule out the possibility of transfers that exceed 50% of the endowment, because there is ample evidence that only a very small fraction of dictators gives more than that (Engel, 2011).

rules of the game have been understood, before starting the experiment, every subject had to answer three control questions on the experiment correctly. Moreover, subjects were never informed about the other subjects' identities and they interacted anonymously. Each subject participated in the experiment only once, i.e, the games were played one-shot. The study was conducted with the experimental computer software z-Tree (Fischbacher, 2007). For observers' punishment decisions, we implemented the strategy method (Selten, 1967) at the second stage: Observers indicated for all three strategies of the dictator the amount (in taler) by which she wants to reduce the dictators payoffs. This means that the observer had to decide before actually knowing the dictator's choice. This procedure has the advantage of gathering the complete action set of the observer. For example, if all dictators in our experiment chose the same recipients payoff we would lose the data on the observers' behavior for all other strategies.[11]

The experiment was conducted in June 2014 in the Cologne Laboratory of Experimental Research (CLER) with 276 (mean age=23.7 (SD=5.24); 63% female) participants majoring in different disciplines. In total, we conducted 11 sessions. Participants were invited using the online recruitment system ORSEE (Greiner, 2003). The experiment was followed by a post-experiment questionnaire. Earned talers were converted to Euro at the end of the experiment at a conversion rate of 10 taler to 1 Euro. The experiment took approximately one hour and participants earned on average 6.88€(minimum 2.50€, maximum 12.50€), including the show-up fee.

Theoretical considerations and behavioral hypotheses on framing and publicity

Although the goal of this paper is not to differentiate between competing economic theories, it is worth discussing some existing models and their predictions with regard to altruistic punishment.

Neither the standard economic model of narrow self-interest nor models of social preferences predict any differences in punishment between treatments. In fact, if we assume a solely self-interested observer, we would expect to observe

[11] Obviously, we are not able to discard objections that the strategy method might influence behavior in our experiment. However, a survey by Brandts and Charness (2011) demonstrates that the likelihood that the strategy method influences behavior in economic experiments is rather small.

no moralistic punishment due to its cost. Similar minded dictators would give nothing (in *Give*) and take everything (in *Take*), respectively. Moreover, only some models of social preferences are able to predict positive punishment rates, but these are silent about potential effects of framing or publicity.[12] As our null hypothesis we state:

> ***Hypothesis 1:*** Moralistic punishment is not influenced by framing and publicity.

Research in experimental economics and social psychology, however, has shown that the representation of a decision has an impact on the final decisions. Besides the standard economic approach, framing effects are usually explained with prospect theory (Kahneman and Tversky, 1979), which assumes that individuals evaluate a decision problem relative to a reference point. Outcomes below the reference point are perceived as losses, while outcomes above the reference point are perceived as gains. According to prospect theory, individuals experience a larger disutility from a loss and engage in more risk loving compared to the utility derived from a gain of the same magnitude and consequently engage in risk averse behavior. These effects of framing on behavior in individual lottery choices are well established. In strategic interactions, such as the dictator game with altruistic punishment, it is not clear what prospect theory would predict. Although past research has shown that subjects carry a preference for the payoff of others (e.g. Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000) it is unclear whether observers take the losses of others more into account than similar sized gains of others. Second, it is

[12]In the Fehr and Schmidt (1999) model (FS) an observer's utility is positively influenced by his own payoff but can be negatively influenced if payoffs of other actors are larger (disadvantageous inequality), and if payoffs of other actors are smaller (advantageous inequality). Empirically, the authors find that usually the former inequality leads to larger utility losses. Under certain parameter constellations, it is straightforward to show that observers might reduce a dictator's payoff in order to minimize disadvantageous inequality towards the dictator. The Bolton and Ockenfels (2000) model assumes that observer's utility is influenced by his own material payoff and the own relative share of the total payoffs. If the observer's payoff is lower than the relative share she experiences a disutility. Irrespective of what the dictator leaves for the recipient in our setup the initial share of the observer is always 1/3 (50 is endowment of the observer / 150 is the sum of all endowments). Thus, assuming this type of preferences we would not expect any punishment to occur. In addition, models of pure reciprocity (Dufwenberg and Kirchsteiger, 2004; Rabin, 1993) (which assume that observers punishment choice is driven by negative intention) do not predict any altruistic punishment because the dictator's action is not affecting the observer directly.

unclear what the reference point is.[13] One natural candidate for a reference point in the present experiment would be the status-quo, i.e., the endowments given to our participants. In that case, for example, taking 100 taler from the recipient might be perceived as more blameworthy than giving 0 taler. For this to occur, however, one has to additionally assume that 1) observers take the changes in payoffs of recipients and dictators into account, i.e., they have social-preferences, and 2) that observers perceive the losses of recipients more negatively than similarly-sized gains. In the social psychology literature there is a discussion on how acts of commission are perceived differently compared to acts of omissions. One main result from this research is that harmful acts of commission are judged as more immoral than harmful omissions (Spranca et al., 1991). With respect to our experiment, assuming that giving 0 taler is considered as an omission and taking 100 taler is a commission this reasoning would lead to similar predictions.

Hypothesis 2 (Framing): In the *Take* treatments we observe more moralistic punishment than in the *Give* treatments.

Effects of publicity on pro-social behavior are usually explained with signaling models (Bénabou and Tirole, 2002; Andreoni and Bernheim, 2009; Grossman and van der Weele, 2013).[14] Numerous studies show that individuals behave more pro-socially if their actions are made public (see also Schram and Charness, 2011; Charness et al., 2007). In a meta study, Engel (2011) documents that dictators allocate money more equally when their identity is disclosed ex-post.[15] Two factors make the study at hand qualitatively different from the existing research. First, our study deals with costly punishment and sanctioning behavior and not with pro-social behavior. Second, in most of these studies dictators announced their decision in front of a group of participants including recipients and other dictators. Several factors, such as

[13] In fact, there is a lively debate on what constitutes the reference point in decisions under risk, while some scholars argue that the status-quo might be one candidate others assume that individuals expectations can also represent a reference point (Kőszegi and Rabin, 2006).

[14] Social psychologists usually refer to this behavior and motivation as "self-representation" or "self-image" (see, for example, Kurzban and Aktipis, 2006).

[15] Even when dictators just have the feeling of being observed their pro-sociality altered. Nettle et al. (2013) review studies on dictator game giving when subjects have the feeling of being observed during their decision-making. A majority of studies show that dictators give more when they have the feeling of being observed.

group-pressure or norm conformity, may have led to the observed differences in behavior. Thus, prima facie, it is unclear whether observed pro-sociality might be due to signaling pro-social behavior to recipients or conforming to a social norm of other dictators. In our experiment the differences between *Public* and *Private* are more clearly defined. The only variable that changes between the treatments is whether the person affected by a a norm violation is informed or not, while the dictator is always informed. We hypothesize that the positive effect of publicity on pro-social behavior is mainly driven by the fact that victims or beneficiaries of ones action see it. Thus, we come to our third hypothesis on moralistic punishment:

> ***Hypothesis 3 (Publicity):*** In the *Public* treatments we observe more moralistic punishment than in the *Private* treatments.

5.3 Results

The presentation of our results starts with an analysis of observers' punishment decisions and is followed by a closer look into recipients' expectations about punishment and will end with an analysis of dictators' behavior.

Since we elicited observers punishment decision with the strategy method we start our analysis by taking the average amount of punishment for all three dictators choices for each observer. The results of this calculation can be found in Table 5.1. We see that observers reduce dictators payoffs on average by 18.52 taler in the *Give-Public* treatment compared to only 9.48 taler in the *Take-Public* treatment (Mann-Whitney U test henceforth MWU test, p=.0136).[16] In the *Private* treatments, we do not find a similar effect of framing. In *Give-Private* dictators payoffs are reduced by 8.78 taler compared to 11.13 in *Take-Private* (p=.3762, MWU test). In *Give-Private,* the punishment by observers is significantly lower compared to the *Give-Public* treatment (p=.0292, MWU test). We find no significant difference when comparing *Take-Private* and *Take-Public* (p=.2466, MWU test).

After having compared the average levels of punishment we now focus in more detail on the punishment assigned for each dictators action. In Figure

[16] If not mentioned otherwise for all non-parametric statistical comparisons we report one-sided p-Values.

Table 5.1: Average received punishment

Treatment	Average received punishment (in taler)			
	Public		*Private*	*Framing* (pooled)
Give	18.52 (1.3)	$>^{**}$	8.78 (.87)	13.65 (1.09)
	$\vee^{**}(^{\dagger\dagger})$		$\wedge$	$\vee(^{***})$
Take	9.48 (.65)	$<$	11.13 (1.1)	10.3 (.89)
Publicity (pooled)	14 (.98)	$>$	9.96 (1)	11.98

Notes: Average punishment (left panel) and recipients payoff across treatments. In brackets we display the average number of instances (between 0 to 3) of observers punish. Stars (Crosses) display significance levels of a one-sided Mann-Whitney U test (χ^2-test). $^{***}(^{\dagger\dagger\dagger})$ Significance at $p<.01$, $^{**}(^{\dagger\dagger})$ Significance at $p<.05$, $^{*}(^{\dagger})$ Significance at $p<.1$.

5.1 we show the average level of observers' punishment, i.e., for each recipient's payoff the amount deducted from the dictator's payoff. In the left panel we depict the data for the *Public* treatments, while in the right panel one can find the results from the *Private* treatments; dashed lines show the *Give* frame, and solid lines the *Take* frame. We see that moralistic punishment decreases in the payoff of the recipient.[17]

We find that the stated differences from above mainly result from differences in punishment levels for the most selfish action of the dictator, i.e., whether she gives nothing or takes everything. Observers reduce dictators payoffs on average by 35.35 taler in *Give-Public* compared to only 17.48 taler in *Take-Public*, when dictators leave nothing for the recipient (p=.0117, MWU test). In the *Private* treatments, observers reduce dictators payment by 16.69 taler under the *Give* frame, and 18.39 taler under the *Take* frame when recipients receive nothing (p=.3886, MWU test). In fact, observers seem to significantly reduce their punishment in *Give-Private* compared to *Give-Public* (p=.0163, MWU test). No such difference is observed in the *Take* treatments (p=.267, MWU test).[18]

[17] Coefficients of GLS regressions with robust standard errors clustering on the individual level explaining punishment with recipients payoff (and a constant) separately for each treatment are all negative and highly significant with at least -.27 magnitude. Thus, if the dictator behaves more generously, i.e., the recipient's payoff increases, moralistic punishment decreases.

[18] An analysis for the other transfer levels can be found in Table 5.4 in the appendix. Further regression analyzes controlling for gender, study major and age in Table 5.5

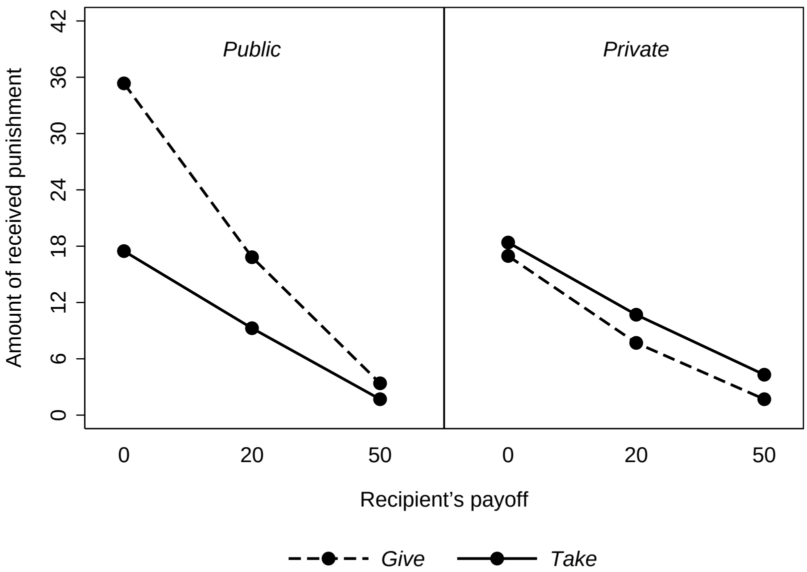


Figure 5.1: Average amount of received punishment

In addition to the amount of punishment, we are able to analyze how often observers punish. We do this by counting for every observer the instances in which observers choose to punish the dictator for the three possible cases. We display these average figures in brackets of the of Table 5.1. The statistical analysis reveals that observers punish the dictator on average 1.3 times *Give-Public*. This constitutes a statistically significant difference compared *Take-Public* (.65%) (p=.036, χ^2-test). Hence, we state our first observation:

Observation 1: Observers punish dictators more and more severely when dictators behave selfishly in *Give* compared to *Take*. When recipients are not informed, in *Private*, about punishment, no such framing effect is observed.

Surprisingly, this partially contradicts our initial hypothesis that losses of others weigh more than equal-sized gains of others and thus lead to more punishment. More evidence on this rather surprising punishment pattern comes from the analysis of recipients expectations about observers' punishment. When observers and dictators make their choices we elicit recipients' expectations by asking them for every dictators action how much the observer

in the appendix confirm this observations.

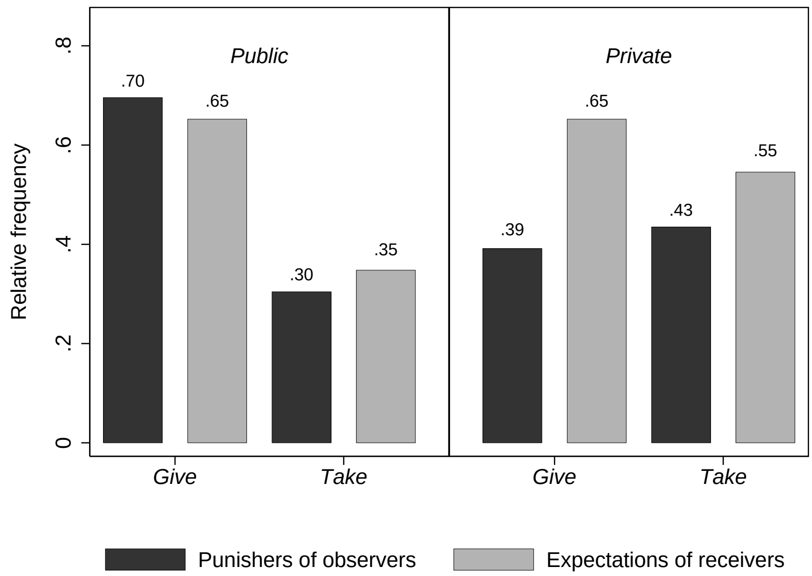


Figure 5.2: Relative frequencies of punishers (black bars) among observers and expected number of punishers (gray bars) for the most selfish action of the dictator among recipients.

would punish. The analysis of their expectations might be especially insightful, because recipients cannot decide in the dictator game. Thus, their expectations are not confounded with prior decisions. In Figure 2, the relative frequency of recipients who think that observers will punish the dictator for the setting the recipients payoff to zero (gray bars) is depicted.[19]

For comparability reasons we display the actual number of observers who punish in black bars.[20] First, we observe that recipients anticipate the treatment effects reported above: Recipients expect that more observers punish under the *Give* than under the *Take* frame, when punishment will be made public (p=.039, χ^2-test). This is not the case when comparing recipients' expectations between *Give* and *Take* frame in the *Private* treatments (p=.465,

[19]For this analysis we focus on the expectations for the lowest recipients' payoff because the analysis of actual punishment behavior showed that our treatment differences are mainly driven by the difference in the lowest recipients payoff. We calculate this frequencies by counting the number of recipients that expect the observer to exert a positive punishment level. An analysis of recipients' expectations for the other payoffs can be found in Table 5.4 in the appendix.

[20]In a comparable experiment by Fehr and Fischbacher (2004) "At each transfer level below 50 (taler) roughly 60% (n=22) of players C [observers] choose to punish the dictator" (p.68). It should be noted, that their experiment is only comparable to our *Give-Public* treatment.

χ^2-test). Second, recipients' expectations and observers' punishment choices are not statistically different in the *Public* treatments (*Give*: p=.753, *Take*: p=.753, χ^2-test), whereas recipients expect that more observer will punish in the *Private* treatment (*Give*: p=.077, *Take*, p=.458, χ^2-test). It seems that recipients correctly anticipate the effect of framing on punishment, , but tend to slightly overestimate observers' likelihood to punish in *Give-Private.* [21]

Observation 2: Recipients expect observers to punish more in *Give* than in *Take.*

Although it is not the main focus of this paper, we also provide evidence of how dictators' behavior might be affected by framing and publicity. Do dictators anticipate the observed punishment patterns and try to circumvent punishment by being more generous? Table 5.2 shows dictators behavior, i.e., the average payoff she left for the recipient in our experiment. A look at the Table 5.2 shows that recipient's payoffs tend to be higher in the *Public* treatments compared to the *Private* treatments. When punishment is public, recipient's payoffs in the *Give* (*Take*) frame are on average 19.13 (23.91) talers whereas when punishment remains private, recipients in *Give* (*Take*) receive 12.61 (6.96) talers on average. A statistical analysis, however, reveals that this tendency is only significant for a comparison between *Take-Private* and *Take-Public* (p=.019, χ^2-test). A corresponding analysis for the *Give* frame yields no statistically significant difference (p=.312, χ^2-test).[22] A supplementary regression analysis in Table 5.6 in the appendix, where we control for other observables (Sex, Age and Major), confirms this observation.

Observation 3: Dictators behave more generous in *Public* than in *Private.*

Our final observation is made by a comparison of average total payoffs, i.e., a measure for efficiency realized by every three player group. For every dictator-

[21] An analysis of dictators expectations, although they are confounded by their own prior decisions, tend to underline this observation but remain insignificant. For the most selfish action in *Public* more dicators think that observers will punish in *Give* (.70) than in *Take* (.65) (p=.753, χ^2-test). When punishment is *Private* in *Give* a fraction of .61 thinks observers will punish, whilst this figure is .52 for *Take* (p=.552, χ^2-test).

[22] If we pool the data from *Give* and *Take* treatments we find, however, that payoffs for recipients are significantly higher in the *Public* than in the *Private* treatments (p=.008, χ^2-test)

Table 5.2: Average recipients payoffs

Treatment	Average recipients payoffs (in taler)			
	Public		*Private*	*Framing* (pooled)
Give	19.13	>	12.61	15.87
	$\wedge^{\dagger}$		$\vee^{\dagger\dagger}$	$\vee^{\dagger\dagger\dagger}$
Take	23.91	$<^{\dagger\dagger}$	6.96	15.43
Publicity (pooled)	21.52	$>^{\dagger\dagger\dagger}$	9.78	15.65

Notes: Crosses display significance levels of a one-sided χ^2-test.
††† Significance at p<.01, †† Significance at p<.05, † Significance at p<.1.

observer-recipient group we sum up the payoffs (in taler) that they received at the end of the experiment (excluding the show-up fee of 2.5€which has been paid out to every participant). If no punishment would have been exerted the highest total payoff of a group would have been 150. A reduction of one taler of dictators payoff, however, reduces the total payoff by 4/3 taler.

As the observations from above and the corresponding figures from Table 5.3 suggest on average the maximally efficient outcome is not reached. In fact, four Wilcoxon signed rank tests testing the average total payoff against 150 confirm that the average total payoff in every treatment is significantly different from the maximally reachable level (*Give-Public*: p=.0011, *Give-Private*: p=.0018, *Take-Public*: p=.0257, *Take-Private*: p=.0029).

Considering efficiency for each treatment shows that the highest level of efficiency is reached in *Take-Public*. Here average total payoffs are by 14 talers higher compared to *Give-Public* (p=.0809, MWU test, two-sided). A similar comparison for the *Private* treatments tend to go in the same direction but remains insignificant (p=.8439, two-sided).

Observation 4: Average total payoffs tend to be higher in *Take* treatments compared to the *Give* treatments.

Table 5.3: Average total payoffs

Treatment	Average total payoffs (in taler)			
	Public		*Private*	*Framing* (pooled)
Give	124.96	<	129.83	127.39
	∧*		∧	∧
Take	138.69	>	132.45	135.57
Publicity (pooled)	131.83	>	131.13	131.48

Notes: Stars display significance levels of a two-sided MWU test. *** Significance at $p<.01$, ** Significance at $p<.05$, * Significance at $p<.1$.

5.4 Discussion and concluding remarks

This paper provides evidence that framing of unfair behavior and publicity of punishment affects individuals' propensity to punish unfairness. We set up a modified version of the dictator game with moralistic punishment, in which observers do not have and cannot expect any material benefit from disciplining unfair dictators.

We find that they are more likely to punish selfish behavior in the treatment where dictators give nothing, compared the treatment where dictators take everything, although both actions lead to similar payoff consequences for the victim of unfair behavior. Additionally, we provide evidence that punishment decreases substantially when recipients will not be informed about the observers punishment choice.

In line with existing studies, we find that a large fraction of observers is willing to punish unfair dictators. However, as reviewed in the beginning, most studies employing this paradigm considered a dictator game solely under a give-frame and a scenario where the recipient is informed about the observer's punishment choice of the observer. When transforming the game in a take-frame, keeping the consequences of dictators actions constant, and vary the information the recipient receives the prevalence of moralistic punishment changes drastically. While the effects of publicity on moralistic punishment confirm evidence from the existing literature the effect of framing seems rather surprising. What are potential reasons for the observed effects of framing on punishment? Why is taking large amounts from the recipient punished less compared to giving low amounts to the recipient, although it leads to the same payoff consequences?

One explanation might result from the distribution of initial endowment,s that changes across the framing conditions. In the *Give* treatment each dictator has 100 talers and the recipient has 0 talers; while it is exactly the opposite in the *Take* treatment. In the *Give* treatment, observers may perceive the dictator as a relatively wealthy individual who behaves selfish versus the perception of a relatively poor individual who behaves selfishly in the *Take* treatment. Thus, selfish behavior of a "poor" individual might lead to lower condemnation than selfish behavior of a "rich" individual. This argument might be strengthened by the fact that endowments, i.e., roles in our experiment, were allocated randomly. Observer's might think that a dictator with 100 talers might was very fortunate, whereas a dictator with 0 talers was very unfortunate. Being fortunate might come with greater responsibility and thus a higher obligation to share the talers with the recipient. In the literature on charitable giving, for example, we find that the acknowledgment of being fortunate is a strong driver of individual giving (Hibbert and Home, 1996). Thus, observers who think that dictators have been very lucky in receiving the higher endowment might think that this goes hand in hand with an obligation to give, and violating this obligation triggers punishment more than compensating the bad luck by being selfish when having the lower endowment.

An alternative explanation for this framing effect could be found in the view that punishment is a way to "choose sides" (DeScioli and Kurzban, 2013) in conflicts. These scholars argue that punishment is a tool for observers to show support for one side in in disputes. This means that an observer who punishes the dictator signals to the recipient that she is on the recipient's side. An observer that does not punish in our paradigm could be interpreted as choosing the side of the dictator. The different initial endowments of dictators and recipients in our experiment might create different feelings of closeness towards the other players. While in the *Give* treatment observers can support the recipient with the lower endowment by punishing the dictator, in the *Take* treatment some observers might choose to support the lower endowed dictator by not punishing him.[23]

[23]Interestingly, in a rather different setup Cubitt et al. (2011) find that free-riding in a social dilemma is evaluated as worse when the social-dilemma is played under a Give frame

Although, decision problems outside the laboratory are often more complex than those presented in the study above our work has several applications. Think, for example, of a bureaucrat who decides on which company will receive a public order to build a hospital. The bureaucrat's management is observed by another colleague. An unfair action, for example given the order to a company of the bureaucrat's wife, although it leads to the same consequences could be perceived differently and thus lead to different reaction of bystanders. One can think of the bureaucrat's behavior as that he is taking something from the common good (*Take* treatment), or that he fails to give the proper treatment to a company that deserves it (*Give* treatment). This bystanders reaction, however, might also depend whether the victim of the bureaucrats action will ever be informed about his courageous act (*Public* treatment) or not (*Private* treatment). In general it might be worthwhile for designers of organization to consider these effects when designing compliance policies or whistle-blowing systems. They might be well advised to stress that potential wrongdoers fail to give something to the company in order to elicit stronger reactions of whistle-blowers.

Although framing and publicity in strategic interactions have been fairly neglected in economic theory it might be an interesting avenue for future research to enrich these models to incorporate these effects. The results suggests that bystanders or third-parties do not only care about the unfair treatment of others but also how this unfairness emerges.

compared to a strategically equivalent social dilemma under a Take frame.

5.5 Appendix

Tables and Figures

Table 5.4: Average received punishment and recipients expectations about punishment

	Punishment of observers			Expectations of recipients		
	Recipients payoff 0					
Treatment	*Public*		*Private*	*Public*		*Private*
Give	35.35 (.7)	$>^{**}$ (††)	16.96 (.39)	24 (.65)	$>$	19.7 (.65)
	$\vee^{**}$(†††)		$\wedge$	$\vee^{*}$(††)		$\wedge$
Take	17.48 (.3)	$<$	18.39 (.43)	11.48 (.35)	$<^{*}$	32.45 (.55)
	Recipients payoff 20					
Treatment	*Public*		*Private*	*Public*		*Private*
Give	16.83 (.52)	$>^{*}$	7.7 (.39)	14.48 (.57)	$>$	8.74 (.52)
	$\vee$		$\wedge$	$\vee$(†)		$\wedge$
Take	9.26 (.30)	$<$	10.7 (.43)	10.57 (.3)	$<$	19.36 (.45)
	Recipients payoff 50					
Treatment	*Public*		*Private*	*Public*		*Private*
Give	3.39 (.09)	$>$	1.7 (.09)	3.91 (.13)	$>^{*}$ (†)	0 (0)
	$\vee$		$\wedge$	$\vee$		$\wedge^{***}$(††)
Take	1.7 (.04)	$<^{**}$	4.3 (.26)	3.52 (.22)	$<$	6.41 (.27)

Notes: Average received punishment of observers and recipients expectations about punishment across treatments for each recipients payoff. In brackets we display the relative frequencies of observers who punish. For recipients we show the relative frequencies who expect the observers to punish. Stars (Daggers) display significance levels of a Mann-Whitney U test (χ^2-test). ***(†††) $p<.01$,**(††) $p<.05$, *(†) $p<.1$.

Table 5.5: Explaining received punishment

	Dependent variable: Received punishment			
Independent variables:	(1)	(2)	(3)	(4)
Payoff recipient 0	31.96***	15.26***	15.26***	14.09***
	(7.14)	(5.82)	(5.82)	(4.84)
Payoff recipient 20	13.43***	6**	6**	6.391**
	(4.31)	(2.51)	(2.51)	(3.16)
1 if *Take*	-2.064	3.120		
	(3.80)	(3.29)		
Take x payoff recipient 0	-16.17*	-1.174		
	(9.50)	(7.57)		
Take x payoff recipient 20	-5.870	0.391		
	(5.20)	(4.04)		
1 if *Public*			0.200	-2.223
			(5.13)	(2.95)
Public x payoff recipient 0			16.70*	1.696
			(9.21)	(7.92)
Public x payoff recipient 20			7.435	1.174
			(4.99)	(4.30)
Constant	21.12	-18.16	15.68	-10.90
	(16.90)	(11.42)	(16.12)	(12.25)
Observations	138	138	138	138
R^2	.222	.12	.218	.104
Treatments	Public	Private	Give	Take
Controls	Yes	Yes	Yes	Yes

Notes: GLS regression with robust standard errors in parentheses (clustered on individuals). Controls variables include dummy variables for gender and whether the subject is majoring in economics/business and a variable for subjects' age. Significance tests are two-sided.
*** Significance at $p<.01$, ** Significance at $p<.05$, * Significance at $p<.1$.

This panel regression analysis further elaborates on observation 1. In models (1) - (4) of Table 5.5 we explain the punishment levels. The models (1) and (2) recapitulate the effects of framing, and model (3) and (4) of publicity on punishment. In every model we include dummy variables for the three potential recipient payoffs, where a payoff of 50 taler is the reference category. The positive and significant coefficients of "Payoff recipient 0" and "Payoff recipient 20" reveal that punishment is higher for lower transfers to the recipient.

To uncover the effects of framing on punishment for the different payoff-levels, we include interaction terms and interact the framing dummy "1 if *Take*" with the different payoff levels for Public (model 1) and Private (model 2), and include additional control variables. In line with the non-parametric analysis above, we observe a negative and weakly significant coefficient for the interaction term "*Take* x payoff recipient 0", indicating that punishment is lower for a payoff of zero in the *Take* treatment compared to the *Give* treatment. A similar analysis for the *Private* treatment yields no significant differences of framing on punishment and thus confirms the results from the non-parametric analysis discussed above. Interestingly, in model (1) the negative and weakly significant (p=.093) coefficient for the dummy variable "1 if female" indicates that women tend to punish less altruistically in the *Public* treatments. In all other models the control variables remain insignificant.

The second set of regressions explains punishment with publicity (models 3 and 4). Again we interact the dummy "1 if *Public*" with the different payoff levels for *Give* (model 3) and *Take* (model 4) separately and include additional controls. For the *Give* treatments we observe that punishment for the lowest payoff is higher when the recipient is informed. Although the coefficients seem to go into the predicted direction, we do not find evidence for a similar effect of publicity on punishment in the *Take* treatments (model 4).

Table 5.6: Explaining recipients payoff

	Dependent variable: Recipients payoff		
Independent variables:	(1)	(2)	(3)
1 if *Take*	-0.0774	-2.165**	-2.112***
	(0.63)	(0.87)	(0.76)
Take x Public			2.055**
			(0.97)
Observations	46	46	92
$Pseudo - R^2$	.14	.16	.19
Audience	Public	Private	pooled
Controls	Yes	Yes	Yes

Notes: Coefficients of an ordered logit regression with robust standard errors in parentheses. Control variables include dummy variables for gender and whether the subject is majoring in economics/business and a variable for subjects' age. *** Significance at p<.01, ** Significance at p<.05, * Significance at p<.1.

Experimental instructions (translated from German)

General explanations

Thank you for your participation in the experiment. For showing up at the experiment you receive a benefit of 2,50 €. During the experiment you are able to earn additional money. Therefore, it is important to read the instructions carefully.

It is very important that you do not talk to others during the experiment. In addition to that, we ask you not to use your smart phone or mobile phone. A contravention to those rules results in an exclusion of the experiment and all payments.

During the experiment we talk about Taler instead of Euros. Your entire income will be primarily charged in Taler and afterward converted into Euro and then will be paid in addition to the benefit. Thereby, we use the following conversion rate:

1 Taler = 0,1 €

You will be randomly assigned to two other group members. During the whole experiment you will exclusively interact with those two participants. Neither you nor they will get to know the identity of the other group members, at no point of the session. There are three different types of participants: A, B and C. At the beginning of the experiment the type of participant you will be will be **randomly** assigned and it will be shown on your monitor. The whole experiment only takes one round which means that you only have to make a decision once.

Before the participants make their decision, each one will receive an endowment.

- Participant A receives an endowment of {*Take*:0}{*Give:*100} Taler.
- Participant B receives an endowment of 50 Taler.
- Participant C receives an endowment of {*Take*:100}{*Give*:0} Taler.

Decision of participant A

After every participant got to know whether he will be participant A, B or C, participant A starts. He decides which share of {*Take*: C's endowment he wants to deduct. He has three opportunities: He can either take 100, 80 or 50 Taler from C. The amount of Taler that he takes from C will be deducted from C's endowment (100 Taler) and added to A's own endowment (0 Taler).}{*Give*: his own endowment he wants to give to participant C. He has three opportunities: He can either give 0, 20 or 50 Taler to participant C. The amount of Taler that he will give to participant C will be deducted from A´s own endowment and added to participant C´s endowment}. Afterward, it is participant B's turn.

Decision of participant B

Participant B can decide whether he wants to deduct Taler from participant A or not. Hereby participant B can decide whether he wants to deduct 0, 3, 6, 9, 12, 15, 18, 21, 24... or 99 Taler from participant A. Therefore, the deduction of Taler is only possible in steps of 3. The costs for the deduction of 3 Taler for participant B are 1 Taler, for 6 Taler the costs are 2 Taler and so forth.

- Example 1: If participant B wants to deduct 15 Taler from participant A, participant B's endowment will be reduced by 5 Taler and participant A's payoff will be reduced by 15 Taler.
- Example 2: If participant B wants to deduct 48 Taler from participant A, participant B's endowment will be reduced by 16 Taler and participant A's payoff will be reduced by 48 Taler.

Please note that it is only possible for participant B to deduct a maximum of Taler so that participant A receives at least 0 Taler at the end of the experiment. Hence, experiments´ participants cannot make any losses. Participant B makes his decision before getting informed about participant A´s actual decision. Thus, participant B makes a decision how many Taler he would potentially deduct from participant A for each possible amount {*Take*: that participant A could take from participant C}{*Give*: that participant A could give to participant C} Participant B also makes a decision, whether he would like to inform participant C about his decision. After participant B has made his decisions about the Taler deduction for all of participant A´s three possible decisions, these will be compared to the actual decisions made and the payment resulting from the one actual decision will be determined. The ta-

Decision	Participant A {*Take*: takes from} /{*Give*: gives to} participant C	Participant C receives	Participant A receives	How many taler do you want to deduct from participant A?
1	100 / 0	0	100	
2	80 / 20	20	80	
3	50 /50	50	50	

Notes: In the left part of the table you can see all of the three possible decisions participant A can make and the resulting payoffs for C and A. Participant B can fill in the amount of Taler he wants to deduct from participant A for each possible decision in the right column.

ble shows all possible decisions of Participant A as well as an example of the decision monitor of participant B:

Decision of participant C

Participant C cannot make any decisions in this experiment. {Take: He receives his endowment of 100 Taler minus the amount of Taler participant A has taken from him.} {Give: He receives his endowment of 0 Taler plus the amount of Taler participant A has given to him.}

Who will find out about the participant B´s decision to deduct the Taler?

{*Public*: Participant A and C will be told about the participant B´s decision to deduct Taler from participant A. Thus, participant C will find out how participant B has decided at the end of the experiment.}{*Private*: Participant A will be told about the participant B´s decision to deduct Taler from participant A, however, participant C will not be informed about it. Thus, participant C will at no point of time find out about participant B´s decision.}

How are the payments determined?

Participant A receives his endowment of {*Take*: 0 Taler plus the amount of Taler that he has taken from participant C}{*Give*: 100 Taler minus the amount of Taler that he has given participant C} minus the amount of Taler that were deducted by B. Expressed in a formula: Payoff participant A = {*Take*: 0 Taler +}{*Give*: 100 Taler -} Taler of C - Taler deduction by B

Participant B receives his endowment of 50 Taler minus the amount of Taler he had to spend in order to deduct Taler from participant A. Please note the costs of deducting 3 Taler are 1 Taler for participant B etc.

Expressed in a formula: Payoff participant B = 50 Taler – (Taler deduction/3)

Participant C receives his endowment of {*Take*: 100 Taler minus the amount of Taler participant A has deducted from him}{*Give*: 0 Taler plus the amount of Taler participant A has given to him} Expressed in a formula: Payoff participant C = {*Take*: 100 Taler – Taler to A}{*Give*: 0 Taler + Taler from A}.

Once all participants will have made their decisions, we kindly ask you to fill in a questionnaire. At the end of the experiment, the Taler converted to Euros as described above in addition to the benefit will be disbursed. Do you have any questions? If yes, please raise your hand. We will come to your place. If you do not have any questions, please answer the following comprehension questions.

Control questions (translated from German)

1. Participant A {*Take*: takes 50 taler from}{*Give*: gives 50 taler to} participant C. Participant B decides to reduce participants A's payoff by 15 taler.

2. Participant A {*Take*: takes 100 taler from}{*Give*: gives 0 taler to} participant C. Participant B decides to reduce participants A's payoff by 45 taler.

3. Participant A {*Take*: takes 80 taler from}{*Give*: gives 20 taler to} participant C. Participant B decides to reduce participants A's payoff by 0 taler.

For each control question subjects have been asked to calculate the payoffs of participant A, B and C after the described behavior.

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Publications

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